PROCEEDINGS

OF THE

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February 4, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Reduction Division in Ferns." By R. GREGORY. Communicated by Professor H. MARSHALL WARD, F.R.S.

II. "Cultural Experiments with 'Biologic Forms' of the *Erysiphaceæ*." By E. S. SALMON. Communicated by Professor H. MARSHALL WARD, F.R.S.

III. "On the Origin of Parasitism in Fungi." By GEORGE MASSEE. Communicated by Sir W. T. THISELTON-DYER, K.C.M.G., F.R.S.

IV. "On the Effects of joining the Cervical Sympathetic Nerve with the Chorda Tympani." By. Professor J. N. LANGLEY, F.R.S., and Dr. H. K. ANDERSON.

V. "Conjugation of Resting Nuclei in an Epithelioma of the Mouse." By Dr. E. F. BASHFORD and J. A. MURRAY. Communicated by Professor J. ROSE BRADFORD, F.R.S.

February 11, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, followed by Professor G. D. LIVEING, Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Compressibilities of Oxygen, Hydrogen, Nitrogen, and Carbonic Oxide between One Atmosphere and Half an Atmosphere of Pressure; and on the Atomic Weights of the Elements concerned.—Preliminary Notice." By LORD RAYLEIGH, O.M., F.R.S.

II. "A New Method of detecting Electrical Oscillations." By Dr. J. A. EWING, F.R.S., and L. H. WALTER.

III. "On the High Temperature Standards of the National Physical Laboratory.—An Account of a Comparison of Platinum Thermometers and Thermojunctions with the Gas-thermometer." By Dr. J. A. HARKER. Communicated by Dr. R. T. GLAZEBROOK, F.R.S.

IV. "Constant Standard Silver Trial-Plates." By EDWARD MATTHEY. Communicated by Sir WILLIAM CROOKES, F.R.S.

V. "On Certain Properties of the Alloys of Silver and Cadmium." By Dr. T. KIRKE ROSE. Communicated by C. T. HEYCOCK, F.R.S.

VI. "Sunspot Variation in Latitude, 1861—1902." By Dr. W. J. S. LOCKYER. Communicated by Sir J. NORMAN LOCKYER, K.C.B., F.R.S.

February 18, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Further Researches on the Temperature Classification of Stars." By Sir J. NORMAN LOCKYER, K.C.B., F.R.S.

II. "Theory of Amphoteric Electrolytes." By Professor J. WALKER, F.R.S.

III. "Note on the Formation of Solids at Low Temperatures, particularly with regard to Solid Hydrogen." By Professor M. W. TRAVERS. Communicated by Sir WILLIAM RAMSAY, K.C.B., F.R.S.

IV. "Atmospherical Radio-activity in High Latitudes." By G. C. SIMPSON. Communicated by Professor A. SCHUSTER, F.R.S.

February 25, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Electromotive Phenomena in Mammalian Non-Medullated Nerve." By Dr. N. H. ALCOCK. Communicated by Dr. A. D. WALLER, F.R.S.

II. "Further Observations on the *Rôle* of the Blood-Fluids in connection with Phagocytosis." By Dr. A. E. WRIGHT and Captain S. R. DOUGLAS. Communicated by Sir J. BURDON SANDERSON, Bart., F.R.S.

III. "On Mechanical and Electrical Response in Plants." By Professor J. C. BOSE. Communicated by Professor S. H. VINES, F.R.S.

IV. "On the Compressibility of Solids." By J. Y. BUCHANAN, F.R.S.

V. "A Contribution to the Study of the Action of Indian Cobra Poison." By R. H. ELLIOT, Major, I.M.S. Communicated by Sir THOMAS FRASER, F.R.S.

March 3, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, followed by Professor J. W. JUDD, C.B., Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read, as follows :—

Brodie, Thomas Gregor.
Burrard, Sidney Gerald.
Dixon, Alfred Cardew.
Dobbie, James Johnstone.
Holland, Thomas Henry.
Joly, Charles Jasper.
Marshall, Hugh.
Meyrick, Edward.
Muirhead, Alexander.
Nuttall, George Henry F.
Shipley, Arthur Everett.
Travers, Morris William.
Wager, Harold.
Walker, Gilbert Thomas.
Watts, William Whitehead.

The following Papers were read :—

I. "An Inquiry into the Nature of the Relationship between Sunspot Frequency and Terrestrial Magnetism." By Dr. C. Chree, F.R.S.

II. "The Optical Properties of Vitreous Silica." By J. W. Gifford and W. A. Shenstone, F.R.S.

III. "A Radial Area-scale." By R. W. K. Edwards. Communicated by Professor Greenhill, F.R.S.

IV. "The Spectra of Antarian Stars in Relation to the Fluted Spectrum of Titanium." By A. Fowler, A.R.C.S., F.R.A.S. Communicated by Professor Callendar, F.R.S.

March 10, 1904.

Professor LIVEING, Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Electric Resistance Thermometry at the Temperature of Boiling Hydrogen." By Professor J. DEWAR, F.R.S.

II. "A Study of the Radio-activity of certain Minerals and Mineral Waters." By Hon. R. J. STRUTT. Communicated by Lord RAYLEIGH, O.M., F.R.S.

III. "Some Uses of Cylindrical Lens-Systems." By G. J. BURCH, F.R.S.

March 17, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read.—

I. "Physical Constants at Low Temperatures. (1) The Densities of Solid Oxygen, Nitrogen, Hydrogen, etc." By Professor J. Dewar, F.R.S.

II. "The Specific Heats of Metals, and the Relation of Specific Heat to Atomic Weight.—Part III." By Professor W. A. Tilden, F.R.S.

III. "On the Construction of some Mercury Standards of Resistance, with a Determination of the Temperature Coefficient of Resistance of Mercury." By F. E. Smith. Communicated by Dr. R. T. Glazebrook, F.R.S.

IV. "On the Effect of a Magnetic Field on the Rate of Subsidence of Torsional Oscillations in Wires of Nickel and Iron, and the Changes produced by Drawing and Annealing." By Professor A. Gray, F.R.S., and A. Wood.

V. "On a Criterion which may serve to Test various Theories of Inheritance." By Professor K. Pearson, F.R.S.

March 24, 1904.

Sir M. FOSTER, K.C.B., Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Croonian Lecture: "The Chemical Regulation of the Secretory Process," was delivered by Professor E. H. Starling, F.R.S., and Dr. W. M. Bayliss, F.R.S.

The Society adjourned over the Easter recess to Thursday, April 28.

April 28, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Royal Medal awarded to Sir David Gill, in 1903, was handed to the recipient by the President.

The following Papers were read :—

I. "Further Experiments on the Production of Helium from Radium." By Sir WILLIAM RAMSAY, K.C.B., F.R.S., and F. SODDY.

II. "The Effects of Changes of Temperature on the Modulus of Torsional Rigidity of Metal Wires." By Dr. F. HORTON. Communicated by Professor J. J. THOMSON, F.R.S.

III. "The Sparking Distance between Electrically-charged Surfaces. Preliminary Note." By Dr. P. E. SHAW. Communicated by Professor J. H. POYNTING, F.R.S.

IV. "Studies on Enzyme Action. Part II.—The Rate of the Change conditioned by Sucro-clastic Enzymes, and its Bearing on the Law of Mass Action. Part III.—The Influence of the Products of Change on the Rate of Change conditioned by Sucro-clastic Enzymes." By Dr. E. F. ARMSTRONG. Communicated by Professor H. E. ARMSTRONG, F.R.S.

V. "Studies on Enzyme Action. Part IV.—The Sucro-clastic Action of Acids as contrasted with that of Enzymes." By Dr. E. F. ARMSTRONG and R. J. CALDWELL. Communicated by Professor H. E. ARMSTRONG, F.R.S.

VI. "Enzyme Action as bearing on the Validity of the Ionic-Dissociation Hypothesis, and on the Phenomena of Vital Change." By Professor H. E. ARMSTRONG, F.R.S.

VII. "On the Changes of Thermo-electric Power produced by Magnetisation, and their Relation to Magnetic Strains." By Dr. S BIDWELL, F.R.S.

VIII. "The Behaviour of the Short-Period Atmospheric Pressure Variation over the Earth's Surface." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and Dr. W. J. S. LOCKYER.

May 5, 1904.

Annual Meeting for the Election of Fellows.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Professor Liversidge and Professor Minchin were, with the consent of the Society, nominated Scrutators, to assist the Secretaries in the examination of the balloting lists.

The votes of the Fellows present were collected, and the following Candidates were declared duly elected into the Society:—

Brodie, Thomas Gregor.
Burrard, Sidney Gerald.
Dixon, Alfred Cardew.
Dobbie, James Johnstone.
Holland, Thomas Henry.
Joly, Charles Jasper.
Marshall, Hugh.
Meyrick, Edward.
Muirhead, Alexander.
Nuttall, George Henry F.
Shipley, Arthur Everett.
Travers, Morris William.
Wager, Harold.
Walker, Gilbert Thomas.
Watts, William Whitehead.

Thanks were given to the Scrutators.

May 5, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following papers were read:—

I. "Experiments on a Method of Preventing Death from Snake Bite, capable of Common and Easy Practical Application." By Sir LAUDER BRUNTON, F.R.S., Sir JOSEPH FAYRER, Bart., F.R.S., and Dr. L. ROGERS.

II. "A Research into the Heat Regulation of the Body by an Investigation of Death Temperatures." By Dr. E. M. CORNER, and Dr. J. E. H. SAWYER. Communicated by Professor J. N. LANGLEY, F.R.S.

III. "A Note on the Action of Radium on Micro-organisms." By Dr. A. B. GREEN. Communicated by Sir MICHAEL FOSTER, K.C.B., F.R.S.

IV. "Further Note on Some Additional Points in Connection with Chloroformed Calf Vaccine." By Dr. A. B. GREEN. Communicated by Dr. W. H. POWER, F.R.S.

V. "On Certain Physical and Chemical Properties of Solutions of Chloroform in Water, Saline, Serum, and Hæmoglobin. A Contribution to the Chemistry of Anæsthesia.—Preliminary Communication." By Professor B. MOORE and Dr. H. E. ROAF. Communicated by Professor C. S. SHERRINGTON, F.R.S.

VI. "Note on the Lymphatic Glands in Sleeping Sickness." By Captain E. D. W. GREIG, I.M.S., and Lieutenant A. C. H. GRAY, R.A.M.C. Communicated by Colonel BRUCE, F.R.S.

VII. "Corrigenda in Mr. W. Shanks's Tables 'On the Number of Figures in the Reciprocal of a Prime.'" By Lieutenant-Colonel A. CUNNINGHAM, R.E. Communicated by the Secretaries.

May 19, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

Professor Ernest Rutherford, elected 1903, was admitted into the Society.

Dr. T. G. Brodie, Professor A. C. Dixon, Professor J. J. Dobbie, Mr. A. E. Shipley, Professor M. W. Travers, and Mr. W. W. Watts were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

THE BAKERIAN LECTURE: "The Succession of Changes in Radioactive Bodies" was delivered by Professor ERNEST RUTHERFORD, F.R.S.

The following Papers were read:—

I. "The Spectrum of the Emanation of Radium." By Sir WILLIAM RAMSAY, K.C.B., F.R.S., and Professor COLLIE, F.R.S.

II. "Experimental Determinations for Saturated Solutions." By the EARL OF BERKELEY. Communicated by F. H. NEVILLE, F.R.S.

III. "On the Liquefied Hydrides of Phosphorus, Sulphur, and the Halogens, as Conducting Solvents.—Part I." By D. MCINTOSH and B. D. STEELE. Communicated by Sir WILLIAM RAMSAY, K.C.B., F.R.S.

IV. "On the Liquefied Hydrides of Phosphorus, Sulphur, and the Halogens, as Conducting Solvents.—Part II." By E. H. ARCHIBALD and D. MCINTOSH. Communicated by Sir WILLIAM RAMSAY, K.C.B., F.R.S.

V. "On the General Theory of Integration." By Dr. W. H. YOUNG. Communicated by Dr. HOBSON, F.R.S.

The Society adjourned over the Whitsuntide Recess to Thursday. June 2.

June 2, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

Mr. Harold Wager and Dr. G. H. F. Nuttall were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Electric Equilibrium of the Sun." By Professor SVANTE ARRHENIUS. Communicated by Sir WILLIAM HUGGINS, Pres. R.S.

II. "Colours in Metal Glasses and in Metallic Films." By J. C. MAXWELL GARNETT. Communicated by Professor LARMOR, Sec. R.S.

III. "On a Direct Method of Measuring the Coefficient of Volume Elasticity of Metals." By A. MALLOCK, F.R.S.

IV. "A Method of Measuring Directly High Osmotic Pressures." By the EARL OF BERKELEY and E. G. J. HARTLEY. Communicated by W. C. D. WHETHAM, F.R.S.

V. "The Advancing Front of the Train of Waves emitted by a Theoretical Hertzian Oscillator." By Professor A. E. H. LOVE, F.R.S.

VI. "On the General Circulation of the Atmosphere in Middle and Higher Latitudes." By Dr. W. N. SHAW, F.R.S.

VII. "On the Magnetic Changes of Length in Annealed Rods of Cobalt and Nickel." By Dr. SHELFORD BIDWELL, F.R.S.

VIII. "On the Electric Effect of Rotating a Dielectric in a Magnetic Field." By Dr. H. A. WILSON. Communicated by Professor J. J. THOMSON, F.R.S.

June 9, 1904.

Professor J. W. JUDD, C.B., Vice-President, in the Chair.

Mr. Edward Merrick, Professor Charles J. Joly, and Dr. Hugh Marshall were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Notes on the Statolith Theory of Geotropism. (I) Experiments on the Effects of Centrifugal Force. (II) The Behaviour of Tertiary Roots." By F. DARWIN, For. Sec., R.S., and Miss D. F. M. PERTZ.

II. "The Fossil Flora of the Culm Measures of North-West Devon, and the Palæobotanical Evidence with regard to the Age of the Beds." By E. A. NEWELL ARBER. Communicated by Professor T. MCKENNY HUGHES, F.R.S.

III. "On the Structure and Affinities of Palæodiscus and Agelacrinus." By W. K. SPENCER. Communicated by Professor W. J. SOLLAS, F.R.S.

IV. "On the Ossiferous Cave-Deposits of Cyprus, with Descriptions of the Remains of *Elephas Cypriotes*." By Miss D. M. A. BATE. Communicated by Dr. H. WOODWARD, F.R.S.

V. "On the Physical Relation of Chloroform to Blood." By Dr. A. D. WALLER, F.R.S.

VI. "Contributions to the Study of the Action of Sea-Snake Venoms." By Sir THOMAS R. FRASER, F.R.S., and Major R. H. ELLIOT, I.M.S.

VII. "On the Action of the Venom of *Bungarus cœruleus* (the Common Krait)." By Major R. H. ELLIOT, I.M.S., W. C. SILLAR, and G. S. CARMICHAEL. Communicated by Sir THOMAS R. FRASER, F.R.S.

VIII. "On the Combining Properties of Serum Complements, and on Complementoids." By Professor R. MUIR and C. H. BROWNING. Communicated by Sir J. BURDON SANDERSON, Bart., F.R.S.

June 16, 1904.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

The Right Hon. Donald Alexander Smith, Baron Strathcona, was balloted for and elected a Fellow of the Society.

Dr. Alexander Muirhead was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. " The Origin and Growth of Ripple-mark." By (Mrs.) HERTHA AYRTON. Communicated by Professor W. E. AYRTON, F.R.S.

II. " On the Seismic Effect of Tidal Stresses." By R. D. OLDHAM. Communicated by Professor J. W. JUDD, C.B., F.R.S.

III. " On Flame Spectra." By C. DE WATTEVILLE. Communicated by Professor A. SCHUSTER, F.R.S.

IV. " An Experiment illustrating Harmonic Undertones." By H. KNAPMAN. Communicated by Dr. G. J. BURCH, F.R.S.

V. " A Probable Cause of the Yearly Variation of Magnetic Storms and Auroræ." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and Dr. W. J. S. LOCKYER.

VI. " On the Relation between the Spectra of Sun-spots and Stars." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

VII. " On the Action of Wood on a Photographic Plate in the Dark." By Dr. W. J. RUSSELL, F.R.S.

VIII. " The Retardation of Combustion by Oxygen." By Professor H. E. ARMSTRONG, F.R.S.

IX. " The Absorption and Thermal Evolution of Gases occluded in Charcoal at Low Temperatures." By Professor J. DEWAR, F.R.S.

X. " Direct Separation of the Most Volatile Gases from Air without Liquefaction." By Professor J. DEWAR, F.R.S.

XI. " On the Influence of the Time Factor on the Correlation between Barometric Heights at Two Stations 1,000 Miles apart." By Miss F. E. CAVE-BROWNE-CAVE. Communicated by Professor K. PEARSON, F.R.S.

XII. "The Decomposition of Ammonia by Heat." By Dr. E. P. PERMAN and G. A. S. ATKINSON. Communicated by Sir WILLIAM RAMSAY, K.C.B., F.R.S.

XIII. "On the Action of Radium Emanations on Diamond." By Sir WILLIAM CROOKES, F.R.S.

XIV. "The Lethal Concentration of Acids and Bases in respect of *Paramœcium aurelia*." By J. O. WAKELIN BARRATT, M.D. Communicated by Sir VICTOR HORSLEY, F.R.S.

XV. "A Memoir on the Theory of Order as defined by Boundaries." By EDWARD T. DIXON. Communicated by Major MACMAHON, R.A., F.R.S.

The Society adjourned over the Long Vacation to Thursday, November 17.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

"The Third Elliptic Integral and the Ellipsotomic Problem."

By A. G. GREENHILL, F.R.S. Received December 22, 1903,—Read January 21, 1904.

(Abstract.)

The elliptic integral of the third kind, which makes its appearance in a dynamical problem, is of the circular form in Legendre's classification, and thus the Jacobian parameter is a fraction of the imaginary period, so that the expression by means of the theta function can no longer be considered as reducing the variable elements from three to two.

Burkhardt* has given a series rapidly convergent for the numerical calculation of such cases; but the object of this memoir is to develop the exact expression by means of an idea of Abel, given in the first volume of 'Crelle's Journal,' 1826, "Ueber die Integration der Differential-Formel

$$\rho dx / \sqrt{R}, \tag{1}$$

wenn R and ρ ganze Functionen sind."

Abel proves practically that when the elliptic parameter is an aliquot μth part of a period, the third elliptic integral and the associated theta functions depend on the μth root of an algebraical function.

Thus, as shown in the paper, if we take the Jacobian elliptic parameter

$$v = 2K'i/\mu,\ \mu = 2n+1, \text{ an odd integer}, \tag{2}$$

the third elliptic integral in the form

$$I(v) = \int \frac{P(v)\,t^2 - \frac{1}{2}xy}{t^2} \frac{dt^2}{\sqrt{(T_1 T_2)}}, \tag{3}$$

---

* 'Elliptic Functions,' § 126.

subject to the condition

(4) $$\gamma_\mu = 0$$

is such that

(5) $$\begin{aligned}&2t^{n+\frac{1}{2}} \exp. (n+\tfrac{1}{2}) \,\mathrm{I}(v)\, i \\ &= (t^{n-1} + h_1 t^{n-2} + h_2 t^{n-3} + \;.\;.\;.\;) \sqrt{\mathrm{T}_1} \\ &+ i\,(t^{n-1} - h_1 t^{n-2} + h_2 t^{n-3} - \;.\;.\;.\;) \sqrt{\mathrm{T}_2},\end{aligned}$$

where

(6) $$\mathrm{T}_1,\ \mathrm{T}_2 = 2t^3 \pm (1+y)\, t^2 + 2xt \pm xy,$$

and x, y, γ_n are the functions employed by Halphen.*

The calculation of the coefficients $h_1, h_2, \ldots$ can be carried out by the method of *réduites*,† avoiding the continued fractions employed by Abel, which make the order of the result higher than is necessary.

Here P (v) is of the nature of a zeta function, and equations are given for its calculation, as an algebraical function of a parameter.

But in most dynamical problems, such as Poinsot's herpolhode and the associated motion of the symmetrical top, the Jacobian parameter is of the form

(7) $$v = \mathrm{K} + 2\mathrm{K}' i/\mu,$$

and this requires the resolution of T_1 and T_2, equivalent to a change to an even value, $4n+2$, of μ.

Then if ρ, ϖ denote polar co-ordinates in the invariable plane of a Poinsot herpolhode, or of the angular momentum vector of a top, and t' denotes the time, we can take

(8) $$pt' - \varpi = \mathrm{I}(v), \qquad \mathrm{M}\rho/k = t,$$

so that in such a curve

(9) $$2\,(\mathrm{M}\rho/k)^{n+\frac{1}{2}} \exp. (n+\tfrac{1}{2})\,(pt' - \varpi)\, i$$

is an algebraical function; and the curve is purely algebraical when the constants are so chosen as to cancel the secular term pt'.

The projection of the path of the centre of gravity of the top is the hodograph of the herpolhode of angular momentum, and is thus obtainable by a differentiation of the above; some of these applications are developed in a memoir on the top, now appearing in the 'Annals of Mathematics.'

With $\mu = 8n+4$ or $8n$ a further reduction is possible in degree. We find for $\mu = 8n+4$,

(10) $$\begin{aligned}&(\mathrm{D} - x^2)^{n+\frac{1}{2}} \exp. (2n+1)\, \mathrm{I}(v)\, i \\ &= [\mathrm{R}_0 + \mathrm{R}_1 x + \;.\;.\;.\; + (-1)^n x^{2n}] \sqrt{\tfrac{1}{2}\mathrm{X}_1} \\ &+ i\,[\mathrm{R}_0 - \mathrm{R}_1 x + \;.\;.\;.\; + (-1)^n x^{2n}] \sqrt{\tfrac{1}{2}\mathrm{X}_2}.\end{aligned}$$

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* Halphen, 'Fonctions Elliptiques,' vol. 1, p. 102.

† 'Fonctions Elliptiques,' vol. 2, p. 576.

$$(11) \qquad X_1,\ X_2 = 1 \pm (o^{-1} - o)\,x - x^2,$$

where o denotes the octahedron-irrationality, $o = \sqrt{\kappa}$.

But with $\mu = 8n$, this changes to

$$(12) \qquad (\mathrm{D} - x^2)^n \exp. 2n\mathrm{I}(v)\,i$$
$$= [\mathrm{R}_0 + \mathrm{R}_1 x + \ \ldots\ + (-1)^n x^{2n-1}]\sqrt{\tfrac{1}{2}\mathrm{X}_1}$$
$$+ i\,[\mathrm{R}_0 - \mathrm{R}_1 x + \ \ldots\ - (-1)^n x^{2n-1}]\sqrt{\tfrac{1}{2}\mathrm{X}_2}$$

$$(13) \qquad X_1,\ X_2 = 1 \pm (o^{-1} + o)\,x + x^2.$$

The results are worked out in the memoir for numerical values as far as possible, starting with the simplest, 3, 4, 5 . . . , and the application is shown to other associated mechanical problems, such as central orbits, the spherical catenary, the elastica and velarium.

Provided with a list of these integrals, the student of Applied Mathematics will be able to effect the complete discussion of many mechanical problems now abandoned in an unfinished state; at the same time the exploration along the simplest line of progress is effected of the general analytical field, and mathematical research is guided along a road likely to lead to useful development in the theory of elliptic functions.

Incidentally the elliptic section or division values (Theilwerthe) are determined, as well as those of the zeta and theta functions, as algebraical functions of a parameter, in a form such that

$$(14) \qquad \left(\frac{\Theta 2\mathrm{K}/\mu}{\Theta 0}\right)^{\mu} \quad \text{and} \quad \left(\frac{\mathrm{H}2\mathrm{K}/\mu}{\mathrm{HK}}\right)^{\mu}$$

are of simple algebraical character; and it is shown that this, the Ellipsotomic Problem, as it may be called by analogy, depends essentially on the discussion of the curve given by (4), which may be called the ellipsotomic equation and curve in the co-ordinates x and y, or on that of a reduced form, involving the determination of its class, and the expression of its co-ordinates as functions of a parameter.

The coefficients in the Transformation of elliptic functions are symmetric functions of these section values, so that the Transformation may be considered as determined incidentally, but as the Transformation theory has no utility in dynamical applications, this branch of pure analysis has not been pursued.

"On the Structure of the Palæozoic Seed, *Lagenostoma Lomaxi*, with a Statement of the Evidence upon which it is referred to *Lyginodendron*."

By Professor F. W. OLIVER and Dr. D. H. SCOTT, F.R.S.

Received December 15, 1903,—Read January 21, 1904.

(Abstract.)

The present communication deals with the structure of *Lagenostoma Lomaxi*, a fossil seed from the lower coal-measures, and with the evidence upon which the authors refer it to the well-known carboniferous plant, *Lyginodendron*.

It is found that this species of *Lagenostoma*, especially in its young form, was inclosed in a husk or cupule, borne on a short pedicel.

The seed, which is of Cycadean character, is fully described, and its relation to other fossil and recent seeds discussed.

The cupule inclosing the seed was borne terminally on a pedicel; it formed a continuous, ribbed cup below, and divided above into a number of lobes or segments. Externally, both pedicel and cupule were studded with numerous prominent multicellular glands of capitate form. The anatomy indicates that the whole organ was of a foliar nature.

A comparison with the vegetative organs of *Lyginodendron Oldhamium*, with which the seeds are intimately associated, demonstrates a complete agreement in the structure of the glands and in the anatomy of the vascular system. Where vegetative and reproductive organs, presenting identical structural features, not known to occur in other plants, are thus found in close and constant association, the inference that the one belonged to the other appears irresistible.

As regards the position of the seed on the plant, two possibilities are discussed: the cupule, with its pedicel, may either represent an entire sporophyll, or a modified pinnule of a compound leaf. Either view is tenable, but various comparative considerations lend a somewhat greater probability to the second alternative.

In the concluding section of the paper, the systematic position of *Lyginodendron* is discussed. On the whole of the evidence, the position of the genus as a member of a group of plants transitional between Filicales and Gymnosperms appears to be definitely established. While many Filicinean characters are retained, the plant, in the organisation of its seed, had fully attained the level of a Palæozoic Gymnosperm. There are many indications that other genera, now grouped under Cycadofilices, had likewise become seed-bearing plants. It is proposed to found a distinct class, under the name *Pteridospermæ*, to embrace those Palæozoic plants with the habit and

much of the internal organisation of Ferns, which were reproduced by means of seeds. At present, the families Lyginodendreæ and Medulloseæ may be placed, with little risk of error, in the new class, Pteridospermæ.

"Some Experiments in Magnetism." By T. C. PORTER. Communicated by LORD RAYLEIGH, O.M., F.R.S. Received November 9,—Read November 26, 1903.

[PLATE 1.]

For many years the writer has, from time to time, been engaged in studying the effect of a powerful magnetic field upon crystals in the act of their formation and growth. It seemed to him probable that if the molecules of substances have magnetic poles, they might group themselves differently when under the influence of a powerful magnetic field, thus producing an orientation of the crystals, or even an alteration in the form or optical characteristics of the crystals themselves. It seemed also possible that if the supposed polar properties of the molecules were the result of atomic polarity, a powerful external magnet might produce some appreciable effect in the chemical combination of the atoms, changing the rate of chemical reaction, if it did not change the character of the compounds formed. Many effects were observed, and were at first erroneously attributed to the influence of magnetism; later, when, by specially contrived apparatus, the influence of variation in temperature, humidity of the air, and above all of the history and character of the surfaces upon which the crystallisation took place, were investigated, these effects were one after another traced to causes other than magnetic, so that the results must, on the whole, be taken as negative. Cases of orientation of crystals growing in the magnetic field and watched under the microscope from their first visibility till they had attained considerable size, such cases of orientation were found in two compounds of iron, but even these orientations were found to depend, at any rate in some cases, upon the direction in which the surface of the glass slide had been rubbed before cleaning for the experiment. The main result of this long, difficult, and expensive research has only been to prove that if there be any such effects as those looked for, they require, to show them *indisputably*, more powerful fields than those of the very powerful electro-magnets used by the writer.

A photograph of the orientation of Marignac's basic sulphate of ammonium and iron $3Fe_2O_3.5(NH_4)_2O.12SO_3.18H_2O$ is given with this paper.

This comparatively little known salt is formed by dissolving in 50 c.c. of water, say, 20 grammes of crystallised ferric ammonium sulphate (iron alum), and adding drop by drop, with constant shaking, 2·33 c.c. of ammonia solution, sp. gr. 0·88. The mixture must be left to evaporate spontaneously, when it deposits hexagons of the very interesting pleo-chroic salt; generally, some quantity of another basic iron salt is also formed.

Marignac's salt, which is strongly magnetic, is also formed when a solution of iron alum evaporates spontaneously above a certain temperature, and the orientation has been, as a rule, more noticeable when the crystals have been deposited from an iron alum solution than when deposited from the solution first mentioned, probably because the rate of deposition from the alum has been very much slower. Figs. 1 and 2 are photographs of the salt being deposited from a solution of ammonium iron alum by spontaneous evaporation on glass slides: fig. 1 in a powerful magnetic field, fig. 2 under conditions as like as possible to those of fig. 1, but under the earth's magnetism only. Numerous trials have satisfied the writer that other conditions being the same, crystals of the alum and of Marignac's salt form in the powerful magnetic field as readily as they do in the weak one. The position of the magnet poles is not indicated on the photographs, but it will be seen that, in many obvious cases, where the hexagons rest on one of their prismatic sides, the principal axes of the hexagonal prisms are parallel (to the magnetic lines of force). Many of the crystals, however, if not most of them, are resting in their most stable position, viz., on one of the hexagonal faces. This is noteworthy, because if a crystal of the salt which has grown to a large size (and away from the magnet) be suspended by a thread of unspun silk attached to the centre of one of its prismatic faces by a minute morsel of wax, between the blunt poles of the electro-magnet, it sets itself in exactly the same position as the orientated crystals in the photograph, viz., with its principal axis parallel to the lines of force. When the same crystal is suspended from the centre of one of its hexagonal faces, it sets itself with the planes of an opposite pair of the prismatic faces perpendicular to the lines of force, so that if in figs. 1 and 3 the visible hexagons are orientated, they should be disposed with a pair of opposite sides perpendicular to the lines of force, and therefore parallel to the longest sides of the rectangles which are the observer's view of the hexagonal crystals resting on one of their prismatic sides. There are certainly signs of some of the hexagons being thus orientated, but since there are notable exceptions, and, moreover, since there are six positions favourable to an orientation hypothesis in one complete revolution of the crystal in its own plane, one cannot say definitely whether they are orientated or not.

FIG. 1.

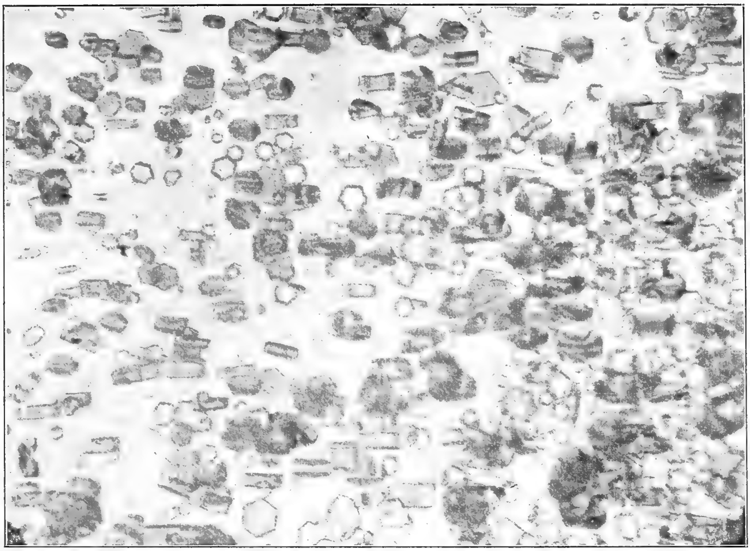
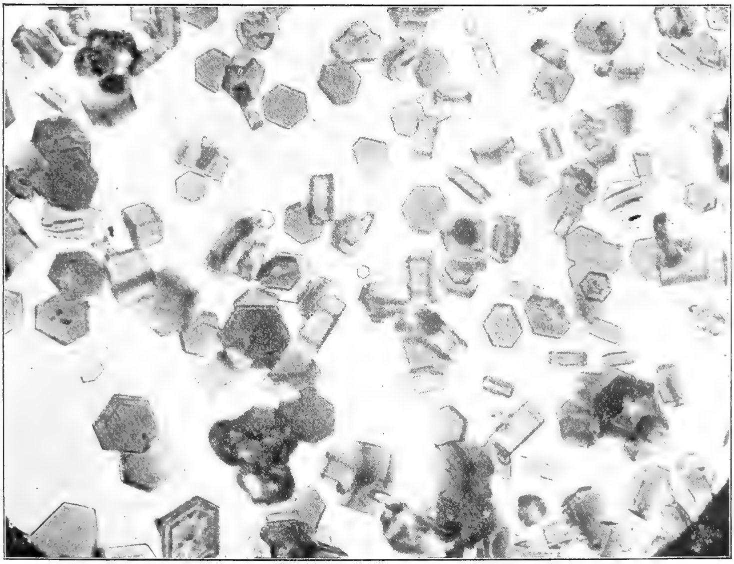


FIG. 2.



For some time the writer regarded these results, and many others of the same kind obtained from the same salt, as clear indications that these crystals are orientated from their earliest growth, since very few ever changed their position unless the crystals were crowded, and that the molecule, or the crystal element, is magnetic, but he has failed again and again to obtain *any* indication of orientation with the same strongly magnetic salt, under what were, so far as could be judged, similar circumstances, and lastly, by wiping the glass slide in a given direction, he found it possible to produce a species of orientation in other directions. The very fact that the orientation in these photographs is such that the length of the rectangles lies parallel to the length of the microscope slide, which is presumably the direction in which it is most often wiped or dusted, makes him the more unwilling to accept the photographs as evidence of any magnetic effect, *but he feels it only wise to point out that the orientation produced by scratches has never in his experience produced quite the effect seen in this photograph*, the grouping of the crystals has been different, leaving no difficulty in tracing the line which gave rise to the orientation, whereas on these photographs this is scarcely possible; the distribution of the orientated crystals themselves is not linear. So that, in the mind of the writer, in spite of the arguments adduced, and the fact that no such clear case of orientation as that shown has been observed with any other substance except the corresponding potassium salt (although very many have been tried), it is still an open question whether the orientation visible in fig. 1 is, or is not, due to magnetism.

