"excursion" indicating a change of potential *in a negative direction* at the movable contact amounting to 3·5 to 5·0 $d$ $\left(d = \frac{1 \text{ De La Rue element}}{100}\right)$.

If a similar plug is applied to the internal surface, so as to cover the whole of it, the result is the same, but the extent of the excursion is somewhat less. Hence it may be generally stated that during the electrical disturbance *the surface of the leaf becomes more negative*[1] *as compared with any other surface of which the potential is constant*, and that on the external surface the change is greater than on the internal. This electrical disturbance is limited to the leaf and ceases at the point dividing the petiole from the isthmus or bridge, by which it is united with the leaf; on the petiole side of this point no sign of electrical disturbance is shown by the electrometer.

For various reasons the authors determined to direct their attention to the middle third of the leaf. The following were selected as representative points of contact:—(1) a point (*i l*) on the internal surface of the leaf equidistant from the three sensitive hairs; (2) a point on the external surface (*e l*) exactly opposite to *i l*; (3) and (4) points on the internal (*i m*) and external (*e m*) surfaces of the midrib, where the line joining the points *i l* on either lobe cuts the midrib; (5) the petiole (*p*); and (6) the bridge or isthmus (*b*) already mentioned. The letter P denotes the potential at any point, and V the variation of the potential during the electrical disturbance.

In four leaves the potentials and variations of the external surfaces of the midrib and lobe were severally in hundredths of a De La Rue cell:—

| | | | | |
|---|---|---|---|---|
| *em* P as compared with *p* P ... | 0 | 0 | 0 | 0 |
| *el* P ,, *p* P .. | 1·6 | 0 | 0 | 1·6 |
| *em* V ... ... ... ... | −5·0 | −6·5 | −4·2 | −4·5 |
| *el* V ... ... ... ... | −2·0 | −6·5 | −4·0 | −4·0 |

The external variation is usually greater than the internal of a corresponding point, and the variation at *e m* is usually greater than that at any other point; thus in six leaves—

$$el\,V = -3{\cdot}6 \quad -4{\cdot}0 \quad -4{\cdot}2 \quad -4{\cdot}0 \quad -4{\cdot}0 \quad -4{\cdot}5$$
$$il\,V = -1{\cdot}5 \quad -1{\cdot}7 \quad -1{\cdot}6 \quad -1{\cdot}8 \quad -2{\cdot}2 \quad -2{\cdot}2$$

and

$$im\,V = -3{\cdot}0 \quad -3{\cdot}5$$
$$em\,V = -5{\cdot}5$$

When a leaf is excited at intervals of a minute or oftener by single shocks from a du Bois-Reymond's induction coil,[2] which are of just sufficient intensity to produce a response, it invariably happens that after a time the electrical variation ceases. The variation can be reproduced either by (1) shifting the needle-points to a fresh spot, (2) by increasing the strength of the induction-current, or (3) by allowing the leaf to rest for a longer interval. With relation to electrical stimuli, it is shown that the excitability of the leaf resembles that of the terminal organs of the higher animals, in this respect, viz., that *relatively feeble* stimuli, if applied at very short intervals and repeatedly, are competent to elicit a response.

If a leaf be excited at short intervals by faradisation, the excitations (makes *and* breaks) being continued each time until an excursion is produced, the combined effects of summation and gradually increasing exhaustion can be readily observed. At first the leaf responds after eight to ten excitations, but gradually the number of excitations required to awaken the tissues to action increases, the effect being postponed for longer and longer periods, until it finally fails to occur. When a leaf is excited at regular intervals by single shocks of such intensity as to be just beyond the limit of adequacy, the effects sometimes become rhythmical.

The time which intervenes between an excitation and the beginning of the electrical disturbance varies in different leaves, and is very much affected by variations of temperature. This time the authors have called the *period of electrical delay*.

As a mean of many experiments it was found that when the fixed electrode was on the petiole and the movable electrode on *e m*, the *delay* was 0·295 second. If the movable electrode was at *e l* or *i l*, the delay varied according to the proximity of the sensitive hair touched to the point of application of the movable electrode. Thus if the movable electrode was at *e l* and a sensitive hair on the same lobe was touched, the delay was 0·231 sec.; but if a hair on the opposite lobe was touched the delay was 0·403 sec, the disturbance having to make its way from the sensitive hair on the opposite lobe through and across the midrib and up to the electrode. It is obvious that by measuring the distance between the hair touched and the electrode we can ascertain, more or less exactly, the rate of the transmission of what may be called the "wave of negative variation" through the leaf. From many experiments, the stimulation being sometimes mechanical and sometimes electrical, it was found that the wave traversed a distance of about 8 mm. in 0·18 sec., or at a rate of about 4·4 centims. per second. When the *period of delay* at *e l* was compared with that at *i l*, it was found that it was shorter at *e l* than *i l*; *e.g.*, in some experiments (the excitation being weak faradisation and the excursions being taken from *e l* and *i l* alternately), the following numbers were obtained:—

| | | | | | |
|---|---|---|---|---|---|
| Inside ... ... | 0·71 | 0·61 | 0·68 | 0·75 | 0·95 sec. |
| Outside ... ... | 0·48 | 0·50 | 0·52 | 0·65 | 0·49 ,, |

