condition under the Third Empire. Working in such fields, his attention was early directed to statistics, and in his important work "La Population Française" he not only treats of the geographical aspects of the question, but deals with it statistically and points out the importance of the proper use of statistics in all questions of applied geography. In all that relates to human geography, the definiteness which we meet in physical geography is only attained with difficulty, and by the careful and prudent use of the best statistical material available. In this direction Levasseur's work furnishes many examples of the highest value as giving a truly scientific form to investigations which, from the numerous factors involved, are too often treated superficially.

H. G. L.

THE BRITISH ASSOCIATION AT PORTSMOUTH.

As we went to press last week, the concluding meeting of the British Association at Portsmouth was being held; and the thanks of the Association were being expressed to the local authorities for the work they had done and the trouble they had taken to make the visit to Portsmouth pleasant and memorable. Everyone agrees that the Portsmouth meeting was most enjoyable; and well it might be, considering that it was held at a seaside resort during the season when "sunny Southsea" is full of attractions. In spite of great difficulties, the Mayor (Alderman T. Scott Foster) was able to arrange for the accommodation of the secretaries and other officials in the best hotels, and to provide hospitality for distinguished visitors. There was little private hospitality, and the grant of 3500*l.* made to the Mayor by the Corporation of Portsmouth for the entertainment of the visitors can scarcely have covered the expenses involved.

The best thanks of the world of science are due to the Mayor and the Corporation for the public spirit they have shown in making provision for the meeting, in spite of a certain amount of local opposition to the necessary expenditure. The actual work of local arrangements has, of course, fallen largely upon the shoulders of the local secretaries, namely, the town clerk (Mr. G. Hammond Etherton) and the medical officer of health (Dr. Mearns Fraser). Only those who have had to be responsible for the organisation of a meeting such as that concluded last week can understand how well the thanks received by the local secretaries are merited. The total attendance at the meeting was 1,241.

Of the attractions provided for the entertainment of the visitors, the naval display, which was viewed from the battleship *Revenge*, was most impressive; and it will long remain in the memories of the large party privileged to see it. The Association owes this distinctive feature to the Commander-in-Chief, Admiral Sir William Moore, who, in acknowledging the vote of thanks to him and the officers and men of the Royal Navy for the display, paid a generous tribute to the work of science. "When I am at sea," said Sir William, "Lord Kelvin's compass and sounding-line make me think of him with gratitude twenty times a day"; and, referring to Sir William White, who proposed the vote of thanks, he remarked, "He has given us the greatest essential in the design of our ships, namely, a steady gun-platform."

On account of the naval display, it was impossible for the Committee of Recommendations to decide upon the grants for scientific purposes on the Monday, so the subjoined list, which was adopted by the General Committee on Wednesday afternoon, was not available for publication last week. The total is 40*l.* less than last year.

GRANTS OF MONEY APPROPRIATED FOR SCIENTIFIC PURPOSES BY THE GENERAL COMMITTEE AT THE PORTSMOUTH MEETING.

	£
Section A.—Mathematical and Physical Science.	
Seismological Observations	60
Upper Atmosphere	30
Magnetic Observations at Falmouth	25
Establishing a Solar Observatory in Australia	50
Grant to the International Commission on Physical and Chemical Constants	30
Tabulation of Bessel Functions	15
Section B.—Chemistry.	
Study of Hydro-aromatic Substances	20
Dynamic Isomerism	30
Transformation of Aromatic Nitro-amines	10
Electroanalysis	10
Plant Enzymes	30
Section C.—Geology.	
Erratic Blocks	5
Palæozoic Rocks	10
Composition of Charnwood Rocks	2
Igneous and Associated Sedimentary Rocks of Glensaul	15
List of Characteristic Fossils	5
Sutton Bone Bed	15
Benbridge Limestone	20
Section D.—Zoology.	
Index Animalium	75
Table at the Zoological Station at Naples	30
Belmullet Whaling Station	20
Secondary Sexual Characters in Birds	10
Section E.—Geography.	
Equal Area Maps	20
Calculation of Areas on the Spheroid	25
Section G.—Engineering.	
Gaseous Explosions	60
Section H.—Anthropology.	
Glastonbury Lake Village	5
Age of Stone Circles	15
Anthropological Notes and Queries	40
Artificial Islands in Highland Lochs	13
Physical Character of Ancient Egyptians	40
Excavations in Easter Island	15
Anthropometric Investigations in British Isles	5
Section I.—Physiology.	
The Ductless Glands	35
Table at the Zoological Station at Naples	20
Anæsthetics	20
Calorimetric Observations	40
Section K.—Botany.	
Structure of Fossil Plants	15
Experimental Study of Heredity	35
Survey of Clare Island	20
Jurassic Flora of Yorkshire	20
Section L.—Education.	
Mental and Physical Factors	5
Overlapping	5
Industrial and Poor Law Schools	10
Influence of School Books on Eyesight	5
Corresponding Societies Committee.	
For Preparation of Report	25
Special Grant.	
Collections to illustrate Natural History, &c., of the Isle of Wight	40
Total	1050

SECTION G.

ENGINEERING.

OPENING ADDRESS BY PROF. J. H. BILES, LL.D., D.Sc., M.INST.C.E., PRESIDENT OF THE SECTION.

It has happened during recent years that accidents have happened to ships and they have mysteriously disappeared. The complete disappearance without leaving any trace has led to the assumption that the vessel has capsized. The circumstances of such cases obviously preclude the existence of any direct evidence. The only subjects of investigation can be (1) the condition of the ship prior to the accident, and (2) the probability that such a condition could be one which in any *known possible circumstances* could lead to disaster. The first is determinable by evidence in any particular case. The second involves a consideration of the whole question of the behaviour of ships at sea. What is the effect upon any given ship of a known series of waves? What waves is a ship likely to meet?

This subject has occupied the attention of scientific engineers, and it may be said to have been considered a solved problem. We have thought that if a ship has a certain metacentric height and a certain range of positive stability she is quite safe from the action of a series of waves of any kind which we know to exist. If, however, a known ship (and perhaps more than one) has these safety-ensuring qualities and mysteriously disappears, it may be desirable to review the grounds of our belief to see whether any *known possible combination* of circumstances may cause disaster.

Let us then first briefly review the grounds of our belief. Fifty years ago Mr. Wm. Froude showed that the large angles occasionally reached in rolling are not due to a single wave-impulse, but are the cumulative effect of the operation of successive waves. The period T of a small oscillation of a ship in water free from wave disturbance and resistance is $\pi\sqrt{\frac{k^2}{gh}}$, where k is the radius of gyration and h is the metacentric height (*i.e.*, the height of the metacentre above the centre of gravity). The period T of a wave is $\sqrt{\frac{2\pi l}{g}}$, where l is the length of the wave and g is the acceleration due to gravity. The line of action of the resultant of the supporting pressures acting on a ship in undisturbed water is the vertical through the centre of gravity of the volume of the water displaced by the ship. In wave-water it is in the normal to the effective wave-slope (which is approximately the wave-surface). The oscillation of this normal as the waves pass causes a varying couple tending to incline the vessel. If the vessel is very quickly inclined by this couple she will place herself in or near the normal and the inclining couple will be of zero value. If, however, her movements are very slow, the normal may make one or more oscillations before any appreciable effect is produced on the vessel. The tendency to incline in one direction caused by the normal acting on one side of the vertical is checked by the rapid oscillation of the normal to the other side of the vertical. It is, therefore, evident that the relation between the period of the ship and that of the wave normal is a dominating feature in the resulting movement of the ship. Mr. W. Froude's mathematical solution of this relation is the basis of our belief that we understand the behaviour of a ship in the *uniform* system of waves when the vessel is placed broadside on to the waves. To obtain this solution he assumed that within the limits considered, the moment of stability varied as the angle of inclination. In the curve of righting levers of a ship, usually known as a curve of stability, this condition holds generally for angles up to about 10°. The curve usually reaches a maximum value at about 30° to 40° and vanishes at 60° to 80°, so that for large angles of roll the assumption does not hold. On this assumption, however, he showed that the motion of a ship amongst such a system of waves is the same as for still water plus a motion composed of two sine terms. The amplitude of this latter motion depends upon the maximum slope of the waves and the ratio $\frac{T}{T_1}$ (the period of the ship in undisturbed water to the period of the wave). If the ship starts from rest in the upright, θ is the maximum angle of inclination of the ship and θ_1 the maximum wave-slope; then

$$\theta = \theta_1 \frac{1}{1 - \frac{T^2}{T_1^2}}$$

He considered several solutions of the equation of motion:—

(1) $T = T_1$; this is synchronism and the angle of inclination gradually increases. Each wave-impulse adds something to the ship's inclination and without any resistance to rolling the vessel would capsize.

(2) $\frac{T}{T_1} = 0$; this is the case of the ship's period being very small compared with that of the wave. θ will then be positive and equal to θ_1. In other words, the ship will place herself normally to the wave-slope. The maximum amplitude will only be the maximum wave-slope.

(3) $\frac{T}{T_1} < 1$. In this case the wave-period is greater than that of the ship and θ is always positive and greater than θ_1. The vessel always inclines away from the wave-slope. If

$\frac{T}{T} = \frac{1}{4}$, $\theta = \frac{16}{15}\theta_1$. If $\frac{T}{T_1} = \frac{1}{2}$, $\theta = \frac{4}{3}\theta_1$. If $\frac{T}{T_1} = \frac{3}{4}$, $\theta = \frac{16}{7}\theta_1$

The nearer $\frac{T}{T_1}$ is to unity the larger is the maximum amplitude.

(4) $\frac{T}{T_1} > 1$. In this case the wave-period is less than that of the ship, and θ is always negative. The vessel inclines towards the wave-slope.

If $\frac{T}{T_1} = 1 \cdot 1$, then $\theta = -4 \cdot 76\ \theta_1$;

$\frac{T}{T_1} = 1 \cdot 26$, then $\theta = -\theta_1$;

$\frac{T}{T_1} = 2 \cdot 0$, then $\theta = \frac{1}{3}\theta_1$;

$\frac{T}{T_1} = 2 \cdot 235$, then $\theta = \frac{1}{4}\theta_1$.

This shows the advantage of having T greater than T_1.

He showed that the ship goes through a cycle of changes and considered the effect of variations of $\frac{T}{T_1}$ upon these cycles. He showed that $\frac{T}{T_1} = \frac{5}{4}$ is better than $\frac{T}{T_1} = \frac{4}{5}$, so that it is better to lengthen T rather than to shorten it. Similar results for $\frac{T}{T_1} = 2$ and $\frac{1}{2}$ respectively gave better results by lengthening than shortening T. In each of the cases $\frac{T}{T_1} = \frac{5}{9}$ and $\frac{T}{T_1} = \frac{9}{5}$ the results show baulked oscillations in which, while the vessel swings towards the vertical, she does not reach it but swings back again. The lengthened value of T here also gave better results than for shortening it. The results given above are greater than would be obtained in practice, because resistance has been neglected. Later he determined the effect of resistance upon rolling in still water free from waves. He determined the law of resistance and found it to vary partly as the angular velocity and partly as the square of it. He rolled a ship, and after she was allowed to roll free from disturbance he measured the angle of inclination at the end of each roll. These showed the rate of extinction of the rolling due to the resistance. The loss of extreme angle of roll between one roll and the next represented the work done by the ship in rolling. It is possible to calculate the work done in inclining the vessel to any angle, and the difference between the amount of work thus done in two different angles represents the difference in work necessary, and therefore work done in resistance to bring the ship to these angles of inclination. Hence the work done by resistance between the two consecutive rolls can be actually measured by measuring the extreme angle of inclination in successive rolls.

Having determined the resistance in terms of angles of roll and time, it was easy to determine the law which represented the resistance in terms of the angular velocity.

In applying this to waves, all that is necessary is to take account of the fact that the position of equilibrium about which the ship oscillates is the normal to the effective wave-slope. This normal has a definite oscillation about a fixed vertical. It is, therefore, possible to determine the angle of inclination in terms of time.

As these angles of roll may be considerable, the assumption upon which the general solutions for unresisted rolling, already given, were based will not hold. The actual moments of stability depend upon the form of the ship and the position of its centre of gravity, and as these vary in different ships it is only possible to obtain the relation between inclination and time by a special investigation in each case. A solution by a method of graphic integration was devised by Mr. W. Froude and has been applied to a very small number of cases. The information necessary to obtain a solution in any one case is as follows:—

(1) A curve of righting levers in terms of angle of inclination. This is called a curve of statical stability.

(2) The form and period of the wave on which the ship is supposed to be placed broadside on.

(3) The constants which determine the actual value of the resistance moment in terms of the angular velocity. These can be obtained by rolling the ship in still water and observing the rate of extinction of rolling when that extinction is due to resistance only. The form of the curve of extinction can be obtained by rolling a model of the ship, but the actual ordinates of the curve for an actual ship can only be obtained by experiment on the ship herself, or by inference from a similar ship of approximately the same size, form, and arrangements.

A consideration of these three necessities for the solution of one particular case shows that a considerable amount of work is necessary for determining the angle of inclination in terms of time. Even this solution can only be made for one assumed maximum angle of inclination as a starting condition. For instance, in any case where a ship is assumed to start with a maximum inclination of 20° it is only possible to obtain one solution of angles of inclination in terms of time. If we take another maximum angle of inclination, another complete solution is necessary. The work of each solution is considerable.

