

part of a flame, must have great influence over the combinations there taking place" (656). The last sentence is interesting in connection with the work of Prof. Bone on surface combustion.

It is, perhaps, rather to be wondered at, since Faraday had gone so far in the interpretation of the phenomena, that he did not take the further step and bring them into relation with surface tension, to which Thomas Young had already directed attention.

In the application of these facts to the theory of enzyme action, the view was first definitely put forward by myself, so far as I know, in a paper in the *Biochemical Journal*, vol. i. (1906), pp. 222-27, that the "combination" between an enzyme and its substrate is of the nature of an adsorption. This view has received more and more support from work done by various investigators since that time. To mention one fact only, it has been found in the cases of several different enzymes that their activity is exercised in liquids in which they are completely insoluble, so that it must be the surface of the particles which is concerned. We know also that, in water, enzymes in general are in the colloidal state, a state in which chemical reactions obey laws which interfere with that of mass action in its simple form.

It is very doubtful whether intermediate compounds of a chemical nature play any part in catalysis by enzymes. None, at all events, have been shown to exist. Moreover, such an explanation is of very rare application to catalysis of any kind. The hypothesis of the action of enzymes which is most in agreement with all the facts known at present may be stated somewhat as follows: the molecules which are to enter into reaction are condensed by adsorption on the surface of the colloidal particles of the enzyme and their final state of equilibrium is brought about at a greatly accelerated rate. Whether, as Faraday seems to hold, the close approximation, and high concentration, is in itself sufficient to account, by mass action, for the increased rate of reaction is a matter for future investigation. It may well be, as Hardy points out (*Proc. Roy. Soc.*, vol. lxxxviii. B, pp. 174 and 175), that it is in the actual process of condensation itself that the molecules are subject to stresses which result in exceptional chemical activity; their chemical potential may very well be raised in the process. It appears to be a phenomenon of very general occurrence that it is in the very act of change of state that special activities are manifested. This is particularly obvious in living organisms, where a system in equilibrium is dead, but it applies also to non-vital systems.

I would finally point out that it should not be stated that the action of enzymes does not obey mass action. Mass action is universal in its application; but, in heterogeneous systems it is controlled by other factors, such as diffusion and surface adsorption, the latter factor playing the chief part in the velocity of reaction in micro-heterogeneous systems, such as those of colloids. The rate of the reaction is conditioned by the relative masses of the molecules condensed on the surface at any one moment of time. It will be seen that the difficulty of applying the law of mass action consists in the determination of the real active masses.

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#### Tidal Friction and Ice Ages.

A CAREFUL study of the conditions of land height during the earlier stages of the Quaternary glacial period seems to show that the earth was then less oblate, *i.e.* the north and south polar regions stood higher than now, to the extent of as much as 10,000 ft. in places, while the equatorial regions stood lower, by

some 500 ft. The spreading of ice-sheets from these high lands may well have been the initial cause of the cooling which produced the Glacial epoch.

Going further back in geological time, we find, as is well known, a series of long periods of epeiro-genetic movement alternating with long periods of marine transgression. The last great change seems to have been continuous from Carboniferous times, with a girdle of land round the equator high enough to be heavily glaciated, through Mesozoic times, with a marine transgression, to Pliocene times, when the high land emerged at the poles.

Qualitatively, at least, it seems possible to read the late Sir G. H. Darwin's theory of tidal friction into these changes. By that theory, owing to the differential attraction of the moon on the tidal protuberances, the rotational momentum of the earth is gradually decreasing. This decrease may manifest itself in two ways, either by an actual decrease in the rotational velocity, or by a decrease in oblateness, *i.e.* by a movement of matter towards the poles.

It is probable that the earth's crust has a certain tendency to slide on its nucleus, and since the effect of tidal friction is chiefly felt in the crust, while the bulk of the momentum must lie in the heavy nucleus, it follows that the effect of tidal friction must be to tend to slide the crust round the nucleus parallel with the equator. As the crust must vary both in thickness and in the closeness with which it is attached to the nucleus, this means a thrust against the deeper and more closely attached portions. The thrust would tend to force some of the crust poleward on either side from the equator, *i.e.* to decrease the equatorial bulge.