After these experiments made at ordinary temperatures, the writer turned his attention to the formation of well-known magnetic bodies in the magnetic field. Flowers of sulphur and levigated soft iron particles were taken in the proportion in which they combine to form the magnetic sulphide of iron Fe_3S_4, and were thoroughly mixed together. The mixture was poured into a small paper tube, loosely corked at the lower end, and this tube was placed vertically between the poles of a powerful electro-magnet, arranged so that the tube stood parallel to the lines of force between the poles, the last being bored, and the little tube standing in the position of the heavy glass in Faraday's celebrated experiment. The current was then switched on, and, at the same time a flame was applied to the mixture where it projected into the air at the top of the tube. As soon as the iron and sulphur began to combine, the flame was withdrawn, and the wave of combination descended right through the mixture. The current through the magnet coils was maintained until the tube had had time to cool; the latter was then removed, the paper carefully taken off, disclosing a fairly tough rod of sulphide. This was found to be a magnet, weak, but unmistakable, its poles being disposed as one would naturally expect. Similar experiments were then made with mixtures of iron and sulphur in the proportions necessary to the sulphides FeS, Fe_2S_3, and FeS_2. In each experiment two tubes of the mixture were made, one for the magnet, and one to be fired at a distance from the magnet; this was done to find out whether there were any persistent differences, other than magnetic, between the two. The mixture for FeS_2 refused to burn in *both* the tubes, as indeed might have been expected. The polarity of the Fe_2S_3 bar was decided, that of the FeS distinct, though very weak. In every case the magnetised bars have *remained* unmistakable magnets, but this *may* be due to part of the soft iron remaining uncombined, or possibly to the formation of small quantities of magnetite from the air between the particles of the powder at starting. The bars were powdered very finely, and their specific gravity taken, with the results shown in the table given in the addenda.

It is scarcely necessary to point out that though there is no doubt that one or more of the compounds FeS, Fe_3S_4, and Fe_2S_3 was really formed, we do not know the proportion of each present in each case. Probably the products were purer in the first and last cases. The specific gravities were taken with all care, and the first two places of decimals are reliable; the powders were thoroughly well mixed, and the experiments were made from the same samples of mixture, in similar tubes. The writer hopes to repeat them, and will not comment at present on the differences of density in the magnetised and unmagnetised products.

Similar experiments were next tried with the magnetic oxide of iron Fe_3O_4.

(*a*) By allowing a considerable quantity of the levigated iron to form a thick chain between the solid conical poles of the electromagnet, and pressing them together until they formed a dense mass between the poles, and then burning them *in situ*, in one experiment with the flame of an ordinary Bunsen burner, and in another by that of an oxy-coal gas flame, blowing on the still very hot mass with pure oxygen to complete the oxidation as far as possible; in both cases the resulting mass of magnetic oxide was of very marked polarity.

(*b*) By placing between the poles of the electro-magnet, so that its upper surface was in the strongest part of the field, a charcoal block, having cut in it, parallel to the lines of magnetic force, a shallow groove, which was filled with previously prepared magnetic oxide, and whilst the current traversed the magnet coils, fusing these fragments into one bar, by means of the oxy-gas blow-pipe, afterwards blowing on the still white hot mass with pure oxygen. On cooling the mass and testing it, it proved distinctly polar, but far less so than in the experiments (*a*). In this experiment the oxide had been heated to a brilliant whiteness, and cooled somewhat quickly by being wetted with water.

These experiments seem to prove that it is possible to prepare in this way, and quickly, artificial lodestones of some strength, for the products of the (*a*) experiments were able to pick up small pieces of soft iron.

Thinking that in these cases part, at any rate, of the polarity might be due to particles of the levigated iron which had escaped combination with either the sulphur or the oxygen, the writer next attempted to make a magnet out of the particles of levigated iron themselves. This was done by half filling a small test tube with solid paraffin, melting it, loosely corking the tube, and placing it in a horizontal position until the paraffin had solidified. The remaining air space, of semicircular section, was then filled with the levigated iron, the tube again corked, and placed horizontally through the bored poles of the electro-magnet; the current was switched on, and whilst the tube was in position, with the iron uppermost in the tube, the paraffin below it was melted by cautiously heating it with a Bunsen. As the paraffin melted, the tube being gently tapped meanwhile, the iron particles were for the time capable of small movements, arranging themselves along the lines of force. The paraffin was then allowed to solidify and grow cool, the current being maintained the while, the tube removed and the glass gently peeled off, by first cutting it round with a diamond, leaving a bar of soft iron particles embedded in paraffin, which was a magnet, and has remained so ever since, without showing any signs of losing its magnetism, and sufficiently strong to develop tufts at either pole when dipped into a heap of the levigated iron.

As it still seemed possible that the permanent polarity of this magnet might be due to carbon in it, which it certainly contained, though in very small quantity, it seemed well to try a similar experiment with particles of pure electrolytic iron, and to try it with *molecules* of the iron instead of *assemblages of molecules*, already under the influence of forces opposing the development of the magnetic polarity, and these considerations at once suggested that the iron should be deposited electrolytically from the solution of one of its compounds in a powerful magnetic field on some "non-magnetic" substance, such as platinum. The photograph shows the arrangement actually used, and, which it may be stated at once, proved entirely successful.

The apparatus consisted of the electro-magnet, with two special pole pieces, designed to concentrate the magnetic lines of force into a space of length, the distance between them; of height, the height of the pole pieces as shown, and of thickness one-eighth of an inch. The poles thus tapered to what may be called a blunt vertical line. Between the poles is the glass cell, in the centre of which hangs the strip of platinum foil, previously proved to have no visible effect upon a magnet, so that its plane is in the paramagnetic position, the platinum approaching close to the sides of the glass vessel, but not touching them. Two pieces of platinum foil, about 3 inches long and $\frac{3}{4}$ inch wide, hang at each end of the glass vessel, facing the central strip on both its sides; the central strip is the cathode, on which the iron is deposited, the other two being the anodes. The current having been switched on to the coils, and the single Groves' cell used, to the electrodes, the iron solution is poured in, till it reaches as high as the top of the pole pieces. After three minutes there was a discoloration of the platinum from the *very* thin film of iron formed upon it, and on testing this, by withdrawing it from the solution and applying it edgeways to a rather heavy compass needle, by no means delicately mounted, it was found to give distinct evidence of polarity, the repulsion and attraction being most clearly visible, and on tearing off a strip of the platinum along its base, and drying it and balancing it on a fine sewing needle, it quickly set itself in the magnetic meridian, and has continued to do so with apparently equal activity ever since, although the film of iron must be exceedingly thin. The main portion of the strip was replaced in the solution, and the deposition of the iron continued, its polarity being tested from time to time. Its magnetic intensity increased, though certainly at a diminishing rate. Finally, when all the iron in the solution had been deposited the platinum was taken out, washed with distilled water, the part covered with iron torn off (so that it did not touch any other iron) and put into dry air for preservation. It has remained a magnet ever since, and as yet gives no sign of diminished intensity, though it has been made to vibrate, and hit with a wooden rod repeatedly.

The whole experiment has been repeated, with the same results.* If during the deposition the platinum be reversed, side for side, after a certain time the iron deposited shows no polarity, and after that its polarity is reversed. Whether this is due to the reversal of polarity in the molecules first deposited, as well as to the deposition upon them of molecules having opposite polarity, or to the latter cause only, the writer has not yet determined, nor has it been possible in this paper to give measurements either of the magnetic intensities, or of the thickness of the thinnest film of pure electrolytic iron which is capable of making its polarity felt; nor again is it yet possible to say how long the polarity of these artificial magnets will continue. Answers to these questions, by the very nature of the investigation, must be left for a future communication.

The solution from which the iron was deposited is constituted as follows:—

20 grammes $FeSO_4.(NH_4)_2SO_4.6H_2O$,
64 grammes $(NH_4)_2C_2O_4.H_2O$.
Made up to 960 c.c. with distilled water.

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* The writer was unaware at the time that a similar experiment had been made by Beetz, and quoted by Maxwell.

This solution is used for the *quantitative* estimation of iron by electrolysis, and though carbon is present in the oxalate, the deposited iron is soluble in dilute hydrogen chloride, without any visible residue, being pure to all chemical tests, but for possibly occluded hydrogen.

ADDENDA: *November* 24, 1903.

(1). It should be stated that when the crystals growing in a powerful magnetic field, and apparently orientated as described and shown in the photograph, become large, and thus approach each others' surfaces, they are often seen to leave their first position and to take up another; sometimes the crystal will be moved bodily nearer its neighbour, sometimes it is swung round only; but it follows that where the crystals are numerous and close together, the signs of orientation, which were evident during the early stages of their growth, have almost, if not quite, disappeared at a later stage.

(2). Some experiments were carried out early in the investigation in weak magnetic fields, and also in a field of as nearly zero intensity as could be managed, by compensating the earth's field, but no difference between these could be detected with any certainty, though in most cases as many as six crystallisations were carried on at one time, three in the strong field and three in a weak or zero field.

(3). When it was found that the direction in which the glass slides used had been rubbed (before they were finally cleaned with acid, alkali, and distilled water) was not in all cases negligible, experiments were made in which the glass had been purposely rubbed hard in different directions, with the result already stated, but in no case was the orientation or arrangement of the crystals the same in appearance as that shown in the photograph, for they formed close together along the lines. The strongest argument, in the writer's opinion, *against* a magnetic explanation of the orientation in the photograph, is his repeated failure to obtain the same result under what were apparently precisely similar circumstances, and also the fact that no other substance has given similar indications, at any rate, such evident indications of orientation, though very many have been tried.

(4). Since the paper was sent in, a number of experiments have been made on the densities of the substances produced by "firing" the small "squibs" of the mixtures of iron and sulphur described in the paper, with the results embodied in the following statement. The quantities taken were in most cases between 4 and 2·5 grammes, and the weighings were certainly correct to milligrammes; the greatest error possible in any single estimation of density does not exceed ± 0·02, and is probably less than 0·005. All the bars fired in the strong magnetic field are magnets, those in the earth's field are not perceptibly so, though they *may* possess very weak polarity.

Mixture in the proportion of					
Fe + S.		3Fe + 4S.		2Fe + 3S.	
Magnet.	Away from magnet.	Magnet.	Away from magnet.	Magnet.	Away from magnet.
4·455	4·593	4·229	4·364	4·358	4·179
4·581	4·710	4·519	4·301	4·443	4·565
4·763	4·579	4·394	4·420	4·377	4·506
	4·648	4·407*		4·703*	
	4·653				
Mean of 3 experiments. 4·600	Mean of 5 experiments. 4·637	Mean of 3 experiments. 4·381	Mean of 3 experiments. 4·362	Mean of 3 experiments 4·393	Mean of 3 experiments. 4·417

Two experiments were made to find out how the density varied in any one bar. The mixtures used were the second and third of the above three; each was placed in a tube more than twice the length of those used in the experiments already described, and both were fired separately in the strong magnetic field, the tubes passing right through the bored poles of the magnet, the distance between the poles being 1 inch. The rod formed was broken into three portions, called the top, middle, and bottom respectively. The results were as follows:—

3Fe + 4S ... top, 4·384; middle, 4·533; bottom, 4·305; mean, 4·407.
2Fe + 3S ... ,, 4·664 ,, 4·718; ,, 4·728; ,, 4·703.

My thanks are due to Mr. R. W. Kennedy for help in the determination of the densities.

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* The tube used in these experiments was much longer than in the others.

"On the Distribution of Stress and Strain in the Cross-section of a Beam."

By JOHN MORROW, M.Sc. (Vict.), Lecturer in Engineering, University College, Bristol. Communicated by Professor HELE-SHAW, F.R.S. Received October 27,—Read November 26, 1903.

Introduction.

Our knowledge of the strains produced in materials by different kinds and combinations of stress rests mainly on theoretical considerations. Much accurate experimental work has been done in the observation of direct tensile strains, but little attention has been given to the lateral strain accompanying a simple tensile or compressive strain, or to the lateral strain occurring in a bar under bending forces. The latter, indeed, has, perhaps, never before been measured in metal specimens.

For some time past the writer has been making experiments with the object of showing that instruments can be constructed capable of measuring these lateral strains with considerable accuracy. The subject is of great interest to the elastician, as it not only provides a method of determining elastic coefficients, but shows the degree of applicability of mathematical results; and, further, in view of the well-known discrepancies which exist between experiment and theory, the subject is also of no small practical importance.

The work here described is confined mainly to experiments on iron beams, and has had for its objects the following:—

1. The design of a comparatively simple instrument which can be used for the accurate measurement of the lateral displacements in the section of a beam;
2. The determination of the amount and distribution of this strain in iron beams; and
3. The determination of Poisson's ratio from the observed lateral displacements.

Description of the Apparatus.

Whilst making some experiments on the lateral constraction of tie-bars,* it occurred to the author that, with a modified form of the apparatus then used, measurements of the deformation of the section of a beam might be made.

After trials of several different forms of instrument, and various ways of applying the load, it was decided that a long bar should be placed in the testing machine, and the readings taken with the mirror apparatus shown in fig. 1. The specimen S is in section

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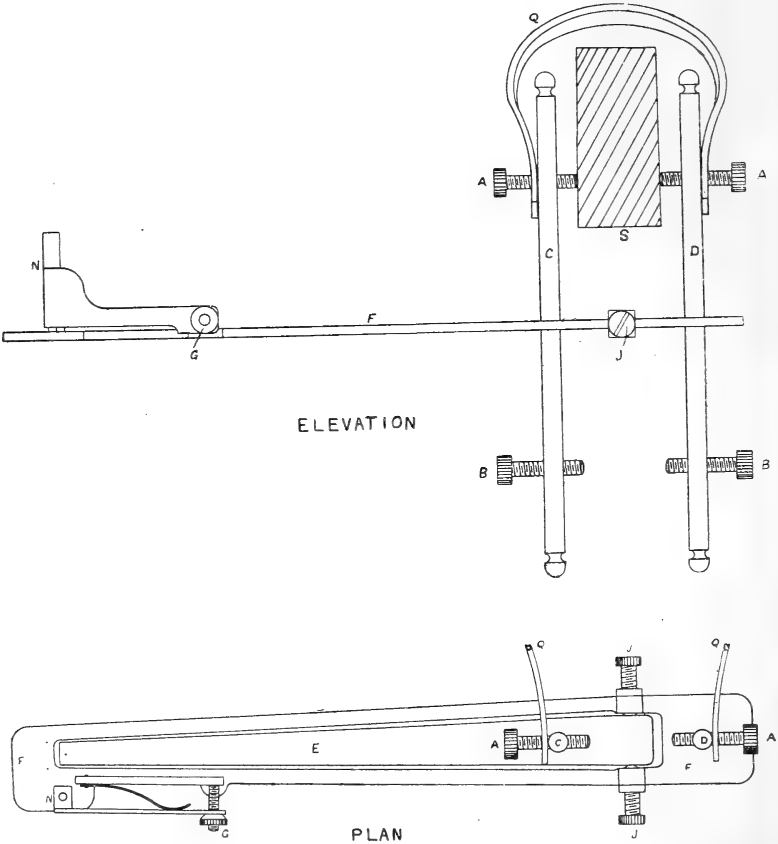
* 'Phil. Mag.,' Sixth Series, vol. 6, No. 34, p. 417.

in the elevation. It is gripped by the two set-screws AA, each of which is screwed through one of the vertical rods C and D. These rods pass respectively through the pieces E and F, and are rigidly attached to them. CE and DF thus form a pair of levers pivoted together at JJ. Any motion of AA, therefore, will be transmitted to the extreme ends of E and F. Thus if the specimen contract, AA approach one another, and E will rise relatively to F; the relative motion of E and F will be a measure of the contraction of the specimen.

Fig. 1.



To obtain the amount of this relative motion, two mirrors, M and N, are used. They are made of specially prepared optically plane glass. The former is supported on three hard steel needle points, two of which rest on F, and the third on E, as shown in the plan. The other mirror, N, is attached to F on a vertical spindle, about which it can be turned by the fine adjustment screw G.

When the instrument is set up, a scale is placed in an upright position in front of the mirrors, and some distance away, and the two images of the scale are brought together in the field of a telescope by means of the screw G. A convenient mark on the N image is then taken as an index, and the reading coinciding with it on the other image is noted each time the load on the beam is altered. From the difference between the readings before and after applying a load, the transverse strain is calculated across the breadth of the beam.

The instrument may be placed either below the specimen, when the screws AA are used, or above, when BB perform exactly the same functions.

The pressure between the screws and the specimen is maintained by a stiff spring, Q and the weight of the instrument taken up by attaching a thin cord to C, which, passing over a light pulley, carries a balance weight. It will be seen that the instrument is free from strains, which vary with the load on the specimen.

The dimension from the axis to the plane of the tilting mirror was 20 cm., while that between the centre-line of the set-screws and the axis was 5 cm. A magnification of four was thus obtained in the instrument itself. For a long time consistent reading could not be obtained, and it was only after very many trials of the instrument and alterations in its design that the causes of the inconsistency were successively eliminated.

The apparatus was very sensivive to vibrations, and it was soon found that accurate readings could only be obtained when the machinery in the adjacent laboratories was at rest. Vibrations caused by traffic some distance away were quite noticeable.

The most troublesome factor was probably the small unavoidable amount of jerk accompanying the application of the load. Occasionally this would cause a very slight displacement of the tilting mirror, which could not be detected on looking in the telescope, but nevertheless was sufficient to vitiate the accuracy of the readings. Under these circumstances it was impossible ever to depend on a single observation. The load was invariably applied and removed several times, and if the readings were consistent, and returned each time to their original value when the load was taken off, they were accepted as correct.

Any imperfection in the adjustment of the instrument or in the manner of applying the load was always made evident by the erratic nature of the readings.

Method of Making the Experiments.

For the first series of experiments, a long cast-iron bar, 2·833 cm. broad and 6·452 cm. deep, was placed in the "Wicksteed" testing machine, in the Engineering Laboratory at University College, Bristol. It was supported on knife edges at each end of a span of 91·44 cm. (36 inches), and was loaded in the centre in the ordinary way.

At a distance of from about 7·5 to 9·0 cm. from the centre of the span lines were carefully scribed on the sides of the beam, marking the places where the set-screws of the strain-measuring instrument were to rest. One of these lines was at the middle of the depth, and the others at distances of 0·635, 1·905, and 3·175 cm., both above and below the mid-point.

The instrument was attached just clear of the ends of these lines, and 36·80 cm. from one of the planes of supports. Thus the section examined was at a distance from the point of application of the load exceeding the depth of the beam, and it was hoped that this would be sufficient to secure immunity from the effects of local strains due to surface loading. Under these circumstances, each ton applied at the centre would produce a bending couple of 18,694 kilogram-centimetres at the section under observation.

The effective distance between the needle points of the tilting mirror, as given by a reading microscope, was 0·6160 cm., and the normal distance of the scale from the mirrors 202·7 cm.

The total magnification was, therefore, 2632 for the first experiments, but it was, of course, re-determined whenever the position of the scale was altered.

The scale was divided to $\frac{1}{40}$th of an inch, and, with the telescope used, $\frac{1}{10}$th of these divisions could be readily estimated. The observed numbers are, therefore, given to the nearest $\frac{1}{414560}$ of a centimetre.

When taking a series of readings for a certain change of load, the instrument was first applied with the points AA at the lowest mark, and a number of readings taken each time the load was applied or removed. The set-screws were then moved up to the next mark, and the readings continued. When the centre was reached, the instrument was taken off and re-applied above the specimen, with the screws BB at the top marks, and observations were continued down to the centre again.

The log sheet was kept as follows :—

Table I.—Loads 1 to 1½ tons. Cast-iron Beam No. 1.

Load.	Scale reading.	Diffs.	Mean diff.
Tons.			
1·0	110·0		6·15
1·5	103·9	6·1	
1·0	110·1	6·2	
1·5	104·0	6·1	
1·0	110·2	6·2	
1·5	104·0	6·2	
1·0	110·1	6·1	

Position of instrument, 2nd line from top. Index mark, 50. Right-hand scale descends when load is increased.

Results of the Experiments.

The experiments for each load were carried out in the manner described above, and the load was at first advanced by ½-ton intervals. Some difficulty was experienced, however, in obtaining satisfactory results for the first ½ ton, and ultimately the zero readings were rejected and a series taken between ¼ and ½ ton. This is summarised in Table II.

Table II.—Load ¼ to ½ ton.

Number of mark.	Distance from centre.	Mean scale differences.	Lateral strain in specimen.
1	3·175	4·68	$3 \cdot 92 \times 10^{-5}$
2	1·905	3·24	2·71
3	0·635	1·20	1·00
4	0·00	0·00	0·00
5	− 0·635	− 1·10	− 0·92
6	− 1·905	− 2·95	− 2·47
7	− 3·175	− 4·87	− 4·08

The minus sign denotes that the lateral strain was a contraction.

The values, of course, refer to the elastic state of the bar only, permanent set having been removed as far as possible before the readings were commenced, by alternate application and removal of the load.

The experiments were then continued in an exactly similar manner, the loads being increased by ½ tons until 2 tons was reached, and the last set of readings being taken between 2 and 2¼ tons.

Fracture took place at 2·46 tons. It showed sound material and occurred about 1 cm. from the middle of the span.

Table III contains the results of these experiments. At the centre two sets of readings were obtained at each load. These, in general, differed but slightly, and the mean values are given. The slight differences were probably due to the limits of accuracy having been reached.

Table III.

Position of instrument.	Increments of strain at different loads. (Scale readings.)				
	$\frac{1}{4}$ to $\frac{1}{2}$ ton.	$\frac{1}{2}$ to 1 ton.	1 to $1\frac{1}{2}$ tons.	$1\frac{1}{2}$ to 2 tons.	2 to $2\frac{1}{4}$ tons.
1	4·68	9·63	10·12	10·30	5·73
2	3·24	5·82	6·15	6·17	3·07
3	1·20	1·92	2·09	2·19	0·84
4	0·00	0·06	0·03	−0·12	−0·31
5	−1·10	−1·88	−2·13	−2·29	−1·69
6	−2·95	−5·25	−5·60	−6·00	−3·45
7	−4·87	−9·40	−9·00	−8·55	−3·96

The column headed "Position of the Instrument" refers to the number of the mark on the side of the beam in line with which the instrument was attached.

A minus sign denotes that the lateral strain was a contraction.

Distribution of Strain.

At the lower loads the maximum lateral strain per unit of load added is on the tensile side of the beam, slightly greater than that on the compressive side. As the load increases, however, the maximum lateral strain per unit of added load in the tensile fibres decreases, whereas that in the compressive fibres constantly increases. Hence, at fracture, this strain is very much greater on the compressive side than on the tensile.

The curves in fig. 2 are plotted from the second and sixth columns of Table III, and show the increment of lateral strain at different points in the depth of the beam. The full line refers to loads of $\frac{1}{4}$ and $\frac{1}{2}$ ton, and the broken line 2 to $2\frac{1}{4}$ tons. For the other loads the curves are given in fig. 3. They would be intermediate between the two curves of fig. 2.

Considering the strain curves for the tensile side of the beam given in figs. 2 and 3, it will be seen that at the lower loads a straight line is obtained, and that as the load is increased the lines become more and more curved in the direction of decreasing strain in the outer layers. This curvature is first noticed in Numbers 3 and 4, but not till the last curve, Number 5, is it very remarkable. On the compression side at the

FIG. 2.

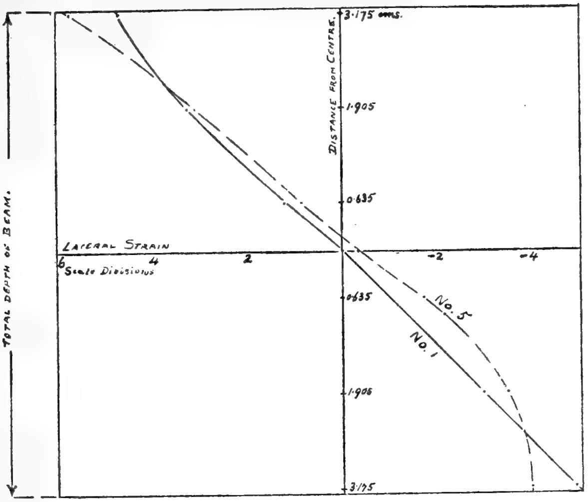
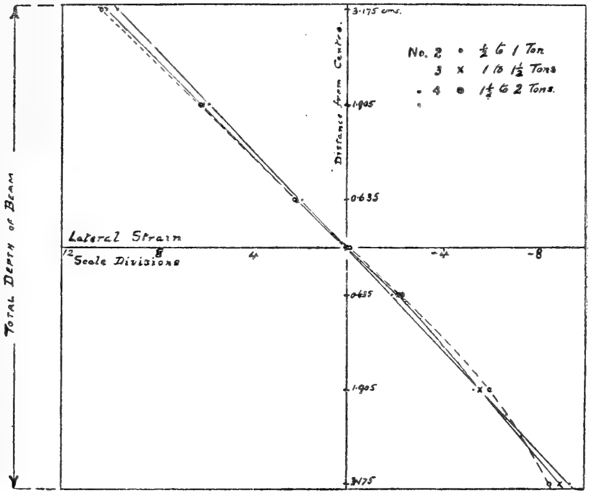


FIG. 3.



lowest load a similar curvature exists (see fig. 2), and although all the readings were carefully checked, the result was practically the same.* In Numbers 2, 3, and 4, however, the line is straight, whilst in Number 5 there is a pronounced curvature in the opposite direction.

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* It has been suggested that this unexpected curvature might be accounted for by additional strains due to the comparative smallness of the distance from the loaded part.

The general conclusions to be drawn from these curves are, firstly, that as the load increases, the increment of lateral strain in the outer tensile layers becomes less, and that this is accompanied by a much increased increment in the less stressed fibres nearer the neutral surface; and, second, that on the compression side, with higher loads, the increment of lateral strain in the outer fibres increases.

To what extent these actions take place before fracture can only be inferred from the experimental results. The greatest changes occurred between 2 and 2¼ tons, that is with a bending moment at the section under observation of not more than four-fifths of that which the beam was capable of resisting before fracture at the middle of the span; and between these limits the effects noticed would become much more remarkable.

The most striking fact brought out is, as will be seen later, that the values of the strains thus obtained are considerably lower than would be expected from theoretical considerations.

This fact appeared so important that it was decided to further verify the result by the examination of a second cast-iron beam.

Experiments on Cast-iron Beam No. 2.

The beam used for this further investigation was 7·578 cm. deep and 3·212 cm. broad. The span was 91·44 cm. as before, and the section examined 37 cm. from one of the supports. Readings were taken at the middle points of the depth and 1·2, 2·4, and 3·6 cm. both above and below these points.

The load was varied between ½ and 1 ton only. In all other respects the procedure of the previous experiments was repeated.

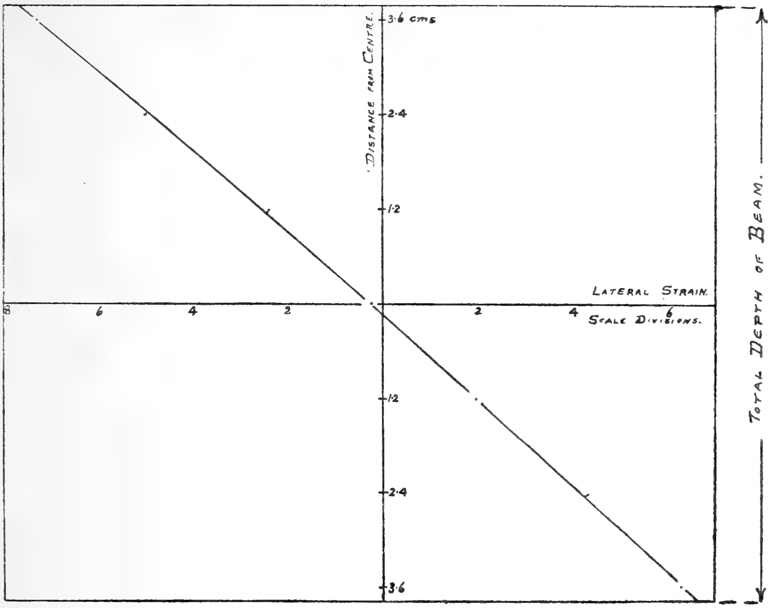
Table IV contains the results in differences of scale readings, the first column showing the distance of the point under observation from the middle of the depth. The curve is given in fig. 4.

It will be shown that these results corroborate those obtained from the beams previously described.

Table IV.

Position of instrument.	Differences of scale readings.
3·6	+7·30
2·4	+5·00
1·2	+2·40
0·0	+0·25
1·2	−1·97
2·4	−4·34
3·6	−6·28

Fig. 4.



Experiments on a Wrought-iron Beam.

The following experiments were made on a wrought-iron bar 3·122 cm. broad and 7·614 cm. deep. The span was the same as before, namely 91·44 cm., and the section under observation was 37 cm. from one of the supports.

In this case the readings were taken at the middle of the depth and at distances above and below of 1·2, 2·4, and 3·4 cm.

The method of procedure was exactly the same as that with the cast-iron beams.

Table V.

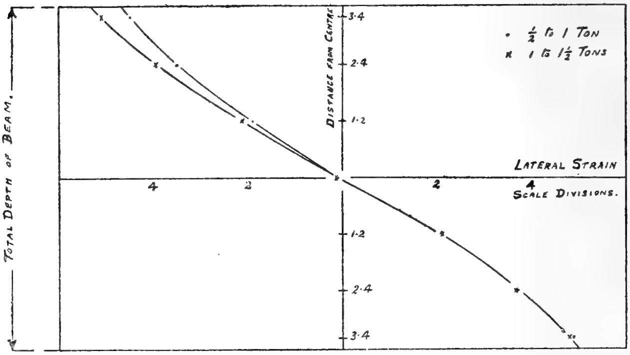
Distance from centre in cms.	Difference of scale readings.		Increments of strain per ½ ton.	
	½ to 1 ton.	1 to 1½ tons.	½ to 1 ton.	1 to 1½ tons.
3·4	+4·50	+5·10	$+3·30 \times 10^{-5}$	$+3·74 \times 10^{-5}$
2·4	+3·52	+3·96	+2·58	+2·91
1·2	+1·90	+2·12	+1·39	+1·56
0·0	+0·10	+0·12	+0·07	+0·09
1·2	−2·08	−2·12	−1·53	−1·56
2·4	−3·73	−3·68	−2·74	−2·70
3·4	−4·87	−4·80	−3·57	−3·52

The experiments were carried out with two increments of load, namely from $\frac{1}{2}$—1 ton and 1—$1\frac{1}{2}$ tons.

Table V contains the results both in differences of scale readings and in increments of strain per half ton.

The curves are plotted in fig. 5, and it will be seen that, whilst on the tension side the two curves practically agree, on the compression side the increment of strain is greater for the higher load.

FIG. 5.



Relations between Stress and Lateral Strain.

In order to appreciate the results of the foregoing experiments, and to compare them with those which would be expected from purely theoretical considerations, it became necessary to investigate the relations which obtain between the lateral strain and the stress producing it, in specimens under direct tensile and compressive forces.

To obtain these relations for the material of the first cast-iron beam specimens were cut from the less strained portions of the beam itself.

The tensile piece was 1·956 cm. in diameter, and was of uniform cross-section for a length of about 25·0 cm. The ends were screwed, and gripped in the testing machine by the ordinary ball and socket arrangements.

The instrument used was very similar in principle to that employed for measuring the lateral strain in the beams. It has been fully explained and illustrated in a paper published in the 'Philosophical Magazine' (6th series, vol. vi, p. 417, October, 1903).

The total magnification was 2744·5, and the load was advanced $\frac{1}{2}$ ton at a time.

The lateral strain per $\frac{1}{2}$ ton of load was then calculated by dividing the difference of scale readings by 84540, and the corresponding stress was 169·07 kilogrammes per square centimetre.

The results are tabulated and reduced in Table VI (Appendix).

The method of applying the load was similar to that described in the paper quoted above, so that the lateral strains do not include that due to permanent set. The specimen was not tested to fracture.

It will be seen that the elastic lateral strain is proportional to the stress. The ratio may be expressed

Lateral strain per kilogramme per square centimetre = $20 \cdot 66 \times 10^{-8}$.

The compression piece was cylindrical in shape, 2·436 cm. in diameter, and about 7·5 cm. high. The measurement of change of diameter was made about the middle of the specimen, in order that the effects of any suppression of the dilatation at the ends might be avoided.

The load was applied exactly as in the case of the tensile piece. Table VII (Appendix) contains the results. The total magnification was 3002.

The ratio of stress to strain is not quite independent of the stress. Near the origin the lateral strain per kilogramme per square centimetre = $20 \cdot 91 \times 10^{-8}$.

In conjunction with my colleague, Mr. E. L. Watkin, M.A., I have made a large number of experiments on these stress-strain relations for cast-iron. We hope to publish an account of them shortly. Some of the specimens used were cast at the same time and from the same ladle as the second beam described in this paper.

In Table VIII (Appendix) are embodied the results obtained from two of these, the particular ones chosen giving average and fairly representative values. The tension piece was 1·958 cm. in diameter, and the compression specimen 2·499 cm. diameter and 7·5 cm. high.

For comparison with the wrought-iron beam the relations were obtained from separate specimens of similar material.

In the case of tension the lateral strain has been found to be directly proportional to the stress when within the elastic limit, and the experiments described in a previous paper* gave, for the material in question, the value $13 \cdot 80 \times 10^{-8}$ for the lateral strain per kilogramme per square centimetre.

The wrought-iron compression specimen was similar to those of cast iron, 2·404 cm. diameter and 7·5 cm. high. The load was applied by $\frac{1}{2}$ ton intervals up to 2 tons, and then by tons to 4 tons. The lateral dilatations were not quite proportional to the stresses applied. The results are given in Table IX (Appendix).

The relations between stress and lateral strain for all three cases are shown graphically in the curves of fig. 6. The origin of the curve for comparison with Cast-iron Beam No. 2 is displaced to the right a distance of 10×10^{-5} unit.

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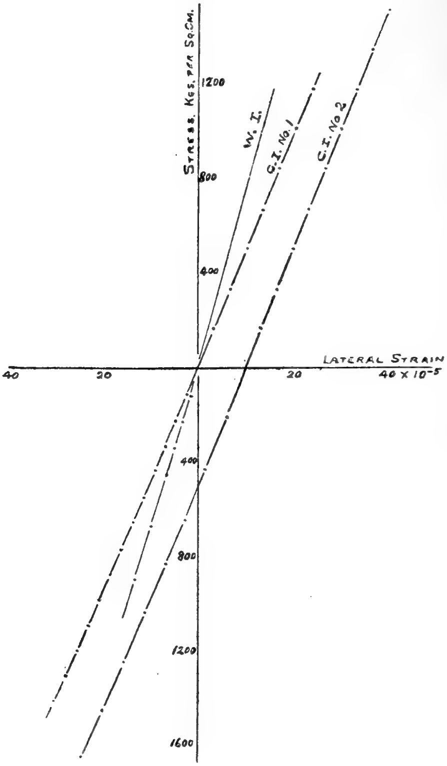
* See 'Phil. Mag.,' Sixth Series, vol. 6, No. 34, p. 417.

Distribution of Stress in Beams.

Assuming, as in the Coulomb-St. Venant theory, that the relations which hold between direct stress and lateral strain in tensile and compressive tests are, at the lower loads, the same as those which obtain between the stresses parallel to the axis of a beam and the corresponding strains at right-angles to that direction, the distribution of stress over the cross-section of a beam may be inferred from the observed lateral strains.

Fig. 6.



