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