For ships which vary much in draught and condition of loading it is evident that for each ship the work of complete investigation for all the conditions of loading of different waves and different angles of maximum inclination is very great. For this reason the investigation of rolling by the Froude graphic method has only been made for a very small number of cases, and our knowledge of the actual angles of inclination of ships obtained by this method is very small.

The curve of statical stability is worked out for many ships in a few conditions of draught and position of centre of gravity. These curves are of little practical value, because they only serve as comparisons between ships. It is assumed that if a ship has a fair range of statical stability, *i.e.*, that the angle of vanishing statical stability is not less than, say, 60°, and the maximum righting lever is not unlike some previous ship which "has been to sea and come home again" safely, this ship will be safe. This assumption is based on the belief that only what has happened to previous ships will happen to the one in question; that is, that the contingencies of waves will be the same in all cases. But when we find that occasionally ships are missing we are compelled to ask ourselves the question—is it possible that some occasional contingencies of sea or ship, or both, may exist which will produce a dangerous and perhaps fatal roll?

Mr. W. Froude's investigations were made for a uniform system of waves. He showed that in unresisted rolling a ship initially at rest and in the upright position is acted upon by a uniform series of waves such that $\frac{T}{T_1} = \frac{p}{q}$ where p and q are the smallest whole numbers which express this ratio, then the rolling of the ship will be in cycles, the maximum inclination in each roll gradually increasing, and again gradually diminishing, and so on. The period of occurrence of the maximum of maxima will be $2qT$. The number of times the ship passes through the upright in each complete cycle is $2p$ or $2q$, whichever is the smaller. The ship is upright at the middle of the cycle, and on either side of this middle there is an equal maximum which is approximately $\theta_1 \frac{q}{q-p}$, and never exceeds this value (where θ_1 is maximum wave-slope). If T is much larger than T_1, and therefore p is much larger than q, then the value of $\theta_1 \frac{q}{q-p}$ approaches $\theta_1 \frac{q}{p}$ and is less than the wave-slope. If T is much smaller than T_1, then the value of $\theta_1 \frac{q}{q-p}$ approaches θ_1. If T is nearly equal to T_1, then $\theta_1 \frac{q}{q-p}$ approaches a high value.

From this it is seen that our investigations in unresisted rolling may be over a very wide field, but would produce no definite result in the matter of finding cases of large angles of roll in practice. We can only obtain valuable results when resistance is included.

Mr. R. E. Froude in 1896 was led to deal with the subject of non-uniform rolling of ships in an assumed uniform system of waves which did not synchronise with the ship, such as is dealt with above for unresisted rolling, and he dealt with the effect of resistance in such a case. He pointed out that there is a particular phase-relation between the ship and the wave which will produce uniform rolling, just as there is in the case of a synchronous system of waves. If at any stage for any reason the rolling is of the cyclic character considered in non-resisted rolling, then the resistance must gradually introduce uniformity, because the rolling is made up of two seas of oscillations—

(1) That due to the rolling relatively to the water-surface, such as would occur in undisturbed water.

(2) That due to the oscillation of the water-surface itself, caused by the passage of the wave.

We have already seen that the resisted oscillation in undisturbed water gradually decreases when the vessel is left free to oscillate, but takes place in practically uniform time T. The oscillation of the water-surface is forced on the ship and causes a simple harmonic oscillation of the ship in time T_1, in algebraic addition to that due to the free resisted oscillation. When the maximum angle of a roll due to the free oscillation coincides with the maximum angle due to the forced oscillation of the wave, we shall have a maximum extreme inclination which is the sum of that due to the free and the forced. When they are in opposition we shall have a minimum extreme oscillation which is the difference of these two. At stages between coincidence and opposition we shall have extreme angles varying between maximum and minimum. As time goes on the extreme angle due to the free oscillation gradually decreases under resistance, and the sum and the difference referred to above approximate to each other, and the rolling becomes that due to the wave alone. We have seen that in the case of unresisted rolling where the wave and the ship synchronise there is an addition to the angle of inclination for each passage of the wave, and were it not for resistance these accumulated increases would cause the vessel to upset. But in the case of resisted rolling each increase of extreme angle of roll causes an increase in the work done upon the resistance of the ship, and when the increase in work done in increasing the angle of heel by each passage of the wave equals the work done in increasing the resistance incurred in swinging through this greater angle, then we shall have a balance of condition and a uniform angle of roll. The angle at which this balance takes place depends on the period and maximum slope of the wave and the coefficients of resistance between the ship and the water. For instance, with a maximum wave-slope of 3° and with a ratio of ship to wave-period of 1·1 the value of the angle of ultimate uniform rolling in the case of H.M.S. *Revenge* was found to be 13·9° without bilge-keels and 10·8° with them. In the case of synchronism of the ship and the wave, the rolling is uniform always and reaches a maximum of 41·1° without and 14·85° with bilge-keels. The nearer the wave and ship are to synchronism, the larger is maximum inclination reached before uniform rolling sets in and during uniform rolling. Resistance is of much more importance in the case of synchronism. If the ratio of ship to wave-period be 1·3, the maximum angle before uniform rolling is reached is 8·25° without and 6·6° with bilge-keels, while that due to uniform rolling is 4·35° without and 4·24° with. We see, therefore, the important part that the near approach to synchronism plays in creating large angles of roll and the value of bilge-keels in reducing the rolling in conditions approaching synchronism. When on waves of smaller period, when small angles of roll may be expected, the bilge-keels give but small advantage. The assumption in these cases is that the vessel starts from rest in the upright in the mid-height of the wave, and that the rolling is caused by the assumed uniform swell. The vessel will go through the cyclic change already described and will reach a maximum inclination of not more than double that which she reaches when uniform rolling has set in.

A later investigator, Colonel Russo, of the Italian Navy, found by experiment that by varying the assumption as to starting condition of the ship, by letting the wave-action begin to operate first when the vessel is upright and at rest on the crest of the wave, the maximum angle before uniform rolling sets in can be more than four times that due to uniform rolling if the time of the ship is greater than that of the swell. There is an infinite number of solutions of rolling amongst waves because there is an infinite number of initial circumstances, but, whatever these may be, the rolling in a uniform swell will always soon degenerate into a series of uniform forced oscillations in the wave-period.

From this discovery of Colonel Russo's, we see that the region of investigation of possible causes of upsetting is removed from that of uniform rolling even in a non-synchronous sea. The following table shows for the *Revenge* with bilge-keels the variation in maximum angle of inclination before and during uniform rolling in terms of the period and length of the swell :—

Period of swell in seconds...	8	10	12	13·3	15	17	19
Length of swell in feet ...	328	512	738	910	1153	1481	1850
Maximum angle in degrees before uniform rolling ...	6·3	8·0	14·7	21·4	17·1	13·0	11·0
Maximum angle in degress during uniform rolling ...	2·5	4·2	12·6	21·4	15·4	11·0	8·7

The period of free rolling of the *Revenge* through small angles for a double roll was about 16 seconds. The foregoing shows that the maximum rolling (which occurs at synchronism) took place at a period of swell of 13·3 seconds. The period of roll was less at large than small oscillations. The above figures are for waves varying from $\frac{1}{37}$th to $\frac{1}{54}$th of their length in height. The length of wave which corresponds to maximum inclination is 910 feet and height is about one-fiftieth. The maximum wave-slope for such waves is 3·6°. We are in the habit of dealing with waves of one-twentieth of their length in height for strength calculations. Observers have recorded waves in the open ocean of 600 to 800 feet in length and of 30 to 45 feet in height, so that we know that the slope of the waves assumed by Colonel Russo is much less than may be encountered at sea. A wave the length of which is twenty times its height has a maximum slope of 9°. Records of waves having a ratio of height to length of as great as one-thirteenth have been published. The maximum slope of wave corresponding to these proportions is 14°. If it is admissible to take much larger angles of wave-slope we may expect to get much larger angles of maximum inclination both before uniform rolling sets in and when it does. In a case given by Mr. Froude in which the maximum inclination in the *Revenge* before uniform rolling was 12·0°, he showed by calculation that the corresponding maximum wave-slope must have been 5·09°. For 20° maximum inclination the wave-slope was 10·3°. Both these cases were for periods of ship and wave of 16 and 13 seconds respectively. For similar periods of 16 and 14·6 seconds the wave-slope to produce 20° maximum before uniform rolling is only 7°. These figures give some idea of the effect of the wave-slope on the maximum inclination. It is to be remembered that these are the maximum angles obtained by Mr. Froude; but if we take Colonel Russo's maximum angles, which in some cases are double those obtained by Mr. Froude, it is easy to see that large wave-slopes may produce very large angles of roll.

Summarising, we see that :—

(1) With wave-slopes of 3·6° the angles of maximum roll obtained by them in the *Revenge* with bilge-keels may be taken at 22°.

(2) This roll takes place when synchronism exists between the wave and the ship, when the wave is 910 feet long and 18½ feet high and has a wave-slope of 3·6°.

(3) Waves exist which are of this length, but which may have a height of 50 feet, and possibly more, and a wave-slope of 10°.

(4) In such steeper waves we should expect to get much larger angles of roll.

(5) Each ship has peculiarities of rolling due to its form as well as to its lading and bilge-keels, &c.

(6) These peculiarities and the effect they have upon rolling, and the effect different waves will have upon the rolling of the ship, can best be studied experimentally.

It was my intention when you appointed me as your President to have placed before you the results of an experimental study made on lines somewhat similar to those carried out by Colonel Russo, but extended to a wide range of types of ship, waves, and resistance.

The machine for carrying out these experiments is practically complete, but having met with an accident at the end of April last which incapacitated me for some time, I was prevented from being able to do anything to this subject since then. I am, therefore, obliged to ask you to be content with the general *résumé* of the subject which has been given.

I think enough has been said to show what a field of investigation is open to the experimenter. The little that has been done and published by Colonel Russo is only for three battleships of about the same size. For the great bulk of the ocean wayfarers nothing has been done. If it is possible to determine the kind of rolling which is likely to take place under stated conditions it seems to be desirable to do so.

In all that has been said it will be seen that it is possible to determine experimentally the kind of rolling which will take place in a ship which is snug and seaworthy. But it is also possible to study the effect of loose water in a ship under the same set of conditions as to waves, lading, and form of ship. This part of the subject has not received any experimental treatment except in a very limited number of full-sized ships. It is quite conceivable that some conditions of loose water associated with some conditions of sea may produce large angles of inclination.

The subject has been treated as one in which it is probable that the kind of waves met with at sea will be uniform in size and period. That this is not so is a fact with which we are all more or less familiar. The effect of a uniform system of waves is to rapidly induce a condition of uniform rolling. But any deviation from uniformity of sea immediately introduces non-uniformity of rolling, and generally greater extreme angles of roll. Any experimental study of the action of waves upon a ship must include a variation in the character of the waves. The field of investigation is thereby widened and the search for large angles of inclination made more laborious. But the work is of a kind which can be done by many people, and can be done fairly rapidly, so that there seems to be no insuperable objection to doing it. The details of the apparatus need not be described, but the study of the objects attained may be of interest.

(1) Wave-motion is simulated by the revolution about parallel axes of two parallel cranks of different lengths. The line joining the ends of the arms of the cranks is always in the line of the normal to the wave-surface, and a line perpendicular to it is therefore parallel to the wave-surface.

(2) From the form of the ship are determined curves which are the shape of rollers which roll on a straight line parallel to the wave-surface. The form of these rollers is such that the model of the ship in rolling maintains the position in relation to the wave-surface (*a*) which cuts off constant volume of displacement at any angle of inclination; (*b*) in which the perpendicular to the straight line parallel to the wave-surface through the point of contact is the line of the resultant of the water-pressures acting on the vessel.

(3) The resistance to rolling is obtained by (a) electro-magnets, the current to which is generated by the motion of the model, (b) secondary electro-magnet, the current for which is in the first magnet. (a) represents the resistance due to the angular velocity, (b) represents the square of that velocity.

The variations in the lengths of the cranks and the speed of revolution give the variation in the wave-form assumed. The variation in the electric current by resistances in the circuit gives the variation in the resistances to rolling of the ship. For instance, the current necessary to represent the resistance of a ship with bilge-keels is very different from that for one without.

It is hoped that sufficient has been said to direct attention to the possibility of extended study of the rolling of ships at sea, so that some valuable work may be done in this important subject.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY W. H. R. RIVERS, M.D., F.R.S., PRESIDENT OF THE SECTION.

The Ethnological Analysis of Culture.

DURING the last few years great additions have been made to our store of the facts of anthropology—we have learnt much about different peoples scattered over the earth, and we understand better how they act and think. At the same time we have, I hope, made a very decided advance in our knowledge of the methods by means of which these facts are to be collected, so that they may rank in clearness and trustworthiness with the facts of other sciences. When, however, we turn to the theoretical side of our subject, it is difficult to see any corresponding advance. The main problems of the history of human society are little if at all nearer their solution, and there are even matters which a few years ago were regarded as settled which are to-day as uncertain as ever. The reason for this is not far to seek; it is that we have no general agreement about the fundamental principles upon which the theoretical work of our science is to be conducted.

In surveying the different schools of thought which guide theoretical work on human culture, a very striking fact at once presents itself. In other and more advanced sciences the guiding principles of the workers of different nations are the same. The zoologists or botanists of France, Germany, America, our own and other countries, are on common ground. They have, in general, the same principles and the same methods, and the work of all falls into a common scheme. Unfortunately, this is not so in anthropology. At the present time there is so great a degree of divergence between the methods of work of the leading schools of different countries that any common scheme is impossible, and the members of one school wholly distrust the work of others, whose conclusions they believe to be founded on a radically unsound basis.