Thus, Table III gives the differences of scale readings measuring the lateral displacements in the first cast-iron beam. In Table X we have the total scale differences due to the different loads. This has been obtained directly from Table III by addition; and for the zero readings it has been supposed that from 0—$\frac{1}{4}$ ton the displacements would follow the same law as those between $\frac{1}{4}$ and $\frac{1}{2}$ ton. The actual lateral strains are then calculated as in Table XI. They are given by dividing the numbers in Table X by the magnification and the breadth of the beam, and multiplying the result by the scale unit.

By comparing these strains with Tables VI and VII (Appendix), or with the curve in fig. 6, we can find the direct stresses which must have accompanied them. These are given in Table XII for loads up to $1\frac{1}{2}$ tons.

Proceeding in a similar way for Cast-iron Beam No. 2 and for the wrought-iron beam, we obtain the second column of Table XIII and the last two columns of Table V for the actual strains per $\frac{1}{2}$ ton, and comparing these with fig. 6, we get the stresses given in the Appendix in the third column of Table XIII for the cast-iron beam, and in Table XIV for the wrought-iron beam.

The degree in which these stresses agree with those obtained by the ordinary theory of the bending prism, as given by Coulomb and generalised by Saint Venant, may be taken as a measure of the applicability of that theory. Thus Table XIV contains the theoretical values of the stresses calculated for the wrought-iron beam, and it will be seen that the agreement with the experimental results is not very good at the higher stresses.

The theoretical stresses for the cast-iron beams are given in Tables XV and XIII (Appendix).

Discussion of the Results for Cast Iron.

The theoretical stresses are not calculated for loads higher than $1\frac{1}{2}$ tons, as above that point some of the values obtained would be greater than the actual tensile strength of the material.

At the lower loads the lateral strains, and the stresses inferred from them, are generally lower than those obtained theoretically.

The amount of this discrepancy can best be seen by comparing Tables XII and XV.

It is also noteworthy that as the load is increased the results show a distinct shifting of the position of the neutral axis from the tensile towards the compressive side of the beam.

For loads over $1\frac{1}{2}$ tons not only is the theoretically calculated maximum stress greater than the ultimate stress the material will stand before fracture, but the values obtained by measuring the strains are also considerably greater than the maximum lateral strain which occurs in a tensile specimen.

The strain at fracture of a tensile specimen is about 35×10^{-5}. This is nearly equalled by the lateral strain noted at $2\frac{1}{4}$ tons load on the beam, whilst the maximum lateral strain at fracture would be greatly in excess of this value.

Saint Venant* assumes that, since all materials are capable of a small

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* See Saint Venant's 'Navier' (Paris, 1864), p. 178.

amount of flow when nearing the point of rupture, the curve of distribution of stress over the cross-section of the beam becomes parallel to the plane of the section when rupture commences.

This suggests that the higher lateral strains may be due to the amount of plasticity of the material just before fracture.

In a tensile test no such plasticity can be observed. This, however, does not invalidate the assumption, as it would be necessary to check the plastic elongation, in order to measure it before complete rupture occurred.

One effect of the permanent set in the direct strains may be to produce a more or less profound change in the distribution of stress over the section. This might give rise to the existence of lateral strains, which, though perhaps elastic in their nature, would not be included in the observed values of the experiments.

It has been pointed out* that, when the load on an overstrained beam is reduced, initial stresses may result. The strain in the outer layer might then be reduced by an amount greater than the real elastic strain due to the load removed.

It appears, however, to be far more probable that the departures from theory which have been noticed are due to the inapplicability of the theory employed; that is, mainly to the facts that (1) the Saint Venant solution implies different conditions of loading and end-fixing,† and (2) it is based on an assumption of the absolute proportionality of stress to strain.

In this connection it should be remarked that a different distribution of stress and strain over the section would not necessarily alter the deflection or curvature of the centre line of a beam due to a given bending couple.‡

The author hopes to deal more fully with these and other questions in a future paper.

Note on the Determination of Poisson's Ratio from Bending Experiments.

The instrument described in this paper provides a method not hitherto available for the determination of the values of the "stretch-squeeze" ratio for different materials.

The method appears at present only suitable for those materials and stresses to which the ordinary theory of bending may be applied.

It depends on the relation between the lateral strain at any section, and the deflection of the centre line produced by the applied bending couple.

Consider a beam, of uniform rectangular section, supported at the

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* See 'Encycl. Brit.,' vol. 22, article "Strength of Materials."

† See paper by L. N. G. Filon, 'Phil. Trans.,' A, vol. 201, p. 83.

‡ See Wiedemann's 'Annalen der Physik und Chemie,' vol. 52, 1894, paper by W. Voigt, p. 536.

ends, and subjected to bending by applying a transverse force at the centre of its span. Let the origin be at one end and let

x be taken in the direction of the length,
y = the distance from the neutral surface,
P = the stress at (x, y),
W = the load applied at the centre,
δ = the deflection of the centre line at x,
E = Young's modulus for the material,
σ = Poisson's ratio,
l, b, d = the length, breadth, and depth of the beam.

If at any section, $x \not> l/2$, the change produced in the breadth of the beam be measured when the load is applied, and, at the same time, the deflection of the centre line of the beam at that point be observed, then the angle turned through by the side of the beam

$$= \tan^{-1} \frac{\sigma \mathrm{P} b}{2 \mathrm{E} y},$$

and the deflection, δ, is given by

$$\delta = \frac{\mathrm{W} l^2}{16\mathrm{EI}} x - \frac{\mathrm{W}}{12\mathrm{EI}} x^3.$$

Combining these we obtain an expression from which σ can be at once determined, namely, angle turned through by side of beam

$$= \tan^{-1} \frac{12\sigma\delta b}{3l^2 - 4x^2}.$$

The left-hand side of this equation is obtained from the mean straight line through the points representing the lateral strain in the beam.

In conclusion the author must express his thanks to Messrs. F. C. Prentice and C. M. Rushton for assistance rendered at various times during the preparation of this paper.

APPENDIX.

Table VI (see fig. 6).—Tension and Lateral Contraction of Test Piece cut from Cast-iron Beam No. 1.

Load.	Lateral strain per ½ ton (scale readings).	Total lateral strain (scale divisions).	Lateral strain (elastic only).	Stress, kilos. per sq. cm.
Tons.				
½	2·95	2·95	$3·49 \times 10^{-5}$	169
1	2·95	5 90	6·98	338
1½	2·94	8·84	10·46	507
2	2·95	11·79	13·95	676
2½	2·97	14·76	17·46	845
3	2 96	17·72	20·96	1014
3½	2·95	20·67	24·45	1183

Table VII (see fig. 6).—Lateral Dilatation of Compression Specimen cut from Cast-iron Beam No. 1.

Load.	Increments of strain (scale divisions).	Total lateral strain (scale divisions).	Lateral strain (elastic only).	Stress, kilos. per sq. cm.
Tons.				
½	2·60	2·60	$2·26 \times 10^{-5}$	109
1	2·56	5·16	4·48	218
1½	2·63	7·79	6·76	327
2	2·63	10·42	9·05	436
2½	2·60	13·02	11·31	545
3	2·73	15·75	13·68	654
3½	2·75	18·50	16·06	763
4	2·67	21·17	18·38	872
4½	2·70	23·87	20·73	981
5	2·73	26·60	23·10	1090
5½	2·70	29·30	25·44	1199
6	2·75	32·05	27·83	1308
6½	2·80	34·85	30·26	1417

Table VIII (see fig. 6).—Stress and Lateral Strain in Cast Iron for comparison with Beam No. 2.

Tension.		Compression.	
Stress kilos. per sq. cm.	Lateral strain (elastic only).	Stress kilos. per sq. cm.	Lateral strain (elastic only).
169	$3{\cdot}37 \times 10^{-5}$	207	$3{\cdot}95 \times 10^{-5}$
337	6·80	414	8·12
506	10·25	621	12·48
675	13·65	828	16·83
843	17·08	1035	21·28
1012	20·45	1243	25·70
1181	23·85	1450	30·14
1350	27·32	1657	34·53
1518	30·75	1864	38·98
1687	34·25	2071	43·45
		2278	47·85
		2485	52·30

Table IX (see fig. 6).—Stress and Lateral Strain in Wrought Iron under Compression.

Load.	Increments of strain (scale divisions).	Total strain, scale divisions (elastic only).	Elastic lateral strain.	Stress, kilos. per sq. cm.
Tons.				
$\frac{1}{2}$	1·80	1·80	$1{\cdot}58 \times 10^{-5}$	112
1	1·84	3·64	3·20	224
$1\frac{1}{2}$	1·85	5·49	4·83	336
2	1·90	7·39	6·50	448
3	3·80	11·19	9·85	672
4	3·82	15·01	13·21	895

Table X.—Cast-iron Beam No. 1.

Point in depth of beam.	Total strains at different loads. (Scale divisions.)				
	0 to $\frac{1}{2}$ ton.	1 ton.	$1\frac{1}{2}$ tons.	2 tons.	$2\frac{1}{4}$ tons.
1	9·36	18·99	29·11	39·41	45·14
2	6·48	12·30	18·45	24·62	27·69
3	2·40	4·32	6·41	8·60	9·44
4	0·00	0·06	0·09	− 0·03	− 0·34
5	−2·20	− 4·08	− 6·21	− 8·50	−10·19
6	−5·90	−11·15	−16·75	−22·75	−26·20
7	−9·74	−19·14	−28·14	−36·69	−40·65

Table XI.—Cast-iron Beam No. 1.

Point in depth of beam.	Total strains at different loads.				
	½ ton.	1 ton.	1½ tons.	2 tons.	2¼ tons.
1	$7{\cdot}83 \times 10^{-5}$	$15{\cdot}89 \times 10^{-5}$	$24{\cdot}36 \times 10^{-5}$	$32{\cdot}98 \times 10^{-5}$	$37{\cdot}78 \times 10^{-5}$
2	5·42	10·29	15·44	20·60	23·17
3	2·01	3·62	5·36	7·20	7·90
4	0·00	0·05	0·08	− 0·03	− 0·28
5	−1·84	− 3·41	− 5·20	− 7·11	− 8·53
6	−4·94	− 9·33	−14·02	−19·04	−21·93
7	−8·15	−16·02	−23·55	−30·70	−34·02

Table XII (Table XI converted to Stresses).

Point in depth of beam.	Stresses in kilos. per sq. cm.		
	½ ton.	1 ton.	1½ tons.
1	−375	−760	−1152
2	−259	−492	− 738
3	− 96	−173	− 257
4	0	− 2	− 3
5	90	165	252
6	239	452	679
7	395	775	1140

− sign indicates compressive stress.

Table XIII.—Comparison of Theoretical Distribution of Stress with that obtained from the Lateral Strains in Cast-iron Beam No. 2.

Distance from centre in cm.	Stresses corresponding to observed lateral strains.		Calculated stress, kilos. per sq. cm.
	Lateral strain.	Stress.	
3·6	$5{\cdot}21 \times 10^{-5}$	−270	−290
2·4	3·57	−187	−194
1·2	1·71	− 90	− 97
0·0	0·18	− 9	00
1·2	−1·41	69	97
2·4	−3·10	153	194
3·6	−4·48	221	290

Table XIV.—Comparison of Theoretical Distribution of Stress with that obtained from the Lateral Strains in the Wrought-iron Beam.

Distance from centre in cm.	Stresses corresponding to observed lateral strains.		Calculated stress, kilos. per sq. cm.
	½ to 1 ton.	1 to 1½ tons.	
3·4	−231	−253	−278
2·4	−181	−199	−196
1·2	− 99	−107	− 98
0·0	− 5	− 6	0
1·2	111	113	98
2·4	198	196	196
3·4	259	255	278

Table XV.—Theoretically-calculated Stresses in Cast-iron Beam No. 1.

Distance from centre in cm.	Calculated stresses in kilos. per sq. cm.		
	½ ton.	1 ton.	1½ tons.
3·175	−460	−920	−1380
1·905	−276	−552	− 828
0·635	− 92	−184	− 276
0·00	00	00	00
0·635	92	184	276
1·905	276	552	828
3·175	460	920	1380

"Observations on the Sex of Mice.—Preliminary Paper." By S. Monckton Copeman, M.A., M.D., F.R.S., and F. G. Parsons, F.R.C.S. Received December 1, 1903,—Read January 28, 1904.

In this communication we record the results obtained, during a period of fifteen months, from the breeding of fancy mice. The experiments were commenced with the object of determining the extent, if any, to which the relative proportion of the sexes is capable of being influenced by varying conditions of age, nutrition, inter-breeding, etc. The work is still in progress, but in the hope of obtaining help and criticism from other observers, we think it desirable to put on record the experimental work that has already been carried out.

Our paper consists of two parts: (*a*) a list of the various crossings and their results, a record which we believe to be perfectly trustworthy; and (*b*) a series of conclusions at which we have arrived after careful study of the figures. As these conclusions form the most generally interesting portion of the paper, we have decided to place them first, especially as the mere statistics are only likely to be of use in the criticism of our deductions or in furnishing material by the aid of which others, not at first apparent, may perhaps be formulated.

In using these tables an explanation of our symbols may be necessary; it should, for instance, be noticed that all the bucks are indicated by small Greek letters—the does by Roman capitals. When a numeral follows the letter representing a particular doe, it shows that the bearer is the daughter of the doe whose indication is the letter alone. Thus, B^2 represents the second daughter which we kept of B, while $B^{2 \cdot 3}$ refers to the third daughter kept of B^2, and, consequently, the granddaughter of B. This method, of course, gives no clue to the male ancestry of a mouse, but this can always be ascertained by referring to the record of the particular animal. If we translate one line taken at random from the doe's record it will, perhaps, make our system clear:

C^1 (to ϵ). Sept. 22/02. 2 ♂, 4 ♀ (♀ $3\frac{1}{2}$ m., ♂ at least 4 m.)

This means that on September 22, 1902, C^1 (a daughter of C) bore two males and four females to the buck ϵ, and that at the time of conception (some 20 days before) the mother was $3\frac{1}{2}$ months old, while the father was at least 4 months. On looking at the top of the paragraph devoted to C^1 in the doe's record, her percentage will be seen, while all that is known of the ancestry of ϵ will be found at the top of the paragraph devoted to him in the buck's record.

Part I.—Generalisations.

The first question as to which we are desirous of obtaining information concerns the possibility of the male or female parent, in any particular instance, exerting a marked influence in the direction of a preponderance of male or female offspring. In seeking an answer to this question it is necessary to state that out of the total number of 493 young produced in the course of our experiments, 258, or 52·3 per cent., were of the male sex, and 235, or 47·7 per cent., of the female sex.

Taking the buck's descendants first, we have the following records :—

	♂.	♀.
α	71 (48 per cent.)	77 (52 per cent.)
β	18 (66·5 „)	9 (33·5 „)
γ	28 (46·5 „)	32 (53·5 „)
δ	52 (57 „)	39 (43 „)
ε	25 (43 „)	33 (57 „)
ζ	6	2
η	33 (66 „)	17 (34 „)
θ	7	6
ι	18 (51 „)	17 (49 „)
κ	0	3
	258	235

The cases of ζ, θ and κ may be left out of consideration, as the numbers of their offspring are so few. Of the rest, α and ι closely approach the normal, but β, δ and η have male offspring in excess, in connection with which fact it must be remembered that δ was the son of β. On turning to the record of β, we find that in the case of all five does with which he was mated, the male offspring was in excess, and in the case of δ also when put to five different does in succession, more males were produced than females in every instance. η was put to seven does—five times males were in excess, once the sexes were equal, while only once (with $C^{1\cdot2\cdot2\cdot1}$) were there more females than males produced. γ and ε on the other hand had female offspring in excess of male to a rather marked degree, and in each case the record is taken from more than fifty young. With γ the females were in excess of the males in four litters out of nine, while in two others the sexes were equal, so that in only three out of nine litters were there more males than females. In the case of ε the females were in excess in five litters out of nine, while in one other the sexes were equal. In this case too there were more males than females in only three litters out of nine. When it is remembered that all these bucks were put to at least five different does selected at random, it certainly does appear that some bucks have a tendency to beget more male, and others more female offspring. This, we believe, is the experience of many breeders of animals. It may be objected that these bucks were not placed with exactly the same series of does in each instance, and we regret that this is so, but by looking through the records of δ (a male producer) and ϵ (a female producer) it will be noted that they both had young by the three does B^2, C^1 and D^1, with the following results :—

	δ.		ε.	
	♂	♀	♂	♀
B^2	2	1	4	2
C^1	3	2	1	6
D^1	9	6	3	4
	14	9	8	12

so that their tendency to produce an excess of male or female offspring would appear to have had no relation to the particular does with which they were mated.

With regard to β (a male producer) and γ (a female producer), both had young by the four does A B D and F with the following results :—

	β.		γ.		
	♂	♀	♂	♀	
A	4	3	4	0	
B	1	4	7	9	(in 3 litters)
D	3	2	8	8	(in 2 ,,)
F	4	3	4	3	
	12	12	23	20	

This is not nearly so satisfactory a result for our contention, as with the same does γ (the female producer) actually produced a larger proportion of males than did β (the male producer).

We shall, therefore, content ourselves by saying that δ and η are instances of bucks which tend to produce an excess of male, and ϵ of one producing excess of female offspring.

On looking through the record of the does the point that is most striking is the behaviour of C^1 and her descendants :—

		♂.	♀.
C^{1}	produced	9	17
$C^{1\cdot 2}$	”	3	11
$C^{1\cdot 2\cdot 1}$	”	2	3
$C^{1\cdot 2\cdot 2}$	”	2	5
$C^{1\cdot 2\cdot 2\cdot 1}$	”	5	10
		21	46
		(31 per cent.)	(69 per cent.)

On the other hand $C^{1\cdot 3}$ had 17 male and 6 female young, but she was a daughter of ϵ not of α. There is, however, an influence at work in this family which may possibly account for the excess of females over males, and it is that the same buck α was largely responsible for the results, as each of the does was put to him; thus $C^{1\cdot 2\cdot 2\cdot 1\cdot}$ was his daughter on the male side, and his great, great granddaughter on the female. It has already been noted that α was a buck who produced practically the normal proportion of young, so that his influence alone is not likely to account for the excess of females. But the practice of inbreeding a buck with his daughter, granddaughter, &c., for several generations may perhaps account for an excess of female offspring; this theory being strengthened by the fact that $C^{1\cdot 2\cdot 2\cdot 1\cdot}$ was the only doe with which the male-producing buck η had more females than males in a litter. The clue is one which we are now following up and which we would suggest to other breeders as worthy of further investigation. On the whole, our statistics seem to point to the fact that certain bucks and does tend to produce a preponderance of one sex, but that the influence is greater in the male parent; also that a doe which is the result of prolonged inbreeding is more likely to produce female than male offspring.

The next point inquired into refers to the possibility of the number of young in a litter exerting any influence on the proportion of the sexes. If there is any basis of fact in the theory that the amount of nourishment an embryo receives affects the determination of its sex, we should expect that large litters would show a predominance of one sex and small litters a predominance of the other. Up to the present we have neglected the young which were eaten by their mothers before their sex had been determined, because we have no reason to believe that the mother preferred to eat male or female young, and, in taking large numbers, we have presumed that as many male as female young would be eaten in this way. We have, however, kept a record of the total number in each litter when first seen; often an hour or two, and never more than 12 hours after the birth. A doe hardly ever eats the whole of a young one at once, as she apparently prefers to first eat the viscera and brains of several, leaving their carcases for a future meal. So that, even after 12 hours from the birth, it is easy to see, by the remains, how many young have been eaten. The record of these litters, with the proportions of the surviving young, are as follows:—

Number.	Doe.	Number in litter.	Surviving males.	Surviving females.	Eaten.
1......	A	4	4	—	
2......	,,	7	4	3	
3......	,,	4	4	—	
4......	B	7	4	3	
5......	,,	5	4	1	
6......	,,	5	3	2	
7......	,,	5	2	3	
8......	,,	6	2	4	
9......	C	5	2	2	1
10......	,,	3	3		
11......	D	4	1	3	
12......	,,	6	3	2	1
13......	,,	8	4	4	
14......	,,	8	4	4	
15......	,,	7	7		
16......	E	8	4	4	
17......	F	9	3	1	5
18......	,,	7	4	3	
19......	,,	7	4	3	
20......	,,	9	1	6	2
21......	B^1	7	2	3	2
22......	B^2	6	2	1	3
23......	,,	8	4	2	2
24......	,,	3	2	1	
25......	,,	10	4	4	2
26......	B^3	8	2	5	1
27......	,,	9	4	5	
28......	,,	7	2	3	2
29......	C^1	7	3	2	2
30......	,,	7	1	6	
31......	,,	9	2	7	
32......	,,	7	3	2	
33......	C^2	2	1	1	
34......	,,	9	5	1	3
35......	,,	6	2	4	
36......	,,	7	3	3	1
37......	,,	6	3	3	
38......	,,	3	—	3	
39......	D^1	7	2	5	
40......	,,	7	3	4	
41......	,,	8	5	3	
42......	,,	7	5	2	
43......	,,	8	4	4	
44......	D^2	5	1	4	
45......	,,	8	4	4	
46......	,,	11	4	3	4
47......	,,	9	3	3	3
48......	D^3	6	3	3	
49......	,,	5	1	4	
50......	,,	8	3	5	
51......	$B^{2\cdot1}$	8	2	3	3
52......	,,	4	1	2	1
53......	,,	9	6	3	
54......	,,	5	3	2	

Number.	Doe.	Number in litter.	Surviving males.	Surviving females.	Eaten.
55	$B^{2\cdot1}$	6	4	2	
56......	$B^{2\cdot2}$	7	5	2	
57......	$B^{3\cdot1}$	5	4	1	
58......	$B^{3\cdot2}$	10	6	4	
59......	$C^{1\cdot2}$	7	1	5	1
60......	"	8	2	6	
61......	$C^{1\cdot3}$	8	5	3	
62......	"	8	6	2	
63......	"	7	6	1	
64......	$D^{1\cdot1}$	5	2	1	
65	$D^{2\cdot3}$	5	5		
66......	$D^{3\cdot3}$	4	2	2	
67......	$C^{1\cdot2\cdot1}$	5	2	3	
68......	$C^{1\cdot2\cdot2}$	7	2	5	
69	$C^{1\cdot2\cdot2\cdot1}$	10	2	6	2
70......	"	7	3	4	
71......	G	3	1	2	
72	"	7	5	2	
73......	H	8	6	2	
		487*	231	211	45

This table shows that 487 young were produced in 73 litters, thus giving an average of 6·7 for a litter. There are 45 litters of 7 or over, and 28 of under 7. These we may speak of as large and small litters respectively. If the number of young in the 45 large litters is added up it amounts to 356, and of these 164 (46 per cent.) were males; 155 (43·5 per cent.) females; while 37 (10·5 per cent.) were eaten and their sex undetermined. Similarly if the number of young in the 28 small litters is taken we get a total of 131; of these 67 (51 per cent.) were males; 56 (43·7 per cent.) females; while 8 (6 per cent.) were eaten. Tabulated these results are as follows:—

	♂ per cent.	♀ per cent.	Eaten per cent.
Large litters (over 6)	46	43·5	10·5
Small litters (6 or under) ...	51	43	6

This result suggests two conclusions—firstly, that in a large litter a greater percentage of young is eaten by the mother, which is, perhaps, what one would expect, and, secondly, that in small litters there is a slightly greater percentage of males than in large ones. Other things being equal, one might fairly suppose that, in a small litter, each individual embryo would be better nourished than in a large litter, and this

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* The records of the six does with which the buck δ was placed in a large cage have not been added in, because we have no means of knowing how many were eaten.

supposition is strengthened by our experience that, in small litters, the young are individually of greater size than we have found to be the case in larger families. The difference between the percentages of the large and small litters is not great enough to enable any general conclusion to be drawn from it, but, so far as it goes, it suggests that ample nourishment is more likely to result in an excess of male rather than of female offspring.

The next point to be discussed is whether the age of either parent affects the proportion of sexes in the young. As an aid to the determination of this question the records of all the does of two months or under at the time of conception, may be compared with those of all the does of 6 months or over.

We have records of 21 litters produced by does up to 2 months of age. These give a total of 108 young, of which 55 (51 per cent.) are males, and 53 (49 per cent.) females. With does over 6 months old we have also records of 21 litters, with a total of 134 young; of these 74 (55 per cent.) are males and 60 (45 per cent.) females. These figures show that there is an increase in the proportion of males to females in the progeny of those does over 6 months of age. It will now be worth while noticing that the does at intermediate ages, that is, from 2½—5½ months (inclusive), produced 27 litters, giving a total of 173 young, of which 85 (49 per cent.) were males, and 88 (51 per cent.) females.

Tabulated, we arrive at the following results:—

	♂ per cent.	♀ per cent.
Does up to and including 2 months	51	49
,, between 2½ and 5½ months............	49	51
,, of 6 months and over	55	45

This suggests that, in does over 6 months old, the proportion of males to females increases, but we are unable to deduce anything from this knowledge at present, for the reason that we do not know the duration of the breeding period of a doe, nor at what stage the young are likely to receive the greatest amount of nourishment. Moreover, the difference in the proportion of the sexes among the young is not a very great one, and, doubtless, there are many other influences at work, the effect of which it is difficult to eliminate. One of these is the tendency, of which proof has been adduced, that certain bucks produce a preponderance of male or female offspring, but this tendency, to a certain extent, has been neutralised by the fact that our statistics have been drawn from the pairing of ten bucks with twenty-eight does. It is unfortunate that the buck of which we possess most records should have been paired so extensively with does under 6 months of age, as had he been put with an equal number of old and young does, it would have been interesting to have determined whether the percentages of male and female young would have varied. Some little information may, however, be obtained from the record of δ, who when he was mated with five does of an average age of 7 months, produced 65 per cent. of male offspring, while with six does averaging 2 months old the male offspring was only 53 per cent. Though the numbers here are small (eighty-three young in all), the record certainly supports the suggestion that an adult doe is more likely to bring forth an excess of male offspring than a very young one.

With regard to the effect of the age of the buck we are unfortunately unable to give any definite opinion, since the bucks from which we chiefly bred were apparently fully grown when purchased, though we had no means of actually determining their age. It will be seen, on looking at the record of α, that in his later offspring females predominated, but there three factors at least are concerned: (1) The advanced age of the buck; (2) the predisposition of C^1 and her descendants to produce females; and (3) the possibility that the excessive inbreeding to which these mice were subjected may have led to an increase of females.

The last factor we propose to consider at the present time is that of external temperature. The mice were kept in an unheated greenhouse in which the temperature usually ranged between 80° and 100° F. during the day-time in summer, while in winter it often descended several degrees below freezing point. It may be interesting to contrast the records of the young born in July, August and September with those born in December, January, February and March, and both of these with the total records for the whole 15 months over which our experiments extended. During the 3 hot months 136 young were born, of which 75 (55 per cent.) were males, and 61 (45 per cent.) females, while during the 4 cold months 127 young were born, of which 65 (51 per cent.) were males and 62 (49 per cent.) females.

	♂.	♀.
Three hot months...............	55 per cent.	45 per cent.
Four cold months	51 ,,	49 ,,
Total records for 15 months...	53·3 ,,	46·7 ,,

These results do not seem to indicate that temperature or time of year exerts any marked effect on the proportion of the sexes in the young.

In conclusion it must be confessed that we have learnt comparatively little from this 15 months' experimental work on mice, but we are hopeful that the labour has not been entirely expended in vain, and that at least certain clues have been obtained which may usefully be followed up, both by ourselves and other breeders. So far as our experiments have gone, the chief points of interest would seem to be :—

1. That the number of males born is slightly larger than that of females.

2. That certain males beget a markedly large proportion of male, and others of female offspring.

3. That there is some evidence that this tendency is hereditary.

4. That certain does tend to bear an excess of either male or female offspring, but the evidence of this is not so conclusive as in the case of the male.

5. That mice bear inbreeding between a male and his offspring for five generations without loss of fertility or apparent bodily degeneration—this inbreeding in our one series of experiments being attended with a large excess of female offspring.

6. That the average number of young in a litter, judged from seventy-three litters, is 6·7.

7. That in large litters more of the young are likely to be eaten by the mother than in small ones.

8. That in large litters the proportion of females is greater than in small ones.

9. That more males are produced by does over 6 months than is the case with does under that age.

10. That the temperature and time of year at which impregnation occurs seem to exert little or no influence on the proportion of male and female offspring.

Of course the larger the number of experiments the greater will be the likelihood of obtaining reliable statistics, so that it will be interesting to determine whether another year's breeding confirms or neutralises the results now recorded, but we think it desirable to publish our observations at this stage, for two reasons, firstly, to invite criticism on our methods and suggestions for future work, and, secondly, to indicate to other breeders clues which would appear worth while following up.

It should, perhaps, be mentioned that, in each instance, careful record has been kept of the *colour* of the individual mice mated together, and also of that of their progeny. These results we have handed over to Mr. Bateson, by whom they have been utilised in connection with his investigation of Mendel's theory of inheritance of parental characteristics.*

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* 'Zool. Soc. Proc.,' 1903, vol. 2, p. 83.

Part II.—Records.

A. *Does' Records.*

1. Doe A (bought on April 18, 1902, under breeding age).

		♂.	♀.	♀.	♂.
To α	May 9/02	4	0	About 2 m.	At least 4 m.
,, β	June 22/02	4	3	,, 3 ,,	,, 5 ,,
,, γ	Oct. 2/02	4	0	,, 7 ,,	,, 4 ,,
		12	3		

2. Doe B (bought April 18, 1902, under breeding age).

		♂.	♀.	♀.	♂.
To α	May 11/02	4	3	About 2 m.	At least 4 m.
,, β	July 16/02	4	1	,, 3½ ,,	,, 4½ ,,
,, γ	Sept. 8/02	3	2	,, 5 ,,	3 m.
,, γ	Oct. 18/02	2	3	,, 6½ ,,	4½ ,,
,, γ	Nov. 11/02	2	4	,, 7½ ,,	5½ ,,
		15	13		

3. Doe C (bought April 18, 1902, under breeding age).

		♂.	♀.	♀.	♂.
To α	May 17/02	2	2	About 2 m.	At least 4 m.
,, β	July 2/02	3	–	,, 3 ,,	,, 4 ,,
		5	2		

4. Doe D (bought April 18, 1902, under breeding age).

		♂.	♀.	♀.	♂.
To α	May 22/02	1	3	About 2 m.	At least 4 m.
,, β	July 1/02	3	2	,, 3 ,,	,, 4 ,,
,, γ	Aug. 20/02	4	4	,, 4½ ,,	2 m.
,, γ	Oct. 10/02	4	4	,, 6 ,,	3½ ,,
,, α	Nov. 25/02	7	–	,, 7 ,,	At least 10 m
		19	13		

5. Doe E (bought April 18, 1902, under breeding age).

		♂.	♀.	♀.	♂.
To α	May 26/02	4	4	About 2 m.	At least 4½ m.
		4	4		

6. Doe F (bought April 18, 1902, under breeding age).

		♂.	♀.	♀.	♂.
To α ……	June 4/02	0	1	About 2 m.	At least 4½ m.
,, β ……	July 23/02	4	3	,, 3½ ,,	,, 4 ,,
,, γ ……	Sept. 19/02	4	3	,, 5½ ,,	,, 3½ ,,
,, α ……	Oct. 30/02	1	6	,, 7 ,,	,, 9 ,,
		12	13		

7. Doe B^1 ($B+\alpha$, born May 11, 1902).

		♂.	♀.	♀.	♂.
To α ……	July 14/02	2	1	2 m.	At least 6 m.
		2	3		

8. Doe B^2 ($B+\alpha$, born May 11, 1902).

		♂.	♀.	♀.	♂.
To α ……	July 12/02	2	1	2 m.	At least 6 m.
,, ε ……	Sept. 25/02	4	2	4½ ,,	,, 4 ,,
,, δ ……	Feb. 2/03	2	1	9½ ,,	,, 6 ,,
,, η ……	May 30/03	4	4	12 ,,	,, 4 ,,
		12	8		

9. Doe B^3 ($B+\alpha$, born May 11, 1902).

		♂.	♀.	♀.	♂.
To α ……	July 29/02	2	5	2 m.	At least 6½ m.
,, ε ……	Oct. 1/02	4	5	4 ,,	,, 4 ,,
,, θ ……	Nov. 13/02	2	3	5 ,,	2 m.
		8	13		

10. Doe C^1 ($C+\alpha$, born May 17/02).

		♂.	♀.	♀.	♂.
To α ……	July 25/02	3	2	2 m.	At least 6½ m.
,, ε ……	Sept. 27/02	1	6	4 ,,	,, 4 ,,
,, γ ……	Nov. 25/02	2	7	6 ,,	,, 5½ ,,
,, δ ……	Feb. 2/03	3	2	8 ,,	,, 6 ,,
		9	17		

11. Doe C^2 ($C+\alpha$, born May 17, 1902).

		♂.	♀.	♀.	♂.
To α	Aug. 1/02	1	1	2 m.	At least 6½ m.
,, α	Aug. 22/02	5	1	2½ ,,	,, 7 ,,
,, ε	Sept. 22/02	2	4	3½ ,,	,, 4 ,,
,, ι	Dec. 20/02	3	3	6½ m.	3½ m.
,, ι	Feb. 4/03	3	3	8 ,,	5 ,,
,, κ	May 20/03	–	3	12 ,,	2 ,,
		14	15		

12. Doe D^1 ($D+\alpha$, born May 22, 1902).

		♂.	♀.	♀.	♂.
To α	July 31/02	2	5	1½ m.	At least 6½ m.
,, ε	Oct. 5/02	3	4	3½ ,,	,, 4 ,,
,, θ	Nov. 18/02	5	3	5 m.	2½ m.
,, δ	Jan. 13/02	5	2	7 ,,	5½ ,,
,, δ	Feb. 24/03	4	4	8½ ,,	7 ,,
		19	18		

13. Doe D^2 ($D+\alpha$, born May 22, 1902).

		♂.	♀.	♀.	♂.
To α	Aug. 7/02	1	4	2 m.	At least 7 m.
,, ε	Sept. 30/02	4	4	3½ ,,	,, 4 ,,
,, ε	Oct. 23/02	4	3	4½ ,,	,, 5 ,,
,, ι	Dec. 21/02	3	3	6½ ,,	3½ m.
		12	14		

14. Doe D^3 ($B+\alpha$, born May 22, 1902).

		♂.	♀.	♀.	♂.
To α	Aug. 2/02	3	3	2 m.	At least 7 m.
,, ε	Sept. 29/02	1	4	3 ,,	,, 4
,, γ	Dec. 12/02	3	5	5½ ,,	6 m.
		7	12		

15. Doe $B^{2\cdot 1}$ ($B^2+\alpha$, born July 12, 1902).

		♂.	♀.	♀.	♂.
To α	Sept. 11/02	2	3	2 m.	At least 8 m.
,, α	Nov. 17/02	1	2	4 ,,	,, 10 ,,
,, δ	Jan. 15/03	6	3	6 ,,	5½ m.
,, α	May 21/03	3	2	10 ,,	At least 16 m.
,, η	July 8/03	4	2	12 ,,	,, 5 ,,
		16	12		

16. Doe $B^{2\cdot2}$ ($B^2+\eta$, born May 30, 1902).

		♂.	♀.	♀.	♂.
To η	July 27/02	5	2	5 w.	At least 6 m.
		5	2		

17. Doe $B^{3\cdot1}$ ($B^3+\alpha$, born July 29, 1902).

		♂.	♀.	♀.	♂.
To α	Oct. 5,02	4	1	2 m.	At least 9 m.
		4	1		

18. Doe $B^{3\cdot2}$ ($B^3+\alpha$, born July 29, 1902).

		♂.	♀.	♀.	♂.
To ι	Oct. 30/02	6	4	2½ m.	1½ m.
		6	4		

19. Doe $C^{1\cdot2}$ ($C^1+\alpha$, born July 25, 1902).

		♂.	♀.	♀.	♂.
To α	Oct. 2/02	1	5	7 w.	At least 9 m.
„ α	Jan. 10/03	2	6	5 m.	„ 12 „
		3	11		

20. Doe $C^{1\cdot3}$ ($C^1+\epsilon$, born September 27, 1902).

		♂.	♀.	♀.	♂.
To δ	Feb. 6/03	5	3	4 m.	6 m.
„ ζ	May 20/03	6	2	7 „	2 „
„ η	June 22/03	6	1	8 „	At least 5 m.
		17	6		

21. Doe $D^{1\cdot1}$ ($D^1+\alpha$, born July 31, 1902).

		♂.	♀.	♀.	♂.
To ϵ	Dec. 5/02	2	1	3½ m.	At least 6 m.
		2	1		

22. Doe $D^{2\cdot3}$ ($D^2+\alpha$, born August 7, 1902).

		♂.	♀.	♀.	♂.
To α	Oct. 26/02	5	—	2 m.	At least 9½ m.
		5	—		

23. Doe $D^{3\cdot3}$ ($D^3 + \alpha$, born August 2, 1902).

		♂.	♀.	♀.	♂.
To ι	Oct. 28/02	2	2	2 m.	1½ m.
		2	2		

24. Doe $C^{1\cdot2\cdot1}$ ($C^{1\cdot2} + \alpha$, born October 2, 1902).

		♂.	♀.	♀.	♂.
To α	Dec. 22/02	2	3	2 m.	At least 11½ m.
		2	3		

25. Doe $C^{1\cdot2\cdot2}$ ($C^{1\cdot2} + \alpha$, born October 2, 1902).

		♂.	♀.	♀.	♂.
To α	Dec. 27/02	2	5	2 m.	At least 11½ m.
		2	5		

26. Doe $C^{1\cdot2\cdot2\cdot1}$ ($C^{1\cdot2\cdot2} + \alpha$, born December 27, 1902.)

		♂.	♀.	♀.	♂.
To α	May 13/03	2	6	4 m.	At least 16 m.
,, η	June 24/03	3	4	5 ,,	,, 5 ,,
		5	10		

27. Doe G (age and ancestry lost, but under 3 months).

		♂.	♀.	♀.	♂.
To ι	May 22/03	1	2	About 3 m.	8½ m.
,, η	June 29/03	5	2	,, 4 ,,	At least 5 m.
		6	4		

28. Doe H (age and ancestry lost, but under 3 months).

		♂.	♀.	♀.	♂.
To η	July 17/03	6	2	About 3 m.	At least 6 m.
		6	2		

B. *Bucks' Record.*

1. α (bought April 18, 1902, at least 4 months old).