But the friction between crust and nucleus is very great, and must in time result in an appreciable decrease in the angular velocity of the latter. Probably the action takes place in alternating steps—periods of constant rotational velocity, combined with a gradual thrusting of the crust poleward from the equator, alternating with periods of stability of the crust and gradual decrease in the rotational velocity. The former are periods of earth movement, mountain forming, and disturbance, resulting in the gradual deepening of the ocean over the equator and emergence at the poles; the latter are associated with a slow retirement of the ocean towards the poles, resulting in marine transgressions in middle and higher latitudes.

I have been able to accumulate a great deal of evidence which supports the theory I have outlined above on the geological side. I am, however, not sufficiently a mathematician to be able to satisfy myself that the cause, tidal friction, is commensurable with the effects, and I am begging the publicity of your columns to ask if someone better situated will help me in that respect.

C. E. P. BROOKS.

"Homeleigh," 3 Roseleigh Avenue,  
Highbury, N., October 10.

At the Editor's request I contribute a few remarks on Mr. Brooks's letter. The suggestion that tidal friction might be a cause of changes in the distribution of land and water is not new. It will be found in a "Note" in *NATURE* of April 25, 1889 (vol. xxxix., p. 613), where it is attributed to M. A. Blytt; and that may not be its first appearance. The character of the changes is that indicated by Mr. Brooks, but the mechanism by which they are effected is a little different. The hypothesis of a crust riding more or less freely on a nucleus is unnecessary, and difficult to reconcile with well-established results. Again, the frictional stresses do not operate directly to cause a flow of material towards or away from the poles, but indirectly by diminishing the speed of rotation.

The mechanical process may be followed very easily

without any mathematics. The surface of the ocean, apart from waves and tides, is at any time a figure of equilibrium answering to the speed of rotation at the time, more oblate when the speed is greater, less oblate when it is slower. Let us imagine that the lithosphere also is at some time a figure of equilibrium answering to the speed of rotation at that time. If the speed remained constant, the lithosphere would retain this figure, and the matter within it would remain always in the same configuration without having to support any internal tangential stress. Now suppose that the speed of rotation gradually diminishes. The surface of the ocean will gradually become less and less oblate. The lithosphere also will gradually become less oblate, but not to such an extent as to make it a figure of equilibrium answering to the diminished speed of rotation, while the matter within it will get into a state of gradually increasing internal tangential stress. The effect on the distribution of land and water will be that the depth of the ocean will gradually diminish in lower latitudes and increase in higher latitudes, the latitudes of no change being  $35^{\circ} 16' N.$  and  $S.$

The internal tangential stress in the matter within the lithosphere may increase so much that it can no longer be supported. If this happens a series of local fractures will take place, continuing until the lithosphere is again adjusted much more nearly to a figure of equilibrium, which will be less oblate than the original figure. The effect on the distribution of land and water will be that the depth of the ocean will increase rather rapidly and spasmodically in lower latitudes and diminish in higher latitudes.

Accordingly the kind of geological change which the theory of tidal friction would lead us to expect is a sort of rhythmic sequence, involving long periods of comparative quiescence, marked by what Suess calls "positive movements of the strand," in the higher latitudes, and "negative movements" in the lower, alternating with comparatively short periods of greater activity, marked by rise of the land around the poles and subsidences in the equatorial regions. It is for geologists to say whether the facts known to them are consistent with this description or not. A. E. H. LOVE.

### The Age of a Herring.

IN the issue of NATURE for September 17 Prof. D'Arcy Thompson states that he is unable to persuade himself of the validity of Dr. Hjort's conclusions based upon the methods of determining the age of herrings by a study of their scale-rings.

