forms of crystals, and the method of notation best adapted for international use, will probably be discussed in the Conference.

I have thus briefly touched upon some of the salient points which occur to the mind when taking a cursory view of an Exhibition such as the present. In doing so I have no doubt passed over many instruments and appliances of even greater importance than those which I have thus succinctly mentioned, and have probably left untouched many topics of the highest interest. Among the subjects, however, which will be discussed on each day of our Conferences there will, I hope, be a sufficient variety to give occasion for any one to call attention to any special features of novelty in the collection. What I have ventured to say must be regarded as merely a short introduction to communications of far greater value, from which I will no longer detain you.

## SECTION—BIOLOGY

*Opening Address by the President, Prof. J. Burdon Sanderson, M.D., LL.D., F.R.S.*

IT having been made a part of the duty of the chairman of each of the sections into which this Exhibition is divided to deliver an opening address, I had no difficulty in selecting a subject. I propose to place before you a short and very elementary account, addressed rather to those who are not specially acquainted with biology than to those who are devoted to the science, in which I shall give you a description of a few of the methods which are used in biological investigation, particularly with reference to the measurement and illustration of vital phenomena. You are aware that the Committee, in order to render these conferences as useful as possible, have thought it desirable that we should devote our attention chiefly to those subjects of which the instruments in the collection contribute the best examples.

Now these subjects are, first, the methods of registering and measuring the movements of plants and animals; secondly, the methods of investigating the eye as a physical instrument; and thirdly, the methods of preparing the tissues of plants and animals for microscopical examination. Of these several subjects it is proposed we should to-day concern ourselves chiefly with the first. I will therefore begin by endeavouring to illustrate to you some of the simplest methods of physiological measurement, particularly with reference to the *time* occupied in the phenomena of life, leaving the description of more complicated apparatus to Prof. Donders, who will address you on Monday, and to my friend, Prof. Marey, who is with you now, and who will give you an account of some of the beautiful instruments which he has contrived for this purpose.

The study of the life of plants and animals is in a very large measure an affair of measurement. To begin, let me observe that the *scientific* study of nature, as contrasted with that contemplation of natural objects which many people associate with the meaning of the word "naturalist," consists in comparing what is unknown with what is known. Whatever may be the object of our study—whether it be a country, a race, a plant, or an animal, it makes no difference in this respect, that the process in each of these cases is a process of comparison, a process in which we compare the object studied in respect of such of its features as interest us, with some known standard, and the completeness of our knowledge is to be judged of in the first place by the certainty of the standard which we use; and secondly, the accuracy of the modes of comparison which we employ. Now, when you think of it, comparison with a standard is simply another expression for measurement; and what I wish to impress is, that in biology, comparison with standards is quite as essential as it is in physics and in chemistry. Those of you who have attended the conferences on those subjects will have seen that a very large proportion of the work of the physical investigator consists in comparison with standards. From his work, our work, however, differs in this respect, that whereas he is very much engaged in establishing his own standards and in establishing the relations between one standard and another, we accept his standards as already established, and are content to use them as our starting-point in the investigation of the phenomena which concern us.

Now I wish to illustrate this by examples. The first objects which strike the eye on entering this collection—the collection in the next room—are certainly the microscopes. But you will say, surely the microscope cannot be regarded as an instrument of measurement. In so far as it is an instrument of research and not merely a pastime, it is emphatically an instrument of measurement, and I will endeavour to illustrate this by referring to one of the commonest objects of microscopic study, namely, the blood of a mammalian animal. Now as regards the blood I will assume that everybody knows that the blood is a fluid mass, in which solid particles float. With reference to the form of those particles, all that we see under the microscope is merely a circular outline. If we wish to find out what form that represents we must use methods which are really methods of measurement. By the successive application of such methods we learn that this apparently circular form really corresponds to a disc of peculiar bi-concave shape. But I will not dwell more upon the application of measurement to the form of the corpuscles, but proceed at once to a subject that can be illustrated by an instrument before you for ascertaining the *number* of the corpuscles. It will be obvious to you—even to those who are not acquainted with physiology and pathology—that the question of the proportion of corpuscles which are contained in the blood must be a matter of very great importance to determine. It has been long known that the colouring matter which is contained in the corpuscles is the most important agent in the most important vital processes of the body, because it is by means of it that oxygen, which is necessary to the life of every tissue is conveyed from the respiratory organs to the tissues. This being the case, it is evidently of very great importance both to the pathologist and to the man who interests himself in investigating the processes of nature, to be able to determine accurately what proportion of corpuscles the blood contains. Well, there are chemical methods of doing this. We can do it by determining how much iron the blood contains, because we know that the proportion of iron in the corpuscles is always nearly the same, and by determining the quantity of iron chemically, we can find out how many corpuscles there are in a certain amount of blood. But this is a long process, requiring first the employment of a considerable quantity of blood, and secondly, difficult chemical manipulations and a long time. Now by a method which has been very recently introduced, we have the means of applying the microscope even to a single drop of blood, to a drop such as one could obtain by pricking one's finger at any moment, or could take, in this way, from any patient in whom it might be desirable to ascertain the condition of the blood as regards the number of its solid particles.