			♂.	♀.
By	A	May 9/02	4	—
,,	B	May 11/02	4	3
,,	C	May 17/02	2	2
,,	D	May 22/02	1	3
,,	D	Nov. 25/02	7	—
,,	E	May 26/02	4	4
,,	F	June 4/02	3	1
,,	F	Oct. 30/02	1	6
,,	B^{1}	July 14/02	2	3
,,	B^{2}	July 12/02	2	1
,,	B^{3}	July 29/02	2	5
,,	C^{1}	July 25/02	3	2
,,	C^{2}	Aug. 1/02	1	1
,,	C^{2}	Aug. 22/02	5	1
,,	D^{1}	July 31/02	2	5
,,	D^{2}	Aug. 7/02	1	4
,,	D^{3}	Aug. 2/02	3	3
,,	B$^{2\cdot1}$	Sept. 11/02	2	3
,,	B$^{2\cdot1}$	Nov. 17/02	1	2
,,	B$^{2\cdot1}$	May 21/03	3	2
,,	B$^{3\cdot1}$	Oct. 5/02	4	1
,,	C$^{1\cdot2}$	Oct. 2/02	1	5
,,	C$^{1\cdot2}$	Jan. 10/03	2	6
,,	D$^{2\cdot3}$	Oct. 26/02	5	—
,,	C$^{1\cdot2\cdot1}$	Dec. 22/02	2	3
,,	C$^{1\cdot2\cdot2}$	Dec. 27/02	2	5
,,	C$^{1\cdot2\cdot2\cdot1}$	May 12/03	2	6
			71	77

2. β (bought May 30, 1902, at least 4 months old).

			♂.	♀.
By	A	June 22/02	4	3
,,	B	July 16/02	4	1
,,	C	July 2/02	3	—
,,	D	July 1/02	3	2
,,	F	July 23/02	4	3
			18	9

3. γ (B + α, born May 11, 1902).

		♂.	♀.
By A	Oct. 2/02	4	—
„ B	Sept. 8/02	3	2
„ B	Oct. 18/02	2	3
„ B	Nov. 11/02	2	4
„ D	Aug. 20/02	4	4
„ D	Oct. 10/02	4	4
„ F	Sept. 19/02	4	3
„ C^1	Nov. 25/02	2	7
„ D^3	Dec. 12/02	3	5
		28	32

4. δ (B + β, born July 16, 1902).

This buck, when 4 weeks old, was placed in a large cage with the following six does, all of which were under breeding age: $B^{1\cdot1}$, F^1, F^2, $D^{2\cdot1}$, $D^{2\cdot2}$, $C^{1\cdot1}$. The young of course were all mixed, but altogether 27 ♂ and 24 ♀ were born between November 9, 1902, and January 17, 1903. The buck was then put separately to five does with the following result:—

		♂.	♂.
By B^2	Feb. 2/03	2	1
„ C^1	Feb. 2/03	3	2
„ D^1	Jan. 13/03	5	2
„ D^1	Feb. 24/03	4	4
„ $B^{2\cdot1}$	Jan. 15/03	6	3
„ $C^{1\cdot3}$	Feb. 6/03	5	3
		21	11
Previous record with young does		27	24
		48	35

5. ε (Bought September 6, 1902, at least 4 months old.)

		♀.	♀.
By B^2	Sept. 25/02	4	2
„ B^3	Oct. 1/02	4	5
„ C^1	Sept. 27/02	1	6
„ C^2	Sept. 22/02	2	4
„ D^1	Oct. 5/02	3	4
„ D^2	Sept. 30/02	4	4
„ D^2	Oct. 23/02	4	3
„ D^3	Sept. 29/02	1	4
„ $D^{1\cdot1}$	Dec. 5/02	2	1
		25	33

6. ζ (ancestry lost, about 2 months old).

		♂.	♀.
By $C^{1\cdot 3}$..........	May 20/03	6	2

7. η (bought May 1, 1903, at least 4 months).

		♂.	♀.
By $C^{1\cdot 3}$	June 22/03	6	1
,, B^2............	May 30/03	4	4
,, $C^{1\cdot 2\cdot 2\cdot 1}$	June 24/03	3	4
,, G	June 29/03	5	2
,, $B^{2\cdot 1}$	July 8/03	4	2
,, H	July 17/03	6	2
,, $B^{2\cdot 2}$	July 27/03	5	2
		33	17

8. θ ($C^2+\alpha$, born August 22, 1902).

		♂.	♀.
By B^3............	Nov. 13/02	2	3
,, D^1	Nov. 18/02	5	3
		7	6

9. ι ($C^2+\alpha$, born August 22, 1902).

		♂.	♀.
By C^2............	Dec. 20/02	3	3
,, C^2............	Feb. 4/03	3	3
,, D^2	Dec. 21/02	3	3
,, $B^{3\cdot 2}$...........	Oct. 30/02	6	4
,, $D^{3\cdot 3}$..........	Oct. 28/02	2	2
,, G	May 22/03	1	2
		18	17

10. κ ($C^2+\iota$, born March 1, 1903).

		♂.	♀.
By C^2...........	May 20/03	0	3

"Observations upon the Acquirement of Secondary Sexual Characters, indicating the Formation of an Internal Secretion by the Testicle."* By S. G. SHATTOCK and C. G. SELIGMANN.

Communicated by Professor J. R. BRADFORD, F.R.S. Received December 14, 1903,—Read January 28, 1904.

The Problem Stated.

The question taken up in the present communication may be concisely stated as follows :—

The most prominent and obvious function of the testicle is the formation of the sperm. Under normal circumstances this is discharged ; it constitutes, that is to say, an external secretion.

In spermatogenesis the male attributes culminate. There is, however, another element in maleness, of a different kind, less essential, yet in many cases well pronounced, viz., the acquirement of certain external characters which distinguish the male from the female in many groups of living forms.

That the development of such secondary characters is related to some function of the testicle, appears from the results which follow castration when carried out before the advent of sexual maturity. On what, then, does the production of these characters depend ?

It is conceivable that the result may be due to a nervous reflex arising out of the physical function of the sexual mechanism. This view our observations seem to us to disprove.

The genesis of the external male characters must, in our opinion, be transferred from the influence of the nervous system to the realm of chemistry. It depends, with more probability, upon the formation of a second secretion by the testicle, the absorption of which into the circulation induces the metabolic changes that reveal themselves as secondary sexual characters.

The suggestion that such an internal secretion might be elaborated by the "interstitial cells," which lie in groups between the tubuli, was put forward by one of us (S. G. S.)† in 1897.

The experiments to be recorded were, in fact, primarily designed with the object of eliminating any part that might be played by the tubuli in this connection, and so of determining whether any function could be ascribed to the cells named.

They consisted in ligation of the vasa deferentia in the young of certain forms in which the male exhibits marked secondary characters.

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* Towards the expenses of this research a grant was made by the Council of the Royal College of Surgeons ; and by the British Medical Association, on the recommendation of the Scientific Grants Committee of the Association.

† 'British Medical Journal,' Feb. 20, 1897.

It appeared possible that the epithelium of the testicular tubuli would, under these circumstances, on proliferating, undergo degeneration and atrophy from the pressure due to its own accumulation, whereas the interstitial cells of the stroma might remain intact. This result, however, did not ensue, but others, which we venture to record as bearing on the problem under consideration.

The forms selected were a breed of sheep (Herdwick), the male of which is furnished with long recurved horns, of which the female is quite destitute, and the common fowl.

Observations upon Sheep.

We owe to the kindness of two friends the opportunity of observing many castrated sheep, as well as a certain number of others on which some form of obliteration of the vas deferens had been practised. Besides the horned (Herdwick) sheep already referred to, we made observations upon the hornless Southdown, in which the results, though less striking, are none the less constant.

The results of occlusion of the vasa deferentia in the Herdwick breed have to be compared with those following castration, and both with the normal standard. Lambs of the same age were selected, and the procedures mentioned were carried out at about the same time.

The occlusion of the vas deferens was effected a short way above the testicle by the application of a silk ligature in two places and division of the duct between. The animals were examined at different periods during their growth, and were killed when fully developed at ages of from 10—14 months.

In those *castrated* either no horns appeared externally, and on preparation the skull exhibited only two low osseous tubercles or horn-cores, or very diminutive horns were produced, and beneath them a slightly more prominent core than in the first case.

In the *ewe* of the Herdwick breed there is no external trace of horn, nor does the prepared skull show any osseous core.

As contrasted with the results of castration, those of *vasotomy* are very striking. The horns attain their full size, and the skull its complete male characters, so that the head in no way differs from that of the normal or intact ram.

The form of the skull is modified by castration, not by double vasotomy, the modification in question being obviously correlated with the absence of horns.

The skull of the castrated sheep, or wether, is less rugged, and the bones thinner, but besides such general differences the plane of the os frontis is continued backwards behind the orbits at a very obtuse angle.

In the intact ram, and equally after vasotomy, the plane of the frontal behind the orbit lies almost at a right angle with the interorbital portion of the bone, the horn-cores arising from the upper or horizontal area.

Although the skull generally is thicker in either case than in the wether, this alone does not account for the difference in external form; the cranial cavity presents a corresponding extension in the frontal region.

In the configuration of its skull, as in the absence of horns, the castrated animal precisely resembles the hornless ewe of the breed.

We have studied the effects of the same procedures upon sheep of a well-known pedigree Southdown herd. The result in such animals is less striking than in the Herdwick, partly because each sex is hornless, and partly because amongst Southdowns individual variations in the form of the head are not uncommon: thus, whilst the head of the wether usually offers a marked contrast to that of the ram, in certain cases the characters of the two so nearly approximate that even an expert may find it difficult to distinguish between them, the ram under such circumstances being commonly called "wether-headed."

In the Southdown there is not (as in the Herdwick) any marked difference produced by castration in the form of the forehead, the angle between the pre- and post-orbital portions of the frontal bone being equally obtuse in the vasotomised sheep and the wether, and this, for the reason that both are equally destitute of horns.

That the occlusion of the vas had been complete in all the cases observed, was proved by a careful dissection of the testicles after the animals were killed.

Seeing that the full development of male characters proceeds in spite of double vasotomy, it becomes interesting to inquire into the condition of the testicles and into the sexual physiology of the animals themselves.

To take the latter first. A Southdown, the subject of double vasotomy when a lamb, and kept apart until full grown from any female, was turned loose with a couple of maiden ewes; he at once copulated, erection and intromission being complete. The two ewes were not again admitted to the flock, but were kept apart, with the result that neither afterwards proved to be in lamb. This animal was killed 18 months after the vasotomy. The testicles had, from the first, grown symmetrically, and had reached the normal size; dissection revealed a complete interruption of each vas close above the gland.

In certain cases one of the testicles underwent a marked diminution, *i.e.*, it not merely failed to grow but rapidly wasted. In the other cases both organs attained the full dimensions. These differences are to be ascribed to differences in the condition of the blood-supply; when the vas is cleanly isolated and divided after ligation without the inclusion of vessels, or without the subsequent occurrence of thrombosis, no atrophy of the gland ensues. When atrophy of one testicle arises, the other suffices singly to bring about the full development of the male characters.

A careful dissection, carried out in all the cases of vasotomy examined and cited, showed that the vas had been completely occluded; not only was its continuity interrupted, but the noose of the ligature was demonstrated on the end of each segment of the divided duct. The epididymis after occlusion of the vas may become notably larger than normal; this is especially obvious in the lower end or globus minor, and is to be ascribed to its over-distention with the secretion transmitted from the body of the gland.

Microscopic sections of the testis of the normal adult Herdwick sheep and of that from the vasotomised animal of the same age and killed at the same date, show similar histological pictures. The tubuli are filled with epithelial cells, and in nearly every one spermatogenesis is in progress.*

Experiments upon Fowls.

Even still more striking are the results of double vasotomy in the cockerel of the common fowl. In the fully grown cock the exposure of the vasa deferentia and their ligation is not particularly difficult, but in the young bird it is otherwise; and the results cited are limited to those cases where careful dissection afterwards proved that this difficult procedure had been successfully carried out, the continuity of the duct being found interrupted, and the noose of the ligature discovered at the site of operation. The method of proceeding was as follows:—The vas is exposed is its course over the kidney by a curved incision carried through the lateral wall of the abdomen; the duct having been ligatured as near to the testicle as possible, is then cut across a short distance below the ligated spot, no ligature being placed on the lower segment. The vas of the other side is afterwards similarly dealt with through a second incision carried through the corresponding side of the abdomen. Owing to the difficulties of this operation, vasotomy was in some cases performed on one side only, the testicle of the other being removed. The anæsthetic used was chloroform, and the material of the ligatures, silk.

The results of double vasotomy, or of one-sided vasotomy combined with one-sided castration, are in all cases alike. When carried out upon the young, immature, bird, or cockerel, the development of the male characters proceeds without any notable interruption, and reaches its full degree.

The birds used in the experiments were so young that it needed an

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* It may be incidentally remarked that whilst in the castrated lamb the prostate fails to grow, in those submitted to vasectomy the gland comes to equal in size that of the intact ram. The same is true of the vesiculæ seminales. If one testicle is removed and the vas of the other ligatured and cut across, the prostate and vesiculæ acquire the full size, and this without asymmetry.

expert to determine their sex: examination, moreover, of the testicles removed from cockerels subjected to the combined castration-vasotomy just referred to, as well as of those removed from birds of the same brood, showed that no spermatogenesis had arisen at the age selected for operation. We may adduce examples in order to give the full grounds for the general statement set forth with regard to these experiments.

Double Vasotomy.—Impure "Plymouth Rock," 7—8 weeks old. Nine months after the operation the head was male in type; neck-hackles well developed; tail beginning to assume male characters; spurs indicated. Twelve months after the date of operation the spurs were stout, though short; head thoroughly male; neck- and saddle-hackles moderately well developed; tail short, male in kind, with sickle feathers.

The bird remained in the same condition, and was killed 12 months after the date of the operation. At the autopsy, the testicles were found to be of full size (about that of a pigeon's egg), and in their general aspect quite normal. In connection with the right there was a spermatocele about as large as a haricot bean; this, on being punctured, gave exit to a whitish fluid which microscopically showed numerous spermatozoa, some of them motile. The superior segment of the divided vas, or that in connection with the testicle, was dilated; the upper end of the lower segment was traceable into scar tissue in which it terminated. On the left side there was no spermatocele in connection with the gland, but the tubuli of the epididymis were abnormally evident. On each side the noose of the silk ligature was found *in situ* on the upper segment of the vas, above the level of the lower border of the testicle.

As a second instance we may recount the following:—

Double Vasotomy.—"Plymouth Rock," about 8 weeks old. In the summer of the year following the operation the head and neck-hackles were typically male, saddle-hackles fairly so; tail short, carried almost vertically, contained a number of short curved feathers; spurs short and stout. In the winter of the same year the neck- and saddle-hackles were typically male; tail short, bushy, feathers curved; spurs long and sharp. The bird was killed in the spring of the following year. Dissection showed the left testicle to be of full size, 4 cm. in longer diameter; in connection with the upper end of the epididymis is a retention cyst filled with white secretion, and about 1 cm. in diameter; the epididymis is, as a whole, enlarged from distension. The upper end of the lower segment of the vas terminates a short distance above the lower border of the testicle. The ligature lies *in situ* on the end of the epididymal segment of the vas, which is separated by a distinct interval from the other.

The right testicle is slightly smaller than the left, the epididymis distended, and the continuity of the vas interrupted; the ligature lies *in situ* on the lower end of the upper segment of the duct.

As an example showing the results of unilateral vasotomy combined with unilateral castration, we may select the following:—

"Buff Orpington," about 8 weeks old. Nine months after the operation the bird was thoroughly male, the comb and wattles being well developed, as well as the neck-hackles and sickle-feathers of the tail.

Twelve months after the date of operation the spurs were sharp. On being put with a hen the bird immediately copulated, although it had had no previous opportunity of approaching one. Eighteen months after the operation it was killed. On dissection the right testicle was found to be of full size, about 3·5 cm. in the longer diameter; the epididymis was slightly distended. A scraping from the divided body of the gland revealed the presence of spermatozoa. The lower segment of the vas was found to taper off and end quite distinctly about a quarter of an inch below the level of the testicle. The noose of the ligature was covered with a thin layer of connective tissue, and lay on the posterior surface of the organ. The position of the ligature may be explained by the general growth of the gland; this growth would naturally lead to an extension in all directions, and that in the downward direction would, relatively to the testicle, raise the site of the ligature. On the left side no trace of testicle was found. Microscopic examination of the body of the testis from the case of double vasotomy first cited shows the tubuli to be full of cells, and spermatogenesis in high activity, all the typical histological pictures being present. The same holds true of the right testicle from the case of combined vasotomy and castration last detailed.

These results offer a striking contrast to those following a double castration when carried out upon the immature bird. Double castration was performed through a lateral incision on each side, the testicle being exposed to view, and afterwards carefully disconnected from its attachments with fine forceps, and withdrawn. In ideal experiments the gland was withdrawn entire; in others rupture occurred during the process of detachment, the organ being then removed piecemeal.

Results of incomplete caponisation.—In certain of our experiments it happened that the testicle gave way during its detachment, and that minute fragments were unintentionally left behind. Sometimes such remnants, as told by subsequent dissection, were left in their normal position; at others they were dislocated and transplanted upon the adjacent viscera, or abdominal wall. Under such circumstances the cockerel assumed in different degrees the character of the male.

The actual number of gland remnants left at such imperfect operations, and the position of the grafts resulting from their displacement, varied considerably. Thus, in one case the dissection of the fully grown bird, which had been castrated when from 6—8 weeks old, showed on the left side a spherodial mass of testicular substance, 2 cm. in diameter, lying in front of the upper part of the kidney, and into the lower end of which the vas deferens is directly traceable. Hanging in the mid line from a loose "mesorchium" is a spheroidal graft 1·5 cm. in diameter. On the right side there is a bi-lobed mass 2·3 cm. in the chief vertical diameter, with the lower end of which the right vas is directly connected; closely adherent to the front of the upper lobe of this, though slightly movable over it, is a spheroidal mass 0·6 cm. in diameter. A further oval nodule 0·7 cm. in chief diameter is closely adherent to the surface of a coil of the small intestine in the neighbourhood of the liver; a scraping from this graft when cut through in the recent state showed large numbers of spermatozoa. Lastly there is a graft of about the same dimensions intimately adherent to the under surface of the liver itself. The external characters acquired by this bird were fully male throughout.

It may be remarked, in passing, that such grafts do not bear classifying with glandular tumours or adenomata, since they do not grow independently of the general requirements of the body. For the whole sum of a series of such grafts and hyperplastic remnants does not exceed the volume of the two fully developed testicles. In this the remnants behave like those of thyroid tissue left experimentally after partial excision of the thyroid gland; or as do the dormant accessory thyroids after the complete removal of a goître, when the accessory gland after attaining a certain size ceases to increase further; or the process, again, resembles the reproduction and hyperplasia of hepatic tissue which follows partial excision of the liver, of a fourth or even half its bulk.*

In the most perfect cases of reproduction, each gland attains its full normal size. A bird was castrated when quite young, 6—8 weeks old. Six months later, the comb and wattles presented a medium degree of development; the spurs were very small. Nine months after the date of operation, the spurs were still small, and the general male characters ill developed. Twelve months after the operation, the spurs were short but stout. Seventeen months after the operation, the comb and wattles were thoroughly male, the neck- and saddle-hackles fully developed, and the spurs long, stout and sharp.

The bird was killed 21 months after the date of the operation. Each testis was found to be of normal form and full size; the epididymis well pronounced, and without retention cysts. Each vas was in every respect normal and filled with white secretion, which microscopically showed countless actively moving spermatozoa. The history, as above given, shows a marked delay in the development of the male characters,

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* Ponfick, 'Centralblatt f. Med. Wiss.,' 1894; Von Meister, 'Centralblatt f. Allg. Path. und Anat. Path.,' 1891.

and indicates that these developed *pari passu* with the reproduction of the testicles, until they ultimately became fully pronounced.

That a comparatively small volume of testicular tissue will suffice to bring about the development of male characters appears from the following result, in which the bird grew to be fully male with the slight exception that the neck-hackles were somewhat less closely set than is normally the case.

"Buff Orpington," of about 8 weeks, at which time double castration was performed. Eight months after the operation, the comb was well developed and bright in colour; the plumage in general, somewhat pale and sparse; neck-hackles moderately developed; spurs small. Eleven months after the operation, the comb and wattles were well developed; neck-hackles moderate; saddle-hackles fairly male; tail feathers beginning to take the male curve; spurs grown to the normal male extent.

The bird was killed 17 months after the operation, its condition being as last noted. Dissection shows on the left side no trace of testicle in its normal position, but an inch and a half lower down, and three quarters of an inch anterior to this spot, there is an oval graft 2·5 cm. in chief diameter, loosely connected with the lateral wall of the abdomen. Above it, separated by a distance of 1·5 cm. and intimately incorporated with the peritoneum, is a second graft 0·5 cm. in chief diameter; and behind or dorsally to this is a further minute nodule 0·2 cm. in diameter, and likewise inseparably adherent to the peritoneum.

The vas is extremely fine and traceable to the vacant, original, site of the testicle. On the right side in the situation of the testis there are two small flattened nodules, the larger, lower, of which, is 0·8 cm. in chief, vertical diameter. Into the lower end of the inferior the vas, diminished in size and empty of secretion, is directly traceable. A third nodule which lay about 1 cm. anteriorly to these and slightly lower in the abdominal cavity was removed for microscopic purposes: scrapings from its divided surface disclosed the presence of spermatozoa.

Histologically the largest graft (that on the left side of the abdomen) shows closely applied tubuli of full size, every one of which presents the histological pictures typical of active spermatogenesis. The lumen of the tubuli contains free spermatozoa. All the cell nuclei are throughout perfectly stained with nuclear dyes, proving that the tissue is living and not in an obsolete or necrotic condition. The amount of inter-tubular stroma is very small, and supports well formed arterioles and other vessels.

The much greater size of the dislocated graft on the left side of the abdomen, and its high state of activity, suggest that it is the chief element concerned in the production of the male characters. This graft is strictly ductless, and is, moreover, entirely disconnected from its proper nervous relations.

But much smaller grafts than any of these may be met with in imperfect castration, and in such circumstances the male characters are correspondingly ill-pronounced. One must in fact regard the external character of maleness as a quantity which varies proportionally with the amount of gland-tissue present. As an example of a minimal development of such characters associated with a correspondingly small amount of gland-tissue, we may adduce the following observation:—

A cockerel (impure breed of Plymouth Rock) was castrated when about 6 weeks old. The bird was killed 10 months after the date of the operation, when it exhibited the following characters. The head presented no male development of comb or wattles. As indications of maleness, however, are the full development of the neck-hackles, a certain development of saddle-hackles, the presence of a few straggling badly curved feathers amongth those of the tail, and the growth of short blunt spurs on the legs. It may be noted that the occurrence of spurs in the hens of this breed is not known, except in the case of old birds. The bird took no notice of the hens with which it was habitually kept.

On dissection, no trace of either testicle was discovered at the normal site, and no graft, with the exception of a minute nodule the size of a hempseed, which was adherent to the surface of one of the coils of intestine. Microscopic examination of this minute nodule proved it to consist throughout of testicular tubuli distended with epithelial cells and large numbers of spermatozoa, spermatogenesis being in active progress.

Conclusions.

From the fact that in the young of the Herdwick sheep and fowl, occlusion of the vasa deferentia does not inhibit the full acquirement of secondary male characters, it is clear, in the first place, that the discharge of the sperm is not in any way the factor responsible for the production of the characters referred to.

This conclusion admits of being extended to mean that the production of secondary characters is not due to metabolic changes set up by a nervous reflex arising out of the mere physical function of the sexual mechanism. This is made still more forcible by the results of incomplete caponisation in those cases where the grafts were found in situations far removed from the normal, and altogether disconnected from the nerve supply proper to the testicle in its natural position and connections.

Such grafts, devoid as they are of any channels communicating externally, and consisting as they do, of tubuli only, are virtually ductless glands, and the metabolic results arising from their function must, as in analogous cases elsewhere, be attributed to the elaboration of an internal secretion and its absorption into the general circulation.

What particular cell elements are concerned in the production of such a secretion cannot as yet be stated. Various possibilities arise which demand the test of further experiment.

The function of spermatogenesis, although not itself the whole or sufficient cause, may be the initial factor of a dual or even a more complex process.

It is quite within the bounds of possibility that certain of the epithelial cells within the tubuli may produce a prò-secretin such as is produced within the intestinal epithelium; that the chemical changes accompanying spermatogenesis in other of the cells of the tubule may lead to the conversion of this pro-secretin into a secretin, much as the acid chyme does in the case of the pro-secretin present in the intestinal cells; and that the secretin so formed may, without being shed into the lumen of the tubule, be transferred to the lymph spaces, and thus eventually reach the general circulation, and incite those metabolic changes in distant parts of the body which disclose themselves as secondary sexual characters. The intimate connection that arises in the process of spermatogenesis between the spermatoblasts and the "sustentacular" cells is a phenomenon not yet explained; this phenomenon possibly coincides with the interaction suggested.

In regard to the interstitial cells of the stroma, they have characters so unmistakeably glandular that some secreting function, probably a sexual one, must be assigned to them, and they may, of course, take a part in the elaboration of such a secretion as that suggested.

But the great variation in the proportion of such cells present in different forms of mammals makes it difficult to formulate any hypothesis to test by way of experiment, and we are not as yet in a position to make any statement in regard to them.

We have to acknowledge our indebtedness to Mr. George Jonas, of Duxford, for much technical information; to Mr. Marcus Van Raalte for generous help in defraying portion of the expense incurred by the work; and to Mr. C. S. Wallace, Mr. H. J. Marriage, and Dr. H. C. Jonas, for assistance in various ways and on various occasions.

Preparations illustrating the various observations referred to are now in the museum of the Royal College of Surgeons, London.

"The Morphology of the Retrocalcarine Region of the Cortex Cerebri."

By G. Elliot Smith, M.A., M.D., Fellow of St. John's College, Cambridge, Professor of Anatomy, Egyptian Government School of Medicine, Cairo.

Communicated by Professor A. Macalister, F.R.S. Received December 1, 1903,—Read January 28, 1904.

Although many writers, amongst whom Henschen, Vialet and Ramon y Cajal may be specially mentioned, have devoted a considerable amount of attention to the study of the white streak in the occipital cortex cerebri, which Gennari first described in 1776 as "lineola albidior admodum eleganter," no one, so far as I am aware, has ever used this feature as a guide to the identification of homologous areas and sulci in different brains. On the contrary, it has been employed as evidence that the furrows on the surface of the hemisphere have no value for the orientation of physiological areas, seeing that it occupies the edges of two adjoining gyri and the floor of the sulcus between them.* In this preliminary note I hope to demonstrate that Gennari's stria is a sure criterion for the identification of three or perhaps four sulci. I began the study of its distribution in the brain of Man and that of the Apes to test the accuracy of the homology which I had suggested between the sulcus occipitalis lunatus (mihi) in the former and the so-called "Affenspalte" of the latter.† In the course of these investigations I found that the stria-bearing cortex (or "area striata occipitalis," as it may be called) presented such definite relations to the calcarine sulcus in the Human brain, that it could be used as a ready and sure means of determining the homologue of this furrow in the Apes and the other Mammalia.

The accompanying diagram of the mesial surface of the right occipital region of the brain of an adult male Egyptian Fellah will make this relationship clear. The drawing is schematic in as much as all the submerged gyri are represented as though they were exposed on the surface; the distribution of the stria Gennari is shown by the punctate shading.

The furrow commonly known as the "fissura calcarina" consisted, in this case, of four separate elements, of which the most anterior one alone was strictly entitled to Huxley's term "sulcus calcarinus." The other three furrows (r^1, r^2 and r^3) represent the sulcus for which in a

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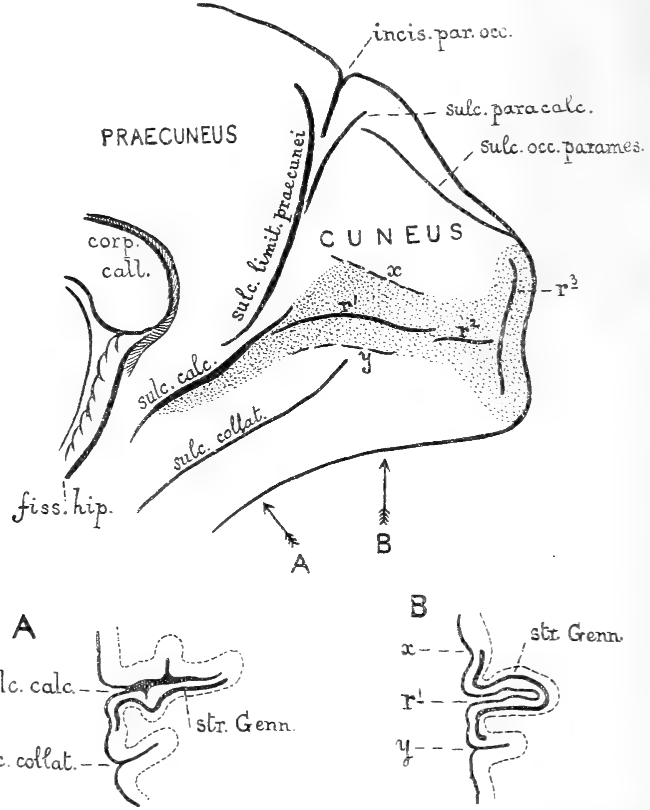
* Oscar Vogt, "Zur anatomischen Gliederung des Cortex Cerebri," 'Journal f. Psychologie und Neurologie,' vol. 2, part 4, 1903, p. 168; see especially Plate 11, fig. 1.

† "The so-called 'Affenspalte' in the Human (Egyptian) Brain," 'Anatomischer Anzeiger,' 1903, pp. 74–83.

previous work I have suggested the name "retrocalcarinus."* In that memoir I have emphasised the fundamental distinction between the calcarine and the retrocalcarine sulci and have discarded the customary

Fig. 1.—Diagram of the Mesial Aspect of the Occipital Region of the Right Cerebral Hemisphere of a Male Egyptian Fellah. The area striata is shaded.



r^1 = Sulcus retrocalcarinus anterior; r^2 = Sulcus retrocalcarinus intercalatus; r^3 = Sulcus retrocalcarinus verticalis; x = Sulcus limitans dorsalis areæ striatæ; y = Sulcus limitans ventralis areæ striatæ. The fossa parieto-occipitalis consists of the depression in which the three sulci—incisura parieto-occipitalis, sulcus paracalcarinus, and sulcus limitans præcunei—are submerged.

application of the term calcarine to the latter furrow, which shares so few features in common with the former, and is of such subsidiary importance. The distribution of the area striata indicates the same distinction in a striking manner. For, whereas the sulcus calcarinus

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* "On the Morphology of the Brain in the Mammalia," 'Linn. Soc. Trans.,' 2nd Series, Zoology, vol. 8, 1902, p. 386.

represents the anterior boundary line of the stria-bearing cortex, the sulcus retrocalcarinus lies wholly *within* that area (fig. 1).

If a section be made through any part of the true calcarine sulcus (except its extremities), the ventro-caudal lip of the furrow will be found to contain the stripe of Gennari, which stops sharply opposite the bottom of the sulcus and does not invade its dorso-cephalic lip (fig. 1, A). I have demonstrated this relationship in a large series of Egyptian and Soudanese brains. It is clear that the true calcarine sulcus accurately marks the line of separation of the area striata and the gyrus fornicatus. In a previous work* I was led to the conclusion that the calcarine sulcus on the mesial surface and the suprasylvian sulcus on the lateral surface of the cerebral hemisphere are probably due to some causes other than the mere general expansion of the neopallium, because they occur with such remarkable constancy in the most diversely-specialised Mammalian Orders, in which the mechanical conditions moulding the growing cortex must be far from uniform. In the case of the suprasylvian sulcus I stated reasons for believing (p. 410) that these other causes were to be found in the unequal rates of growth of the receptive area for auditory impressions and a more dorsal region performing some other function.