I propose to consider in this address one of the most striking of these divergencies, but, before doing so, I will put as briefly as possible what seem to me to be the chief characters of the leading schools of different countries. To begin with that dominant among ourselves. The theoretical anthropology of this country is inspired primarily by the idea of evolution founded on a psychology common to mankind as a whole, and further, a psychology differing in no way from that of civilised man. The efforts of British anthropologists are devoted to tracing out the evolution of custom and institution. Where similarities are found in different parts of the world it is assumed, almost as an axiom, that they are due to independent origin and development, and this in its turn is ascribed to the fundamental similarity of the workings of the human mind all over the world, so that, given similar conditions, similar customs and institutions will come into existence and develop on the same lines.

In France we find that, as among ourselves, the chief interest is in evolution, and the difference is in the principles upon which this evolution is to be studied. It is to the psychological basis of the work of British anthropologists that objection is chiefly made. It is held that the psychology of the individual cannot be used as a guide to the collective actions of men in early stages of social evolution, still less the psychology of the individual whose social ideas have been moulded by the long ages of evolution which have made our own society what it is. It is urged that the study of sociology requires the application of principles and methods of investigation peculiar to itself.[1]

About America it is less easy to speak, because it is unusual in that country to deal to any great extent with general theoretical problems. The anthropologists of America are so fully engaged in the attempt to record what is left of the ancient cultures of their own country that they devote little attention to those general questions to which we, more unfortunately situated with no ancient culture at our doors, devote so much attention. There seems, however, to be a distinct movement in progress in America which puts evolution on one side, and is inclined to study social problems from the purely psychological point of view, the psychological standpoint, however, approaching that of the British school more nearly than that of the French.[2]

It is when we come to Germany that we find the most fundamental difference in standpoint and method. It is true that in Adolf Bastian Germany produced one who was thoroughly imbued with the evolutionary spirit, and the *Elementargedanke* of that worker forms a most convenient expression for the psychological means whereby evolution is supposed to have proceeded. In recent years, however, there has been a very decided movement opposed to Bastian and the whole evolutionary school. In some cases this has formed part of that general revolt, not merely against Darwinism, which is so prominent in Germany, but it seems even against the whole idea of evolution. In other cases the objection is less fundamental, and has been not so much to the idea of evolution itself as to the lines upon which it has been customary to endeavour to study this evolution.

This movement, which by those who follow it is called the geographical movement, but which, I think, may be more fitly styled "ethnological," was originated by Ratzel, who was first led definitely in this direction by a study of the armour made of rods, plates, or laths which is found in North America, northern Asia, including Japan, and in a less developed form in some of the islands of the Pacific Ocean.[3] Ratzel believed that the resemblances he found could only be explained by direct transmission from one people to another, and was led by further study to become an untiring opponent of the *Elementargedanke* of Bastian and of the idea of independent evolution based on a community of thought. He has even suggested that the idea of independent origin is the anthropological equivalent of the spontaneous generation of the biologist, and that anthropology is now going through a phase of development from which biology has long emerged.

The movement initiated by Ratzel has made great progress, especially through the work of Graebner[5] and of P. W. Schmidt. It has resulted in an important series of works in which the whole field of anthropological research is approached in a manner wholly different from that customary in this country.[7] I must content myself with

[1] I refer here especially to the work of the "sociological" school of Durkheim and his followers. For an account of their principles and methods see "L'Année sociologique," which began to appear in 1898; Durkheim, "Les Règles de la Méthode Sociologique," Paris; and Lévy-Bruhl, "Les fonctions mentales dans les sociétés inférieures," Paris, 1910.

[2] See especially A. L. Kroeber, "Classificatory Systems of Relationship," Journ. Roy. Anthr. Inst., 1909, xxxix., 77; and Goldenweiser, "Totemism: An Analytical Study." Journ. Amer. Folk-lore, 1910, xxiii.

[3] *Sitzber. d. Akad. d. Wiss. München*, Hist. Cl., 1886, p. 181.

[4] See especially "Anthropogeographie," 1891, Th. ii., 795, and "Die geographische Methode in der Ethnographie," *Geograph. Zeitsch.*, 1897, iii., 268.

[5] See especially "Methode der Ethnologie," Heidelberg, 1911, and "Die melanesische Bogenkultur und ihre Verwandten," *Anthropos*, 1909, iv., 726. The annual "Ethnologica," edited by W. Foy, is devoted to the illustrations of this school of thought.

[6] See especially "L'origine de l'Idée de Dieu," *Anthropos*, iii.–v., 1908–10, and "Grundlinien einer Vergleichung der Religion u. Mythologie der austronesischen Völker," *Denksch. d. Akad. d. Wiss. Wien*, Phil.-hist. Kl., 1910, liii. Schmidt differs from Graebner in limiting the application of the ethnological method to regions with general affinities of culture. Otherwise he remains an adherent of the doctrine of independent origin. (See "Panbabylonismus und ethnologischer Elementargedanke," *Mitt. d. anthrop. Gesellsch. in Wien*, 1008, xxxviii., 73.)

[7] It must not be understood from this account that all German anthropologists are adherents to the ethnological school. There are still those who follow the doctrines of Bastian, which have undergone an interesting modification through the adoption of the biological principle of Convergence.

one example to illustrate the difference of standpoint which separates the two schools. Few subjects have attracted more interest in this and other countries than the study of primitive decoration. In the decorative art of all lands there are found transitions from designs representing the human form or those of animals and plants to patterns of a purely geometrical nature. In this country it has been held, I think I may say universally, that in these transitions we have evidence for an evolutionary process which in all parts of the world has led mankind to what may be called the degradation and conventionalisation of human, animal, or plant designs, so that in course of time they become mere geometrical forms.

To the modern German school, on the other hand, these transitions are due to the blending of two peoples, one possessing the practice of decorating its objects with human, animal, or plant designs, while the art of the other is based on the use of geometrical forms. The transitions which have been taken to be evidence of independent processes of evolution based on psychological tendencies common to mankind are by the modern German school ascribed to the mixture of cultures and of peoples. Further, similar patterns, even one so simple as the spiral, when found in widely separated regions of the earth, are held to have been due to the influence of one and the same culture.

I have chosen this example because it illustrates the immense divergence in thought and method between the two schools; but the difference runs through the whole range of the subject. In every case where British anthropologists see evolution, either in the form of material objects or in social and religious institutions, the modern German school sees only the evidence of mixture of cultures, either with or without an accompanying mixture of the races to which these cultures belonged.

It will, I think, be evident that this difference of attitude of British and German workers is one of fundamental and vital importance. When we find the chief workers of two nations thus approaching their subject from two radically different and, it would seem, incompatible standpoints, it is evident that there must be something very wrong, and it has seemed to me that I cannot better use the opportunity given to me by the present occasion than in devoting my address to this subject.

The situation is one which has an especial interest for me in that I have been led quite independently to much the same general position as that of the German school by the results of my own work in Oceania with the Percy Sladen Trust Expedition. With no knowledge of the work of this school, I was led by my facts to see how much, in the past, I had myself ignored considerations arising from racial mixture and the blending of cultures, and it will perhaps interest you if I sketch briefly the history of my own conversion.

Much of my time in Oceania was devoted to survey work, in which I collected especially the systems of relationship of every place I visited, together with such other facts concerning social organisation as I was able to gather. I began my theoretical study by a comparison of the various forms of these systems of relationship, disregarding at first the linguistic nature of the terms. From the study of these systems I was able to demonstrate the existence, either in the present or the past, of a number of extraordinary and anomalous forms of marriage, such as marriage with the daughter's daughter and with the wife of the father's father,[1] all of which become explicable if there once existed widely throughout Melanesia a state which is known as the dual organisation of society with matrilineal descent, accompanied by a condition of dominance of the old men which enabled them to monopolise all the young women of the community. Taking this as my starting-point, I was then able to trace out a consistent and definite scheme of the history of marriage in Melanesia from a condition in which persons normally and naturally married certain relatives to one in which wives are purchased with whom no relationship whatever can be traced, and I was able to fit many other features of the social structure of Melanesia into this scheme. So far my work was of a purely evolutionary character, and only served to strengthen me in my previous standpoint.

[1] These terms are used in the classificatory sense.

I then turned my attention to the linguistic side of the systems of relationship, and a study of the terms themselves showed that these fell into two main classes: one class generally diffused throughout Oceania, while the terms of the other class differed very considerably in different cultural regions. Further, it became clear that the terms of the first class denoted relationships which my comparative study of the forms of the systems had shown to have suffered change, while the terms which varied greatly in different parts of Oceania denoted relationships, such as those of the mother and mother's brother, which there was no reason to believe had suffered any great change in status. From these facts I inferred that at the time of the most primitive stage of Melanesian society of which I had evidence there had been great linguistic diversity which had been transformed into the relative uniformity now found in Melanesia by the incoming of a people from without, through whose influence the change I had traced had taken place, and from whose language the generally diffused terms of relationship had been borrowed. It was through the combined study of social forms and of language that I was led to see that the change I had traced was not a spontaneous evolution, but one which had taken place under the influence of the blending of peoples. The combined morphological and linguistic study of systems of relationship had led me to recognise that a definite course of social development had taken place in an aboriginal society under the influence of an immigrant people.

I turned next to a Melanesian institution, that of secret societies, concerning which I had been able to gather much new material, and it soon became probable that these societies belonged properly neither to the aboriginal culture nor to that of the immigrants, but had arisen as the result of the interaction of the two; that, in fact, these secret societies had had their source in the need felt by the immigrants for the secret practice of the rites they had brought with them from their former home. A comparison of the ritual of the secret societies with the institutions of other parts of Oceania then made it appear that the main features of the culture of these immigrants had been patrilineal descent, or at any rate definite recognition of the relation between father and child, a cult of the dead, the institution of taboo, and, lastly, certain relations with animals and plants, which were probably allied to totemism, if they were not totemism itself in a fully developed form.

Further study made it clear that those I have called the immigrant people, though possessing these features in common, had reached Melanesia at different times and with several decided differences of culture, but that probably there had been two main streams: one which peopled Polynesia and became widely diffused throughout Melanesia, which was characterised by the use of kava; another which came later and penetrated much less widely, which brought with it the practice of chewing betel-mixture. Traces of a third stream, the earliest of all, are probably to be found here and there throughout Melanesia, while still another element is provided by recent Polynesian influence. It became evident that the present condition of Melanesian society has come into being through the blending of an aboriginal population with various peoples from without, and it therefore became necessary to ascertain to which of the cultures possessed by these peoples the present-day customs and institutions of Melanesia belong, always keeping in mind the possibility that some of these institutions may not have belonged to any one of the cultures, but may have arisen as the result of the interaction of two or more of the blending peoples.

I must be content with this brief sketch of my scheme of the history of Melanesian society, for my object to-day is to point out that if Melanesian society possesses the complexity and the heterogeneous character I have indicated, and is the resultant of the mixture of three or four main cultures, it cannot be right to take out of the complex any institution or belief and regard it as primitive merely because Melanesian culture on the whole possesses a more or less primitive character. It is probable that some of the immigrants into Melanesia had a relatively advanced culture, possibly even that the institutions and ideas they brought with them had been taken from a culture higher still, and, therefore, when we bring forward any Melanesian institution or belief as an example of primitive thinking or acting, our first duty should be to inquire to which stratum of Melanesian culture it belongs.

To illustrate my meaning, I have time for only one example. No concept of Melanesian culture has bulked more largely in recent speculation than that of *mana*, the mysterious virtue to which the magico-religious rites of Melanesia are believed to owe their efficacy. This word now seems on its way to enter the English language as a term for that power or virtue which induces the emotions of awe and wonder, and thus provides a most important element, not only in the specific mental states which underlie religion, but also plays much the same part in the early history of magic. In recent speculation the idea of *mana* is coming to be regarded as having been the basis of religious ideas and practices preceding the animism which, following Prof. Tylor, we have for long regarded as the earliest form of religion, and *mana* is thus held to be not only the foundation of pre-animistic religion, but also the basis of that primitive element of human culture which can hardly be called either religion or magic, but is the common source from which both have been derived. If I am right in my analysis of Oceanic culture, the Melanesian concept of *mana* is not a suitable basis for these speculations. It is certain that the word *mana* belongs to the culture of the immigrants into Melanesia, and not to that of the aborigines. It is, of course, possible that though the word belongs to the immigrant culture, the ideas which it connotes may belong to a more primitive stratum; but this is a pure assumption, and one which I believe to be contrary to all probability. At any rate, we can be confident that even if the ideas connoted by the term *mana* belong to or were shared by the primitive stratum of Melanesian society, they must have been largely modified by the influence of the alien, but superior, culture from which the word itself has been taken. I believe that the Melanesian evidence can legitimately be used in favour of the view that the power or virtue denoted by *mana* is a fundamental element of religion. The analysis of culture, however, indicates that it is not legitimate to use the Melanesian evidence to support the primitiveness of the concept of *mana*. This evidence certainly does not support the view that the concept of *mana* is more primitive than animism, for the immigrants were already in a very advanced stage of animistic religion, a cult of the dead being certainly one of the most definite of their religious institutions.