Finally, if either *el*, *il*, *em*, or *im* be compared with the bridge *b*, it will be found that the *period of delay* at *b* will be much greater than that at any of the other points;

| | | | | |
|---|---|---|---|---|
| thus *el* | 0·26 | 0·24 | 0·12 | 0·18 sec. |
| bridge (*b*) | 0·87 | 0·65 | 0·85 | 0·83 ,, |

In normally active leaves, in which the disturbance is first seen about a sixth of a second after mechanical stimulation, the excursion attains its maximum in about one second, and the whole disturbance is over in about two seconds after the excitation, so that the electrical disturbance is entirely over before the mechanical effect begins, and consequently occurs in a period which in muscle is called the period of latent stimulation.

All these periods are, however, very much modified by temperature, being shortened if the temperature is raised (within certain limits), and lengthened if the temperature falls.

The following is one of several tables given in the paper, illustrating the effect of temperature on the periods of delay, maximum and total duration of the electrical disturbance:—

| | | In leaf at ordinary temperature. | In warm chamber at 45° C. | Cooled by proximity of a block of ice. |
|---|---|---|---|---|
| Time in seconds after excitation. | To beginning of excursion | 0·23 | 0·11 | 0·44 |
| | To maximum ,, ... | 1·46 | 0·79 | 1·68 |
| | To end ,, ... | 2·2 | 1·37 | 2·94 |

## THE SPONTANEOUS GENERATION QUESTION[1]

AT the meeting of the French Academy of Sciences on January 29, M. Pasteur read the following reply to Dr. Bastian:—

Dr. Bastian, in reply to the communication which I made on January 8, along with M. Joubert, addressed to the Academy last Monday a long note, in which he still contrives, I think, to elude the main point of the debate. In our communication of January 8 there was one word of prime significance, *pure potash*; but, what is surprising, in the reply of three pages of Dr. Bastian there is not even allusion made to that condition of purity, which was everything.

I shall make a new attempt to recall the English *savant* to the criterion, from which he cannot escape, do what he will.

The discussion was raised by his statement, that a solution of boiled potash caused bacteria to appear in sterile urine at 50°, after it had been added to the latter in quantity sufficient for exact neutralisation. Dr. Bastian concluded that he had thus discovered the physico-chemical conditions of the spontaneous generation of certain bacteria.

This is my reply to the learned London professor of pathological anatomy:—

I defy Dr. Bastian to obtain, in presence of competent judges, the result to which I have referred, with sterile urine, on the sole condition that the solution of potash which he employs be pure, *i.e.* made with pure water and pure potash, both free from organic

[1] It is interesting to note that the surface of a frog muscle, during the electrical disturbance which precedes contraction, becomes *positive*.

[2] Two steel needles sheathed in glass, and bound together, were used as exciting electrodes, the points of the needles being thrust through the epidermis of the leaf.

[1] Continued from p. 314.

matter. If Dr. Bastian wishes to use a solution of impure potash, I freely authorise him to take any in the English or any other Pharmacopœia, being diluted or concentrated, on the sole condition that that solution shall be raised beforehand to 110° for twenty minutes, or to 130° for five minutes.

This is clear enough, it seems to me, and Dr. Bastian will understand me this time.

The following reply to the above was read at the Academy on February 12:[1]—

At the *séance* of January 29, M. Pasteur, in reply to a communication which I had made at the previous *séance*, challenges me to cause sterile urine to ferment by the addition of a suitable quantity of *liquor potassæ*, "on the sole condition that this solution shall be raised beforehand to 110° for twenty minutes, or to 130° for five minutes."

In order that M. Pasteur may not attribute to me the least desire "to elude the main point of the debate," and also with the view of testifying the respect which I consider due to the opinions of so distinguished an investigator, I hastened at once to accept his challenge. During the last week I have repeated my experiments several times, and with a degree of precaution going much beyond the severity of the conditions prescribed by M. Pasteur.

I repeated them at first with liquor potassæ which had been previously raised to *110° C. for sixty minutes*, and afterwards with liquor potassæ which had been raised, in the same manner, to *110° C. for twenty hours*. The results have been altogether similar to those produced upon sterile urine by liquor potassæ, which has been raised only to 100°, when added in suitable quantity; that is to say, *in twenty-four to forty-eight hours the urine was in full fermentation and swarmed with bacteria*. The specimens of urine employed had a specific gravity ranging from 1,020-1,022, and they required about 3 per cent. of liquor potassæ for neutralisation.

If M. Pasteur has found himself unable to renounce his interpretation of my experiments on account of "la preuve manifeste," which I have cited in my last communication (p. 189 of the *Compt. Rend.*), I hope he will frankly accept the disproof of his views furnished by the experiments which I have now the honour of communicating to the Academy, and which have been made in acceptance of his own challenge. These experiments I hope in a short time to repeat before competent judges.

*Verbal Reply of M. Pasteur.*

I thank Dr. Bastian for having accepted the proposition which I made to him at the *séance* of the 29th of January. In consequence, I have the honour to beg the Academy to appoint a commission to report upon the fact which is under discussion between Dr. Bastian and myself.