The true calcarine sulcus is also probably the result of the disproportionate expansion of two neighbouring areas of different physiological significance.

If we admit the validity of the teaching of Henschen, Vialet and others,† who regard the visual centre as being exactly limited to the region distinguished by the stripe of Gennari, we can conclude that the true calcarine sulcus is caused by the unequal rates of growth of the visual area and the cortex in front of it, which performs some other function.

If the brain of any large Carnivore or Ungulate be examined (in sections either of the fresh brain or after staining according to Weigert's method), the area striata will be found to be limited anteriorly on the mesial surface of the hemisphere by the retrosplenial part of Krueg's "splenial sulcus." The stria extends into the caudal lip of the sulcus and ceases abruptly opposite the bottom of the furrow. This affords a striking confirmation of the view, which I put forward last year‡ on general morphological grounds, that the retrosplenial part of Krueg's splenial sulcus in the Ungulata, Carnivora, and other Mammalia is the strict homologue of the carcarine sulcus (in the restricted sense just explained) of the Human brain.§

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* *Op. cit., supra,* 'Linn. Soc. Trans.,' 1903.

† See S. E. Henschen, "Revue Critique de la Doctrine sur le Centre Cortical de la Vision," XIIIe Congrès International de Médicine, p. 130, Paris, 1900, translated by Dr. Dor.

‡ *Op. cit., supra.*

§ *Op. cit.,* 'Linn. Soc. Trans.,' 1903, p. 376 *et seq.*

If a section be made through any part of the retrocalcarine sulcus (or sulci) the stria Gennari will be found to occupy *both* lips of the furrow (fig. 1 B). Moreover, it extends upward for a short distance into the cuneus and downward for a similar extent into the gyrus lingualis. It sometimes happens (as in the specimen which I have represented in fig. 1) that superior (x) and inferior (y) limiting furrows of this area striata make their appearance. When the posterior end of the retrocalcarine sulcus is bifid, or when it is represented by a separate vertical furrow (fig. 1, r^3), the area striata becomes extended so as to completely surround the depression. In some cases the retrocalcarine sulcus may be placed close to the superior margin of the hemisphere; its anterior part may be less than 1 cm. distant from the fossa parieto-occipitalis, in fact, this sulcus or its separate constituents may occupy in different human hemispheres any position within the triangle bounded below by the tentorial margin, above by the dorsal edge, and in front by the fossa parieto-occipitalis. But wherever this retrocalcarine sulcus or any of its sub-divisions may happen to be placed, it will be found to be invariably *within the area striata.* The obvious inference is that the sulcus retrocalcarinus is produced by the folding of the visual cortex itself. The variability of its form and constitution points to the conclusion that it is the result of the mechanical conditions to which a limited cortical area must be subjected when it begins to expand.

In his great monograph on the "Surface Anatomy of the Cerebral Hemispheres,"* Cunningham has expressed the opinion that there is no "posterior calcarine fissure" (sulcus retrocalcarinus mihi) in the Apes, the so-called "calcarine fissure" being the representative of the "stem" only (*i.e.*, the sulcus calcarinus mihi) of the Human brain (p. 42). I have already strongly opposed this interpretation on general morphological grounds, because the retrocalcarine sulcus of the Human brain is often apparently formed by the backward prolongation of the true calcarine sulcus, as Gustaf Retzins has demonstrated.† The distribution of the area striata in the Apes enables us to settle this matter decisively. I have examined sections of more than 50 hemispheres of the genera *Cebus*, *Cercopithecus*, *Macacus*, *Papio*, *Cynopithecus*, *Semnopithecus*, *Hylobates* and *Simia* to determine the distribution of the stria Gennari, and have found that it occupies *both lips* of the greater portion of the sulcus commonly called calcarine. This shows that the greater part of this furrow represents the sulcus retrocalcarinus of the Human brain.

The examination of fœtal brains of *Semnopithecus* seems to indicate that the retrocalcarine sulcus is very precocious in those brains in which the area striata is of great extent. It may develop con-

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* 'Royal Irish Academy Memoirs,' 1892.

† 'Das Menschenhrin,' 1896.

temporaneously with or perhaps even before the true calcarine sulcus, or, as seems to be the commonest method, in most Apes (certainly in *Hylobates*) one great fossa develops as the representative of the conjoint calcarine and retrocalcarine sulci. It is certain that the mere chronological order in which sulci develop in different brains is of little value in the identification of their homologies, so that we cannot place such implicit reliance on this method as Cunningham* seems to do when he regards it as the basis on which the accurate comparison of the sulci in the Human and Simian brain must rest.

In many Apes the area striata, which forms the walls of the retrocalcarine sulcus, may not reach the surface of the cuneus or the lingual gyrus.

In the brains of certain large Ungulates and Carnivores there is a furrow behind the calcarine (splenial) sulcus, which occupies a position analogous to that of the retrocalcarine in the Primates. In the case of the Camel's brain, I suggested the definite homology of this furrow with the primate retrocalcarine.† In a series of hemispheres of *Camelus dromedarius*, I have found that the distribution of the area striata closely follows this sulcus. When, as often happens, this retrocalcarine sulcus bends upward to the dorso-caudal angle of the mesial surface, the stria-bearing cortex has a similar peculiar distribution. This places beyond all doubt the question of the identity of the retrocalcarine sulci in the camel and the primates.

It has been stated by Henschen‡ that the stripe of Gennari does not extend on to the lateral aspect of the Human brain. This is true in some, but by no means all, cases. The exact extent of the caudal prolongation of the stria is subject to a wide range of variation. As the area striata is traced backward alongside the sulcus retrocalcarinus, it will be found to expand near the caudal pole of the hemisphere (fig. 1), but in some cases it does not reach the true lateral aspect. In more than half of the specimens which I have examined, however, it does extend on to the lateral surface (fig. 2) and reaches almost (in some cases quite) as far forward as the sulcus occipitalis lunatus (or "Affenspalte"). I have chosen to represent as fig. 2 a case in which the sulcus lunatus is placed far back, because this presumably presents the nearest resemblance to the common European type of brain. But it often happens in the Egyptian brain that a large occipital operculum is present, as in the Apes, and in these cases the stria Gennari is prolonged far forward on the lateral aspect into the operculum.

It has been known for many years that the stria extends into the occipital operculum in the Apes, but I have been unable to find any

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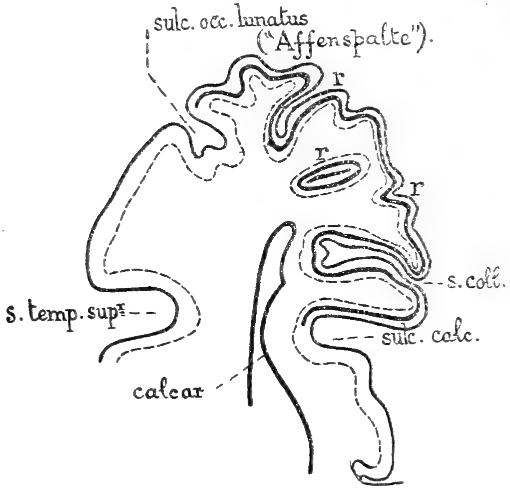
* *Op. cit.*, p. vi.

† *Op. cit.*, 'Linn. Soc. Trans.,' p. 389, fig. 55.

‡ *Op. cit.*, *supra*.

reference to a more detailed account of its distribution. In most of the brains of Apes that I have examined, the area striata extended forward on the surface of the operculum, but often failed to reach as far as its anterior free margin; in some cases it ceased abruptly at a point 4 or 5 mm. behind the edge of the operculum. In the Cercopithecidæ (in which the operculum is relatively biggest) the stria Gennari extended upward as far as the dorso-mesial edge of the hemisphere, and laterally (or ventrally) as far as the sulcus occipitalis inferior. The sulcus occipitalis superior lies in the midst of the area striata. Thus, if the whole stria-bearing cortical region were spread out in one plane, it would present a racquet-like shape—the handle of the racquet corresponding to the retrocalcarine cortex and the expanded part to the occipital operculum. In the Human brain the "handle of the racquet" becomes greatly enlarged at the expense of the rest of the area striata. But its form is subject to a very wide range of variation, which I shall describe in detail in a forthcoming memoir.

FIG. 2.—Horizontal Section through the Posterior Part of the right Cerebral Hemisphere of an Egyptian.



r = Sulcus retrocalcarinus.

The thick line represents the stria Gennari. It is quite exceptional for the sulcus collateralis to be prolonged into the area striata as in this diagram.

[*Addendum*, January 11.—The so-called "calcarine fissure" in the Apes is a complete involution of the *whole* of the mesial part of the area striata. The so-called "calcarine fissure" (of most writers) in the Human brain usually consists of an anterior part, which is an anterior limiting sulcus of the area striata, and a posterior part, which is a mere indentation (or indentations) of *part* of the mesial area striata, therefore it is not exact to speak of these similarly named furrows as being strictly homologous.]

"On the Acoustic Shadow of a Sphere." By LORD RAYLEIGH, O.M., F.R.S. With an Appendix, giving the Values of Legendre's Functions from P_0 to P_{20} at Intervals of Five Degrees. By Professor A. LODGE. Received December 28, 1903,—Read January 21, 1904.

(Abstract.)

The problem here considered is that of the intensity of sound at the various points of a rigid and fixed sphere on which plane waves impinge, or reciprocally the intensity at a distance in various directions due to a source of sound situate upon the surface of the sphere. The analytical solution is readily given, but in the interpretation everything depends upon the ratio of the wave-length ($2\pi/k$) to the circumference ($2\pi c$). If kc be small, the sphere has little effect. In my book on the "Theory of Sound," § 328, I have considered (but only for certain special directions) the case of $kc = 2$. The extension to various directions is now given; and the calculation is pushed to the case of $kc = 10$, about as far as is practicable. For this purpose the values of Legendre's Functions up to P_{20} are required.

$kc = 10.$

θ.	$4(F^2+G^2)$.	θ.	$4(F^2+G)$.
0	3·8300	105	1·06117
15	3·8176	120	0·56815
30	3·7148	135	0·27890
45	3·4978	150	0·13338
60	3·1098	165	0·09492
75	2·4984	170	0·12591
90	1·7510	175	0·69395
		180	1·09263

The table gives the intensity in directions making angles θ with the radius which passes through the source. On the same scale the intensity would be unity were the sphere removed. The most interesting feature is the existence of a fairly good shadow between 135° and 170°, and the subsequent rise of intensity in the neighbourhood

of 180°. This corresponds in some degree with the bright spot in the centre of the optical shadow of a circular disc.

The problem which arises when both the source and the point of observation are situated upon the sphere is more difficult. It is treated less completely, but some results of interest are obtained for the case of $kc = 10$.

The Appendix by Professor A. Lodge contains tables of Legendre's Functions up to P_{20} for angles ranging at intervals of 5°, accompanied by a statement of the method of calculation. It is believed that these values may prove useful in other physical investigations.

"The Significance of the Zoological Distribution, the Nature of the Mitoses, and the Transmissibility of Cancer."

By E. F. Bashford, M.D., and J. A. Murray, M.B., B.Sc. Communicated by Professor J. Rose Bradford, F.R.S. Received January 12,—Read January 21, 1904.

[Plate 2.]

The object of this communication is to relate some results of the work conducted under the immediate direction of the Executive Committee of the Cancer Research Fund during the past year. We believe that these results will convince others of the important practical assistance which biologists generally can give in the further elucidation of certain problems of cancer which must be settled before preventive and curative measures can be devised. It will also be made evident that the elucidation of cancer is something more than a problem of human pathology.

We shall adduce evidence tending to show that the wide zoological distribution, the character of the mitoses, and the transmissibility of cancer, are nearly related phenomena with a common basis.

The fundamental significance of ascertaining the extent of the zoological distribution of cancer was recognised by the Cancer Research Fund from the first, and determined the prosecution of definite lines of inquiry, not only with the object of eliciting new facts in regard to the zoological distribution itself, but also with the object of discovering cancer in animals well adapted to cytological and experimental observations.

Zoological Distribution.

Within the past year specimens of malignant new growths have accumulated from all the domesticated animals and from many other vertebrates. The appended List shows the abundance of the material which has thus been examined. The List includes also specimens which we have been privileged to examine through the courtesy of investigators abroad, who, subsequently to the inauguration of the Cancer Research Fund, have published descriptions of malignant tumours in the lower vertebrates. It is noteworthy that such growths have been obtained, not only in domestic animals, but also in animals living in a state of nature : wild mouse, codfish, gurnard.

The clinical, pathological, anatomical, and microscopical characters of these new growths are identical with those found in man in all essential features, although the animals themselves are drawn from the different classes of the vertebrate phylum. A detailed histological description of the various tumours examined will not be attempted here. Only the general significance of the observations in relation to the incidence of cancer in man will be emphasised.

The great diversity of the habitat, food, and conditions of life generally of the forms in which malignant new growths occur relegates such external conditions to a subsidiary *rôle* in determining the incidence of the disease, and shows that the essential factors must be sought in the potentialities which reside in the cells constituting the living body.

The list of specimens, while giving no safe basis of deduction as to the relative incidence of cancer in the different classes of vertebrates, or of the comparative susceptibility of the different sites of the body, is extremely suggestive.

The large number of epitheliomata obtained in the horse and dog indicates very clearly that malignant new growths are recognised according to the ease with which animals can be examined, and the length of time they are kept under observation. In the same way, numerous malignant new growths have been discovered in the internal organs of cows during the inspection at abattoirs.

Stated generally, it may be said that malignant new growths are known to occur chiefly in animals which are habitually examined with care, and are unrecorded in forms which are difficult to examine or do not reach old age in considerable numbers.

The figures are not sufficiently extensive to determine accurately the age incidence of the various types of new growths in different animals, but a relatively higher incidence in old age is apparent.

The Phenomena of Cell-division in Malignant New Growths.

The progressive increase in size of malignant tumours is due to the division and increase in size of their constituent cells. The process of cell-division is usually indirect, mitotic division of the nuclei preceding the division of the protoplasm. The protoplasm division is frequently omitted, and multi-nucleated cells are formed, and these may subsequently enter on mitosis, giving rise to pluripolar figures. Amitosis or direct nuclear division also occurs, but its full significance has not yet been determined. It is, however, evident that the occurrence of amitosis does not signify degeneration. The amount of chromatin entering into the equatorial plate of the mitoses of malignant new growths had long been recognised as subject to variation (hyperchromatosis, hypochromatosis, of von Hausemann, 1893), but a new light has been thrown on this phenomenon by a paper communicated to the Royal Society on December 10, 1903, by J. B. Farmer, F.R.S., J. E. S. Moore, F.L.S., and C. E. Walker, entitled "Resemblances exhibited between the Cells of Malignant Growths in Man and those of Normal Reproductive Tissues."

These observers found that while the growing margin of carcinomata and sarcomata presented mitoses similar to those found in other tissues in repair and inflammation, certain cells in the deeper layers, after a slight increase in size, entered on mitosis with ring chromosomes similar to those found in the heterotype division of spore mother-cells of plants and spermatocytes of animals, and like these, with chromosomes in number only half that characteristic of the mitoses of somatic cells. Mitoses similar in character to the somatic divisions, but with reduced number of chromosomes, were also seen (homotype), corresponding to the divisions in the sexual generation of plants and the second ripening divisions of animals. From these observations the authors concluded that malignant new growths were virtually reproductive tissue arising in abnormal situations and possessed of an independence and power of growth like that of the testis in the mammalian body.

This striking sequence of characteristic mitoses had been found in all malignant tumours examined, and was absent in those of benign character. We at once determined to communicate with the authors, who with great courtesy afforded us an early opportunity to examine their preparations. It was then decided to determine how far similar phenomena were characteristic of the malignant new growths occurring in animals. The result has been a complete confirmation of Farmer, Moore, and Walker's observations in tumours from the trout (Mr. Gilruth's and Miss Plehn's cases of adeno-carcinoma), the mouse (two cases of adeno-carcinoma, Jensen's epithelioma), and the dog (mixed cell sarcoma, round cell sarcoma, squamous cell carcinoma). In the columnar cell carcinoma of the trout the phenomena were especially distinct, the small number of chromosomes* (24, 12), the striking contrast between the long slender

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* The achromatic figure has always been carefully studied as a control to the observations made on the chromatin of the mitosis. When the chromosomes have been counted, this has been done on Sections 5—10 μ thick, mounted in series.

chromosomes of the somatic mitoses, and the rings of the heterotype division, being of diagrammatic clearness. Homotype mitoses occurred, but were very few in number. Mitoses in the stroma are relatively scanty in the tumours at our disposal, but such as have been seen are somatic in character. Farmer, Moore, and Walker record a similar result. In the mouse it has been possible to compare the mitoses in the testis, and those occurring in the irritation produced by iodine, with the result that the significance of the mitoses in cancer has been further confirmed.

FIG. 1.—Adeno-carcinoma of Trout. Homotype amphiaster. Reduced number of chromosomes, arranged transversely in spindle, and showing longitudinal splitting.

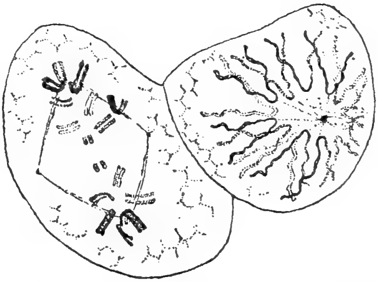

FIG. 2.—Epithelioma of Mouse. Somatic prophase and amphiaster.

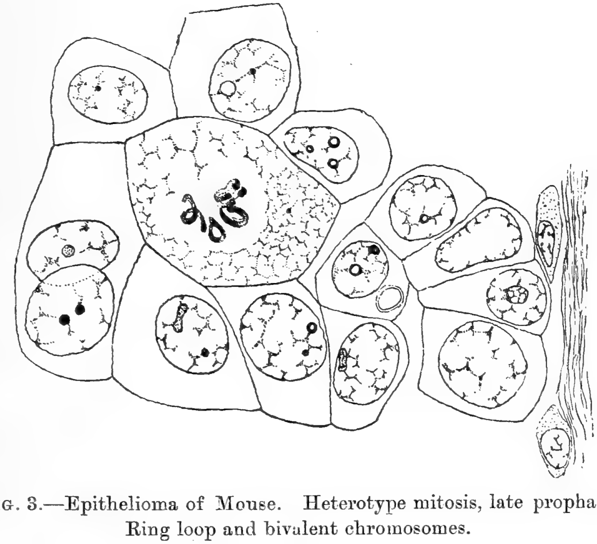

FIG. 3.—Epithelioma of Mouse. Heterotype mitosis, late prophase. Ring loop and bivalent chromosomes.

The following points are of great importance in these observations. A complicated sequence of cell-changes has been found to be characteristic of carcinoma and sarcoma alike. This sequence is the same as that which initiates the origin of the sexual generation in plants from the asexual, and is terminal in the history of the sexual cells in animals. It must be noted also that all the cells of the malignant new growths do not undergo the reducing division, a certain number differentiate in the direction of the tissue among which they have arisen, and in the secondary growths when present, somatic mitoses occur in the growing margin, which it will subsequently be shown is also a feature in the growth of cancer when transferred to a new host.

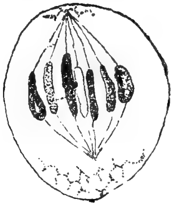

FIG. 4.—Heterotype amphiaster, chromosomes arranged longitudinally on spindle. Reduced number. All the chromosomes are not figured.

FIG. 5.—Heterotype amphiaster. Ring chromosomes, 12 in number. Adenocarcinoma of Mouse. (The nucleus was contained in two consecutive sections).

The Transmissibility of Malignant New Growths from one Animal to Another.

The transmission of cancer from man to animals, or from one animal to another of different species, has never been successfully performed. Successful transplantation experiments have been made, however, from animals suffering from malignant new growths to others of the same species. The most exhaustive observations in this connection have been made by Jensen and Borrel on mice. Professor C. O. Jensen, of Copenhagen, most generously placed at the disposal of the Cancer Research Fund a portion of one of his experimental tumours,* and with this, and another tumour occurring naturally in a tame mouse, similar experiments have been performed. We have thus been able to confirm Jensen's observations by microscopical examination of the tissues at the site of inoculation at short intervals, and have found that the new tumours which develop arise from the actual cells introduced. While many of these degenerate, a few

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* Sent by post under such precautions as to preserve sterility. The transplantations were effected by the Cancer Research Fund five days after the tumour was posted in Copenhagen.

remain of normal appearance, and these gradually increase in number. In the earliest transplantations mitotic division is absent, and it is not till later, when a considerable mass has arisen, that mitoses appear. The earliest mitoses we have been able to observe have been of the somatic type.

Transplantation is, in fact, identical with the process of metastasis as it occurs in the individual providing the tumour. It is remarkable, however, that the tumour of Jensen's experiments does not produce metastases naturally, and its malignancy is only evidenced by its progressive growth, and the undifferentiated character of the cells. The process is in no sense an infection, the tissues of the new host not participating in the formation of the new parenchyma. In this interpretation we are in agreement with Jensen and differ from Borrel, who conceives the results to be due to the agency of a *virus cancéreux.*

The origin of the stroma has not been accurately determined. The power of growth of this tumour is remarkable. In every mouse in which the transplantation succeeds, the new growth may attain a weight equal to that of the animal itself, and over 400 such transmissions have been effected by Jensen in Copenhagen and the Cancer Research Fund in London. A mass of tumour, 16 lbs. in weight, has thus actually arisen from the original one, and that without participation of the cells of the various hosts and without manifest change in structure. This great power of growth is a phenomenon unparalleled in the mammalia, and indicates the potentialities in cases in which widespread dissemination has occurred before death in a human patient.

The experimental transmission of carcinoma shows that we must distinguish between the problem of the genesis of a malignant new growth, and that of the conditions which permit of its continued existence. While the conditions leading to the initiation of malignant tumours are relatively infrequent, we have examined upwards of 1000 tame mice, and have discovered two with cancer; once begun, this proliferative activity can, under favourable conditions, persist for a long time unaltered, and can give rise to masses of tissue of great size, having no relation to the restrictions which limit the growth of adult organisms in a large proportion of healthy animals.

The phenomena of cell division, indicating a similarity to the normal reproductive tissues, may help to explain the nature of this great power of multiplication, but leave the problem of cancer genesis practically untouched. They give, however, important indications of the character of the processes on whose elucidation the solution of the question depends. The wide zoological distribution of malignant new growths—its limits are not yet determined—indicates that the cause of cancer is to be sought in a disturbance of those phenomena of reproduction and cell-life, which are common to the forms in which it occurs.

Our observations on animals show that malignant new growths are always local in origin and of themselves produce no evident constitutional disturbances whatsoever. These facts are in full accord with accumulated clinical experience in man. In connection with diagnosis and statistics we have already emphasised the importance of the absence of specific symptoms. The evidence we have advanced that cancer is an irregular and localised manifestation of a process, otherwise natural to the life-cycle of all organisms, probably explains why it is that malignant new growths and their extensive secondary deposits, *quâ* cancer, are devoid of a specific symptomatology.

We desire to add the accompanying dated note because we find that conclusions which have been drawn by others are attributed to us.

[NOTE.—We find that the guarded terms in which the points of importance are emphasised may lead to a misconception of our interpretation of the facts. The cells which have undergone the reducing division are not responsible for the active invasion of surrounding tissues, nor for the production of metastases; the cells dividing somatically are responsible for both. The number of heterotype mitoses may not stand in any relation to the degree of malignancy and their absence is only presumptive evidence of the benign character of a tumour. We postulate nothing as to the future of the cells which have undergone the reducing division, though we believe the latter to be a terminal phase in the life cycle of cancer cells as it is in the history of sexual cells in animals. The local origin, and the expansive and infiltrating growth of cancer in its relation to surrounding tissues, while respecting its own proper elements, is the criterion of its malignancy. This stamps it as belonging to a new cycle comparable in its entirety to the whole organism which it is invading, rather than to any one of its tissues, reproductive or otherwise.

We intentionally restricted our original statement to recording the facts, and only such general conclusions as could be irrefutably drawn from them.—*January* 25, 1904].

We cannot here make full acknowledgment to those who have assisted our inquiry in this country, but our indebtedness may be expressed to those observers abroad who have recorded isolated instances of cancer in animals, and have so generously furthered our investigations by placing material or specimens at our disposal.

In particular, we desire to thank Professor Borrel, of the Pasteur Institute; Professor C. O. Jensen, of Copenhagen; Mr. J. A. Gilruth, Chief of the Veterinary Department, New Zealand; Professor Landau and Dr. L. Pick, of Berlin; and Dr. Marianne Plehn, Munich.

Without the generous co-operation of these and many others it would not have been possible within so short a time to have covered the extensive ground indicated in this paper.

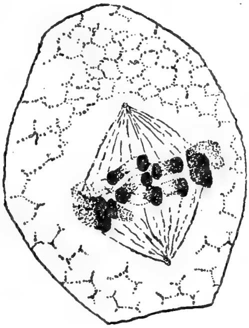


Fig. 6.—Homotype amphiaster. Epithelioma Mouse. Transversely arranged chromosomes, longitudinal splitting, reduced number. All the chromosomes are not figured.

List of Specimens of Malignant New Growths examined by the Cancer Research Fund Committee during the year 1903.

Animal.	Age.	Primary site.	Microscopic character.
Cow	Aged	Orbit	Carcinoma, large polygonal cells.
,,	,,	Rumen	,, spheroidal cell.
,,	,,	,,	,, squamous cell.
,,	,,	,,	,, ,, ,,
,,	,,	Liver	,, cubical cell.
,,	,,	,,	,, ,, ,,
,,	,,	? Pleura (secondary)	,, squamous cell.
,,	,,	? Gastric gland (secondary)	,, ,, ,,
Heifer	1	Perineum	Melanotic, sarcoma.
Cow	Aged	Ovary	Sarcoma.
,,	..	Suprarenal	,,
,,	Aged	Ovary	Carcinoma.
,,	..	Jaw	Osteo-sarcoma.
,,	..	Adrenal	Sarcoma.
,,	Aged	Liver	Carcinoma, cubical cell.
,,	..	Bowel	Sarcoma, spindle cell.
,,	..	Neck	,, ,, ,,
,,	Aged	Face and neck	Carcinoma, squamous cell.
Heifer	2	Side of thigh	Melanotic sarcoma.
Dog	14	Mammæ	Carcinoma (scirrhus).
,,	10–11	Upper lip	Epithelioma.
,,	..	Testis	Sarcoma, mixed cell.
,,	..	Upper lip	Fibro-sarcoma.
,,	10–11	,,	Carcinoma, squamous cell
,,	..	Abdominal gland	Sarcoma, round cell.
,,	..	Sympathetic glands	Fibro-sarcoma.

List of Specimens of Malignant New Growths—*continued.*

Animal.	Age.	Primary site.	Microscopic character.
Dog	10	Liver	Carcinoma, columnar cell.
,,	..	Mouth	,, squamous cell.
,,	..	Anus	,, ,, ,,
,,	12?	Leg (subcutaneous)	Sarcoma, round cell.
,,	..	Sup. max., orbit, gld., l. jaw	,, spindle cell.
,,	10	Anal tumour	Carcinoma (sebaceous adenoma).
,,	11	Spleen, liver, stomach	Sarcoma, spindle cell.
,,	12	Leg	,, small round cell.
,,	10	Palate, cervical gland	Carcinoma, squamous cell.
,,	..	Mammæ	Osteo-sarcoma.
,,	10–12	Axilla	Carcinoma, squamous cell.
Horse, gelding	10	Penis	Epithelioma.
,, ,,	7	,,	Carcinoma, squamous cell.
,, stallion	15	,,	,, ,, ,,
Mare	10	Vulva	,, ,, ,,
Horse	Aged	Lung	,, columnar cell.
,,	10	Penis	,, squamous cell.
Mare	14	Vaginal growth	,, ,, ,,
Horse	..	Parotid	Medullary carcinoma.
Sheep	2	Mandible	Osteo-sarcoma.
,,	..	Liver	Carcinoma, cubical cell.
Pig	..	Sub-max glands	Sarcoma, mixed cell.
White mouse	..	Inguinal mammæ	Adeno-carcinoma (localised keratinisation).
Yellow mouse	..	Axillary tumour	Adeno-carcinoma.
Wild mouse	..	Mammary glands	Carcinoma, spheroidal cell.
Mouse (Jensen)	..	Leg (subcutaneous)	Epithelioma.
,, (Borrell)	..	Axilla ,,	Adeno-carcinoma.
,, ,,	..	Groin ,,	,,
,, ,,	..	,, ,,	,,
,, ,,	..	Jaw ,,	Epithelioma.
Mouse (Pick)	..	Back ,,	Adeno-carcinoma (sweat glands).
Cat	10	Tongue	Carcinoma, squamous cell.
Hen (Pick)	..	Floor of mouth	Squamous cell epithelioma.
Indian parakeet	..	Pectoral muscle	Myxo-sarcoma.
Giant salamander (Pick)	..	Testis	Cystic adenoma, malignant.
Cod	..	Air-bladder	Sarcoma, spindle cell.
Gurnard	..	Abdominal tumour	Adenoma, malignant.
Trout (Gilruth)	..	Floor of mouth	Carcinoma, columnar cell.
,, (Plehn)	..	,, ,,	,, ,, ,,
,, ,,	..	,, ,,	,, ,, ,,

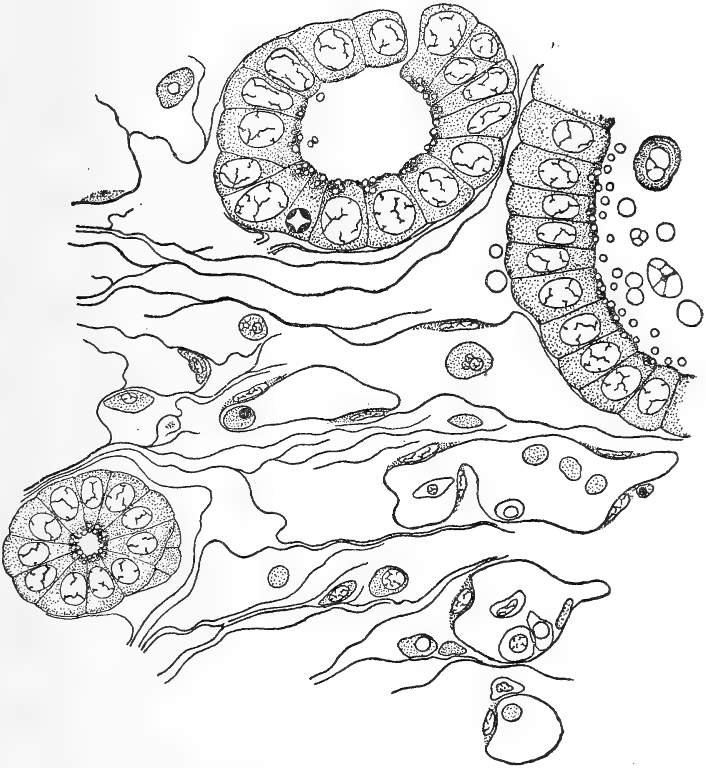


Fig. 7.—Malignant adenoma of Gurnard. Primary tumour. Peritoneal cavity. × 2250 (reduced).

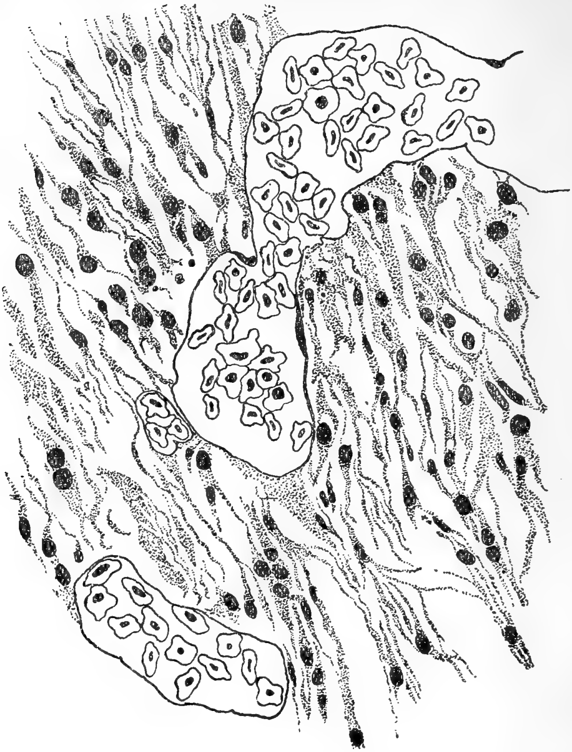


FIG. 8.—Spindle cell sarcoma. Codfish. Secondary nodule, wall of swim bladder. × 1500 (reduced).

DESCRIPTION OF PLATE.

ADENO-CARCINOMA OF TROUT.

(1) The chromosomes in the stroma mitoses are long slender V-shaped loops, which split longitudinally, 24 in number. They are somatic in type; compare fig. 2 from the tumour.

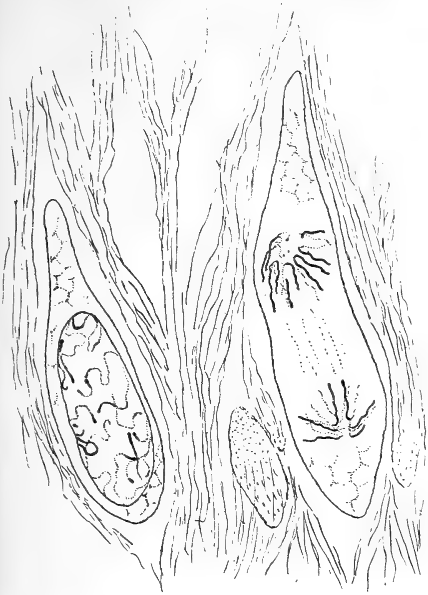
(2) Somatic equatorial plate in margin of tumour, seen from the pole, slender V-shaped chromosomes 24 in number arranged transversely on the spindle, and showing longitudinal splitting. All the chromosomes are not reproduced in the figure.

(3) Microphotograph (untouched) of somatic equatorial plate.

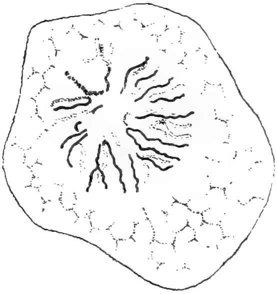
(4) Microphotograph (untouched) of heterotype amphiaster seen from pole.

(5) Heterotype amphiaster (polar view). Small ring chromosomes present in reduced number (12). All the chromosomes are not reproduced in the figure. This is a drawing of the mitosis photographed in fig. 4.

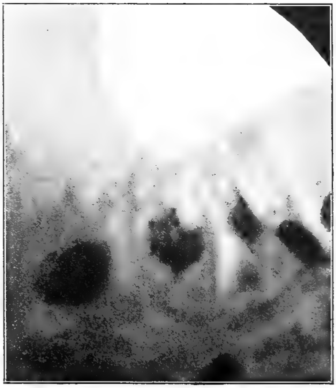
(6) Heterotype amphiaster, lateral view (one centrosome only in this section). Ring chromosomes in reduced number arranged longitudinally on the spindle. All the chromosomes are not figured.