Further, I believe that the use of the term *mana* in Melanesia in connection with magic, as a term for that attribute of objects used in magic to which they owe their efficacy, is due to an extension of the original meaning of the term, and that it would only be misleading to use the Melanesian facts as evidence in favour of the concept of *mana* as underlying primitive magic. Here, again, I do not wish to deny that a concept such as that denoted by *mana* may be a primitive element of magic; all that I wish to point out is that the Melanesian evidence cannot properly be used to support this view, for the use of the term in connection with magic in Melanesia is not primitive, but secondary and relatively late.

The point, then, on which I wish to insist is that if cultures are complex, their analysis is a preliminary step which is necessary if speculations concerning the evolution of human society, its beliefs and practices, are to rest on a firm foundation.

I have so far dealt only with Melanesia. It is obvious that the same principle that analysis of culture must precede speculations concerning the evolution of institutions is of wider application; but I have time only to deal, and that very briefly, with one other region.

No part of the world has attracted more attention in recent anthropological speculation than Australia, and at the bottom of these speculations, at any rate in this country, there has usually been the idea, openly expressed or implicitly understood, that, in the culture of this region, we have a homogeneous example of primitive human society. From the time that I first became acquainted with Australian sociology, I have wondered at the complacency with which certain features of Australian social organisation have been regarded, and especially the combination of the dual organisation and matrimonial classes with groups closely resembling the totemic clans of other parts of the world. This co-existence of two different forms of social organisation side by side has seemed to me the fundamental problem of Australian society, and I confess that till lately, obsessed as I see now I have been by a crude evolutionary point of view, the condition has seemed an absolute mystery.[1] A comparison, however, of Australia and Melanesia has now led me to see that probably we have in Australia, not merely another example of mixture of cultures, but even another resultant of mixture of the same or closely similar components as those which have peopled Melanesia, viz. a mixture of a people possessing the dual organisation and matrilineal descent with one organised in totemic clans, possessing either patrilineal descent, or at any rate clear recognition of the relation between father and child. This is no new view, having been already advanced, though in a different form, by Graebner[2] and P. W. Schmidt.[3] If further research should show Australian society to possess such complexity, it will at once become obvious that here also ethnological analysis must precede any theoretical use of the facts of Australian society in support of evolutionary speculations.

It may be objected that we all recognise the complexity of culture, and, indeed, in the study of regions such as the Mediterranean, where we possess historical evidence, it is this complexity which forms the chief subject of discussion. Further, where we possess historical evidence, as in the cases of the Hindu and Mohammedan invasions into the Malay Archipelago, all anthropologists are fully alive to the complexities and difficulties introduced thereby into the study of culture; but where we have no such historical evidence, the complexity of culture is almost wholly ignored by those who use these cultures in their attempts to demonstrate the origin and course of development of human institutions.

I have now fulfilled the first purpose of this address. I have tried to indicate that evolutionary speculations can have no firm basis unless there has been a preceding analysis of the cultures and civilisations now spread over the earth's surface. Without such analysis it is impossible to say whether an institution or belief possessed by a people who seem simple and primitive may not really be the product of a relatively advanced culture forming but one element of a complexity which at first sight seems simple and homogeneous.

Before proceeding further I should like to guard against a possible misconception. Some of those who are interested in the ethnological analysis of culture regard it not only as the first, but as the only, task of the anthropology of to-day. I cannot too strongly express my disagreement with this view. Because I have insisted on the importance of ethnological analysis, I hope you will not for a moment suppose that I underrate the need for the psychological study of customs and institutions. If the necessity for the ethnological analysis of culture be recognised, this psychological study becomes more complicated and difficult than it has seemed to be in the past, but that makes it none the less essential. Side by side with ethnological analysis there must go the attempt to fathom the modes of thought of different peoples, to understand their ways of regarding and classifying the facts of the universe. It is only by the combination of ethnological and psychological analysis that we shall make any real advance. To-day, however, time will not allow me to say more about this psychological analysis, and I must continue the subject from which I have for a moment turned aside.

Having shown the importance of ethnological analysis, I now propose to consider the process of analysis itself and the principles on which it should and must be based if it in its turn is to have any firm foundation. In the analysis of any culture a difficulty which soon meets the investigator is that he has to determine what is due to mere contact and what is due to intimate intermixture, such intermixture, for instance, as is produced by the permanent

[1] I may note here that Mr. Lang, after having considered this problem from the purely evolutionary standpoint ("Anthropological Essays presented to E. B. Tylor," p. 203), concludes with the words, "We seem lost in a wilderness of difficulties."

[2] *Zeit. f. Ethnol.*, 1905, xxxvii., 28, and "Zur australischen Religionsgeschichte," *Globus*, 1909, xcvi., 341.

[3] See especially *Zeitsch. f. Ethnol.*, 1909, xli., 340.

blending of one people with another either through warlike invasion or peaceful settlement. The fundamental weakness of most of the attempts hitherto made to analyse existing cultures is that they have had their starting-point in the study of material objects, and the reason for this is obvious. Owing to the fact that material objects can be collected by anyone and subjected at leisure to prolonged study by experts, our knowledge of the distribution of material objects and of the technique of their manufacture has very far outrun that of the less material elements. What I wish now to point out is that in distinguishing between the effects of mere contact and the intermixture of peoples, material objects are the least trustworthy of all the constituents of culture. Thus in Melanesia we have the clearest evidence that material objects and processes can spread by mere contact, without any true admixture of peoples and without influence on other features of the culture. While the distribution of material objects is of the utmost importance in suggesting at the outset community of culture, and while it is of equal importance in the final process of determining points of contact and in filling in the details of the mixture of cultures, it is the least satisfactory guide to the actual blending of peoples which must form the solid foundation of the ethnological analysis of culture. The case for the value of magico-religious institutions is not much stronger. Here, again, in Melanesia there is little doubt that whole cults can pass from one people to another without any real intermixture of peoples. I do not wish to imply that such religious institutions can pass from people to people with the ease of material objects, but to point out that there is evidence that they can and do so pass with very little, if any, admixture of peoples or of the deeper and more fundamental elements of the culture. Much more important is language; and if you will think over the actual conditions when one people either visit or settle among another, this greater importance will be obvious. Let us imagine a party of Melanesians visiting a Polynesian island, staying there for a few weeks, and then returning home (and here I am not taking a fictitious occurrence, but one which really happens). We can readily understand that the visitors may take with them their betel-mixture, and thereby introduce the custom of betel-chewing into a new home; we can readily understand that they may introduce an ornament to be worn in the nose and another to be worn on the chest; that tales that they tell will be remembered, and dances they perform will be imitated. A few Melanesian words may pass into the language of the Polynesian island, especially as names for the objects or processes which the strangers have introduced; but it is incredible that the strangers should thus in a short visit produce any extensive change in the vocabulary, and still more that they should modify the structure of the language. Such changes can never be the result of mere contact or transient settlement, but must always indicate a far more deeply seated and fundamental process of blending of peoples and cultures.

Few will perhaps hesitate to accept this position; but I expect my next proposition to meet with more scepticism, and yet I believe it to be widely, though not universally, true.[1] This proposition is that the social structure, the framework of society, is still more fundamentally important and still less easily changed except as the result of the intimate blending of peoples, and for that reason furnishes by far the firmest foundation on which to base the process of analysis of culture. I cannot hope to establish the truth of this proposition in the course of a brief address, and I propose to draw your attention to one line of evidence only.

At the present moment we have before our eyes an object-lesson in the spread of our own people over the earth's surface, and we are thus able to study how external influence affects different elements of culture. What we find is that mere contact is able to transmit much in the way of material culture. A passing vessel, which does not even anchor, may be able to transmit iron, while European weapons may be used by people who have never even seen a white man. Again, missionaries introduce the Christian religion among people who cannot speak a word of English or any language but their own, or only use such European words as have been found necessary to express ideas or objects connected with the new religion. There is evidence how readily language may be affected, and here again the present day suggests a mechanism by which such a change takes place. English is now becoming the language of the Pacific and of other parts of the world through its use as a *lingua franca*, which enables natives who speak different languages to converse not only with Europeans, but with one another, and I believe that this has often been the mechanism in the past; that, for instance, the introduction of what we now call the Melanesian structure of language was due to the fact that the language of an immigrant people who settled in a region of great linguistic diversity came to be used as a *lingua franca*, and thus gradually became the basis of the languages of the whole people.

But now let us turn to social structure. We find in Oceania islands where Europeans have been settled as missionaries or traders perhaps for fifty or a hundred years; we find the people wearing European clothes and European ornaments, using European utensils, and even European weapons when they fight; we find them holding the beliefs and practising the ritual of a European religion; we find them speaking a European language, often even among themselves, and yet investigation shows that much of their social structure remains thoroughly native and uninfluenced, not only in its general form, but often even in its minute details. The external influence has swept away the whole material culture, so that objects of native origin are manufactured only to sell to tourists; it has substituted a wholly new religion and destroyed every material, if not every moral, vestige of the old; it has caused great modification and degeneration of the old language; and yet it may have left the social structure in the main untouched. And the reasons for this are clear. Most of the essential social structure of a people lies so below the surface; it is so literally the foundation of the whole life of the people that it is not seen; it is not obvious, but can only be reached by patient and laborious exploration. I will give a few specific instances. In several islands of the Pacific, some of which have had European settlers on them for more than a century, a most important position in the community is occupied by the father's sister.[1] If any native of these islands were asked who is the most important person in the determination of his life-history, he would answer, "My father's sister"; and yet the place of this relative in the social structure has remained absolutely unrecorded, and, I believe, absolutely unknown, to the European settlers in these islands. Again, Europeans have settled in Fiji for more than a century, and yet it is only during this summer that I have heard from Mr. A. M. Hocart, who is working there at present, that there is the clearest evidence of what is known as the dual organisation of society as a working social institution at the present time. How unobtrusive such a fundamental fact of social structure may be comes home to me in this case very strongly, for it wholly eluded my own observation during a visit three years ago.

Lastly, the most striking example of the permanence of social structure which I have met is in the Hawaiian Islands. There the original native culture is reduced to the merest wreckage. So far as material objects are concerned, the people are like ourselves; the old religion has gone, though there probably still persists some of the ancient magic. The people themselves have so dwindled in number, and the political conditions are so altered, that the social structure has also necessarily been greatly modified, and yet I was able to ascertain that one of its elements, an element which I believe to form the deepest layer of the foundation, the very bedrock of social structure, the system of relationship, is still in use unchanged. I was able to obtain a full account of the system as actually used at the present time, and found it to be exactly the same as that recorded forty years ago by Morgan and Hyde, and I obtained evidence that the system is still deeply interwoven with the intimate mental life of the people.

If, then, social structure has this fundamental and deeply seated character, if it is the least easily changed, and only changed as the result either of actual blending of

[1] There are definite exceptions in Melanesia; places where the social structure has been transformed, though the ancient language persists.

[1] See "Folk-Lore," 1910, xxi., 42.

peoples or of the most profound political changes, the obvious inference is that it is with social structure that we must begin the attempt to analyse culture and to ascertain how far community of culture is due to the blending of peoples, how far to transmission through mere contact or transient settlement.

The considerations I have brought forward have, however, in my opinion, an importance still more fundamental. If social institutions have this relatively great degree of permanence, if they are so deeply seated and so closely interwoven with the deepest instincts and sentiments of a people that they can only gradually suffer change, will not the study of this change give us our surest criterion of what is early and what is late in any given culture, and thereby furnish a guide for the analysis of culture? Such criteria of early and late are necessary if we are to arrange the cultural elements reached by our analysis in order of time, and it is very doubtful whether mere geographical distribution itself will ever furnish a sufficient basis for this purpose. I may remind you here that before the importance of the complexity of Melanesian culture had forced itself on my mind, I had already succeeded in tracing out a course for the development of the structure of Melanesian society, and after the complexity of the culture had been established, I did not find it necessary to alter anything of essential importance in this scheme. I suggest, therefore, that while the ethnological analysis of cultures must furnish a necessary preliminary to any general evolutionary speculations, there is one element of culture which has so relatively high a degree of permanence that its course of development may furnish a guide to the order in time of the different elements into which it is possible to analyse a given complex.

If the development of social structure is thus to be taken as a guide to assist the process of analysis, it is evident that there will be involved a logical process of considerable complexity in which there will be the danger of arguing in a circle. If, however, the analysis of culture is to be the primary task of the anthropologist, it is evident that the logical methods of the science will attain a complexity far exceeding those hitherto in vogue. I believe that the only logical process which will in general be found possible will be the formulation of hypothetical working schemes into which the facts can be fitted, and that the test of such schemes will be their capacity to fit in with themselves, or, as we generally express it, "explain" new facts as they come to our knowledge. This is the method of other sciences which deal with conditions as complex as those of human society. In many other sciences these new facts are discovered by experiment. In our science they must be found by exploration, not only of the cultures still existent in living form, but also of the buried cultures of past ages.

And here is the hopeful aspect of our subject. I believe our present store of facts, at any rate on the less material sides of culture, to form but a very small part of that which is yet to be obtained, and will be obtained, unless we very wilfully neglect our opportunities. Waiting to be collected there is a vast body of knowledge by means of which to test the truth of schemes of the history of mankind, not only of his migrations and settlements, but of the institutions and objects which have arisen at different stages of his history and developed into various forms throughout the world.