The method consists in this. In order that you may understand it I will ask you to fix your attention upon this cube which I draw on the board. Suppose this cube is not of the size actually represented, but that it is a cube of one millimetre, *i.e.*, the $\frac{1}{25}$ part of an inch. How many blood corpuscles do you suppose are contained in a cube of that size? Such a cube we know to contain in normal blood about 5,000,000 corpuscles. Supposing we had a method by which we could count those 5,000,000 particles it is obvious that the task would be endless, and even if we were to take a cube $\frac{1}{125}$ part of that size, namely, a cube of one-fifth of a millimetre in measurement the enumeration would be somewhat easier, but still impossible, for the number contained in such a cube would be enormous; and therefore, it is necessary to diminish the bulk of the blood in which you make your counting very much further. This you can only effect by a process of dilution. In order to get at your result you have not only to diminish the bulk of the quantity which you contemplate and in which you count, as much as possible, but also to dilute the blood so that your liquid may contain a very much smaller proportion of blood corpuscles. You dilute it then 250 times, and in this way you divide the cube of a millimetre from which you started, into about 31,000 parts, and count the blood corpuscles in the thirty-one thousandth part of a cubic millimetre. Supposing you find it contains about 160 corpuscles you will find by calculation that they amount to about 5,000,000 in the whole cube from which you started. This being the case the question is how we effect the division. We do it in this way: You first dilute your blood in the exact proportion required, and for this purpose one uses the apparatus which is on the table. You take a capillary pipette which will only take an extremely small quantity, in fact, a cubic millimetre of blood. Then having filled your pipette you discharge it into a little eprouvette, into which has been introduced 250 times, or rather 249 times, the bulk of some liquid with which blood can be diluted without its corpuscles being destroyed. Having thus got this diluted liquid which contains blood in the proportion I have mentioned, all that you have to do is to place under the microscope a layer of a definite thickness—one-fifth of a millimetre—and count the number of corpuscles in a square of the same measurement. That is effected by this very ingenious arrangement, which was introduced by M. Potain, and has been finally perfected by Messrs. Hayem and Nachet. The way it is done is this: An object-glass is covered by a perforated plate; the perforated plate is of the thickness I mentioned, namely, exactly one-fifth of a millimetre. Consequently if a very small drop of the mixture of the blood with serum (the diluting liquid) is placed within this space, you have a layer of the thickness I have mentioned which you can contemplate. You can cut off a cubic millimetre of that stratum of blood perfectly easily by means of a micrometer eyepiece, and in that way accomplish the required enumeration. You have in short before you a quantity of liquid which contains about the thirty-one thousandth of a cubic millimetre of blood, and consequently would obtain, if the blood were normal, 160 corpuscles. These can be very readily counted, and the whole process can be done in a very few minutes—in a much shorter time, in fact, than I have taken to describe it to you, and you get results which are not only equal to those obtained by chemical investigation, but more accurate. This, I think, is a good example of the application of the microscope as an instrument of measurement to an important question.