(1)



(2)



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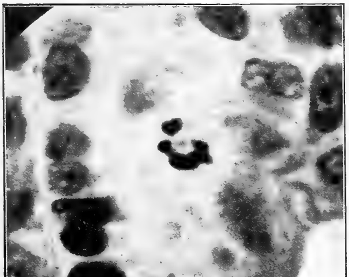
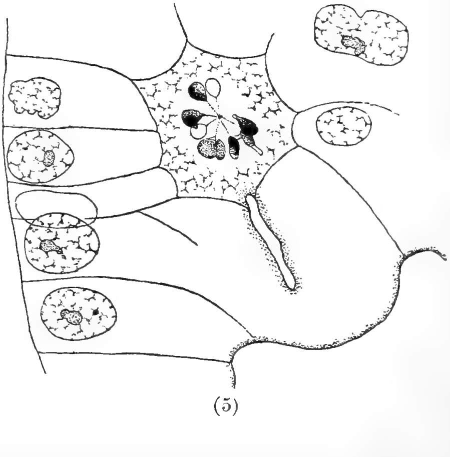
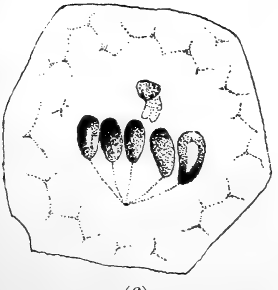


(4)



(5)



(6)

"Conjugation of Resting Nuclei in an Epithelioma of the Mouse."

By E. F. Bashford, M.D., and J. A. Murray, M.B., B.Sc.
Communicated by Professor J. Rose Bradford, F.R.S.

Received January 28,—Read February 4, 1904.

In a previous communication we have drawn attention to the fact that the power of cell proliferation, which has been proved to occur in an epithelioma of the mouse (Jensen), is a phenomenon unparalleled in the mammalia. A mass of tumour, 16 lbs. in weight, has been produced by artificially transplanting portions of the original growth and its descendants. In seeking to throw light on this fact, we have studied carefully the phenomena which follow the transplantations of portions of the tissue to new sites, and have found that the tumours which arise are the genealogical descendants of the cells introduced. We have studied the growth of the tumours which arise at successive stages of 24 hours. In a tumour removed on the eighth day, and less than half a split pea in size, conjugation of resting nuclei has been observed. To take a specific case, the nuclei of two adjacent cells are continuous through the cell wall by a tube-like bridge, in the middle of which a strand of nucleolar substance with fusiform swellings in either cell is visible. The cells of this particular case are adjacent to the stroma, and close to the outer surface of the young tumour.

"On the Part played by Benzene in Poisoning by Coal Gas." By R. Staehelin, M.D., Senior Assistant in the Medical Clinic at Basle. Communicated by Professor E. H. Starling, F.R.S. Received December 11, 1903,—Read January 28, 1904.

(From the Physiological Laboratory, University College.)

In a recent paper Vahlen* has maintained that a difference exists between the poisonous action of coal gas and of carbon monoxide, and Kunkel† has also drawn attention to a similar difference in the case of frogs. In the course of a research which I was undertaking in University College for other purposes, at the suggestion of Professor Starling, I have come across facts which may serve to explain the difference noted by these observers.

My first object was to investigate the effect of deprivation of oxygen on the fatigue curve of muscles. To this end, a muscle was hung up in a closed chamber and the atmospheric air driven out by a stream of some other gas. When coal gas was used for this purpose, it was noticed that the muscle rapidly went into *rigor mortis*, whereas, in nitrogen, it remained excitable for many hours. I set myself, therefore, to find out which constituent of the coal gas was responsible for this poisonous effect.

The experiments were carried out on the frog's sartorius, since the relatively large surface of this muscle renders it particularly accessible to gaseous poisons. The muscle was hung up in a glass tube, closed above and below by rubber corks. To the upper cork a hook was fastened on which was hung the bony insertion of the sartorius. Through the lower cork a glass tube passed. The movements of the muscle were transmitted to a recording lever by means of a steel needle hooked into the lower end of the muscle and passing through the glass tube. The lever was weighted near its axle, and was afterloaded, and its movements were recorded on a slowly rotating drum, on which the time was also marked by means of a signal. The closed tube was provided with two small lateral openings by which any gas desired could be passed through it, the opening through which the needle passed to the lever being made sufficiently air-tight by means of a small plug of vaseline. In every experiment two similar

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* Ferchland und Vahlen, "Ueber Verschiedenheit von Leuchtgas- und Kohlenoxydvergiftung," 'Archiv für exper. Pathologie und Pharmakologie,' 48, p. 106; Vahlen, "Ueber Leuchtgasvergiftung," 'Archiv für experimentelle Pathologie und Pharmakologie,' 49, p. 245.

† Kunkel, "Ueber Verschiedenheit von Leuchtgas- und Kohlenoxydvergiftung," 'Sitzungsberichte der Physikalisch-medizinischen Gesellschaft zu Würzburg,' 1902, Nr. 4, 5, p. 61.

apparatus were employed, so that the influence of two gases could be compared at one and the same time on the two sartorii of the same frog.

When coal gas was led through the tube the muscle began to contract 1—7 minutes after the beginning of the passage of the gas, and in ½—1 hour the muscle was maximally contracted and had an opaque appearance. The rapidity with which the contraction came on was proportional to the rapidity with which the gas was passed through the chamber, and varied with the temperature, warmth quickening and cold slowing the process. Once the muscle was fully contracted no recovery took place. This phenomenon could not be due to the CO in the coal gas, since, on passing pure CO through the muscle chamber, the muscle remained excitable as long as in nitrogen, and the fatigue curve took the same course as in that gas. Among the remaining more important constituents of coal gas which might produce this phenomenon, benzene merited first attention, since all aromatic bodies are more or less potent poisons. I therefore tried the effect of passing air through benzene into the muscle chamber. I found that in less than a minute the muscle began to contract, and the contraction reached its maximum height within a short time, the muscle becoming opaque and dead. The other sartorius of the same frog was placed at the same time in coal gas. It began to contract only after several minutes, and the contraction took half an hour to reach its maximum. Benzene vapour therefore showed itself much more poisonous for a muscle than coal gas, evidently on account of its greater concentration. According to Letheby, London coal gas contains only 3·8 per cent. of condensible hydrocarbons, of which benzene forms only a small proportion. I therefore, in another experiment, passed air into the muscle chamber, not through pure benzene, but through water which had been shaken up with benzene. In this case the contraction began 6 minutes after the beginning of the experiment and took 38 minutes to reach its maximum, while the control muscle in coal gas began to contract in 7 minutes and reached its maximum contraction in 40 minutes.

It seemed, therefore, highly probable that benzene was really the constituent of coal gas which was responsible for its toxic properties on muscle. If that were the case the passage of coal gas through oil, which absorbs benzene, ought to deprive it of its deleterious effects. This was found to be the case. Coal gas, passed through oil, showed no difference in its effects from pure CO or nitrogen.

In the same manner as benzene I investigated the effects of xylol and toluol. If the muscle was supplied with air blown through either of these fluids no poisonous effect was observed even when the fluids were warmed. The absence of effect in these two cases is perhaps to be ascribed to the lower vapour tension of these two substances. The muscle was equally unaffected when supplied with air which had been passed over heated naphthaline.

I also investigated the effect of certain hydrocarbons of the fatty series, namely, methane, prepared by heating sodium acetate and sodium hydrate, acetylene produced by the action of water on calcium carbide, as well as the mixture of substances obtained by blowing air through petroleum ether. In none of these cases was any effect produced on the muscle. If, however, a trace of benzene was added to petroleum ether, *rigor mortis* of the muscle was almost immediately produced. In this case the volatility of the petroleum ether apparently aids the evolution of the benzene. On the other hand, the addition of small quantities of benzene to xylol does not impart poisonous qualities to the air bubbled through the mixture, the benzene being apparently held fast by the less volatile xylol.

It seemed, therefore, most probable that the specific poisonous effect of coal gas on frog's muscle was due to benzene only, and that it was to the presence of this substance in coal gas that the differences observed by Kunkel and Vahlen between the action of coal gas and carbon monoxide were to be referred. Vahlen states that warm-blooded animals and frogs die more rapidly in coal gas than would be expected from its percentage of CO, and also that frogs in coal gas present excitatory phenomena which are absent in pure CO. Although Kunkel denies the presence of any difference between the action of these two gases on warm-blooded animals, he also draws attention to the peculiar effects on frogs of coal gas, which are not produced by other gases free from oxygen, and describes them as "choreiform twitchings and spasms in the neck and legs." To decide this question the following experiments were carried out:—

I. Three frogs were placed in air-tight bell jars, through each of which coal gas was conducted at constant rate. The coal gas had to pass in each case through a wash bottle, which in A was water, which, of course, left the composition of the gas unchanged, in B oil, which would absorb the benzene. In front of C were two wash bottles, the first one containing oil, the second one benzene.

The passage of the gas through the three bell jars was begun at 11.20. At 11.25 all three frogs were restless. Frog C remained then still for a short time, and the breathing became irregular, and twitching occurred in the extremities and back. Movements were chiefly co-ordinated, though there were some twitchings of isolated muscles. After a little time the movements became shorter and less co-ordinated, the legs remained stretched out, and breathing ceased. Frog A betrayed phenomena similar to C, but the spasms were less evident, and came on more slowly. Frog B became quite quiet, the respiration becoming irregular and shallower.

At 11.40 B was sitting up in normal position, though the breathing was somewhat irregular, while A and C were lying on their bellies, with legs stretched out. At 11.45 all three frogs were taken out. C gave no signs of life, and in ten minutes was quite rigid; B still reacted slightly to stimulation, showed shallow respiratory movements, and gradually recovered, so that at 1.30 it was apparently normal. Frog A at first showed no response to stimulus, and no respiratory movements. By 11.55 it had recovered sufficiently to show both these phenomena, and at 1.30 it was so far recovered that it could recover its position when turned on its back, and in a couple of hours later was apparently normal.

II. In a second experiment the arrangements were the same as in the first, except that in C the gas which had passed through the oil was allowed to pass through water saturated with benzene instead of through pure benzene. In this case the phenomena in A and C were practically identical, and were similar to those observed in A in the first experiment. We need not, therefore, give fuller details of this experiment.

We thus see that, when frogs are exposed to coal gas, motor phenomena are produced, which are absent if the coal gas be previously purified by passage through oil, and that these phenomena can be reproduced if the purified gas be made to take up benzene vapour. The poisonous properties of the gas can be increased by increasing the tension of the benzene vapour. We are, therefore, justified in concluding that the differences between the effects of CO and coal gas observed by Kunkel and Vahlen, depend on the presence in the latter of benzene. The slight motor excitation observed in frogs in coal gas, which had been purified by passage through oil, is exactly similar to that described by Kunkel, as the result of deprivation of oxygen, produced by placing the frogs in nitrogen or CO, as is shown by the following experiment:—

III. Two frogs were placed, one in a bell jar through which CO gas was led, the other in a similar jar through which coal gas was led after passing through oil. Both animals in a short time showed slight twitchings of isolated groups of muscles in the extremities and back, and occasional extensor movements of the hind limbs, which gradually diminished. No difference was observable between the two frogs. In three quarters of an hour they were taken out of the jars, and both recovered within a short time.

In order to be certain of the part played by benzene in coal gas poisoning, we must have some idea of the effect of pure benzene on the frogs. I have been unable to find any published experiments over the effects of inhalation of benzene on the frog. Beyer* states that xylol acts as a narcotic poison, like the other odorous substances investigated by him. I have, therefore, made some experiments on the influence of benzene vapour in the presence of oxygen on frogs.

IV. A frog was placed in a bell jar, in which a beaker of benzene was hung up. Eight minutes after the beginning of the experiment spasmodic movements and twitchings began in various parts of the body, accompanied by a considerable secretion of mucus. After a few minutes the spasms ceased, the frog lay still with extended limbs, and respiration, which at first was irregular, became gradually shallower and less frequent. Twenty minutes after the beginning of the experiment all respiratory and other movements had ceased. The frog was taken out of the jar, and recovered in a few hours.

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* Beyer, "Narkotische Wirkungen von Riechstoffen und ihr Einfluss auf die motorischen Nerven des Frosches." 'Archiv für Anatomie und Physiologie, physiologische Abteilung,' 1902, p. 201.

In other experiments in which the frog was left longer exposed to the action of benzene vapour, *rigor mortis* came on first in the hinder and then in the fore extremities before the heart had ceased to beat. One peculiarity was observed with regard to the reflex irritability of the animals, under these conditions, which is worthy of notice. Very early in the experiment, at the very beginning of the spasmodic movements, the frog reacted very slightly and incompletely to changes in its position, that is, such as would be produced by holding the vessel in which they were placed in an oblique position or turning them on their sides or backs. On the other hand, the reactions to tactile stimuli of the skin were much more pronounced than usual, so that a tap on the one foot might evoke muscular contractions throughout the whole body. When the animals began to become rigid, stimulation of a toe of the rigid limb could evoke contractions of the limbs which had not yet become stiff. Thus, in all these experiments the first result of the poisoning was motor excitation, which showed itself at first by co-ordinated movements affecting large portions of the body, and later by twitchings of isolated groups of muscles. A little later the respiration became irregular and finally ceased. The higher reflexes, *e.g.*, the reaction to changes in position, were abolished, while the lower were increased. Finally, however, the paralysis became universal, so that also the spinal reflexes were abolished. This stage was followed by a rigidity of the muscles, and last of all the heart ceased to beat. The rapidity of onset of these phenomena is naturally dependent on temperature, being quicker the higher the external temperature. It is evident that we have, therefore, to deal with an action of the poison on the central nervous system. The general spasmodic movements are abolished by previous destruction of the brain and spinal cord; the twitchings of the muscles and the rigor of the extremities persist, however, in the complete absence of the central nervous system, and occur in a hind limb, the nerve of which has been divided, as rapidly as on the opposite side. The onset is not prevented by curarisation, and must, therefore, be ascribed to a direct action of the benzene on the muscles themselves, as has been described at the beginning of this paper.

In the poisoning by coal gas this rigor of the muscles was not observed either by myself or Vahlen and Kunkel, probably because the animals die of asphyxia before the small amounts of benzene present in the coal gas have time to bring about their direct effect upon the muscular tissue. As Experiment I shows, the increase in the percentage of benzene in the coal gas is followed by the onset of rigidity in the muscles.

Rather more difficult is the explanation of the increased reflex excitability in the later stages of intoxication. It may be that the poisonous effects are first confined to the higher centres. On the other hand, the increased irritability of the muscles themselves is the chief factor in the spinal reflex hyper-excitability. In coal gas poison, when the lower centres are also paralysed by asphyxia, the reflex excitability disappears.

It is thus possible to refer the difference between intoxication by coal gas and that by CO entirely to the influence of benzene, which determines in its first stage vigorous excitatory motor phenomena. In warm-blooded animals the conditions are quite different. Lorraine Smith* found that an addition of 0·65 per cent. of benzene to air had no effects on a guinea-pig, and Santesson† did not succeed in producing either acute or chronic poisonous effects by administering benzene to a rabbit by inhalation. In man, too, cases of poisoning by benzene are few and far between. It is possible that the minute quantities, which are absorbed by the lungs, are rapidly oxidised and excreted as an aromatic sulphate. At any rate, in man, benzene plays no part in the poisonous effects of coal gas.

Summary of Results.

(1) Coal gas produces first excitation and then rigor of the isolated frog's muscle.

(2) Frogs exposed to coal gas show excitatory phenomena which are absent when the animal is placed in an atmosphere of CO or nitrogen.

(3) The specific effects of coal gas on frogs are determined by the presence of benzene in the gas, and can be produced by air containing the same percentage of benzene.

(4) There is no reason to suppose that the poisonous effect of coal gas on mammals is determined by anything except its content in CO.

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* Lorrain Smith, "The Poisonous Action of Coal-Gas and Carburetted Water-Gas." 'Report of the Water-Gas Committee,' presented to both Houses of Parliament by command of Her Majesty, 1899, Appendix VII, p. 127.

† Santesson, "Ueber chronische Vergiftungen mit Steinkohlenbenzin," 'Archiv für Hygiene,' 31, p. 336.

"The 'Islets of Langerhans' of the Pancreas." By H. H. Dale, B.Ch., George Henry Lewes Student. Communicated by Professor Starling, M.D., F.R.S. Received December 11, 1903,—Read January 28, 1904.

(From the Physiological Laboratory, University College.)

(Abstract.)

These structures were first described by Langerhans in 1869. They have since been found by many observers in the pancreas of every species of mammal, bird, reptile, and amphibian in which they have been looked for. Kühne and Lea first recognised the peculiarly rich plexus of wide blood-capillaries in the islets. As regards their function, they have been regarded as connected with the nervous system, as lymphatic structures, as embryonic remnants, as patches of exhausted or degenerate pancreatic tissue, as furnishing a particular constituent of the pancreatic juice, and as internally secreting ductless glandular tissue, furnishing a substance necessary for normal carbohydrate metabolism, and quite unconnected with the externally secreting function of the pancreas. This last view has received support from many observations of the degeneration or absence of the islets in diabetes, and from the statement of several observers that, after occlusion of the pancreatic duct, the islets remain intact when the ordinary secretory tissue has disappeared.

Lewaschew, in 1885, first stated that activity of the pancreas led to an increase in the number of the islets, and that intermediate forms between the ordinary secretory tissue and the islets could be found, and were more abundant after activity.

This statement has been confirmed by Pischinger, Maximow, and Tschassownikow, and has also been repeatedly contradicted. Laguesse describes a perpetual change of secretory tissue into islets and *vice versâ*, the islets being, in his view, pancreatic tissue in an internally secreting stage, and representing also the stage during which growth takes place.

My observations have been made on the pancreas of the dog, cat, rabbit, and toad. The pancreas was hardened in a mixture of corrosive sublimate and formaldehyde, sections cut in paraffin and stained with toluidine-blue and eosine. The islets appear, with a low magnification, as relatively unstained areas.

The pancreas was examined in conditions of "rest" (normal activity), of exhaustion produced by prolonged administration of secretin, and of starvation. Exhaustion was produced in the mammals (cat and dog) by repeated injections of secretin into the jugular vein during 6—12 hours, accompanied by bleeding towards the end of the

experiment, until the pancreas ceased or almost ceased to secrete. The animals were anæsthetised with morphia and A.C.E. mixture.

In the toad, secretin was injected into the dorsal lymph-sac by a hypodermic needle during 2—4 days. The effect of starvation was observed in a stray cat, picked up in an emaciated condition and killed immediately, and in toads which had been for several months in the laboratory tank.

In the resting glands of all the species the intermediate forms described by Lewaschew were observed, the islets being formed by an assimilation of the secreting epithelium to the centro-acinary cells and the epithelium of the ductules, with later rearrangement of the cells attended by formation of the wide tortuous blood-capillaries. In the toad evidence was also found of reconstruction of secreting alveoli from islets and of cell-multiplication in the islet stage.

The effect of exhaustion was in all cases the same—a very extensive conversion of the secretory tissue of the gland into large islets, of irregular outline, retaining obvious traces of their former alveolar structure, and containing numerous intermediate forms. Specimens have been obtained from a dog with the greater part of a lobule, and from a toad with the greater part of the whole pancreas thus converted.

The effect of prolonged starvation was, on the whole, very similar to that of exhaustion, but slighter in degree.

Experiments on the dog* and rabbit were also made to observe the effect of occluding the pancreatic duct. There resulted in all cases an interstitial fibrosis. The areas of pancreas not destroyed assumed the islet condition, but the preformed islets showed no special immunity from destruction.

The experiments leave the question of the function of the islets undecided, but the results of occlusion of the duct are in favour of Laguesse's view that they represent an internally secreting stage in the life of pancreatic tissue.

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* On the dog only one experiment was made, in which the operation was performed for other purposes by Professor Starling.

"The Reduction Division in Ferns." By R. P. GREGORY, St. John's College, Cambridge, University Demonstrator in Botany. Communicated by Professor H. MARSHALL WARD, F.R.S. Received January 5,—Read February 5, 1904.

The earlier work upon the spore-formation in Ferns having led to divergent results, a new investigation was begun in connection with other cytological work bearing upon the Mendelian hypothesis. During the progress of this work, Professor Farmer and J. E. S. Moore, in their preliminary communication to the Royal Society upon the reduction-phenomena of plants and animals,* indicated the occurrence of a true reduction at the heterotype division in both plants and animals. The examination of the early stages of spore-formation in Ferns leaves no doubt that the essential features of the phenomena described by the above-mentioned authors are present also in this group of plants.

The species which I have examined are *Pteris tremula*, *Scolopendrium vulgare*, *Asplenium marinum*, the so-called hybrid between *Scolopendrium vulgare* and *Asplenium ceterach*, *Onoclea sensibilis*, *Davallia capensis* and *Fadyenia prolifera*. All these are included among the *Polypodiaceæ*. The processes of spore-formation are identical in all these types. The reduced number of chromosomes, which appears at the heterotype division, is thirty-two.† Owing to the smallness of the nuclei it is not easy to determine exactly the number of the chromosomes in the case of the vegetative cells, but it is about sixty, and I think there is a strong presumption that the number is, as stated by Stevens,‡ sixty-four.

An examination of *Dicksonia davallioides* and of *Alsophila excelsa* (*Cyatheaceæ*) was sufficient to show that it is extremely probable that the following description applies equally to these plants, but as the quantity of material hitherto available was small, only the early stages have been examined as yet.

After the vegetative divisions of the archesporium are complete, the spore mother-cells undergo a period of rest during which the nuclei increase in size. At the end of this period, that is, in the earliest prophase of the reduction division, the spireme thread undergoes a longitudinal fission. The ensuing contraction of the spireme towards one end of the nucleus results in the formation of a series of loops. The double nature of the loops, which is a consequence of the longitudinal fission of the thread, is clear.

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* 'Roy. Soc. Proc.,' 1903, June 18.

† In *Pteris tremula* no exact counts were made, but the number is apparently the same as in the other species described.

‡ "Ueber Chromosomentheilung bei der Sporenbildung der Farne," 'Ber. d. Deutsch. Bot. Gesellsch.,' 1898, p. 263.

As the polarity of the spireme becomes more pronounced the limbs of each loop approach each other, and segmentation into the chromosomes takes place. Each chromosome has its origin in one of the loops of the spireme and thus forms a double **U**-shaped body, the limbs of the **U** being twisted upon one another to varying degrees in the different chromosomes of the same nucleus. The approximation towards one another of the distal ends of the limbs of each **U**, often resulting in the appearance of a "ring" chromosome, is a common feature of the heterotype division in Ferns.

As prophase leads up to metaphase the original longitudinal fission becomes more obscure, and as the chromosomes begin to group themselves in the equatorial plate, each appears to consist of two parallel rods, which represent the approximated limbs of each loop of the spireme, and are joined at one end.

The increasing difficulty during these successive stages of recognising the original longitudinal fission in the limbs of the chromosomes has led to an incorrect interpretation of their structure. The two limbs, of which each chromosome consists, were interpreted as being the result of the original longitudinal fission in the now shortened and thickened chromosomes. A similar conception led to the interpretation of the "ring" form of chromosome as being due to the divergence of the halves into which each chromosome was separated by that fission. The examination of numerous preparations of the stages intermediate between that of the looped spireme and that of early metaphase reveals the incorrectness of this interpretation, inasmuch as the original longitudinal fission can be clearly recognised *in each limb* of the chromosome. In the same way favourable preparations of the "ring" type of chromosome reveal the double nature of the ring, while in many cases the distal ends of the limbs either do not quite meet, or on the other hand may overlap, thus providing forms transitional to the **U** and **X** types of chromosome.

The spindle fibres are attached to the limbs of the chromosomes near the distal ends of the latter; as, therefore, the two daughter-chromosomes are drawn apart, the familiar $\underset{\Gamma}{\text{L}}$-shaped figures are obtained, and the final separation takes place at a point corresponding with the apex of the original loop.

The exact time when the transverse fission, which separates the two limbs of each loop, takes place is not easily determined and appears to be variable. In some cases it appears to have been completed before metaphase is reached, so that the chromosomes as they move to the equatorial plate consist of two separate rods. In others, on the contrary, the separation is synchronous with the commencement of the contraction of the spindle fibres, and consequent divergence of the limbs of the chromosomes.

As the chromosomes move toward the equatorial plate the longitudinal fission of each limb once more becomes clearly apparent, so that, seen in face, the diverging daughter-chromosomes form a ◇-shaped body. A splaying of the ends of the rods at this or a slightly earlier stage often gives rise to those figures, not unlike tetrads, which were assumed by Calkins* to have an origin similar to that of the tetrads characteristic of the heterotype division of, for instance, *Gryllotalpa*, as described by Vom Rath.

In the small chromosomes of the Ferns it is impossible, in the majority of cases, to trace the presence of the original longitudinal fission through the late prophase condition up to the beginning of metaphase. Nevertheless, a study of the successive forms assumed by the chromosomes indicates that the gradual obliteration is apparent rather than real; for it can still be recognised by means of the slightly bifid ends of the limbs of the chromosomes. These appearances are sufficiently convincing as to the correctness of the interpretation of the so-called second longitudinal fission, as nothing more than a reappearance of the original fission undergone by the spireme in the early stages of prophase.

The second (homotype) division follows very rapidly upon the completion of the heterotype division, and is provided for by the longitudinal fission already noticed in the diverging chromosomes of the heterotype division.

The result is, therefore, a transverse true reduction division of the bivalent chromosomes which characterise the heterotype division. This work, therefore, provides an extension to another group of plants of the results obtained by Farmer and Moore† in certain plants and animals.

It is not within the scope of the present paper to discuss the considerations tending to support the belief in the universal occurrence of a reduction division leading to the formation of the gametes. I shall confine myself to a consideration of the significance of the reduction division in connection with Mendelian segregation.

Viewed from this standpoint the occurrence of a qualitative reduction in plants as well as in animals is extremely important as affording a possible provision for that purity of the gametes, in respect of allelomorphic characters, which is demanded by Mendel's hypothesis.

The work of Boveri‡ upon the qualitative differentiation of chromo-

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* "Chromatin Reduction and Tetrad-formation in Pteridophytes," 'Bull. Torrey Bot. Club,' vol. xxiv, 1897, p. 101.

† Farmer and Moore, *loc. cit.*

‡ "Mehrpolige Mitose als Mittel zur Analyse des Zellkerns," 'Verh. d. Phys. Med. Ges. Würzburg,' 1902, vol. 35.

somes, supported by that of Sutton,[*] McClung,[†] and others, affords strong evidence in favour of the theory that the development of certain characters in the zygote corresponds with the presence of certain chromosomes or groups of chromosomes in the nuclei.

Cannon[‡] has suggested a "cytological basis for the Mendelian laws" founded upon the occurrence of a qualitative reduction division, and, at a time when the general concensus of opinion among botanists was adverse to such a conception, went so far as to predict the discovery of a qualitative reduction in plants. A somewhat similar suggestion, based upon work on *Brachystola* (*Orthoptera*), was independently made by Sutton.[§]

Cannon's hypothesis consisted in the assumption that in fertile hybrids, as well as in pure races, "the chromosomes derived from the father and the mother unite in synapsis and separate in the metaphase of one of the maturation divisions. . . so that the end is attained that the chromatin is distributed in such a way that two of the cells receive pure paternal, and two cells pure maternal chromosomes, and no cells receive chromosomes from both the father and the mother."[||]

Thus enunciated the hypothesis is applicable only to "monohybrids" (de Vries); it is insufficient to explain the phenomena observed in the offspring of Mendelian hybrids whose parent races differ from one another in respect of more than one pair of allelomorphic characters.

This was recognised by Sutton,[¶] who was thus led to make a more careful study of the whole division process in *Brachystola*, paying particular attention to the positions assumed by the chromosomes. He says (p. 233), "the results gave no evidence in favour of the parental purity of the gametic chromatin as a whole. On the contrary, many points were discovered which strongly indicate that the position of the bivalent chromosomes in the equatorial plate of the reducing division is purely a matter of chance, that is, that any chromosome pair may be with maternal or paternal chromatid indifferently towards either pole, irrespective of the positions of the other pairs, and hence that a large number of different combinations of maternal and paternal chromosomes are possible in the mature germ-products of an individual."

The view that the gametes may contain both chromosomes of paternal and of maternal origin is strongly supported by the recent results obtained by Valentin Hacker in his study of certain *Copepoda*.

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[*] "On the Morphology of the Chromosome Group in *Brachystola magna*," 'Biol. Bull.,' 1904, vol. 4.

[†] "Spermatocyte Divisions of the Locustidæ," 'Kansas Univ. Quart.,' 1902, vol. 11, No. 8 (contains other references).

[‡] 'Bull. Torrey Bot. Club,' December, 1902.

[§] "The Chromosomes in Heredity," 'Biol. Bull.,' 1903, vol. 4, No. 5, p. 251.

[||] Cannon, *loc. cit.*, p. 660.

[¶] "The Chromosomes in Heredity," 'Biol. Bull.,' 1903, vol. 4, No. 5.

He showed in 1892* that in *Cyclops*, as in *Ascaris* and other forms, the two germ nuclei do not fuse in fertilisation, but give rise to two separate groups of chromosomes which lie side by side in the spindles of the dividing nuclei. Rückert† was able to trace the paternal and maternal groups into the later stages of cleavage. Both observers have shown that the two distinct groups of chromosomes appear also in the germinal vesicle.‡

Hacker§ has since traced the autonomy of the paternal and maternal chromatin in *Cyclops* from fertilisation up to the formation of the mother-cells of the gametes; and the double structure of the nuclei at the maturation divisions is such that he is able to show "das die Vierergruppen auf der einen Seite der Scheidewand väterlichen, auf der anderen mütterlichen Ursprungs sind" (p. 341). In the "secundären Keimbläschen" the paternal and maternal chromosomes pass between one another "in einer ganz gesetzmässigen Quadrillen-ähnlichen Ordnung" (p. 342), with the final result that "Während der Eireifung von Cyclops findet eine Umordnung der Chromatinelemente in der Weise statt, dass die Eizelle in gleichmässiger Mischung grossväterliche und grossmütterliche Elemente erhält" (p. 374).

The regularity observed by Hacker in the movements of the chromosomes at the reduction division, leading as it does to a symmetrical distribution of the chromosomes, may be our first indication of a more comprehensive symmetry which probably underlies the production of the different types of gametes in Mendelian hybrids. "It is impossible to be presented with the fact that in Mendelian cases the cross-bred produces on an average *equal* numbers of gametes of each kind, that is to say, a symmetrical result, without suspecting that this fact must correspond with some symmetrical figure of distribution of the gametes in the cell divisions by which they are produced" (Bateson).‖

On the hypothesis that the segregation of characters occurs at the reduction division, we shall expect that the mitoses in a Mendelian hybrid will be perfectly regular, and in our present condition of inability to recognise qualitative differences between chromosomes alike in form, we should further expect that the mitoses will differ in no *visible* way from those of the pure paternal and maternal races. Cannon¶ has shown this to be the case in race-hybrids of *Pisum*

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* "Die Eibildung bei *Cyclops* und *Canthocampus*," 'Zool. Jahr.,' 1892, vol. 5.

† "Ueber des Selbständigbleiben der väterlichen u. mütterlichen Kernsubstanz, etc.," 'Arch. f. Mikr. Anat.,' 1895, vol. 45.

‡ See Wilson, 'The Cell,' 2nd edit., 1902, p. 299

§ "Ueber das Schicksal der elterlichen u. grosselterlichen Kernanteile," 'Jena Zeitschr.,' 1903, vol. 37, p. 297; a review in 'Zool. Zentralbl.,' 1903, Jahrg. 10, No. 11, p. 365.

‖ 'Mendel's Principles of Heredity,' Cambridge, 1902, p. 30.

¶ "The Spermatogenesis of Hybrid Peas," 'Bull. Torrey Bot. Club,' 1903, vol. 30, p. 519.

sativum, a result which is confirmed by my own observations upon race-hybrids of *Lathyrus odoratus*, for the material of which I am indebted to Mr. Bateson.

Further light upon this question may be expected from the study of hybrids between races which differ from one another either in the morphology or number of the chromosomes. The only observations upon this point are those of Rosenberg* on the hybrid *Drosera longifolia* × *Drosera rotundifolia.*

The number of chromosomes characteristic of the vegetative and of the reduction divisions respectively in the former is 20 and 10; in the latter, 40 and 20. In the vegetative cells of the hybrid 30 (*i.e.*, $10+20$) chromosomes appear. In the formation of the pollen of the hybrid there occur three types of nuclei, all of which may occur in the same pollen-sac. Commonly 15 bivalent chromosomes appear, but in many cases there are 20 (as in *D. rotundifolia*), and in two cases Rosenberg observed 10 (as in *D. longifolia*). He was, however, unable to determine whether dissimilar numbers of chromosomes appear in the daughter nuclei of the same pollen mother-cell. The investigation is, therefore, not sufficiently complete at present to permit a useful discussion of the results.

The sterility which characterises many hybrids follows upon the abortive development of the sex cells, and the suggestion has been made† that this may be due to the inability of the hybrid to separate, in the formation of the gametes, the characters which were united in the hybrid zygote. It is well known that sterile plant-hybrids are particularly characterised by abortive development of the pollen, or (in the case of the hybrid Fern described by Farmer‡) of the spores.

Among the offspring of a race-hybrid of *Lathyrus odoratus* fertilised with its own pollen, Mr. Bateson obtained a number of individuals which failed to form good pollen. In the plants with coloured flowers the sterility was, with a few exceptions, correlated with the development of a somatic character—the sterile plants generally possessing a green leaf axil, while the fertile coloured plants with rare exceptions had red axils. In these plants the divisions of the vegetative cells are quite normal, as are also those of the archesporium up to the formation of the pollen mother-cells. The irregularity makes its appearance only in the heterotype division.

The longitudinal fission of the spireme takes place quite normally, but the segmentation into chromosomes is, if carried out at all, irregular, and the pollen mother-cells degenerate. Since the equation divisions are quite normal, this would seem to indicate that the union

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* "Das Verhalten der Chromosomen in einer hybriden Pflanze," 'Ber. d. Deutsch. Bot. Gesellsch.,' 1903, vol. 21, p. 110.

† Bateson and Saunders, Report to Evolution Committee, I, p. 148.

‡ 'Annals of Botany,' 1897, vol. 11, p. 533.

of the chromosomes in synapsis is such as to prevent any subsequent separation, the result being that no sex-cells can be organised, since the essential condition of a qualitative separation of the chromatin is not fulfilled.

"The Secreto-motor Effects in the Cat's Foot Studied by the Electrometer." By AUGUSTUS D. WALLER, M.D., F.R.S.

Received November 17,—Read November 19, 1903. Received in revised form January 16, 1904.

In a previous communication* it was stated that the electrical signs of secreto-motor action by tetanisation of the sciatic nerve are demonstrable in the pads of a cat's foot after death, best so during the second half-hour after death, when the action of the nerve upon muscles of the limb has ceased.