And this brings me to my concluding topic. I have tried to show that any speculations concerning the history of human institutions can only have a sound basis if cultures have first been analysed into their component elements, but I do not wish for one moment to depreciate the importance of attempts to seek for the origin and early history of human institutions. To me the analysis of culture is merely the means to an end, which would have little interest if it did not show us the way to the proper understanding of the history of human institutions. The importance of the facts of ethnology in the study of civilised culture is now generally recognised. You can hardly take up a modern work dealing with any aspect of human thought and activity without finding reference to the customs and institutions of savage or barbarous peoples. It is becoming recognised that a study of these helps us to understand much that is obscure in our own institutions or in those of other great civilisations of the present or the past. Further, there can be no doubt that we are only at the threshold of a new movement in learning which is being opened by this comparative study.

It is a cruel irony that just as the importance of the facts and conclusions of ethnological research is thus becoming recognised, and just as we are beginning to learn sound principles and methods for use both in the field and in the study, the material of our science is vanishing. Not only is the march of our own civilisation into the hitherto undisturbed places of the earth more rapid than it has ever been before, but this advance has made more easy the spread of other destroying agencies. In many parts of such a region as Melanesia, it is even now only from the old men that any trustworthy information can be obtained, and it is no exaggeration to say that with the death of every old man there and in many other places there goes, and goes for ever, knowledge the loss of which the scholars of the future will regret as the scholars of the past regretted such an event as the disappearance of the library of Alexandria. There is no other science in the same position. The nervous system of an animal, the metabolism of a plant, the condition of the South Pole, for instance, will a hundred, or even a thousand, years hence be essentially what they are to-day, but long before the shorter of those times has passed, most, if not all, of the lower cultures now found on different parts of the earth will have wholly disappeared or have suffered such change that little will be learnt from them. Fortunately, the need for ethnographical research is now forcing itself on the attention of those who have to deal with savage or barbarous peoples. Statesmen have begun to recognise the practical importance of knowledge of the institutions of those they have to govern, and missionary societies are beginning to see, what every wise missionary has long known, that it is necessary to understand the ideas and customs of those whose lives they are trying to reform. Still, we must not be content with these more or less official movements. There is ample scope, indeed urgent need, for individual effort and for non-official enterprise. It is not all who can go into the field and do the needed work themselves, but there are none who cannot in some way help to promote ethnographical research. We have before us one of those critical occasions which must be seized at once if they are to be seized at all: the occasion of a need which to future generations will seem to have been so obvious that its neglect will be held an enduring reproach to the science of our time.

SECTION I.

PHYSIOLOGY.

OPENING ADDRESS BY PROF. J. S. MACDONALD, B.A., PRESIDENT OF THE SECTION.

THE special difficulties of physiology are well known to a large section of my audience, but it may be permissible to illustrate them by reference to an individual case. Take for example those small capsules which are found in the kidneys at the very summit, so to speak, of the problem of renal secretion. These small bodies each occupy a space of less than two thousandths of a cubic millimetre. Within their interior they contain several different kinds of blood-vessels that represent the structures of greatest mechanical interest when dealing with the circulatory system, omitting, of course, the heart. This almost complete sample of the circulatory mechanism, itself formed of a congeries of parts and unitary mechanisms, is enclosed by two or three thousand cells of specific glandular function. Every one of these cells again is a complex of mechanisms about which we cannot rightly think until we reduce our conceptions to the level of molecular dimensions. Enclosed then in this minute space, within a mass that weighs two thousandths of a milligramme, lie quite a series of the problems in which physiology is interested.

The difficulties occasioned by this minuteness of parts, and by the manner in which they are complexly mixed together, render direct investigation of single problems possible only in the very simplest cases, as, for instance, the red blood corpuscle and the nerve-fibre.

A consideration of the dynamic properties of the red blood corpuscle is perhaps the simplest task in physiology. By the aid of the centrifuge these bodies can be obtained free from the embarrassing presence of other cells, may even be washed and immersed in definite solutions of known value. In addition, these compressed discs—the study of the forces normally compressing them open to research by variations in the quality of the surrounding solutions—contain no nuclear reactions, and but the one material of primary dynamic importance.

Everyone knows, however, that even in this case the dynamic conditions are being investigated largely in an indirect fashion. The material of primary importance, hæmoglobin, is stable except with regard to the one well-defined reaction with oxygen to which it owes its utility. This material may readily be obtained pure and its properties examined in homogeneous solutions, and these properties may again be studied after adding to this solution such secondary substances, lipoids and inorganic salts, as are also present in the red blood corpuscle. In the hands of members of this section such studies are not only increasing our knowledge of the properties of hæmoglobin, but are also rapidly leading to a knowledge of those very dynamic conditions with which it is surrounded when present within its microscopical site in the red blood corpuscle. In this very simple instance, the parts of the mechanism being known, it is possible to arrange them in such a fashion as to limit our conceptions of the way in which they are actually arranged within the body.

In cases of greater complexity, where no doubt in course of time the same method of indirect attack will be adopted, in preparation for this event, the necessities of the moment largely confine our attention to a discovery of the various parts present in these mechanisms. In fact, the first requirement is a knowledge of the micro-chemistry of these more complex structures, that is to say, a precise knowledge of the chemical materials distributed in minute spaces of microscopical dimensions. It is well known that my predecessor in this honourable post, Prof. Macallum, of Toronto, has contributed largely to our knowledge of these matters, and that he further assisted us to a right conception of the forces in action between these minute masses of material by his excellent Presidential Address to this Section.

Thinking of the body as no more than a collection of chemical reactions, this elaborate separation of parts in a multiplicity of extremely small spaces protects the individuality of a certain large number of reactions, whilst at the same time securing a rich maintenance of contact with supplies of raw material and a ready means for separating the end-products of reactions from the materials in reaction at each point. Every nucleus, surrounded by its constellation of secondary chemical reactions, is thus given certain limits of size, surface, territory, and environment. These are physical necessities of arrangement possible within the conditions of solution met with in the body, and no doubt largely due to physical states developed by each reaction—that is to say, that the products of each reaction exert a physical influence and produce characteristic physical arrangements. It is not without interest to realise that cell-growth, and the increase in nuclear surface with which it is attended in cell-division, is apparently initiated at every centre by what is doubtless a physical process, and what, as Loeb has shown us, may be accelerated by definite physical change. Such effects of growth are best studied in those early days of enormous expansion when the ovum increases to one thousand million times its original weight, and it is at this time that these separative physical consequences of chemical reactions are most apparent.

During this primary expansion not only have the reactions of nuclear matter been extended to occupy some hundred million times more mass, but it is also true that they have been modified in a very large number of ways, and doubtless this as the consequence of special conditions, extrinsic conditions, existing at the time of formation of each separate part. These modifications are largely shown by differences in appearance and structure, and are each attended by some difference in the function of typical groups of cells. A singular persistence in the similarity of structure and function exhibited by successive generations of similarly placed cells is no doubt sometimes due to the maintenance of those special extrinsic conditions which occasioned their initial modification. In these cases reversion to an original type may occur on immersion in formerly pre-existent conditions, and indeed a whole series of different structures make their appearance as the conditions are further variously modified, as is sometimes seen in the regeneration of parts.

There is, however, seen in some cases a greater degree of persistence, studied, for example, in malignant growths, which is largely retained even when the extrinsic conditions are greatly modified; and in such cases there has doubtless occurred some elimination and refinement—that is to say, rather an abstraction than an addition of character—as the consequence of the initial modification.

In certain places in the adult, physical conditions due to the modification and acceleration of chemical reactions are still frequently provocative of nuclear growth and subdivision: thus in the tonsils, follicles, patches, and lymphatic structures generally, that are embedded in the surface of the alimentary canal. These structures, characterised by their great wealth of nuclear material, experience great nuclear change, to which they are largely stimulated by chemical substances derived from foreign organisms. Specifically affected by each chemical substance, they are probably the site of manufacture of specific neutralising substances that are driven from these sites of activity into the portal system almost as soon as the substances exciting their appearance are driven in from the absorbent surface of the alimentary canal.

In other places in the adult, however, such conditions never recur after a certain date in development. In these places the nuclear material has been so refined as to be irresponsive to conditions that accelerate and modify the reactions of nuclear material in other parts. Permanent sites of monotonous nuclear activity are formed and maintained in such places until the moment when some unusually extreme condition still further limits their activity and terminates their existence. It is significant, too, that this may happen when the condition is not sufficiently extreme similarly to cut short the reactions of other parts.

Now the latter case is typically illustrated by reference to the nervous system, which is thus seen as the site of a severely limited quality of chemical activity. That it is also restricted in amount may be further emphasised by reference to the relatively minute quantity of nuclear material which is present in this system. Thus it is probable that if a direct comparison between the cells of the nervous system and the lymphoid cells to which I have alluded were possible, the essential difference found would be a difference between the stability of certain chemical material in the one case, and a frequently modified wealth of chemical reaction in the other; so that of the two, the nervous system would be the more comparable with the red blood corpuscle.

Thus, if when reviewing the wide array of function in which the nervous system participates, we are led to foresee for each of its cells a great variety of chemical change, or, if when surveying the great differences in function of the organs of the body we are led to expect typical chemical differences between those several parts of the central nervous system with which they are individually associated, we are arrested by this clear evidence of a universally distributed monotony of simple chemical state.

It is true that certain drugs affect some groups of cells within this system more readily than others. None of these instances are, however, of such a kind as to demand the inference that there was any essential difference between different groups of cells. In most cases, indeed, it is probable that differences in relative quantity, and in such simple factors as relative state of solution, are responsible for these effects. Thus there is nothing to refute the statement that all the cells of the nervous system contain chemical materials of an exactly similar kind. Just as every liver-cell is like every other liver-cell in its general chemical character, so in the nervous system are all the cells chemically alike.

Glancing from the liver-cell to the nerve-cell, however, there is at once seen a marked difference of a kind we have not yet considered. The chemical experiences of the liver-cell are multifold, but in the main alike for each cell, and it is thus not surprising that the chemical reactions are in the main the same in every cell, no matter how multifold they may be. The physical experiences of the liver-cells are similarly the same for each cell, and we are not surprised that in physical appearance there is as monotonous a similarity between all the cells in the liver as there is a monotonous chemical similarity between all the cells in the nervous system. In the nervous system, however, there is no monotony in the physical character of the cells. It is a notable physical fact that the cells of the nervous system have diverse shapes and sizes, and still more so that these are such as to bring them into a kind of physical relationship observed in no other epithelial organ. It is a notable physical fact that cells originally separated by considerable distances are brought into close contact by a growth of processes, and that they are in this way arranged into chains forming definite paths for the transmission of physical influence through this system.

Before attempting to explain the manner in which physical conditions give rise to this arrangement, I must briefly sketch the differences in physical state which may be met with in these cells. Thus there are the states of excitation, of rest, and of inhibition. I may simplify matters by saying that there are reasons for considering excitation as associated with an increase in pressure, either due to a temporary increase of particles in motion within the solutions of the cell or to some acceleration in the motion of particles initially present. In rest these particles are in their normal quantity and have their normal motion. During inhibition the particles are decreased in number, or have a retarded motion. Associating excitation with an increase, inhibition with a diminution, and rest with normal degrees of molecular activity, we shall not be far away from the facts.

Everyone is aware that increased molecular activity is associated with a tendency to break bounds, or when taking place behind resistant but distensible bounds with a tendency to expand the region of activity. Thus it happens that the excited cell tends to grow in size, whereas, on the other hand, the inhibited cell tends to diminish, and the resting cell to remain unaltered. These several proceedings are possible so long as the surface membranes of the cells, or of structures within them, which form bounds resistant to the pressure of molecular activity, are at the same time porous to water molecules; and this we know is within limits true—namely, that the cell is enclosed by such semi-permeable membranes. Thus when the excited nerve-cell grows in size, and the region of molecular activity is thus increased, the materials within the cell are diluted by an admission of water.

Attention is now directed to the probability that there is some kind of material in solution within the cell which takes no part in this increase of molecular activity; is, on the other hand, retarded in its motion by agglutination into colloidal clusters, and may finally be precipitated. I, for my part, have no hesitation in saying that there is every probability that this is indeed the primary phenomenon of excitation, this precipitation. Leaving that point, however, alone, it is probable that this tendency towards precipitation occurs. This material, precipitated and diluted, thus loses some of that mass-action formerly holding in check its formation by the particular chemical reaction that is always tending to produce still more of it. More of this material is thus produced within the excited cell, and is in turn precipitated, and still more and more. We may therefore think of these excited cells as laying down a structure which I will ask your permission to describe as a cuticle. The nerve-fibre is the cuticle of the nerve-cell. Once give it such a name, as is in part justifiable, and no one will be surprised that these structures are pushed out to an extraordinary distance from their parent cells, and that their length is measured not like other details of cell-structure in thousandths of millimetres, but sometimes in metres, and therefore on a scale with units one million times larger than usual.

If we entertain this idea, that nerve-fibre growth is proportional to excitation, we are prepared for the statement that the physical characters of the cells within the nervous system and their relations to one another are all due to their relative experience of incidents of excitation. We face the fact that their chemical work is of a universally monotonous type, a drearily slow and respectable type, and that their physical features and arrangements are capable of very simple explanation.

Now structure is everywhere the outcome of function, and those functional developments that lead to the growth and differentiation of structure contain the most interesting and most fundamental problems of physiology. If it is thought that the main relationships of parts within the nervous system are fixed from an early date of development, it would then seem that to the physiologist the nervous system is a place of very limited interest. But this is by no means the case, the relationship of parts is by no means a fixture within the nervous system. In so far as it is fixed, it is the sign of the orderly action of circumstance upon the structures of the body, and the result rather than the cause of the monotony of existence. There is, however, no need to labour this point or to debate our interest in this system. One portion of the nervous system is the seat of the mind, a fact to which I will return later. The whole of it is the very essence of the unity of the organism containing it. It is the rapid transmission of physical states through its individual nerve-fibres, and the modifications in transmission determined by passage into its constituent cells, which serve to weld the actions of the several parts of the body into that phase of common action which is suited to the necessities of the moment.