I hope that Dr. Bastian will seek to induce the Royal Society of London, of which he is a member, to nominate a commission for the same purpose.

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At the *séance* of February 19, it was announced that MM. Dumas, Milne-Edwards, and Boussingault have been appointed to constitute a commission charged to express an opinion on the fact which is under discussion between Dr. Bastian and M. Pasteur.

## OUR ASTRONOMICAL COLUMN

THE NEW COMET.—Elements of the new comet calculated by Dr. Hartwig of Strasburg from observations to February 15 are almost identical with those given in this column last week. Observations have been made at Berlin, Copenhagen, Leipsic, Lund, Paris, and Strasburg. On the 16th the comet appeared to the unaided vision a little brighter than the well-known cluster in Hercules, and in the telescope presented itself as a round nebulosity, ten minutes in diameter, with a small central nucleus: this apparent measure corresponds to a real diameter of 77,000 miles.

The following ephemeris for every second midnight, Greenwich time, may facilitate observations. The intensity of light is assumed, as usual, to be represented by the reciprocal of the product of the squares of the distances from the earth and sun: it will be remarked that on the last date, the degree of brightness is only one-sixth of that on the first date of the ephemeris:—

| | Right Ascension. h. m. | North Polar Distance. ° ′ | Distance from the Earth. | Intensity of Light. |
|---|---|---|---|---|
| March 3 | 3 39·5 | 27 12 | 0·613 | 2·06 |
| 5 | 3 51·1 | 29 56 | 0·683 | 1·59 |
| 7 | 3 59·5 | 32 11 | 0·754 | 1·25 |
| 9 | 4 5·8 | 34 4 | 0·826 | 1·00 |
| 11 | 4 11·0 | 35 38 | 0·898 | 0·81 |
| 13 | 4 15·2 | 36 59 | 0·970 | 0·67 |
| 15 | 4 18·9 | 38 8 | 1·042 | 0·56 |
| 17 | 4 22·2 | 39 9 | 1·114 | 0·47 |
| 19 | 4 25·1 | 40 1 | 1·185 | 0·39 |
| 21 | 4 27·8 | 40 47 | 1·256 | 0·34 |

THE VARIABLE-STAR T CORONÆ BOREALIS.—In No. 2,118 of the *Astronomische Nachrichten*, Prof. Schmidt, of Athens, publishes numerous comparisons of the brightness of this star, the so-called *Nova* of 1866, with a neighbouring star which he satisfied himself is not variable, and finds that during the period 1866-1876 there have been fluctuations of brightness exhibiting a certain regularity, from which he deduces the most probable period 93·7 days. Prof. Schönfeld, at Bonn, has also noted these changes, and has determined the times of maxima at which the star varied from 7·8 m. to 9·0 m. T Coronæ therefore exhibits a similar phenomenon to that already remarked about η Argus, "Nova Ophiuchi, 1848," and the star which is almost precisely in the position of Tycho Brahe's famous object of 1572.

THE RADCLIFFE OBSERVATIONS, 1874.—With the marked regularity which distinguishes the publication of the Oxford observations, the Radcliffe observer has just circulated the thirty-fourth volume of the series, containing the observations made in 1874. The usual contents of the handsome octavo so punctually presented to us by the Rev. R. Main are too well known to require any detailed account here. The heliometer has been chiefly employed, as before, in the measurement of a selected list of double-stars, a number of which were also observed for position with the meridian circle. Observations of shooting-stars in the year 1876 are included in this volume, with the view of placing them early in the hands of those who are interested in the study of meteoric astronomy.

We believe we are correct in stating that the next volume will contain observations of the solar spots, commenced at the Radcliffe Observatory in 1875, and which will therefore be a new feature in the publication.

DUN ECHT OBSERVATORY PUBLICATIONS, VOL. I.—The difficulty of procuring Struve's great work, the "Mensuræ Micrometricæ," has suggested to Lord Lindsay the formation of a summary of the measures of double-stars contained in it in a convenient and portable form, which has been presented to the astronomical world, as the first volume of publications of the Dun Echt Observatory. The positions of the stars are brought up to 1875; in the text Struve's first epoch is given, the subsequent ones being added in foot-notes, or in the case of binaries and other stars frequently observed, in an appendix. The highest and lowest powers used in the measures, the magnitudes and colours of the components, and the page of the original work, where the measures are to be found, are included in the summary.

There can be no doubt that Lord Lindsay's volume will be welcomed by a large number of amateurs, who are interested in double-star astronomy, but to whom Struve's great work is difficult of access, to say nothing of its awkward size for frequent use, when obtained. The transcript and reduction of places from 1826 to 1875, appears to have been made with great care, as we are able to testify from a number of cases examined—including instances where the variation of precession has required to be taken into account. That equal care has been exercised in the correction of the press, is also apparent, and as an admirable specimen of astronomical typography, Lord Lindsay's summary of the "Mensuræ Micrometricæ" is probably unsurpassed.

[1] "On the Fermentation of Urine; reply to M. Pasteur." By M. H. Charlton Bastian.