The subsequent study of these effects, by means of electrometer records, has brought out with great distinctness the chief classical events with which we are familiar in the case of the contraction of voluntary muscle, viz., the latency and course of a single response to a single stimulus, the super-position of two or more responses and the composition of tetanus, summation of stimuli, fatigue and recovery, and the staircase phenomenon. The difference between the muscular and the secreto-motor series of phenomena is principally a difference of time, the former being about 100 times more rapid than the latter.

I may preface the description by stating that I have experimentally satisfied myself that the electrical effects are in reality of glandular origin. The response is completely abolished by atropine, and it is restricted to the pads (glandular) of the skin, being completely absent from the hairy (non-glandular) skin, *i.e.*, it is not a pilo-motor concomitant.

The description itself will be best given by means of the following electrometer records of :—

1. A single response to show the latent period and duration of the response.
2. A series of single responses to show staircase phenomenon.
3. A series of four responses to show composition of tetanus.
4. A series exhibiting *post-mortem* decline.

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* 'Proc. Roy. Soc.,' November, 1901, "On Skin-currents. Part II.—Observations on Cats." The electrical effect of indirect excitation is always ingoing through the skin. This direction has been conventionally indicated throughout this paper by a downward movement of the mercury column.

5. A series to exhibit the relation between magnitude of stimulation and magnitude of response.
6. A single response before and after tetanus to illustrate "facilitation" (Bahnung).
7. A single response before and after tetanus to illustrate fatigue.
8. A series to illustrate summation of stimuli.
9. A series to show the difference between infrequent and frequent stimuli.

No special comment upon the records appears to be necessary, beyond, perhaps, a remark to the effect that "summation of stimuli," as distinguished from "summation of effects," is, by reason of the great length of the latent period, a particularly evident phenomenon. The latent period itself has its seat at the organ of intermediation between nerve and secreting cell, as is shown by the absence of demonstrable lost time to direct excitation and along the nerve itself. The declining excitability of the secreto-motor nerve fibres is very evidently in the centrifugal direction, stimulation of the nerve nearer to the periphery being effective after stimulation further from the periphery has ceased to be effective.

Similar effects are obtainable on nerve-skin preparations of the frog, "summation of stimuli" and "staircase effect" being, as in the case of the cat, particularly evident.

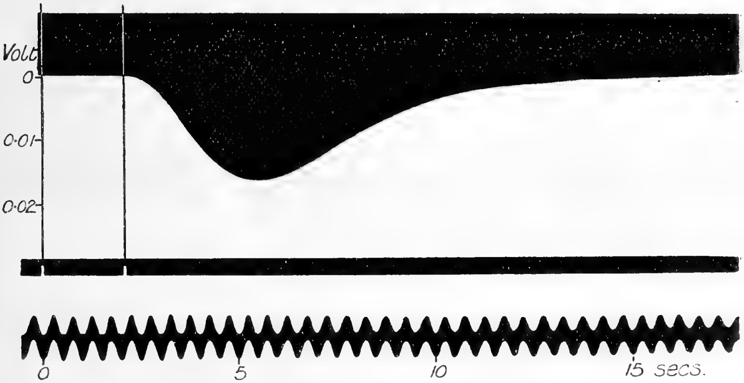

FIG. 1.—Cat. Nerve-skin response to a single induction shock 40 minutes *post-mortem*.

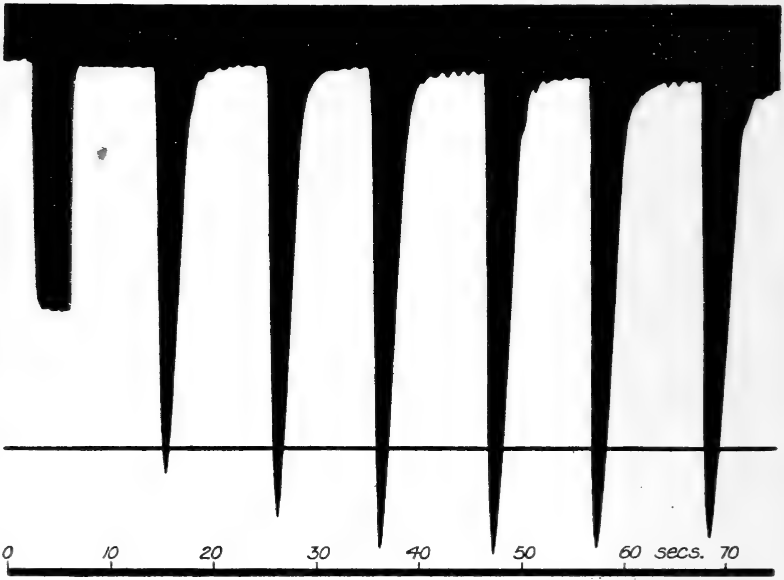

FIG. 2.—Cat. Six nerve-skin responses to single induction shocks of uniform strength. Staircase increase from 0·0165 to 0·0195 volt. (The initial deflection is that of a standard $\frac{1}{100}$th volt.)

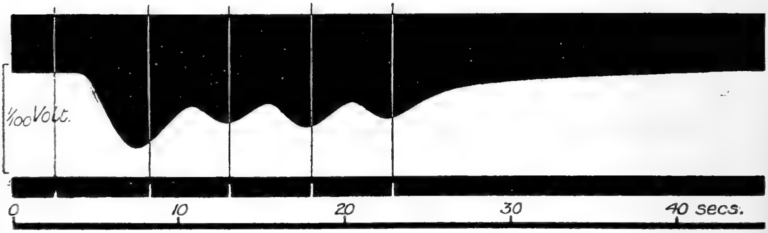

FIG. 3.—Cat. 35 minutes *post-mortem*. Imperfect tetanus by four instantaneous make-break induction shocks at intervals of about 5 seconds.

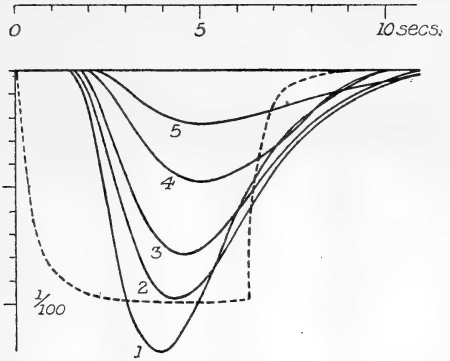

FIG. 4.—Cat's pad. 30 to 65 minutes *post-mortem.* Five single responses to excitation of the sciatic nerve by instantaneous make-break induction shocks at intervals of approximately 10 minutes. The dotted line shows the curve given by $\frac{1}{100}$th volt through the preparation and electrometer.

Time *post-mortem.*	Latency.	Magnitude of response.
30 mins.	1 ·4 sec.	0 ·0123 volt.
40 ,,	1 ·6 ,,	0 ·0100 ,,
48 ,,	1 ·7 ,,	0 ·0080 ,,
55 ,,	1 ·8 ,,	0 ·0048 ,,
65 ,,	2 ·0 ,,	0·0018 ,,

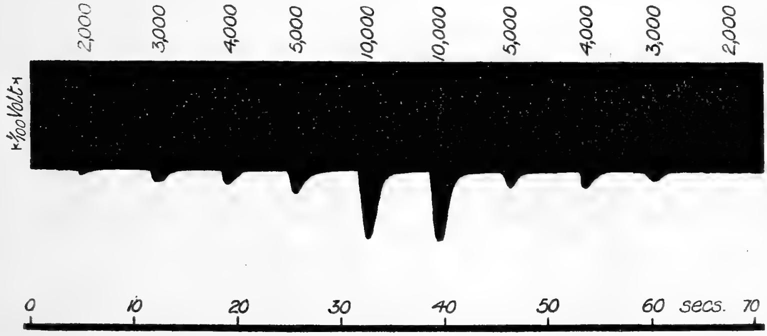

FIG. 5.—Responses to single shocks of increasing and diminishing strengths. 1 hour *post-mortem.*

Strength of stimulation.	Voltage of response.	Strength of stimulation.	Voltage of response.
2,000	0 ·0005	10,000	0·0090
3,000	0 ·0015	5,000	0·0015
4,000	0 ·0020	4,000	0 ·0015
5,000	0 ·0030	3,000	0 ·0010
10,000	0 ·0085	2,000	Nil

N.B.—This electrometer record is not very satisfactory, as the magnification was taken too low. The following series of numbers observed without record with higher magnification 45 minutes *post-mortem* is a better one :—

Strength of stimulation.	Voltage of response.	Strength of stimulation.	Voltage of response.
1,000	Nil	4,000	0 ·0065
2,000	0 ·0015	5,000	0 ·0125
3,000	0 ·0035	10,000	Off scale

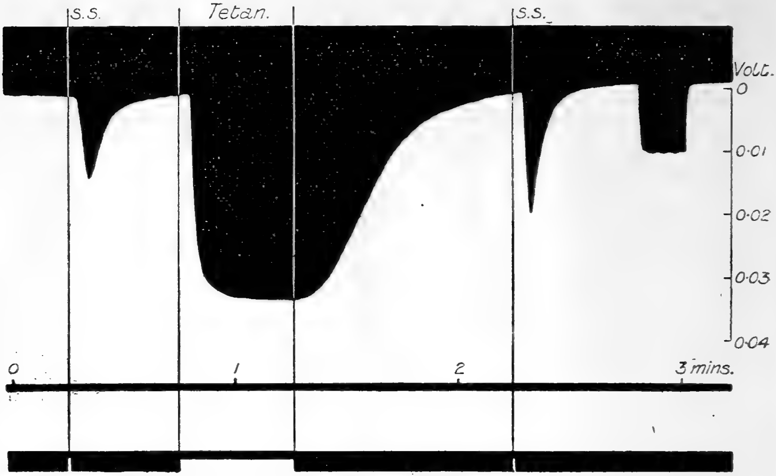

Fig. 6.—Effects of a single shock, S.S., before and after tetanisation for ½ minute.

Before .... = 0·0115 volt.
(During tet. ...... = 0·0300 „)
After .... = 0·0155 „

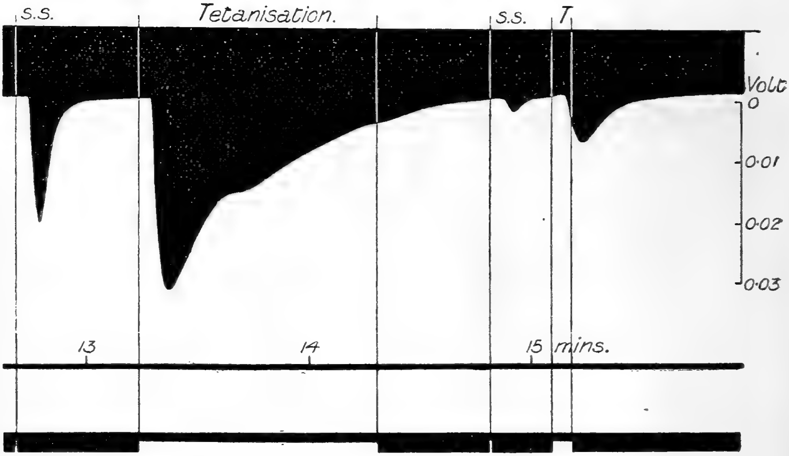

Fig. 7.—Effects of a single shock, S.S., before and after strong tetanisation for 1 minute.

Before .... = 0·0183 volt.
(During tet. ...... = 0·0283 „)
After............. = 0·0017 „

A second tetanisation at T gives only 0·0067 volt.

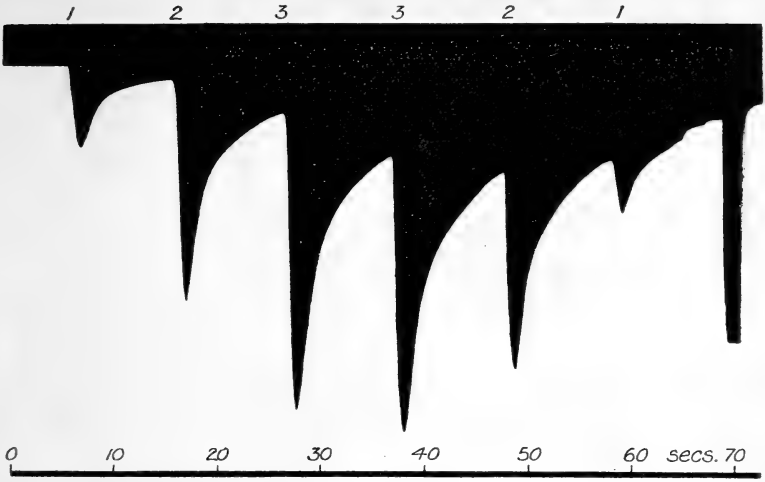

FIG. 8.—To illustrate summation of stimuli. Effects of 1, 2, 3, 3, 2, 1 instantaneous make-break shocks.

Response to 1 shock........ = 0·0035 volt.
" 2 " .... = 0·0097 "
" 3 " .... = 0·0130 "
" 3 " .... = 0·0120 "
" 2 " .... = 0·0087 "
" 1 " .... = 0·0023 "

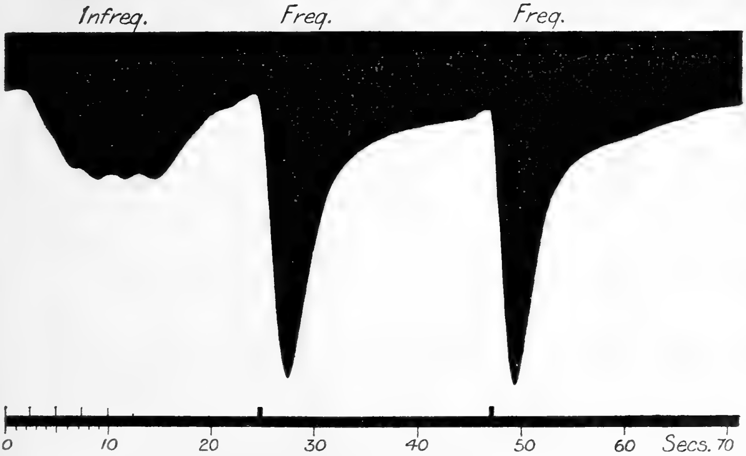

FIG. 9.—To show that the effect of frequent is greater than that of infrequent stimuli. The first response, aroused by five shocks at an interval of 2 seconds, is an incomplete tetanus with a maximum value of 0·0040 volt. The second and third responses are each to five shocks at an interval of about $\frac{1}{10}$th second, and reach maximum values of 0·0123 and 0·0120 volt.

Addendum, December 10, 1903.

The latent period and the duration of the response are smaller at high temperature, greater at low temperature. And under similar conditions the voltage of the response is greater at high than at low temperature. These three points are illustrated by fig. 10 of a single response from the left foot enclosed in a cool chamber at + 9°, and from the right foot enclosed in a warm chamber at + 35°.

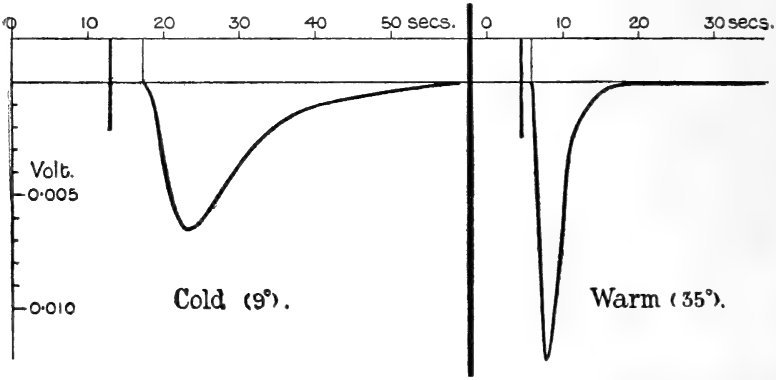

Fig. 10.—Cat. Nerve-skin response to a single induction shock at 9° and at 35° of the surrounding air.

	Cold.	Warm.
Latent period..........	4 secs.	1 ·5 secs.
Duration of response....	30 ,,	10 ·0 ,,
Maximum voltage......	0 ·0065 volt.	0 ·0122 volt.

The progressive alterations exhibited by fig. 10 are no doubt influenced by the falling temperature that normally occurs in a limb after arrest of the circulation.

"On the Effects of Joining the Cervical Sympathetic Nerve with the Chorda Tympani." By J. N. Langley, F.R.S., and H. K. Anderson, M.D. Received January 26,—Read February 4, 1904.

It is well known that the cervical sympathetic nerve and the chorda tympani have opposite actions upon the blood-vessels of the submaxillary gland, the former causing contraction of the vessels, and the latter, dilatation. Evidence has been given by one of us* that the chorda tympani if united with the cervical sympathetic, can in time make connection with the nerve cells of the superior cervical ganglion and become in part vaso-constrictor fibres. Our experiments have been directed to determine whether the cervical sympathetic if allowed an opportunity of becoming connected with the peripheral nerve cells in the course of the chorda tympani will in part change their function from vaso-constrictor to vaso-dilator. Two experiments were made on anæsthetised cats, both give similar results, but one was much more conclusive on the point at issue than the other, and here we shall speak of that only. The superior cervical ganglion was excised and the central end of the cervical sympathetic nerve was joined to the peripheral end of the lingual, which contains the chorda tympani fibres. After allowing time for union and regeneration of the nerves, the cervical sympathetic was stimulated; it caused prompt flushing of the sub-maxillary glands, and the effect was repeatedly obtained.

The experiment shows we think (1) that vaso-constrictor nerve fibres are capable of making connection with peripheral vaso-dilator nerve cells, and becoming vaso-dilator fibres, and (2) that whether contraction or inhibition of the unstriated muscle of the arteries occurs on nerve stimulation, depends upon the mode of nerve-ending of the post-ganglionic nerve fibre.

The cervical sympathetic gave a less scanty and more prolonged secretion than normal, so that some of its nerve fibres had become connected with the peripheral secretory nerve cells of the chorda tympani.

A full account will be published later in the 'Journal of Physiology.'

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* Langley, 'Journal of Physiology,' 1898, vol. 23, p. 267.

"The Longitudinal Stability of Aerial Gliders."

By G. H. Bryan, Sc.D., F.R.S., and W. E. Williams, B.Sc., University College of North Wales.

Received June 18,—Read June 18, 1903,—Received in revised form January 7, 1904.

1. *Introduction.*

The main difficulty connected with the attempt to fly by means of a machine heavier than air is that of longitudinal stability. It is not difficult to construct an aeroplane system which shall be *transversely* stable.

But for this difficulty the problem of artificial flight would probably have been solved already. Experiments in gliding under gravity have been always made with machines not too large to be kept balanced by the skill of the experimenter, and the glides, though undoubtedly successful, have been of short duration. Experiments have invariably stopped short of the performance of continuous flight by a mechanically propelled machine.

The problem of artificial flight is hardly likely to be solved until the conditions of longitudinal stability of an aeroplane system have been reduced to a matter of pure mathematical calculation.

A theoretical investigation, even if calculated under conditions slightly different to those occurring in nature, will serve as a basis of comparison by which experimental results can be co-ordinated and interpreted in their true light.

The object of these investigations is (1) to show that the longitudinal stability of aeroplane systems *can* be made the subject of mathematical calculation; (2) to draw the attention of those interested in the problem of artificial flight to the necessity of acquiring further experimental knowledge concerning the quantities on which this stability is shown to depend.

2. *General Investigation of the Longitudinal Stability of any Symmetrical Aeroplane System.*

Consider any system of aeroplanes, having a plane of symmetry, descending in a vertical plane, in air or in any resisting medium whatever. To specify the motion (which we suppose to be two-dimensional) choose two axes at right angles fixed in the body, having the centre of gravity as origin.

The motion will be completely determined if at every instant we know—

(1) the angle θ which the axis of x makes with the vertical.*

(2) the velocity components u, v, of the body along the two axes.

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* This angle is supposed measured from the *downward drawn* vertical in the positive direction. The axis of y must be drawn *above* the horizontal at an inclination of θ.

The angular velocity of rotation of the body will be $d\theta/dt$ or $\dot{\theta}$.

We shall use m to denote the mass of the body, mk^2 its moment of inertia about the centre of gravity.

Whatever be the law of resistance, the resistances of the air on the several parts of the system will in general be functions of u, v and θ. These resistances are always equivalent to two forces, which we shall call $m\mathrm{X}$ and $m\mathrm{Y}$, along the axes, and a couple $m\mathrm{G}$ about the origin, so that X, Y, G, denote the forces and couple, divided by the mass of the body.

The equations of motion of the body are—

$$\left.\begin{aligned} m\left(\frac{du}{dt} - v\frac{d\theta}{dt}\right) &= mg\cos\theta - m\mathrm{X}, \\ m\left(\frac{dv}{dt} + u\frac{d\theta}{dt}\right) &= -mg\sin\theta - m\mathrm{Y}, \\ mk^2\frac{d^2\theta}{dt^2} &= -m\mathrm{G}. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1).$$

In *steady motion* u, v and θ are constant, and equations (1) give

$$0 = g\cos\theta - \mathrm{X}, \quad 0 = -g\sin\theta - \mathrm{Y}, \quad 0 = -\mathrm{G} \quad \ldots\ldots \quad (2).$$

Knowing the forms of the aeroplanes and other parts of the system and the law of resistance, X, Y, G are known functions of u, v and $\dot{\theta}$. Moreover, in steady motion, $\dot{\theta} = 0$.

Equations (2) thus determine the values of u, v, θ, for steady motion.

Fluctuations about Steady Motion.—We must now examine what happens when the system is slightly disturbed from its state of steady motion. Let the disturbance be represented at time t by small increases δu, δv, $\delta\theta$ in the values of u, v, θ, and let u_0, v_0, θ_0 be their values in steady motion, so that in the disturbed motion,

$$u = u_0 + \delta u, \qquad v = v_0 + \delta v, \qquad \theta = \theta_0 + \delta\theta.$$

Also let X_0, Y_0, G_0 be the forces and couple in the steady state; then in the disturbed state we have, neglecting small quantities of the second order,

$$\mathrm{X} = \mathrm{X}_0 + \delta u\frac{d\mathrm{X}}{du} + \delta v\frac{d\mathrm{X}}{dv} + \delta\dot{\theta}\frac{d\mathrm{X}}{d\dot{\theta}} \quad \ldots\ldots\ldots\ldots\ldots \quad (3),$$

and two similar equations for Y and G.

We shall denote differential coefficients such as $d\mathrm{X}/du \ldots d\mathrm{G}/d\dot{\theta}$ by $\mathrm{X}_u \ldots \mathrm{G}_\theta$.

Substituting in the equations of motion, we obtain, to the first order,

$$\frac{d}{dt}\delta u - v_0\,\frac{d}{dt}\,\delta\theta = -g\sin\theta_0\delta\theta - X_u\delta u - X_v\delta v - X_{\dot\theta}\delta\dot\theta,$$

$$\frac{d}{dt}\,\delta v + u_0\,\frac{d}{dt}\,\delta\theta = -g\cos\theta_0\delta\theta - Y_u\delta u - Y_v\delta v - Y_{\dot\theta}\delta\dot\theta,$$

$$k^2\,\frac{d^2}{dt^2}(\delta\theta) = -G_u\delta u - G_v\delta v - G_{\dot\theta}\delta\dot\theta.$$

To solve these equations, put

$$\delta u = Pe^{\lambda t},\qquad \delta v = Q\epsilon^{\lambda t},\qquad \delta\theta = R\epsilon^{\lambda t}.$$

Substituting, dividing by $\epsilon^{\lambda t}$, and re-arranging the terms, we have

$$\begin{aligned} P(\lambda + X_u) + QX_v + R(\lambda X_{\dot\theta} - \lambda v_0 + g\sin\theta_0) &= 0,\\ PY_u + Q(\lambda + Y_v) + R(\lambda Y_{\dot\theta} + \lambda u_0 + g\cos\theta_0) &= 0,\\ PG_u + QG_v + R(\lambda^2 k^2 + \lambda G_{\dot\theta}) &= 0. \end{aligned}$$

Eliminating P, Q, R,

$$\begin{vmatrix} \lambda + X_u, & X_v, & -\lambda v_0 + \lambda X_{\dot\theta} + g\sin\theta_0 \\ Y_u, & \lambda + Y_v, & \lambda u_0 + \lambda Y_{\dot\theta} + g\cos\theta_0 \\ G_u, & G_v, & \lambda^2 k^2 + \lambda G_{\dot\theta} \end{vmatrix} = 0 \quad (4).$$

If the determinant be expanded in powers of λ we get an equation of the form

$$A\lambda^4 + B\lambda^3 + C\lambda^2 + D\lambda + E = 0 \quad \text{..............} \quad (4a),$$

where

$$\left.\begin{aligned} A &= k^2,\\ B &= k^2(X_u + Y_v) + G_{\dot\theta},\\ C &= k^2(X_uY_v - X_vY_u) + v_0G_u - u_0G_v - X_{\dot\theta}G_u - Y_{\dot\theta}G_v,\\ D &= u_0(X_vG_u - X_uG_v) + v_0(Y_vG_u - Y_uG_v),\\ &\quad - g\sin\theta_0G_u - g\cos\theta_0G_v + Y_{\dot\theta}(X_vG_u - X_uG_v)\\ &\quad - X_{\dot\theta}(Y_vG_u - Y_uG_v) + G_{\dot\theta}(X_uY_v - X_vY_u),\\ E &= g\cos\theta_0(X_vG_u - X_uG_v) - g\sin\theta_0(Y_vG_u - Y_uG_v), \end{aligned}\right\} \quad (5).$$

In many cases it is possible to take the axes of co-ordinates, so that θ_0 shall be zero, and also that Y and its differentials shall vanish. In such cases the coefficients take the following simple forms:—

$$\left.\begin{aligned} A &= k^2,\\ B &= k^2X_u + G_{\dot\theta},\\ C &= v_0G_u - u_0G_v - X_{\dot\theta}G_u,\\ D &= u_0(X_vG_u - X_uG_v) - gG_v,\\ E &= g(X_vG_u - X_uG_v), \end{aligned}\right\} \quad \text{..............} \quad (6).$$

Equation (4) or (4*a*) is the period equation for small fluctuations about steady motion.

In order that the steady motion may be stable, the roots of this biquadratic must either be real and negative, or complex with their real parts negative, and Routh* finds that this will be the case if the six quantities

$$\mathrm{A, B, C, D, E, \text{ and } BCD - AD^2 - EB^2},$$

are all of the same sign. Since A is essentially positive the remaining five quantities must all be positive. We shall denote the last quantity by H.

3. *General Theorems.*†

(1) The general transformation formulæ, connecting the nine coefficients $\mathrm{X}_u \ldots \mathrm{G}_{\dot\theta}$ referred to any given system of axes with those referred to any other system of axes, may be easily written down and need not be discussed here.

(2) The work of calculating these coefficients for a given system may be reduced if the resistance is proportional to the square of the velocity, for X, Y, G will then be homogeneous quadratic functions of u, v, $\dot\theta$, and Euler's theorem of homogeneous functions gives, remembering that $\dot\theta_0 = 0$ and applying (2),

$$u\mathrm{X}_u + v\mathrm{X}_v = 2g\cos\theta,$$

$$u\mathrm{Y}_u + v\mathrm{Y}_v = -2g\sin\theta,$$

$$u\mathrm{G}_u + v\mathrm{G}_v = 0.$$

(3) If V is the velocity of gliding, the coefficients $m\mathrm{X}_u \ldots m\mathrm{G}_{\dot\theta}$ are all linear functions of V for a given angle of gliding. But mg being, in steady motion, equal to the vertical resistance, is proportional to V^2. Hence if m be eliminated, the values of $\mathrm{X}_u \ldots \mathrm{G}_{\dot\theta}$ are inversely proportional to V. In this case

A is independent of V,
B is of dimensions V^{-1},
C is of form $\mathrm{P} + \mathrm{QV}^{-2}$,
D is of dimensions V^{-1},
E is of dimensions V^{-2},

and the expression H or $\mathrm{BCD} - \mathrm{AD}^2 - \mathrm{EB}^2$ assumes the form $\mathrm{PV}^{-2} + \mathrm{QV}^{-4}$. Thus the conditions of stability $\mathrm{C} > \mathrm{O}$ and $\mathrm{H} > \mathrm{O}$ impose limits on the value of V^2, the remaining conditions only

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* Routh, 'Advanced Rigid Dynamics,' p. 167.

† This section and also the subsequent parts enclosed in [......] have been rewritten October 27, 1903.

depend on the form and dimensions of the machine and the angle of gliding.

We shall now show how the nine coefficients $X_u \ldots G_{\dot\theta}$ can be calculated for a system of aeroplanes if the laws of variation of the resultant pressures and of the positions of the centres of pressure are known. We shall assume that the resultant pressure on a plane lamina varies as the square of the velocity, and acts in a direction normal to the lamina.

We may therefore write

$$R = KSV^2 f(\alpha),$$

where S is the area of the lamina, V the velocity, and α the angle between the plane of the lamina and the direction of motion, K a constant depending on the units employed.

The function $f(\alpha)$ has been determined by Langley* for certain rectangular planes.

Also, let the distance of the centre of pressure from the centre of figure be $a\phi(\alpha)$, $2a$ being the breadth of the lamina.

Experiments to determine $\phi(\alpha)$ for square planes have been made by Joesel,† Kummer,‡ and Langley,§ and Kummer has also experimented on oblong planes, with their longer side in the direction of motion.

Their results show a certain amount of discrepancy, and there appears to be considerable difficulty in obtaining consistent results at small inclinations.

Further experiments are very necessary, and it is to be hoped that more attention will be given to determinations of $\phi(\alpha)$, when their importance, as affecting the stability, has been recognised.

Experiments show that $f(\alpha)$, $\phi(\alpha)$ are, to a first approximation, independent of the translational velocity of the lamina, but the effects of a rotational angular velocity, $\dot\theta$, have never been considered. It would not be difficult to determine these effects by experiments with a whirling table, making $\dot\theta$ the angular velocity of the table. Failing such experiments, these effects must be neglected, and, as $\dot\theta$ is zero in the steady motion, and only small oscillations are considered, they are probably small. When the system consists of a number of planes rigidly connected together, we may now write the component forces and couple in the form

$$\left.\begin{aligned} mX &= \Sigma KS_1V_1^2 f(\alpha_1-\beta_1)\cos\beta_1 \\ mY &= \Sigma KS_1V_1^2 f(\alpha_1-\beta_1)\sin\beta_1 \\ mG &= \Sigma KS_1V_1^2 f(\alpha_1-\beta_1)\{p_1+a_1\phi(\alpha_1-\beta_1)\} \end{aligned}\right\} \quad\ldots\ldots \quad (7),$$

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* 'Experiments in Aerodynamics,' p. 62.

† Joesel, 'Mémorial du Genie Maritime,' 1870.

‡ 'Berlin Akad. Abhandlungen,' 1875, 1876.

§ *Loc. cit.*, p. 90.

where S_1 is the area, $2a_1$ the breadth of the lamina, ξ_1, η_1 the co-ordinates of its centre, V_1 the resultant velocity of its centre, $\alpha_1 = \tan^{-1} u_1/v_1$ the angle which the direction of motion makes with the axis of y, β_1 the angle which the plane of the lamina makes with the axis of y, so that $(\alpha_1 - \beta_1)$ is the angle between the plane and the direction of motion of the centre of mass, and finally, $p_1 = \eta_1 \cos\beta_1 + \xi_1 \sin\beta_1$.

The summation is to be extended over all the planes of the system.

If, now, u, v be the component velocities of the centre of gravity the velocities u_1, v_1 of the centre of the lamina will be given by

$$u_1 = u - \eta_1\dot{\theta}, \qquad v_1 = v + \xi_1\dot{\theta},$$

and for steady motion, since $\dot{\theta} = 0$, $u_1 = u$, $v_1 = v$.

We may therefore write the expressions for the forces in the form

$$\begin{aligned} mX &= \Sigma KS_1 (u_1^2 + v_1^2) f(\tan^{-1} u_1/v_1 - \beta_1) \cos\beta_1 \\ &= \Sigma KS_1 (u^2 + v^2 - 2u\eta_1\dot{\theta} + 2v\xi_1\dot{\theta}) f\left\{\tan^{-1}\left(\frac{u - \eta_1\dot{\theta}}{v + \xi_1\dot{\theta}}\right) - \beta_1\right\} \cos\beta_1, \end{aligned}$$

neglecting terms in $\dot{\theta}^2$.

Similarly,

$$mY = \Sigma KS_1 (u^2 + v^2 - 2u\eta_1\dot{\theta} + 2v\xi_1\dot{\theta}) f\left\{\tan^{-1}\left(\frac{u - \eta_1\dot{\theta}}{v + \xi_1\dot{\theta}}\right) - \beta_1\right\} \sin\beta_1,$$

$$\begin{aligned} mG = {} & -\Sigma KS_1 (u^2 + v^2 - 2u\eta_1\dot{\theta} + 2u\xi_1\dot{\theta}) f\left\{\tan^{-1}\left(\frac{u - \eta_1\dot{\theta}}{v + \xi_1\dot{\theta}}\right) - \beta_1\right\} \\ & \left\{p_1 + a\phi\left(\tan^{-1}\left(\frac{u - \eta_1\dot{\theta}}{v + \xi_1\dot{\theta}}\right) - \beta_1\right)\right\}. \end{aligned}$$

Differentiating these expressions with respect to u, v, $\dot{\theta}$ we have :—

$$mX_u = \Sigma\,[KS_1 (2u - \eta_1\dot{\theta}) f(\alpha_1 - \beta_1) \cos\beta_1 + KS_1 v_1 f'(\alpha_1 - \beta_1) \cos\beta_1];$$

or, since $\dot{\theta} = 0$ in the steady motion,

$$mX_u = \Sigma\,[2KS_1 u f(\alpha_1 - \beta_1) \cos\beta_1 + KS_1 v f'(\alpha_1 - \beta_1) \cos\beta_1] \dots (8),$$

Similarly,

$$\begin{aligned} mX_v &= \Sigma\,[2KS_1 v f(\alpha_1 - \beta_1) \cos\beta_1 - KS_1 u f'(\alpha_1 - \beta_1) \cos\beta_1], \\ mY_u &= \Sigma\,[2KS_1 u f(\alpha_1 - \beta_1) \sin\beta_1 + KS_1 v f'(\alpha_1 - \beta_1) \sin\beta_1], \\ mY_v &= \Sigma\,[2KS_1 v f(\alpha_1 - \beta_1) \sin\beta_1 - KS_1 u f'(\alpha_1 - \beta_1) \sin\beta_1], \\ mX_{\dot{\theta}} &= \Sigma\,[2KS_1 (-u\eta_1 + v\xi_1) f(\alpha_1 - \beta_1) \cos\beta_1 \\ &\qquad - KS_1 (v\eta_1 + u\xi_1) f'(\alpha_1 - \beta_1) \cos\beta_1], \end{aligned}$$

$$mY_{\dot\theta} = \Sigma\,[2KS_1(-u\eta_1+v\xi_1)\,f(\alpha_1-\beta_1)\sin\beta_1 - KS_1(v\eta_1+u\xi_1)\,f'(\alpha_1-\beta_1)\sin\beta_1],$$

$$mG_u = \Sigma\,[-2KS_1(u-\eta_1\dot\theta)\,f(\alpha_1-\beta_1)\{p_1+a_1\phi(\alpha_1-\beta_1)\} - KS_1v_1f'(\alpha_1-\beta_1)\{p_1+a_1\phi(\alpha_1-\beta_1)\} - KSv_1f(\alpha_1-\beta_1)\,a_1\phi'(\alpha_1-\beta_1)].$$

Since in the steady motion $G = 0$,

$$i.e.,\quad 2(u^2+v^2)\,\Sigma KS_1f(\alpha_1-\beta_1)\,\{p_1+a_1\phi(\alpha_1-\beta_1)\} = 0,$$

$$mG_u = v\Sigma\,[-KS_1f'(\alpha_1-\beta_1)\,\{p_1+a_1\phi(\alpha_1-\beta_1)\} - KS_1f(\alpha_1-\beta_1)\,a_1\phi'(\alpha_1-\beta_1],$$

$$mG_v = u\Sigma\,[KS_1f'(\alpha_1-\beta_1)\,\{p_1+a_1\phi(\alpha_1-\beta_1)\} + KS_1f(\alpha_1-\beta_1)\,a_1\phi'(\alpha_1-\beta_1)],$$

$$mG_{\dot\theta} = \Sigma\,[-KS_1f'(\alpha_1-\beta_1)\,(-v\eta_1+u\xi_1)\,\{p+a\phi(\alpha_1-\beta_1)\} - KS_1f(\alpha_1-\beta_1)\,(-v\eta_1+u\xi_1)\,a\phi'(\alpha_1-\beta_1)].$$

4. *Numerical Calculations for Particular Cases. Single Laminæ.*

We now consider the stability of certain particular systems, beginning with a single plane lamina.