That there is no moment during life when there are not many paths through the central nervous system engaged in this business of transmission is a statement of commonplace realised by all. There are not, however, in my opinion, a sufficiently large number of persons sufficiently impressed by that greater truth, discovered and analysed by Sherrington: that no path is thus busy without there being at the same time some other path maintained in a condition of enforced rest. Whenever the system is excited at one part it is also inhibited at another, and it is this phenomenon that lies at the root of the harmonious effects produced by this system, and forms the means whereby action suspends antagonistic action.

When considering the influence of states of excitation upon the growth and arrangement of structures within this system, it follows then that I cannot afford to omit some proper consideration of the manner in which this phenomenon of simultaneous inhibition may be explained, and of its influence on the growth and arrangements of structures. To get a clearer view of this process we must think in detail of the probable nature of the structures involved in the simplest case of transmission through the system. It is indeed a simple thing to form a picture of the track entering the system, the structure called the afferent neurone. Here we have a long length of cuticle, or nerve-fibre, stretching right from the surface where it is liable to stimulation by change in circumstance, or—more complicated case, but very usual one—by the maintenance of circumstance. This afferent neurone is mainly cuticle. It is true that its cell-body is placed like a hump somewhere on its back, but this is no more than an index that it is never inhibited. Thus from the site of change of circumstance right into the nervous system transmission is of the simplest kind, since all we know of this nerve-fibre is that it transmits most of the excitations it receives at a rapid pace and without loss from one end to the other. We can therefore see the excitation planted by it into every cell with which it comes in contact within the system. By some of its branches it plants this excitation into nerve-cells, whose nerve-fibres pass out to reach the site of action. It is a simple matter again to picture this first set of efferent neurones as receiving an excitation which they then transmit. That there is a certain complexity in the process is a fact with which we are not at present concerned.

But now, what about the site of antagonistic action, the parts that are held in a state of enforced rest? To them also lead perfectly similar efferent neurones, incapable of producing any other effect in the site of antagonistic action than that of exciting it or transmitting excitations towards it. We must therefore conclude that it is this second set of efferent neurones that are inhibited and maintained in a condition of enforced rest. How then does the change transmitted into the several branches of the afferent neurone, having the same character as it invades every branch, succeed in causing diametrically opposite conditions in two groups of perfectly similar efferent neurones? There is but one answer to this question, namely, that transmission into the second group must be through some intermediate mechanism which reverses the character of the change. Now I have no hesitation in naming definite structures in the nervous system as being alone those to which we can impute this reversal, namely, certain intermediate neurones which have a way of being interpolated between afferent and efferent neurones. Such neurones are seen in the cord sometimes sending their main nerve-fibre towards efferent neurones placed on the other side of the cord, and in the cerebellum the large cells of Purkinje are seen to be approached by afferent nerve-fibres in this double fashion; one set reaching them directly, the other set indirectly through intermediate neurones. We shall then picture neurones with short nerve-fibre processes as placed in these paths that are inhibited, and as sometimes responsible for this singular reversal of the transmitted excitation.

In this connection, too, we must deal briefly with another fact observed by Sherrington, that certain drugs, tetanotoxin and strychnine, affect these intermediate mechanisms in such a way that they lose their power of reversing the character of change transmitted through them. When these drugs are applied to any part of the nervous system action and antagonistic action are simultaneous consequences, and the stronger wins. Of the greatest interest, too, is the fact that this disturbance of the process of reversal may be obtained in a graduated manner by the application of such drugs in varied strengths of solution. It is thus clear that there is nothing peculiar about the nerve-fibre portion of these intermediate neurones, since when given excitations to transmit they transmit them, although it is so frequently their normal business to transmit inhibitions. Clear, too, that their cell-bodies frequently inhibited like those of the efferent neurones may also with a slight modification of condition tend towards excitation, or, as a matter of fact, be excited, again like the efferent neurones. There is no difference discoverable here between these two sets of cells other than a difference of degree. The one salient fact demanding explanation is this difference under normal conditions in which the efferent neurones are seen as excited by identically the same character of transmitted change that inhibits the intermediate neurones.

Now it would be a simple matter to show that all these points might be dealt with adequately on the assumption that nerve-cells invariably contained a mixture of two materials, existing in different proportions in different cells, each of which was forced into a diametrically opposite physical state to the other as the result of changes in physical conditions of the kind transmitted by nerve-fibres.

It is of interest then that there is definite reason to suppose that within nerve-cells there are always two substances which seem to have their states diversely affected by different conditions. One of them is the characteristic constituent of what I have been irreverently terming the cuticle, the nerve-fibre; and the other a complex material which apparently represents the primary product of nuclear activity, and is spoken of as the material of Nissl. It may seem a weak point in my use of the term that this cuticle-stuff is found within the cell-body. Perhaps so, but perhaps also not so; the point is not worth discussing.

The point really worth discussion is as to whether it is true that these substances are affected in diametrically opposite ways by the same change, just as if, for example, one of them was possessed of acid and the other of basic characters; so that the basic was precipitated, and the acid dissolved by the addition of an alkali: since if they exhibit any opposite behaviour in the presence of the transmitted excitation, then it is indeed probable that their admixture is responsible for many of the orderly vagaries of transmission through nerve-cells. I am proceeding as if this is really true to a consideration of its influence on the development of nerve-cells.

Imagine a developing afferent neurone in contact with two other neurones, but by different extents of its surface, so that it transmits a larger quantity of change to the one than to the other. In both cases it affects an algebraical sum of opposing properties, and we might think of it as effecting a compression and an expansion. Now let there be the slightest difference in the forces required to compress and to expand, and it might readily happen that the effect of a minimal dose might be to produce an algebraical sum in favour of compression, whilst a maximal created a general effect of expansion. One of these cells then might be habitually excited and grow a cuticle traversing considerable distances in the central nervous system; whereas the other is inhibited until the accumulation of charges previously received add up to the dose required to tip the algebraical sum in favour of excitation, and then first commences the growth of a short nerve-fibre.

This, however, involves the assumption that these cells of both classes store up all the transmitted energy they receive, that they do not leak, do not transmit, and thus grow their nerve-fibres from the effects of accumulation. Within certain limits this supposition is sound, since we are familiar with that summation which is a leading feature in nerve-cell conduction. Below a certain definite quantity of charge they do not leak, and are found by a second impulse arriving some little time after an apparently ineffectual predecessor in a new state, so that the new-comer is effectual. Now if no new-comer arrives in time we must suppose the energy due to the first as having affected the growth of the cell in one direction or another—that is to say, in one direction if it produced the change characteristic of excitation, and the other if producing to a minimal degree the change characteristic of inhibition. It is legitimate, too, to suppose these limits as set by the capacity and extent of excitable contacts. The larger the extent of contact the sooner and the more effectual must be the leakage. Thus we may readily picture the excited neurone as growing more and more cuticle until this growth is checked by the number, extent, and capacity of the excitable contacts made in course of growth. When a certain measure of growth has occurred we may suppose that residual charges below the margin of leakage are now only just sufficient to maintain the district of cuticle that has been laid down. We have therefore encountered the limits of growth of the nerve-fibre.

As for the second cell, which we have considered as mainly inhibited. In it the mass-action of the products of nuclear change is diminished and we must think of it as enlarging its cell-body by an increased nuclear activity; possessed of a short cuticle but an extending cell-body, possessed of no more than a short nerve-fibre and an extensive set of dendritic processes. As each new dendritic process makes contact with a new branch of the excited afferent neurone its growth will be more and more limited. We have here, then, encountered the limits of growth of the nerve-cell.

There is no difficulty other than that due to the short time at my disposal in compounding these statements so as to cover the whole scale of differential cell-growth, and within each cell of the relative growth of its several parts, that is observed within the nervous system. I may perhaps be permitted this abrupt closure to a development of the probabilities underlying the following expression of opinion.

I hold it as probable that all the individual structures of the nervous system, and so in the brain, have just so much difference from one another in size, in shape, and in function, as is the outcome of that measure of purely physical experience to which each one of them has been subjected: and that the physiological function of each one of them is of the simplest kind. The magnificent utility of the whole system, where the individual units have such simplicity, is due to the physically developed peculiarities of their arrangement in relation to one another, and to the receptive surfaces and motor-organs of the body.

To relieve the monotony of this discussion, let us turn away for a moment to the consideration of certain physical mechanisms found in the body, external to the central nervous system; mechanisms that are placed, so to speak, upon the front of that system so that they are capable rather of affecting it than of being affected by it, and this to such a degree that we must suppose them as rather assisting in the development of the central nervous system than as being assisted to their development by the central nervous system. There are, for example, the lens systems of the eyeball and the sound-conducting and resonant systems of the ear. Now, in dealing with the central nervous system, the suggestion was made that it was developed by just such physical conditions as are transmitted through it in its adult form. In dealing with the eyeball, it is clear that an admission of this sort is not easy. During the evolution associated with natural selection the eyeball is formed by light. It must be so. The eye is as perfect an optical instrument as could be made with a full knowledge of the part played by matter and special arrangements of matter in reflecting, refracting, and absorbing light. Long prior to the development of man, who at a later date acquired sufficient knowledge of these properties to aid him in the formation of crude lenses, there was to be found upon the general surface of the animal world lenses of very great perfection, in fact, complete cameras. Had the first optician then known what was in him he would have been saved infinite pains. Had he indeed known even the lens systems formed on the leaves of plants. Surely there is no escape from the statement that either external agency cognisant of light, or light itself, has formed and developed to such a state of perfection this purely optical mechanism, and that natural selection can have done no more than assist in this process. The influence of natural selection depends upon the frequency of variations, and it is important that there is no variation that has not behind it some cause. In this special case of variation in physical arrangements, it is indeed probable that the most frequent cause of variation would be exerted by physical conditions, since in this case the factors that are thus introduced by variation are not distinguished by any chemical peculiarity. Thinking of the few possible physical causes of variation, there can be little doubt that light itself would produce some change in this optical instrument, and that the variations produced by light would be just those more likely to be adapted to the subsequent traverse of light than such as were accidentally produced by some other physical cause. Accepting such a statement, we may say that in the course of development light formed the eye by its action upon such tissues as those of which the general surface of the body is composed. Now in just the same way there can be little doubt but that sound formed the sound-conducting and resonant portions of the ear. We may perhaps go further than this statement, and say that not only has this mechanism placed in front of the central nervous system been formed in this fashion, but that the parts of the central nervous system behind it have been formed by physical effects transmitted from the ear through this keyboard where sound is transformed into nervous impulses. Thus also, when thinking of the semicircular canals, representing as they do the portion of the surface of the body that is still normally excited by just such changes as affected the whole surface of the animal when its habitat was the sea, there is no need to doubt the view that the structures found there were formed by fluid friction; and that the cerebellum was formed as a consequence of the stimuli which have been transformed by these surface organs into nervous impulses.

But if this was the case during the evolution which led up to man, what occurs in the development of the individual? We can afford to admit the possibility that sound may approach the embryo and that fluid friction is responsible for effects observed, but light is obviously no factor in this process. Here there is no doubt that the eyeball is developed into a very perfect optical instrument in the absence of light, and we must ask: What is the force that in this case imitates the action of light? Some force must be held as arranging the several parts of the eyeball in front of the developing retina, and it is probable that before discovering it we should have to refer to the properties of the retina for an answer. We might indeed say that since the retina is a portion of the central nervous system generally characterised by the undoubted possession of electrically charged surfaces, it is always possible that this cause is of an electrical nature. Leave the statement general and it takes the form that the optical mechanisms of the eyeball are formed in the absence of light by some other definite physical cause or series of causes. Place it temporarily in the form where I would like to leave it, both on general grounds and on the evidence that its development is modified by the addition or subtraction of electrolytes: in the absence of light it is probable that orderly electrical forces arrange the developing parts of the eyeball. Now this is really not a surprising statement, since light may probably, even in the first case, be transformed into some other form of energy such as electrical energy when primarily shaping these surfaces. In any case, however, this is the view, that the individual eyeball is an instrument formed probably by some simple set of physical conditions from which light is absent, and that it is used, after a certain abruptly occurring date, by light, a force that has, up to this time, had no access to it, and yet finds it most beautifully formed for its special use.