The next subject that I wish to draw your attention to is a different one. It is a question of measuring the time occupied in certain simple processes in which the nervous system is concerned. The examples I am going to give you are entirely derived from the physiology of man, and relate to the phenomena which we observe in ourselves. The measurement to which I wish to draw your attention is the measurement of the time occupied in what we call in physiology a "reflex" process. You may reasonably ask that I should endeavour to explain what a reflex process is, and the only way, or at any rate the readiest way in which I can do this is by giving you an example. Supposing this blank card, which has written on it previously some word, say the word "reflex," were suddenly turned over by a second person. It is agreed that at the moment I see the word upon it, I say the word "reflex." In that act it is obvious that there are three stages. First, the reception of the impression by my eye produced by seeing the word; secondly, the process which goes on in my brain in consequence of seeing it; and thirdly, a message sent out from my brain to the muscles which are concerned in articulation, by means of which certain movements are produced which give rise to the sound which you recognise as the word "reflex." That is one example. Let us now take another which is simpler. We cannot take one better than the act of sneezing. Some snuff finds its way into the nose; an impression is received, a change is produced in one's nervous centres, and in consequence of that central change, a certain number of muscles are thrown into the action recognised as sneezing. These are different examples of reflex action. The brain, the highest part of the nervous system, has to do with the first; whilst the other is one in which the nervous centres lower down have to do, and consequently it is simpler. The methods which I am going to illustrate to you are methods intended for the measurement of the time occupied in this process. First, let me draw your attention to the circumstance that you have here three stages. You have the stage of reception; the stage corresponding to the changes which take place in the brain in consequence of the reception of an impression from outside; and thirdly, the process by which you convey the effect to the muscles which act. Now let us agree, in speaking of this, to call the impression the "signal," and to call the muscular effect the "event." In that case the question before us is to measure how much time takes place between the reception of this signal by a certain person and the occurrence of the event, namely, the completion of the muscular action. There are a great many questions involved in this: thus you may measure either the whole process or one of its stages. You may measure, for example, either the time occupied by the reception, the time occupied by the discharge, or, on the other hand, the time which is occupied by the changes which take place in the centre itself. In the first instance I gave you just now—the example of reading a word aloud—the time occupied in the reception is extremely short, and the time occupied in the discharge is also extremely short. Popularly the whole thing is done as quick as thought, but, comparatively, the time the brain takes in going through these changes which connect the reception of the impression with the discharge is a very considerable one. All this we can make out with absolute accuracy by methods of measurement. Most of these methods are founded on this principle, that we measure the duration of a voltaic current which is closed at the moment the signal is given, and opened or broken at the moment that the act takes place. There are a great many instruments constructed on this principle, of which you will find illustrations in the next room. The general principle involved in all of them is shown on this diagram. In the simplest form you can give to such an apparatus you must have a surface of paper so placed that it shall pass horizontally by the point of the lever, and at a uniform rate; thus, for example, it may pass at the rate of 1 metre in the second. Supposing this to be the case, it is obvious that if you arrange the electro-magnet so that when you close the current a certain mark is made, and that at the moment of the break of the current when the magnet ceases to act, another mark is made, you will have a tracing on the surface of the paper which indicates the time. So long as nothing is going on, the paper receives a horizontal mark, but at the moment the signal is given you have the point of the lever descending. At the moment the act takes place the lever assumes its original situation, and you have again a horizontal line. That is the general principle of the apparatus. Now for its action. We have here a voltaic circuit and a key by which we can give the signal. I shall be the subject of the experiment, and you will see what the result is. Here is the recording arrangement. We have two electrical keys, one at the further end intended for making what is called the signal, and one here for breaking, which is placed close to the person who is to be experimented upon. Mr. Page, at any moment he likes, will act upon me by sending an induction flash through my tongue. I shall arrange the electrodes so that they shall be against the tip of my tongue, and at the moment I feel that flash I shall place my finger on the key. Then the clockwork being in motion at the same time, we shall see by the length of the depression in the tracing the duration of the process. If we take different sorts of signals, or if the person to be experimented upon is in different conditions, the time will be very different. Thus we may compare the result which will be produced when I am attending and expecting the signal with the result which will be produced when I am not attending or expecting the signal; or, on the other hand, I may compare those results with that which will be produced when I am expecting it, but Mr. Page, instead of giving it at the time I expect it gives it me at a different time; in that case the time occupied would be longer than in either of the other two cases. A great variety of different cases can be investigated in this way in which we measure the total period occupied in the reflex. The arrangement is perfectly simple. You see when Mr. Page presses on his key, which is the signal key, that a lever is set in vibration and makes a tracing, and at the same moment the voltaic current is made and the coil is acted upon inductively; the result is that an induction flash passes through my tongue which I feel, and the moment I feel it I break the current. Consequently the time between the moment at which Mr. Page makes the current by closing his key and the moment at which I break the current by placing my finger on my key, gives us precisely the time which is occupied by the reflex process. We will make two experiments, first, with the signal expected, and then unexpected; that is, in the one case I shall be on the *qui vive*, and on the other I shall not be so. (The experiments were made accordingly.) We shall now repeat the process, so that instead of my receiving the information of the making of the current by means of the excitation of my tongue, the signal shall consist in my hearing the sound of an electrical bell. In that case we shall find that, although the signal will come in exactly the same way, practically the time occupied will be very considerably longer, showing that a signal received by sound takes longer in producing its effect than one in which the signal is felt by the tongue.