It is, of course, impossible to attempt to deal in a few words with all the evidence brought forward in favour of these methods in recent years by different biologists, and we must, with regard to the herring, refer to our published papers,<sup>1</sup> where arguments are given in favour of the primary assumption that the age of a herring may be determined by counting the rings seen on its scales. The facts supporting this assumption are briefly:—

(1) For young individuals (up to age of three years) the results of age determinations by means of the scale-rings correspond with the results obtained by

<sup>1</sup> Hjort, "Report on Herring Investigations until January, 1910," *Publ. de Circonst.*, No. 53. Copenhagen, 1910.

Hjort and Lea, "Some Results of the Internat. Herring Inv., 1907-11," *Publ. de Circonst.*, No. 61. Copenhagen, 1911.

Hjort, "Fluctuations in the Great Fisheries of Northern Europe," *Rapports et Procès-Verbaux*, vol. xx. Copenhagen, 1914.

Lea, "On the Methods used in the Herring Investigations," *Publ. de Circonst.*, No. 53. Copenhagen, 1910.

Lea, "A Study on the Growth of Herrings," *Publ. de Circonst.*, No. 61. Copenhagen, 1911.

Lea, "Further Studies concerning the Methods of calculating the Growth of Herrings," *Publ. de Circonst.*, No. 66. Copenhagen, 1913.

plotting frequency curves for the length measurements of the individuals.

(2) Scale examination of small herrings continued with short intervals during all seasons showed that the formation of the so-called winter rings took place during the winter, while the formation of the so-called summer belts commenced in the spring and continued during the summer months. That the summer belt is small at the commencement of the formation in May, while it is large on the completion of the formation in the beginning of autumn, has been proved by observations carried on during four years. Regarding the older fish, it has been difficult to proceed in the same manner as for younger fish, as the frequency curves fail to give any hints as to the age groups represented in a sample, while, on the other hand, the fishing season for the old herrings does not extend over all seasons of the year. The following facts point to the correctness of the assumption that the conditions here are strictly homologous to the conditions as regards the younger fish.

(3) Among the Norwegian herrings a great many

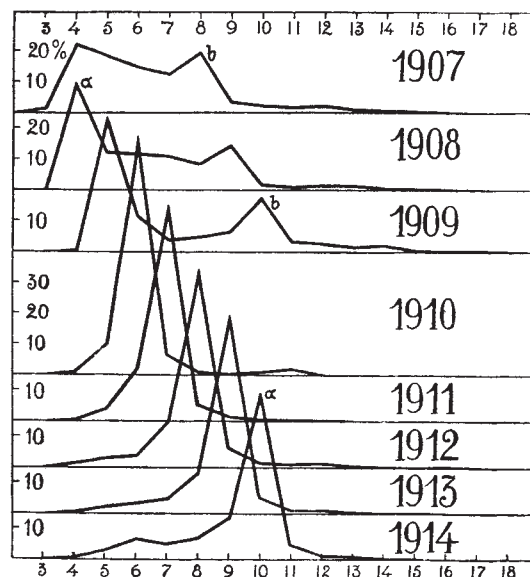


FIG. 1.—Showing the captures of Norwegian mature herrings for eight successive years, arranged in percentage frequency curves according to the number of rings on the scales (the numbers along the abscissa denoting the ring groups).

individuals had an abnormally small third summer belt on their scales. Herrings showing this abnormality have been very frequently observed during all the years from 1907 to 1914, but while the scales of these herrings in the year 1907 had only one summer belt outside the abnormal one in 1908, they showed two summer belts, and so on until the winter 1913-14, when they had seven summer belts outside. Thus these herrings, so easily distinguishable by their abnormality, have during the seven years of observation annually formed one summer belt on their scales, each belt being separated by a winter ring from the preceding and succeeding belts.

(4) By scale investigations on the Norwegian spring herrings (spawning herrings), carried out during the years from 1907 to 1914, results are obtained the main points of which are given in Fig. 1. This diagram is based upon all the material from 1907-13, while for 1914 part of the material was not worked up when the diagram was constructed (still the curve for this year is based upon more than 2000 individuals).