Now development after all is rather a retrograde affair. Consider the fertilised ovum and its possibilities. A physical condition determines an increase in the chemical activity of the nucleus. At the same time an addition is made to the chemical material of the nucleus. The nucleus then divides and forms an ever-increasing site of modified chemical activity. Each new portion of this extending site is surrounded by cell bodies subjected to different sets of physical conditions, and in touch with different qualities and quantities of states. We may take it as certain that not any of the many extraordinary events which take place happen without definite cause. For example, this must be true of every single cell division. Any particular cause bearing similarly on successive generations of cells, or, as we may say, allowed to prolong its action upon a special mass of changing nuclear reaction, must finally produce states of an almost irreversible kind, eliminating possibilities of variation. Thus we might describe the ovum as a possible source of countless variations, whereas it is probable the cells of formed tissues are greatly limited in this possibility. Early in these processes, it is true, a portion of still fairly aboriginal material is shut off, and through some cause protected from changes leading to violent modification; and to this share there still appertains much of the variable character of the original ovum. Part of the remainder, perhaps the whole of the remainder, is under the heavy grip of circumstances which differ widely in different cases, and is step by step slowly driven into something of that deadly monotony of condition which is so evident in the red corpuscle, in the nerve-fibre, and in a somewhat less degree in the nerve-cell. Knowing this, then, we shall only with difficulty be induced to credit any particular kind of subordinate cell with any special character. When, for example, it is stated that the mind is, so far as the evidence will permit the statement, associated with the brain, and with no other part of the central nervous system, we can hardly get behind this statement. Mind, in man, is associated with the brain. It is conceivable that in animals it may be associated with parts of central nervous systems so simple in arrangement that we single out nothing from them as the brain. It is conceivable that there is something of the kind, indeed, in humble uni-cellular organisms. But in man mind is associated with the brain.

There is also the point that even in the case of the brain, such phenomena as sleep and deep anæsthesia familiarise us with the fact that the mind is not necessarily always associated with the brain, but only with this when in a certain condition.

Now there is no scientific evidence to support or to rebut the statement that the brain is possibly affected by influences other than those which reach it by the definite paths proceeding from the sense-organs and from the different receptive surfaces of the body. It is still possible that the brain is an instrument traversed freely, as the ear, by sound, by an unknown influence which finds resonance within it. Possible, indeed, that the mind is a complex of such resonances; music for which the brain is no more than the instrument, individual because the music of a single harp, rational because of the orderly structure of the harp. Consider such a possibility, and the analogy which I have prepared in dealing with the eyeball is seen to have some meaning, inasmuch as an instrument shaped in the embryo by a certain set of conditions may in due course of time become the play of some new influence which has taken no immediate part in fashioning it. I will not dwell upon the point behind this statement, that I find it difficult to refrain from using the word "soul."

If, however, such a view is considered, it must be said that there is no evidence that any individual physico-chemical phenomenon is developed within the brain that is not developed within other parts of the nervous system, and in a more confused manner, indeed, within the limits of every living cell. It is some special arrangement of dynamic states that must be held to form the special characteristic of the waking brain, and it should be possible in time to define the peculiarities of those special arrangements whereby we are assuming that the mind is, so to speak, caught.

It is true, too, that there are great difficulties offered to the expanded presentation of a statement which suggests a mysterious influence provocative of mind as possessed apparently of something of the nature of a physical force, since it is held to be constrained in certain peculiarities of physical environment to behave in a special way. It is, indeed, almost clear that this influence must be held to affect those physical surroundings, since there is little doubt that mind, *per se*, affects human conduct and animal behaviour, just as it is impossible to conceive mind, where present, as exerting no influence in natural selection. This, although the risks of the environment must always play the greater part in natural selection, and the influence of the mind be conceived as only secondarily affecting the organism through the intervention of the nervous system, or through mechanisms that are substituted for that system. Admitting these facts, we should in this case be obliged to regard mind resonating amongst the distributed dynamic states of the brain as influencing them in a way that might possibly be demonstrable in any physical apparatus closely imitating those states and their distribution.

Then, again, one of the main objections to a suggestion of this kind is that the condition might involve a transformation of energy which should have been discovered as an otherwise unexplainable quantity in the energy equations of the body. There may, however, be no real necessity to conclude that any transference of energy would be involved in such a process. The distribution of dynamic states in the central nervous system which are suggested as playing the part of resonators is, as I have already related, a distribution of opposite states. If we consider how these opposite states, excitation and inhibition, are arranged in any given case, it is seen that the installation of an equal number of excitations where inhibitions were present, and of inhibitions where excitations were present, will give rise to a new pattern of a very different meaning. Now such a change in the distribution of states might entail either no more than the transmission of nervous impulses, a process in which exceedingly small quantities of energy are dissipated, or, indeed, an actual cessation in the transmission of certain nervous impulses, since it is one of the curious features in these states that the one tends to recoil into the other. We might, indeed, make the assumption that an alteration in the setting of the instrument, such as was attended with a change in consciousness, was always attended by this cessation of nervous impulses, so that a brilliant display of mind might be associated with no increase in the transformation of physical energy, but actually with a diminution in the transformation. Under cover of such an assumption it might be held that this mysterious influence of which I have spoken absorbed instead of contributing energy to the system, or that it diverted energy without loss from one part of the system to another.

Now, in my opinion, there is no one at the present time who is in a position to discuss the energy transformation of the central nervous system. Further, there is certainly no one capable of dealing with such peculiarities as might arise in the energy transformation of that part of it, the brain, which is associated with the mind. There are many points to be cleared up, as, for instance, the extraordinary relationship of the central nervous system to the general muscular system, upon which I might be allowed for a moment to dwell. The fibres of skeletal muscle form the largest site of energy transformation from the oxidation of food or fuel, a site in which apparently no such transformation takes place without a coincident exhibition of characteristic muscle function and the performance of some mechanical work, and are dominated in this transformation by impulses discharged from the ventral portion of the central nervous system. This exhibition of function is invariably the cause of a dispatch of nervous impulses into the central nervous system again, along the nerve-fibres passing into its dorsal portion. Now, since the energy set free in muscle is out of all proportion to the small sum of energy transmitted from the nervous system, it is capable, amongst other things, of dispatching back again to the central nervous system a compensating or even an additional sum of energy. The musculature might then be supposed to reinforce the nervous system. Until such points are given their due importance, it would be ridiculous to dogmatise about the energy equations of the central nervous system, and to discuss the amount of energy expended in the performance of movements, or stored in the absence of movements, within this system.

I will not labour these points, upon which I can throw no light, but put forward this expression of belief rather than opinion to explain an attitude revealed in the remainder of this address, and not as based on evidence or in any way a statement of demonstrated or demonstrable fact. The essential point for the moment is this—that there is some loophole for the view that mind is not directly associated with life or living matter, but only indirectly with certain dispositions of dynamic state that are sometimes present within certain parts of it. It is a point of view not without interest to physiology, since it would leave that science free to consider all phenomena present in such forms of life and living matter as carry no suggestion of an association with mind, as nothing more or less than physico-chemical phenomena, which, when thoroughly investigated, would be completely translatable into scientific terms. Then, too, when there is evidence of mind, the view is that it represents a force acting from without upon what is still no more than matter involved in certain chemical and physical states. Incidents of function would, in such a view, pass straightway into the realms of physical and organic chemistry, requiring special methods of investigation alone, because of the localisation of processes and punctate states in minute microscopical parts not readily removed from their surroundings into selected experimental surroundings of the same value.

We are at liberty, then, to deal with this series of physico-chemical experiments, boldly giving each observed difference in circumstance a possible importance in the determination of observed differences of character, and each difference in character a probable explanation in terms of simple differences in circumstance; we may boldly consider the causation of variations, and use the term "natural selection" as equivalent to the physico-chemical limits to the successful maintenance of each experiment. Let us, for example, begin with the blood.

It is at once legitimate, in the first place, to ask how this blood tissue has arisen from variation in the chemical reactions of nuclear material. The argument runs that some ascertainable cause must have produced a material variation which has been preserved by natural selection, and quite probably, too, by the persistence of the cause over some long period in the history of nuclear material. There is no harm for the moment in surveying causes and temporarily fixing upon one that seems to possess greater appropriateness than any other. Therefore I suggest that we take this main characteristic of nuclear material in the blood tissue, that it is engaged in the production of a pigment, and that the most efficient cause determining pigment production is the action of light. Remembering that we are probably dealing with nuclear matter in general from which this particular material has been split off and set aside by subsequent causes, we can admit this postulate. The pigment-forming propensity of blood is thus taken as probably due to the initial action of light upon nuclear material placed near the surface of the body, and there exposed to the action of light.

Our next step is to discover any probable fact which, favoured by natural selection, might drive into the interior of the body nuclear matter that had been so modified by light that it persisted in the formation of pigment; in other words, Why should any pigment-forming reaction ever be removed from the direct influence of light, and a valuable transformer of radiant energy be thus driven into a position of disadvantage within the interior of the body?

Now let us consider the value of those particular instances of pigment formation which have been allowed by natural selection to persist upon the surface of the body. These successes represent experiments that have not been detrimental to the general mass of chemical reactions which form collectively what we call the organism, and we are entitled to ask, In what way are these successes likely to differ from the failures? If we take the possibility that some pigments convert all the light which they absorb into heat, and receive per unit of surface a share of solar radiation measured as seven thousand horse-power per acre, we have a picture that the body surface might thus be exposed at any one time to the transformation of an excessive amount of energy. The square metre of surface which might in the human body be exposed at one time to the sun would, provided with such pigment, absorb in one hour as much heat as is produced by the whole body in twelve hours, and the temperature of the body might be raised a further 20° C. by this means in one hour. It must, then, be an important matter in which the risks of life maintenance have certainly acted along the lines of natural selection, that such pigments, transforming the total energy they receive into heat, must be driven from any place they have temporarily occupied on the surface of living matter. As in the plant, in successful cases this energy must be largely diverted into chemical work. It would not, then, be surprising that certain modes of pigment formation have been eliminated, and that certain other modes, finding a utility of some other kind, have been retained by natural selection in seclusion within the interior of the body. Let us take it that blood-pigment represents such a mode of reaction, and that its influence is mainly to convert light into heat, and, secondarily, in some degree to determine the separation of oxygen from certain compounds, thus also performing some chemical work when under the influence of light.

Now since it is also part of the general line of argument that it was inefficient in this chemical aspect, and on that account driven from the surface of the body, it must be held as incapable of separating oxygen from more stable compounds; and we find an explanation for the fact that it is engaged upon unstable compounds of oxygen, not absorbing much energy in the process of reduction nor liberating much on oxidation.

Since, in regard to all such chemically dynamic pigments, with a utility dependent upon their constant association with some molecular group in which a corresponding reduction process can be effected, it will never be surprising to find this group actually forming a constituent part of the molecule. It is, then, not surprising to find these two qualities, pigment and unstable oxygen compound, present in hæmoglobin, nor to find in this special case that the secondary process has assumed the position of major importance, and that hæmoglobin is no longer of use as a pigment so much as an unstable compound of oxygen. Following this line of reasoning, there is nothing extraordinary in the discovery that such pigments, utilised as "oxygen carriers" within the interior of the body, are found in other situations than in blood—for instance, in the nerve-cells of certain animals and commonly in skeletal muscle. Blood tissue represents a special set of nuclear reactions possessed of this persistent quality in marked degree.

If it seems strange that the initial formation of blood in the embryo and its maintained formation in the adult persist in the absence of light, let us return to the instance of the eyeball. Of that instrument it was said that, although it was originally formed by light, yet in the mammalian embryo its formation was continued in the absence of light. Here it was necessary to think of some replacement of one cause by another, and not difficult to adopt such a suggestion, since even in the initial process it was probable that light produced its effects subsequent to transformation into some other form of energy, such as electricity. In that particular case this idea of forces, and substituted forces, in action, is capable of being formulated in fashion readily understood, because of the ease with which we can think of arrangements in gross parts being determined by such forces. Here in this new case we are, however, thinking of parts of a different order of magnitude, a fact which I can best illustrate by reference to a single red blood corpuscle occupying a one-tenth millionth of a cubic millimetre, and containing in a one-hundredth part of that space as many molecules of hæmoglobin as there are present red blood corpuscles in one cubic millimetre of blood—that is to say, five millions.

Now there is, in reality, no difficulty in considering some electrical agency as limited in its action to the minute dimensions in which each pigment-forming reaction is in process; some electrical machine such as, for example, might be energised by electrons derived from the dissolved molecules of a pigment-salt; such a machine as might be capable of transforming both light and heat into electrical energy, and which would maintain a process in the absence of light at the cost of energy obtained in the form of heat.

When thinking of the persistence of such reactions as this, initiated in this way by the action of certain primary causes that are then subsequently removed without any cessation of the reaction, we are concerned with one of the fundamental properties of living matter. Everywhere in living matter numerous instances of this property are being discovered, as in the study of immunity and of protection from infection. Nor is there reason to believe that this persistent quality of such variations will not finally be explained in terms of physical chemistry. The main characteristic of living matter is that it contains machines formed by electrolytes distributed upon the complex surfaces of matter in a state of colloidal solution and in the presence of competitive solvents, and that such machines are multiplied within it. Some of these mechanisms are arranged and perfected by the action of physical conditions operative on the surface of living matter, as, for instance, light. Some by energy derived from internal sources, but in a form that embodies conditions originally derived from the surface. Some are primarily due to internal disturbances in the equilibrium of these complex solutions produced by those chemical reactions which take place there.

Now, returning to the grosser characteristics of blood, we find it possessed of other characters curiously reminiscent of the surface of the body, and especially of glandular invaginations from the surface. Thus it is everywhere confined by cells spread upon its surface, the endothelium, which limit its relationship to the general mass of the interior. Its new-formed cells are again passed into an internal core covered by these surface cells, and from this situation, except as a result of violence, they do not pass. We might, in fact, compare the blood to a gland in which the red blood corpuscles were seen as a secretion occupying a lumen which represents the original external surface of the body. I do not wish to lay any emphasis on this point except in so far as it renders clearer this thought: that blood covered by its endothelium represents a single tissue which tends, like any gland, to grow into every interstice of the body, where the conditions of mechanical pressure permit. I shall render the point clearer by saying that the blood capillaries are no more and no less than blood-tissue.