In order to make all this perfectly plain I shall hand round this tracing. You will see there several experiments made with expected and unexpected signals, which show the different results obtained in the two cases.

The next question which arises, and with that I must conclude what I have to say just now, is this:—You will readily see that the exact measurement of time depends upon the rate at which this clockwork happens to be going. I happen to know that it makes twenty revolutions per second. But suppose I do not know that. In fact one would not trust to the accuracy of clockwork for such a purpose. How should I then be able to measure the duration of time so exceedingly short as the one which now concerns us? In order to do this we always come back to a physical standard, to a standard of absolute invariability which we can depend upon as being true. For this purpose we use a tuning-fork which produces vibrations, the rate of which we know, because we know the tone which the tuning-fork produces, and the arrangement which is always used for this purpose is the one shown here. We have turned off the voltaic current we used for signalling, and turned it on the tuning-fork. There are two electro-magnets on either side of the tuning-fork which react upon it, so that the moment you close the current the fork is thrown into vibration and produces its own characteristic note. All that we have to do is, during the time we are making our record, to bring this tuning-fork, which is now in vibration, into such a position that this little brass pointer shall make a tracing against the paper. If you look at the tracing I have sent round you will find there are tracings on it of a fork, which vibrates at the rate of 100 per second, consequently you have nothing to do but to translate the tracings which you have made and which correspond to the duration of the mental process which you have been investigating, into vibrations of the tuning-fork, and you get an exact measurement of the total duration of the process. While I have been doing this you hear the tuning-fork is in vibration, and Mr. Page has made the tracings. After it is varnished it will be sent round and you will see the tracing made by the fork over the traces corresponding to the different experiments we made just now.

I may observe that although the experiments made on that paper were made with myself, you find that the period occupied by the reflex is considerably longer than in the other which I sent round previously. But that one may very easily explain from the abnormal conditions under which the experiment has been made as regards myself.

I intended to go on from this subject to another mode of investigation, namely, to the very beautiful instruments which have been lately introduced for the purpose of measuring the finest differences of bulk in different organs, as for example, in the human arm, by which you can ascertain the condition of the circulation precisely by a very exact registering-measurement of the bulk of the arm;[1] but as there are several other gentlemen now ready to address you, I will defer that till this afternoon. I will now conclude what I have to say by asking you to listen to Dr. Hooker.

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## SCIENCE IN GERMANY

*(From a German Correspondent.)*

HERR v. OBERMAYER has recently communicated a memoir to the Vienna Academy on the relation of the coefficient of internal friction of gases to the temperature. If we accept for the coefficients of friction $\mu$ at $t°$ C., the formula—

$$\mu = \mu_0 (1 + at)^n$$

where $a$ is the coefficient of expansion of the gas, taken as basis of the calculation, then the experiments of Obermayer give the following results:—

| | |
|---|---|
| For Air … … … … … | $n = 0·76$ |
| ,, Hydrogen … … … … | $n = 0·70$ |
| ,, Oxygen … … … … | $n = 0·80$ |
| ,, Carbonic oxide … … … | $n = 0·74$ |
| ,, Ethylene … … … … | $n = 0·96$ |
| ,, Nitrogen … … … … | $n = 0·74$ |
| ,, Protoxide of nitrogen … … | $n = 0·93$ |
| ,, Carbonic acid … … … | $n = 0·94$ |
| ,, Ethyl chloride … … … | $n = 0·98$ |

The coefficient of friction of the permanent gases is, according to these experiments, approximately proportional to the $\frac{3}{4}$-power of that of the coercible gases, and to the 1-power of the absolute temperature.

For temperatures between 150° and 300° C., air gave the same values of $n$ as between the lower temperatures $-21°·5$ and $53°·5$. In the case of carbonic acid a slow decrease of the exponent $n$ with the temperature was perceptible from the experiments.

W.

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## NOTES

ON Tuesday a visit was paid to the *Challenger* at Sheerness by several Fellows of the Royal Society, foreign men of Science, who are in London in connection with the Loan Collection

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[1] The apparatus was fully described subsequently by Mr. Gaskell.