In its early days this blood-tissue, or, if you will, this capillary network, is pushed into each portion of the body by pressure due to its growth. In its later stage the tissues surrounding it, which form the muscular coat of the heart and the walls of the blood-vessels, are arranged into an external mechanical system providing a new pressure, which still further tends to push the blood-tissue into every available space, a process such as, for example, takes place in tumour development and in the granulation tissue present in wounds.

It is a general postulate that cells long exposed to constant conditions may come to be stamped by those conditions. Special change takes place from the time when the blood grew onward by pressure of its own growth to the time when this movement is more clearly determined by the mechanism of the circulatory system, and divergent results occur in different localities of the blood-tissues which can be attributed to the differences in these causes of onward motion. Thus where growth is the leading cause of this progressive motion, as, for example, in the development of bone, the blood-tissue is later found occupying spaces that are cut off from the general mass except by lines of communication too small to transfer a full share of pressure from the circulatory mechanism. In this isolated space the blood-tissue preserves to a greater degree powers of intrinsic growth than in those places where the tissue bears the brunt of new forces. It is true that other factors induced by the new motion given to the fluid core of this tissue complicate this matter. This notwithstanding, it is, however, clear that certain definite differences in circumstance, and those principally of a purely mechanical kind, leave the blood-tissue in one district possessed of aboriginal properties which are in a large degree lost elsewhere.

As to that other tissue, which forms the circulatory system and embraces the blood-tissue, there is here little room for doubt that the structures found are the result of special local conditions acting upon originally similar cells, and little room for the suggestion that samples of several different kinds of special formative cells are driven into these positions by destiny and not by mechanics. This is an old theme, well extended and illustrated by exact observation, especially by Thoma; that in every blood-vessel the arrangement of structures is an almost immediate guide to the conditions of pressure met with in that vessel. Let us proceed through the structures in the walls of a small artery, giving a definite mechanical origin to each tissue. The elastic tissue first met with in the inner coat of the vessel is the result of periodical or intermittent pressure. In the large arteries, where intermittent pressure is the main phenomenon, and where its influence is felt right through the thickness of the wall, this elastic tissue has the major share in forming the structure of the wall. In the small artery, where the total quantity of the causative phenomenon is small, the innermost structures are affected most. This inner zone, formed under the influence of intermittent pressure, protects from intermittency the tissue formed by constant pressure, involuntary muscle. Both with regard to this tissue and with regard to the elastic tissue, it is to be remembered that the conformation of the material embracing the cylindrical mass of blood-tissue is such as to convert incidents of internal pressure into tension as well as pressure. Thus we may say that elastic tissue varies in quantity with the value of intermittent, involuntary muscle with the value of constant, pressure, and tension. On the outer surface of this case, still more protected by the mechanical value of the structures internal to it, but submitted to the traction and friction of surrounding tissues, comes white fibrous tissue. Again, when windows have been cut in the outermost case of large vessels, leaving the inner case intact, and thus destroying the tensile character of the mechanical conditions and permitting the internal pressure to hammer through these windows, they have been found closed in by plaques of cartilage, and even by true bone. It is true that the explanation offered for such results has been different from that here inferred, it being held that cells specially formative of cartilage and bone have been admitted to this new situation by the brusque strokes of operating instruments. True, too, that the complete ligature of vessels has been followed by developments of bone in unexpected places beyond the walls of the blood-vessels, as in the pelvis of the kidney; but how can you make a better internal hammer and better provide for its constant use than, for example, by tying the renal artery? Let me state it as probable that white fibrous tissue, involuntary muscle, and elastic tissue are produced by tension, whereas bone and cartilage are formed by pressure. If we credit the main statement that they are first formed from originally similar cells by circumstances special to each case, and that the difference lies in the circumstances and not in the cells, together with the statement illustrated in former paragraphs that modifications tend to persist when once introduced, we shall probably get near to the truth of the matter. Now it is impossible to leave this special case of the circulatory system—special because here there is no doubt that mechanical conditions are operative from the earliest days of development and from the first beat of the heart—without touching upon two points: the origination of the heart itself and the formation of valves.

Picture the blood-tissue in its earliest form as a lacery of networks distributed in a layer throughout the embryo, protected better by the greater thickness of material covering the central longitudinal axis than at the edges. In the absence of this protection the peripheral parts are subjected to incidents of compression which set pressure-waves travelling along the meshes of this blood-tissue in all directions from the point primarily affected. Since these waves will tend to be reflected within the tissue, we can think of the disturbance caused by them as possessed of a certain periodic recurrence of rhythm determined in its time-relations by the dimensions of the tissue, and as undergoing a tendency to modification as these dimensions are increased. In the earliest stages, whilst the distance from edge to edge is less than one millimetre, giving these waves the very slow rate of one metre per second, we can imagine these periodic changes in pressure exerting their influence upon the tissues enveloping the blood with a frequency of one thousand per second. It is again not difficult to imagine that the protection afforded to the central axial portion, through which each wave must pass in transit from edge to edge, allows us to think of the tissue there as more pressed upon than pressing, so that in this place our attention is directed to the enveloping tissue-cells receiving this rapidly recurring stimulation and being especially affected in the process into a formation of cardiac muscle. Since cardiac muscle resembles so closely in many minute particulars skeletal muscle, which is developed mainly under the influence of electrical discharge from the central nervous system, we must, if consistent, suppose that here, too, the same force is in action. In this, however, there is no difficulty, since it is a simple matter to explain how mechanical pressure may give rise to electrical change, as, for instance, when a nerve is excited by mechanical pressure. There is, however, probably this distinction between skeletal and cardiac muscle, namely, that the electrical stimulus provocative of the latter is of a high frequency and approximates nearer to what I might describe as a constant electrical current. The heart is not by any means the only site of formation of rhythmical contractile tissues, and in these other cases, so far as I am acquainted with them, a similar state of formative conditions may be described. Thus at those points where the conical apices of that second network, the lymphatic system, are forced by pressure of external parts to flow towards certain points in this blood-tissue, rhythmical lymph-hearts are described as developed in these protected sites prior to the final penetration of the blood-tissues, and the forced commingling of lymph with the fluid core of the blood.

Now give the agency that I have described a certain direction, crediting it with a graduated qualitative influence in different parts in correspondence with the date of their formation and with the altering dimensions of the blood-tissue as a whole, and the peristaltic character of the movement subsequently performed by the contractile tissue may be completely explained. Let us, then, suppose that such a peristaltic contractile mass is formed in these enveloping tissues, and consider how it will affect the blood, again enveloped by its own endothelial cells. When driven forward away through this site, the endothelial covering, which at first will slip upon the enclosing heart, later will acquire some attachment by the precipitation of fibrous tissue due to repeated friction. The movement of the endothelial cells is now only partial. They have become describable no longer completely as the surface cells of blood-tissue, and are in a measure the internal covering of the heart, its "tunica intima." With each pulsation this intima is dragged onwards to some slight degree behind the blood column to which it originally belonged. There is no difficulty whatever in thinking that valves are necessarily formed at every point where the conditions are such as tend to break up the blood column into separate parts. Indeed, we may look particularly at every place where valves are found in the blood-vessels and see similar factors at work. In the arterial system there is no projection forwards of interrupted columns of blood, nor is this the case in any of those veins in which no valves are found, as notably in those veins that are protected from partially distributed results of external pressure by the rigidity or by some other incident in the conformation of the framework in which they are found.

And now let us turn to the main function of this developing system, which is to drive the blood in continual sequence past tissues that contribute to it and tissues that abstract from it certain chemical materials, and let us select the main incident, namely, the carriage of oxygen from the lungs to other parts. That this is a main incident is clearly shown by the fact that the red corpuscles which form so important a feature in the structure of blood are formed in a number directly equivalent to this demand, that the blood should be capable of transferring a certain quantity of oxygen. Thus if these structures are lost by hæmorrhage, or rendered less efficient by the presence of carbon monoxide, or when circumstances for the acquisition of oxygen are peculiarly difficult, as on high altitudes, their formation is proportionally accelerated. That negative pressure of oxygen governs blood-production is a statement which will bear some inspection.

Now here we have a function which for its perfect performance is dependent upon another machine, the respiratory mechanism, which in its turn is governed by a different but correlated factor, namely, the carbonic-acid pressure in the blood. In this case we may say that the positive pressure of carbonic acid dominates the quantity of the respiratory activity. It is well known now that this statement has been set on firm ground.

It is interesting, then, to observe how these two mechanisms are brought into exact correlation by the simple fact that the lung surface, a portion of the respiratory mechanism, is formed accurately to a measure provided by the volume of blood dispatched from the heart, and therefore probably by that second growth of blood-tissue which I have spoken of as due to pressure from the heart. The surface of the lungs is some eighty square metres. The heart at each stroke sends into the lungs somewhere about 100 cubic centimetres of blood containing red corpuscles within a total surface also of eighty square metres. Here, then, we have a mechanical link connecting these mechanisms that is obviously forged by an incident of use.

Within the central nervous system, where development mainly affects the shape and distribution of structures rather than their chemical quality, affecting thus what we might call the geography of the system, interesting geographical facts attest to the same forged linkage of mechanisms. Thus, for example, we have the so-called "sympathetic system," offering at first view a curious anomaly to the more usual, somewhat segmental, distribution of nerve-fibres, since from the region of the cord related to the trunk of the body nerves pass through this system to control tissues placed in the head and in the limbs. This anomalous geographical fact is, however, at once explained when we regard the part played by this sympathetic control in the several parts of the body as merely subservient to the interests of locomotion. Under its influence the eye is set for out-of-door, or, if I might say it, for out-of-cave, vision. The heart is accelerated. The glandular organs, with the exception of those useful in times of much exertion and heat production, like the sweat glands, are set at rest, or else the motor organs of special importance in their sphere of influence are quietened. Regarding the matter in this light, there is an obvious convenience of geographical fact in the situation of this instrument midway between those parts of the central nervous system that are swept at this very time by nervous impulses dominating the movements of the limbs, just as there is some convenience in the chemical linkage which has been discovered between the different parts of this sympathetic system that further tends to permit their unison of activity.

On the other hand, when the muscles are at rest and the condition of the body is of the indoor description, the eye is set for close vision, and various glandular organs are allowed to conduct their functions under the influence of nervous mechanisms placed at some distance from the disturbing centres of nervous activities that are used in locomotion.

Doubtless this useful distribution of parts within the nervous system must find an explanation in the same terms as must the dynamic anatomical relation to which I have drawn attention as linking up the respiratory and circulatory systems, namely, the fact that the heart sweeps past the surface of the lungs at each stroke red corpuscles that have the same extent of surface as the lungs. In both cases it is true that the right adjustment of the several parts of this machine has been arrived at as a consequence of use, and that these mechanical linkages are due to circumstances of a purely physical and chemical nature.

In conclusion, I might say that these instances have been selected to illustrate my opinion that some of the experiments of greatest interest to physiology are in process of conduction within the normal body, and are to be observed by records imprinted on its structures. In feeling for the keys whereby each set of records may be interpreted, it is necessary that someone should frankly attempt to assign a definite meaning to every incident of structure. That this attempt should be limited by precise thinking goes without saying, and I may be allowed the hope that my transgression outside the realms of precision have not been beyond the tolerance of this section of the British Association for the Advancement of Science.

NOTES.

A DEPARTMENTAL committee (consisting of Mr. Angus Sutherland, C.B., chairman, Mr. J. E. Sutherland, M.P., Mr. H. M. Conacher, Dr. T. Wemyss Fulton, and Mr. J. Moffatt) has been appointed by the Secretary for Scotland to inquire into and report upon the character and national importance of the inshore and deep-sea fisheries of Norway and other countries engaged in the North Sea fisheries, and the efforts made for the development of the fishing and fish-curing industry in all its branches, including (1) the systems of fishery administration, including the constitution and function of the local committees formed for this purpose in Norway and of any similar organisations in the other countries; (2) the facilities provided for research and for educating and training those engaged in these industries, by the establishment of technical schools, museums, laboratories, classes, or other special facilities; (3) the nature of the various means of capture employed and the methods (including any use of State credit) by which fishermen obtain the necessary capital to maintain the efficiency of their vessels and equipment; and to report in regard to each of the foregoing matters whether it would be advisable for similar action to be taken, with or without modifications, in the case of the Scottish fishing industry, and, if so, what means should be adopted.

REUTER messages from Catania state that frequent earthquake shocks, some of which were fairly severe, have been recorded at the Etna Observatory. The records in the seismic apparatus are reported to be almost continuous and very distinct. The volcano is throwing up dense clouds of smoke, and a rain of cinders is falling as far as Catania. There is also a broad stream of lava, which is destroying the vineyards in its path.

ON Saturday, September 9, the aërial post was inaugurated by Mr. Gustav Hamel, one of our most brilliant flyers, who carried a sack of letters in a Blériot monoplane from Hendon to Windsor in thirteen minutes. Starting at five minutes to five in the afternoon, he arrived at his destination, nineteen miles distant, at 5.8, so that his speed, the wind being behind him, was about 105 miles an hour. The other aviators who should have started were prevented by the thirty-mile wind, and no further deliveries took place until Monday, when Messrs. Greswell and Driver carried six mail-bags over in the early morning. M. Hubert, in an effort to follow, had a bad fall, damaging his machine and severely injuring himself. The affair has aroused great interest, so much so that it is as well to sound a word of warning and say that the aëroplane post is neither