Since the lamina is falling steadily under gravity, its plane must necessarily be horizontal.

Take the axis of y parallel to this plane, then $\theta = 0$, $\beta = 0$, and, therefore, $Y = 0$; also, for equilibrium, $\eta + a\phi(\alpha) = 0$.

The coefficients, therefore, become

$$\begin{aligned} mX_u &= 2KSuf(\alpha)+KSvf'(\alpha),\\ mX_v &= 2KSvf(\alpha)-KSuf'(\alpha),\\ mX_{\dot\theta} &= -2KS(u\eta-v\xi)\,f(\alpha)+KSf'(\alpha)\,(v\eta+u\xi),\\ mG_u &= -KSvf(\alpha)\,a\,\phi'(\alpha),\\ mG_v &= KSuf(\alpha)\,a\phi'(\alpha),\\ mG_{\dot\theta} &= -KSaf(\alpha)\,(v\eta+u\xi)\,a\phi'(\alpha). \end{aligned}$$

Ex. 1.—Consider a square plane, balanced so as to fly at an angle of 10°, with the centre of gravity in the plane, and suppose its radius of gyration given by $k^2 = \frac{1}{2}a^2$.

Professor Langley's results give for this particular angle

$$f(\alpha) = 0{\cdot}3, \qquad f'(\alpha) = 1{\cdot}6,$$

and from Joesel's formula

$$\phi(\alpha) = 0{\cdot}49, \qquad \phi'(\alpha) = -0{\cdot}59, \qquad \therefore\ \eta = 0{\cdot}49a.$$

also

$$u = \mathrm{V} \sin \alpha = 0{\cdot}17\mathrm{V}, \qquad v = 0{\cdot}98\mathrm{V}.$$

Substituting these values in the above expressions for X_u . . ., we have

$$m\mathrm{X}_u = 1{\cdot}66\mathrm{KSV}, \qquad m\mathrm{G}_u = 0{\cdot}17\mathrm{KSV}a,$$

$$m\mathrm{X}_v = 0{\cdot}32\mathrm{KSV}, \qquad m\mathrm{G}_v = -0{\cdot}03\mathrm{KSV}a,$$

$$m\mathrm{X}_{\dot\theta} = 0{\cdot}73\mathrm{KSV}a, \qquad m\mathrm{G}_{\dot\theta} = 0{\cdot}09\mathrm{KSV}a^2.$$

Substituting these values in the expressions for A, B, C, D, E, given in § (2), and remembering that by the conditions for steady motion $mg = \mathrm{KSV}^2 f(\alpha)$, we obtain

$$\mathrm{A} = \tfrac{1}{2}a^2, \qquad \mathrm{B} = 92\, a^2/\mathrm{V}, \qquad \mathrm{C} = 18a - 1340\, a^2/\mathrm{V}^2,$$

$$\mathrm{D} = 295\, a/\mathrm{V}, \qquad \mathrm{E} = 36400\, a/\mathrm{V}^2,$$

and therefore

$$\mathrm{H} \equiv \mathrm{BCD} - \mathrm{AD}^2 - \mathrm{EB}^2 = 4{\cdot}8 \,.\, 10^5 \frac{a^4}{\mathrm{V}^2} - 3{\cdot}6 \,.\, 10^7 \frac{a^5}{\mathrm{V}^4}$$

$$- 4{\cdot}3 \,.\, 10^4 \frac{a^4}{\mathrm{V}^2} - 3{\cdot}08 \,.\, 10^8 \frac{a^5}{\mathrm{V}^4}.$$

This expression will be positive if $\mathrm{V}^2 > 774a$, and this condition will also make C positive. The glider will therefore be stable if its velocity is greater than $\sqrt{(774a)}$, the units being feet and seconds.

Ex. 2.—Let us now take the angle of gliding to be 35°, and assume as before $k^2 = \frac{1}{2} a^2$. At this angle we have

$$f(\alpha) = 0{\cdot}84, \qquad f'(\alpha) = 0{\cdot}6,$$

$$\phi(\alpha) = 0{\cdot}26, \qquad \phi'(\alpha) = 0{\cdot}5.$$

With these values, we have

$$m\mathrm{X}_u = 1{\cdot}34\mathrm{KSV}, \qquad m\mathrm{G}_u = 0{\cdot}35\mathrm{KSV}a,$$

$$m\mathrm{X}_v = 1{\cdot}1\mathrm{KSV}, \qquad m\mathrm{G}_v = 0{\cdot}21\mathrm{KSV}a,$$

$$m\mathrm{X}_{\dot\theta} = -0{\cdot}1\mathrm{KSV}a, \qquad m\mathrm{G}_{\dot\theta} = 0{\cdot}08\mathrm{KSV}a.$$

Substituting in the expressions for A, B, etc., we have

$$\mathrm{A} = \tfrac{1}{2}a^2, \qquad \mathrm{B} = 27\, a^2/\mathrm{V},$$

$$\mathrm{C} = 15{\cdot}2a + 50\, a^2/\mathrm{V}^2, \qquad \mathrm{D} = 380\, a/\mathrm{V} + 240\, a/\mathrm{V} = 620\, a/\mathrm{V},$$

$$\mathrm{E} = 25600\, a^2/\mathrm{V}^2, \qquad \mathrm{H} = 5600\, a^3/\mathrm{V}^2 - 17400000\, a^5/\mathrm{V}^4.$$

The latter is positive when $\mathrm{V}^2 > 3100a$, which is therefore the condition of stability.

Ex. 3.—Let us now take an oblong plane lamina; the values of $f(\alpha)$

are given by Langley for a lamina of 30″ by 4·8″ the shorter side being in the plane of motion. In the absence of any information as to $\phi(\alpha)$, we shall assume (whether correctly or incorrectly) the same values as for square planes.

Let the moment of inertia be given, as before, by $k^2 = \frac{1}{2}a^2$.

For an angle of 10° we have

$$f(\alpha) = 0{\cdot}44, \qquad f'(\alpha) = 1{\cdot}5,$$
$$\phi(\alpha) = 0{\cdot}49, \qquad \phi'(\alpha) = 0{\cdot}59.$$

Substituting these values in the expressions for the coefficients, we have

$$m\mathrm{X}_u = 1{\cdot}6\mathrm{KSV}, \qquad m\mathrm{G}_u = 0{\cdot}26\mathrm{KSV}a,$$
$$m\mathrm{X}_v = 0{\cdot}6\mathrm{KSV}, \qquad m\mathrm{G}_v = -0{\cdot}4\mathrm{KSV}a,$$
$$m\mathrm{X}_{\dot\theta} = 0{\cdot}56\mathrm{KSV}a, \qquad m\mathrm{G}_{\dot\theta} = 0{\cdot}13\mathrm{KSV}a^2.$$

Substituting again in the expressions for A, B, C, D, and E, we have, assuming $k^2 = \frac{1}{2}a^2$,

$$\mathrm{A} = k^2 = \tfrac{1}{2}a^2,$$
$$\mathrm{B} = k^2\mathrm{X}_u + \mathrm{G}_{\dot\theta} = \frac{1}{m}\left(1{\cdot}6\frac{a^2}{2} + 0{\cdot}13a^2\right)\mathrm{KSV} = 60a^2/\mathrm{V},$$
$$\mathrm{C} = v\mathrm{G}_u - u\mathrm{G}_v - \mathrm{X}_{\dot\theta}\,\mathrm{G}_u = 19{\cdot}4a - 760a^2/\mathrm{V}^2,$$
$$\mathrm{D} = u(\mathrm{X}_v\mathrm{G}_u - \mathrm{X}_u\mathrm{G}_v) - g\mathrm{G}_v = 280a/\mathrm{V},$$
$$\mathrm{E} = g(\mathrm{X}_v\mathrm{G}_u - \mathrm{X}_u\mathrm{G}_v) = 35000a/\mathrm{V}^2.$$

Therefore

$$\mathrm{H} = 3{\cdot}2\,.\,10^5\frac{a^4}{\mathrm{V}^2} - 1{\cdot}2\,.\,10^7\frac{a^5}{\mathrm{V}^4} - 3{\cdot}9\,.\,10^4\frac{a^4}{\mathrm{V}^2} - 1{\cdot}2\,.\,10^8\frac{a^5}{\mathrm{V}^4}.$$

This is positive if $\mathrm{V}^2 > 470a$, which is therefore the condition of stability.

5. *Gliders Formed of two Planes.*

Proceeding now to consider the stability of systems made up of several planes rigidly connected together, we shall first consider the stability of a gliding system supported on two slats S_1 S_2, which are so narrow that displacements of their centre of pressures due to variations of the angles of incidence of the wind may be neglected.

We shall first suppose that the two slats are in the same plane, and that the centre of gravity is also in this plane.

Putting $\phi(\alpha) = 0$ and $\beta_1 = \beta_2 = 0$ in the expressions of § (3), we have all the Y's equal to zero and

$$\begin{aligned} m\mathrm{X}_u &= \mathrm{K}(\mathrm{S}_1+\mathrm{S}_2)(2uf(\alpha)+vf'(\alpha)), \quad \ldots\ldots\ldots\ldots\ldots \quad (9)\\ m\mathrm{X}_v &= \mathrm{K}(\mathrm{S}_1+\mathrm{S}_2)(2vf(\alpha)+uf'(\alpha)),\\ m\mathrm{X}_{\dot\theta} &= -\mathrm{K}(\mathrm{S}_1\eta_1+\mathrm{S}_2\eta_2)(2uf\alpha+vf'\alpha),\\ m\mathrm{G}_u &= -\mathrm{K}vf'(\alpha)(\mathrm{S}_1\eta_1+\mathrm{S}_2\eta_2),\\ m\mathrm{G}_v &= \mathrm{K}uf'(\alpha)(\mathrm{S}_1\eta_1+\mathrm{S}_2\eta_2),\\ m\mathrm{G}_{\dot\theta} &= \mathrm{K}vf'(\alpha)(\mathrm{S}_1\eta_1^2+\mathrm{S}_2\eta_2^2). \end{aligned}$$

Now for equilibrium we must have

$$\mathrm{S}_1\eta_1+\mathrm{S}_2\eta_2 = 0.$$

In this case, G_u, G_v, and $\mathrm{X}_{\dot\theta}$ vanish, and therefore, from (6), we see that the coefficients C, D, E, will be zero, and the equilibrum is critical or neutral.

[This result, which is also evident from first principles, holds good equally when the centre of gravity is not in the plane of the laminæ. In order to make the system really stable, the laminæ must be inclined at small angles to the line joining their centres.

Calling these angles β_1 and β_2, neglecting $\phi(\alpha)$, and writing $\alpha' = \alpha - \beta_1$, $\alpha'' = \alpha - \beta_2$, we get in the equations (8),

$$\begin{aligned} m\mathrm{X}_u &= \mathrm{K}\Sigma\mathrm{S}_1(2uf(\alpha')+vf'(\alpha'))\cos\beta_1,\\ m\mathrm{X}_v &= \mathrm{K}\Sigma\mathrm{S}_1(2vf(\alpha')-uf'(\alpha'))\cos\beta_1,\\ m\mathrm{X}_{\dot\theta} &= -\mathrm{K}\Sigma\mathrm{S}_1\eta_1(2uf(\alpha')+vf'(\alpha'))\cos\beta_1,\\ m\mathrm{Y}_u &= \mathrm{K}\Sigma\mathrm{S}_1(2uf(\alpha')+vf'(\alpha'))\sin\beta_1,\\ m\mathrm{Y}_v &= \mathrm{K}\Sigma\mathrm{S}_1(2vf(\alpha')-uf'(\alpha'))\sin\beta_1,\\ m\mathrm{Y}_{\dot\theta} &= -\mathrm{K}\Sigma\mathrm{S}_1\eta_1(2uf(\alpha')+vf'(\alpha'))\sin\beta_1,\\ m\mathrm{G}_u &= -\mathrm{K}v\Sigma\mathrm{S}_1p_1f'(\alpha'),\\ m\mathrm{G}_v &= \mathrm{K}u\Sigma\mathrm{S}_1p_1f'(\alpha'),\\ m\mathrm{G}_{\dot\theta} &= \mathrm{K}v\Sigma\mathrm{S}_1\eta_1p_1f'(\alpha'). \end{aligned}$$

The conditions for steady motion give

$$\begin{aligned} 0 &= k\mathrm{S}_1f(\alpha')\sin\beta_1+k\mathrm{S}_2f(\alpha'')\sin\beta_2,\\ mg/\mathrm{V}^2 &= k\mathrm{S}^1{}_1f(\alpha')\cos\beta_1+k\mathrm{S}_2f(\alpha'')\cos\beta_2,\\ 0 &= k\mathrm{S}_1f(\alpha')\eta_1\cos\beta+k\mathrm{S}_2f(\alpha'')\eta_2\cos\beta_2. \end{aligned}$$

With these substitutions, those of the nine coefficients $\mathrm{X}_u \ldots \mathrm{G}_{\dot\theta}$ which vanish in the limiting case of coplanar laminæ are given by

$$\frac{\mathrm{X}_{\dot\theta}}{v} = \frac{\mathrm{G}_u}{v} = -\frac{\mathrm{G}_v}{u} = -\frac{g}{\mathrm{V}^2}\frac{\eta_2\eta_1}{\eta_2-\eta_1}\left\{\frac{f'(\alpha')}{f(\alpha')} - \frac{f'(\alpha'')}{f(\alpha'')}\right\},$$

$$\frac{\mathrm{Y}_u}{v} = -\frac{\mathrm{Y}_v}{u} = \frac{g}{\mathrm{V}^2}\frac{\sin\beta_1\sin\beta_2}{\sin(\beta_2-\beta_1)}\left\{\frac{f'(\alpha')}{f(\alpha')} - \frac{f'(\alpha'')}{f(\alpha'')}\right\},$$

and an expression for $\mathrm{Y}_{\dot\theta}$, which is not required.

The remaining coefficients may for a first approximation be taken the same as in (9). We notice the following points.

(1) The expressions for $\mathrm{X}_{\dot\theta}$, Y_u, Y_v, G_u and G_v contain the factor $f'(\alpha')/f(\alpha'') - f'(\alpha'')/f(\alpha'')$. In the laws of resistance commonly assumed $f'(\alpha)/f(\alpha)$ decreases as α increases, and this coefficient, therefore, becomes negative if $\alpha' < \alpha''$, *i.e.*, if the two planes slope towards each other α' referring to the front plane. This is the case in a gliding machine furnished with a rudder, if the rudder is tilted slightly upwards. In such cases η_2 must be negative, hence $\mathrm{X}_{\dot\theta}$ and G_u are positive, Y_u and G_u are negative.

(2) If β_1 and β_2 are of the first order of small quantities, $\beta_1 - \beta_2$ also being of the first order, the expressions $\mathrm{X}_{\dot\theta}$, G_u and G_v are of the first order, but Y_u and Y_v are of the second order, and hence equations (6) give approximately the five coefficients A, B, C, D, E.

(3) The fifth stability condition $\mathrm{H} > 0$ appears at first sight difficult to reconcile with the smallness of the coefficients C, D, E for small inclinations of the planes, as $\mathrm{B}^2\mathrm{E}$ is of a lower order of small quantities than BCD. To satisfy this fifth condition, however, the important thing is to make E small compared with C. Now C will be found to consist of two parts, one independent of V and the other negative and proportional to $1/\mathrm{V}^2$, while E is proportional to $1/\mathrm{V}^2$, hence stability can best be secured by making V^2 sufficiently large.]

(4) If the planes are not infinitely narrow, it will be found that X_u, X_v, Y_u, Y_v are the same as before, and $-m\mathrm{G}_u/v$, and $m\mathrm{G}_v/u$ are both increased by $\Sigma\mathrm{KS}_1 a_1(f'(\alpha')\phi(\alpha') + f(\alpha')\phi'(\alpha'))$. This will have the effect of altering C, E, D in the same proportion, and will, therefore, not alter the stability, except when $\alpha = 0$, when the additional term will prevent $\mathrm{E}_1\mathrm{D}$ from becoming zero. $\mathrm{G}_{\dot\theta}$ is increased by the term $v\Sigma\mathrm{KS}_1 a_1\eta_1(f'(\alpha')\phi(\alpha') + f(\alpha')\phi'(\alpha))$. This term will, in general, be negative, and therefore $\mathrm{G}_{\dot\theta}$ will be diminished, which will have the effect of diminishing B.

6. *Examples of Two-Plane Gliders.*

We shall now assume certain particular values for the dimensions of gliders of this form, and proceed to calculate the conditions of stability.

Ex. 4.—Let us first consider the case of the two slats of equal area set at angles such that

$$\alpha_1 = 15^\circ, \qquad \alpha_2 = 5^\circ.$$

Then we have from Langley's results,

$$f(\alpha') = 0{\cdot}56, \qquad f'(\alpha') = 1,$$

$$f(\alpha'') = 0{\cdot}28, \qquad f'(\alpha'') = 2{\cdot}5\,;$$

for equilibrium

$$\alpha' f(\alpha'') = -\eta_2 f(\eta_1)\,;$$

therefore, if we put $\eta_1 = a$, we have $\eta_2 = 2a$. Let k^2 be assumed $= 2\alpha^2$. Substituting in the expressions for X_u, etc., we have

$$mX_u = 3{\cdot}6\text{KSV}, \qquad mG_u = 4\text{KSV}a,$$

$$mX_v = 1\text{KSV}, \qquad mG_v = 0{\cdot}7\text{KSV}a,$$

$$mX_{\dot\theta} = 4\text{KSV}a, \qquad mG_{\dot\theta} = 11\text{KSV}a^2,$$

and therefore,

$$A = 2a^2, \qquad B = 680\,\frac{a^2}{V}, \qquad C = 156a - 22800a^2/V^2,$$

$$D = 2721\,\frac{a}{V}, \qquad E = 297000\,\frac{a}{V^2}, \qquad H = 2.7\,.\,10^8\,\frac{a^4}{V^2} - 1{\cdot}4\,.\,10^7\,\frac{a^4}{V^2}$$

$$- 2{\cdot}9\,.\,10^{10}\,\frac{a^3}{V^4} - 1{\cdot}3\,.\,10^{11}\,\frac{a^3}{V^4}.$$

This is positive if $V^2 > 590a$.

Ex. 5.—Let us now take the case of two equal square planes inclined at a small angle to each other.

Let $$\alpha = 10^\circ, \qquad \beta_1 = -5^\circ, \qquad \beta_2 = 5^\circ,$$

and let $2a$ be the breadth of either plane.

For these angles we have

$$f(\alpha_1-\beta_1) = 0{\cdot}44, \qquad f'(\alpha_1-\beta_1) = 1{\cdot}7,$$

$$f(\alpha_2-\beta_2) = 0{\cdot}15, \qquad f'(\alpha_2-\beta_2) = 1{\cdot}8,$$

$$\phi(\alpha_1-\beta_1) = 0{\cdot}45, \qquad \phi'(\alpha_1-\beta_1) = 0{\cdot}6,$$

$$\phi(\alpha_2-\beta_2) = 0{\cdot}55, \qquad \phi'(\alpha_1-\beta_2) = 0{\cdot}58.$$

For equilibrium we must have

$$f(\alpha_1-\beta_1)\,\{\eta_1 + a\phi(\alpha_1-\beta_1)\} = f(\alpha_2-\beta_2)\,\{\eta_2 + a\phi(\alpha_2-\beta_2)\},$$

whence

$$3\eta_1 + \eta_2 = 0{\cdot}28a\,;$$

therefore, if we put $\eta_2 = -4a$, we have $\eta_1 = 1{\cdot}4a$. Let k^2 be assumed to be $= 4a^2$. Substituting these values in the expressions for the coefficients, we have

$$mX_u = 3{\cdot}6\text{KSV}, \qquad mY_u = 0{\cdot}02\text{KSV},$$
$$mX_v = 0{\cdot}58\text{KSV}, \qquad mY_v = -0{\cdot}1\text{KSV},$$
$$mX_{\dot\theta} = 1{\cdot}8\text{KSV}a, \qquad mY_{\dot\theta} = 0{\cdot}00\text{KSV},$$
$$mG_u = 2{\cdot}61\text{KSV}a, \qquad mG_v = 0{\cdot}44\text{KSV}a,$$
$$mG_{\dot\theta} = 30\text{KSV}a^2.$$

Substituting again in the expressions for A, B, C, D, E, we obtain, putting $k^2 = 4a^2$,

$$A = 4a^2, \qquad B = 2390\frac{a^2}{V}, \qquad C = 142a - 13700\frac{a^2}{V^2},$$
$$D = 2500\frac{a}{V}, \qquad E = 284000\frac{a}{V^2}.$$

H is positive if $V^2 > 2000a$, which is the condition of stability for this form of glider.

By putting $l = 7{\cdot}4a$, l represents approximately the extreme length of the glider, and the condition of stability reduces to $V^2 > 270l$.

Ex. 6.—Consider next the case of two unequal square planes, inclined at an angle, and in the first case suppose that the smaller plane is in front.

Let S and S′ be the respective areas, and suppose $S = 10S'$.

Let $2a$ be the breadth of the large plane, and $2a/3{\cdot}1$ that of the smaller, also let the distance between the centres be equal to $3a$; and let $k^2 = a^2$.

Let the angle α between the large plane and the direction of motion be 10°, and let the angle between the two planes be also 10°, so that the small plane is inclined at an angle of 20° to the direction of motion.

Then we have

$$f(\alpha) = 0{\cdot}3, \quad f'(\alpha) = 1{\cdot}6, \quad \phi(\alpha) = 0{\cdot}49, \quad \phi'(\alpha) = 0{\cdot}59,$$
$$f(\alpha+\beta) = 0{\cdot}5, \quad f'(\alpha+\beta) = 1{\cdot}57, \quad \phi(\alpha+\beta) = 0{\cdot}4, \quad \phi'(\alpha+\beta) = 0{\cdot}56.$$

For equilibrium we have, if η be the distance between c.g. and centre of large plane,

$$Sf(\alpha)(\eta - a\phi(\alpha)) = S'f(\alpha+\beta)(b + a'\phi(\alpha+\beta) - \eta).$$

Therefore $\eta = 0{\cdot}94a$, so that $\eta_1 = b - \eta = 2{\cdot}06a$.

Substituting in the expressions for X_u, etc.,

$$mX_u = 1{\cdot}87\text{ KSV}, \quad mX_v = 0{\cdot}41\text{ KSV}, \quad mX_{\dot\theta} = 1{\cdot}4\text{ KSV}a,$$
$$mG_u = 0{\cdot}57\text{ KSV}a, \quad mG_v = 0{\cdot}097\text{ KSV}a, \quad mG_{\dot\theta} = 1{\cdot}13\text{ KSV}a^2.$$

Substituting these values in the expressions for A, B, etc., assuming that $k^2 = a^2$, we have

$$A = a^2, \qquad B = 273\frac{a^2}{V}, \qquad C = 50{\cdot}8a - 15050\frac{a^2}{V^2},$$

$$D = 917\frac{a}{V}, \qquad E = 120000\frac{a}{V^2}, \qquad H = 118\,.\,10^5\frac{a^4}{V^2} - 123\,.\,10^8\frac{a^5}{V^4}.$$

This will be positive if $V^2 > 1040a$. This condition will also make C positive, and is therefore the condition of stability.

Ex. 7.—Let us now suppose the small plane to be placed behind the other

being inclined to it at an angle of 5°, the direction of motion again making an angle of 10° with the large plane and k^2 being $= a^2$, as in Ex. 6.

In this case we have

$$f(\alpha) = 0{\cdot}3, \quad f'(\alpha) = 1{\cdot}6, \quad \phi(\alpha) = 0{\cdot}49, \quad \phi'(\alpha) = 0{\cdot}59,$$
$$f(\alpha') = 0{\cdot}17, \quad f'(\alpha') = 1{\cdot}8, \quad \phi(\alpha') = 0{\cdot}55, \quad \phi'(\alpha') = 0{\cdot}6.$$

If the distance between the centres of the planes be $3a$, the conditions of equilibrium give

$$\eta = -0{\cdot}35a, \qquad \eta_1 = -2{\cdot}65a.$$

Substituting, we obtain

$$\begin{aligned} mX_u &= 1{\cdot}9\,\mathrm{KSV}a, & mG_u &= 0{\cdot}33\,\mathrm{KSV}a, \\ mX_v &= 0{\cdot}41\,\mathrm{KSV}a, & mG_v &= 0{\cdot}056\,\mathrm{KSV}a, \\ mX &= 0{\cdot}65\,\mathrm{KSV}a, & mG_{\dot\theta} &= 1{\cdot}32\,\mathrm{KSV}a^2. \end{aligned}$$

$$\therefore\ A = a^2, \qquad B = 233\frac{a^2}{V}, \qquad C = 34{\cdot}8a - 2400\frac{a^2}{V^2},$$

$$D = 1780\frac{a}{V}, \qquad E = 80000\frac{a}{V^2}, \qquad H = 12\,.\,10^6\frac{a^4}{V^2} - 3\,.\,10^9\frac{a^5}{V^4}.$$

H will be positive if $V^2 > 250a$.

The condition of stability is therefore that $V > \sqrt{(250a)}$.

[7. *Effect of Moment of Inertia on Stability.*

It will be seen from (5) that the radius of gyration, k^2, occurs only in the expressions for A, B, the first two coefficients of the determinantal equation. We have $A = k^2$, $B = k^2(X_u + Y_v) + G_{\dot\theta}$. In all the cases considered $X_u + Y_v$ and $G_{\dot\theta}$ are positive, and therefore these two coefficients are positive for all values of k^2.

Taking the expression

$$H = BCD - AD^2 - EB^2,$$

it may be written

$$CDG_{\dot\theta} - EG_{\dot\theta}^2 + k^2\{(X_u + Y_v)\,CD - D^2 - 2\,(X_u + Y_v)G_{\dot\theta}E\} - k^4(X_u + Y_v)^2E.$$

H is therefore increased with increase of k^2 if

$$(X_u + Y_v)\,(CD - 2G_{\dot\theta}E) - D^2 - 2k^2\,(X_u + Y_v)\,E > 0.$$

At the critical velocity, if $H = 0$, this becomes

$$CDG_{\dot\theta} + (X_u + Y_v)^2Ek^4 < EG_{\dot\theta}^2.$$

Unless this condition is fulfilled, the critical velocity given by $H = 0$ increases with increase of k^2.

In all the numerical examples considered above, $CDG_{\dot\theta} > EG_{\dot\theta}^2$ and E is positive, so that the critical velocity is increased by increasing k^2. Thus in Ex. (1) k^2 was taken equal to $\frac{1}{2}a^2$, and the critical velocity obtained was $\surd(774a)$; if, instead, we had taken $k^2 = a^2$, the critical velocity would have been $\surd(1240a)$.—*Jan.*, 1904.]

8. *Character of the Fluctuations about Steady Motion. Mode in which the System Overturns.*

The character of the fluctuations about steady motion depends on the nature of the roots of the biquadratic (4*a*), the expressions for the displacements δu, δv, $\delta\theta$ being evidently of the following forms:—

For roots all real .... $c_1e^{\lambda_1 t} + c_2e^{\lambda_2 t} + c_3e^{\lambda_3 t} + c_4e^{\lambda_4 t}$.
For roots two real, two imaginary ... $c_1e^{\lambda_1 t} + c_2e^{\lambda_2 t} + \gamma e^{\alpha t}\cos(\beta t - \epsilon)$.
For two pairs of imaginary roots ...... $\gamma_1e^{\alpha_1 t}\cos(\beta_1 t - \epsilon_1) + \gamma_2e^{\alpha_2 t}\cos(\beta_2 t - \epsilon_2)$.

The last form indicates two different sets of undulations of different lengths. Photographs of the paths of gliders taken by magnesium light distinctly show these two undulations, thus confirming our theory.

An important further consequence is that a glider may perform undulations decreasing in amplitude, corresponding to a pair of complex roots of the biquadratic with their real part negative, but the motion may be unstable through the other roots having their real part positive, or one or both of them being real and positive. This indicates a real danger in experimenting with gliders.

Stability may be broken either if a real root of the equation (4*a*) changes from negative to positive, or if the real part of a pair of imaginary roots changes from negative to positive.

The condition for the latter is that $H = 0$, whereas a real root changes sign if $E = 0$. If stability is broken by a fall of velocity the only quantities which can vanish are H and C, of these H is more difficult to make positive than C, and hence it appears that the most likely way for a glider to overturn in general is by commencing with a series of oscillations of increasing amplitude. This again agrees with experiments.

9. *Conclusions.*

1. For a glider or other body moving in a vertical plane in a resisting medium of any kind whatever, the small oscillations about a state of uniform rectilinear motion are determined by an equation of the fourth degree, so that the conditions for stable steady motion are those obtained by Routh.

2. The coefficients in the period equation involve, in addition to the ordinary dynamical constants, nine quantities $X_u \ldots G_{\dot{\theta}}$, which, when referred to rectangular axes fixed in the body, represent the differential coefficients of the forces and couple due to the aerial resistances with respect to its translatory and rotatory velocity components.

3. In the case of a system of aeroplanes these nine quantities can be expressed for the separate planes in terms of $f'(\alpha)$ and $\phi'(\alpha)$, where $f(\alpha)$ and $\phi(\alpha)$ are functions determining the resultant thrust, and the position of the centre of pressure when the direction of the relative motion of the air makes an angle α with the plane. These functions have been tabulated for certain different forms of surfaces, but further data are greatly needed.

4. *The longitudinal stability of the gliders is thus seen to be capable of mathematical investigation*, and it is of paramount importance that the present methods should be practically applied to any aerial machines that may be designed or constructed before any actual glides are attempted.

5. The methods of calculation are exemplified by numerical determinations of the criterion of stability in the cases of a single plane lamina and a pair of planes one behind the other. Most of the calculations have been performed for an angle of gliding of 10° with the horizon, and it has been necessary to assume arbitrary values for the moment of inertia of the lamina.

6. The condition that any steady linear motion may be stable in all these cases assumes the form $V^2 > ka$, where a is a constant depending on the linear dimensions of the glider, and k is a constant depending on its shape, the angle of gliding and the law of aerial resistance.

7. For a pair of narrow slats, in which the variations in the positions of the centres of pressure of each are neglected, certain coefficients of stability vanish. If the planes are square so that the displacements of the centres of pressure are not neglected, the system is less stable than a single plane of breadth equal to one of the squares.

8. By inclining the planes at a small angle to each other the stability

is much increased. On the other hand, if they are made to slope away from each other the glider becomes unstable.

9. Two square planes of equal size placed one behind the other at a small angle are less stable in the examples considered than a square equal in size to one of the two, but more stable than a single square whose side is equal to the total length of the glider formed by the pair.

10. A pair of unequal squares of which the smaller forms a rudder are more stable, in the examples considered, when gliding with the rudder behind than with the rudder in front.

11. In general, the stability is increased by making the moment of inertia as small as possible.

"Cultural Experiments with 'Biologic Forms' of the *Erysiphaceæ*." By ERNEST S. SALMON, F.L.S. Communicated by Professor H. MARSHALL WARD, F.R.S. Received December 2, 1903,—Read February 4, 1904.

(Abstract.)

In the introductory remarks the author points out that through specialisation of parasitism "biologic forms" have been evolved in the *Erysiphaceæ* which, both in their conidial (asexual) stage and ascigerous (sexual) stage, show specialised and restricted powers of infection. The powers of infection, characteristic of each "biologic form," are under normal conditions sharply defined and fixed, and hitherto the result of the experiments of numerous investigators—both in regard to the present group of fungi and to the *Uredineæ*, where the same specialisation of parasitism occurs—has been the accumulation of evidence tending to emphasise the immutability of "biologic forms."

The second part of the paper gives the result of cultural experiments with "biologic forms" of *Erysiphe Graminis* DC., carried out during the past summer in the Cambridge University Botanical Laboratory. It has been found that *under certain methods of culture, in which the vitality of the host-leaf is interfered with, the restricted powers of infection, characteristic of "biologic forms," break down.*

In the first method of culture adopted, the leaf, which was either attached to a growing plant, or removed and placed in a damp chamber, was injured by the removal of a minute piece of leaf-tissue. In this operation the epidermal cells on one surface, and all or most of the mesophyll tissue, were removed at the cut place, but the epidermal cells on the other surface (opposite the cut) were left uninjured. Conidia were sown on the cuticular surface of the uninjured epidermal cells over the cut. In a few experiments the conidia were

sown on the internal tissues of the leaf exposed by the cut, and these gave the same results.

Using this method of culture, over fifty successful experiments, of which details are given, were made. In these the conidia of certain "biologic forms" were induced to infect "cut" leaves of host-species which are normally immune against their attacks.

The experiments proved that the range of infection of a "biologic form" becomes increased when the vitality of a leaf is affected by injury, and also that species of plants "immune" in nature can be artificially rendered susceptible.

Further experiments showed that *the conidia of the fungus produced on a "cut" leaf are able at once to infect fully uninjured leaves of the same host-species.*

In other experiments, a method suggested by Professor H. Marshall Ward, with the object of avoiding lesion of the leaf, was adopted. Leaves were injured by touching the upper epidermis for a few seconds with a red-hot knife, and conidia were sown on the injured place. It was found that the cells immediately surrounding the place of injury were rendered susceptible to the attacks of a "biologic form" which is unable to attack uninjured leaves of the plant in question.

In the third part of the paper, dealing with general considerations, the following hypothesis is advanced as to the actual manner in which the injury to a leaf causes it to become susceptible to a "biologic form" otherwise unable to infect it. It is supposed that the leaf-cells of each species of host-plant contain a substance or substances—possibly an enzyme—peculiar to each species which, when the leaf is uninjured and the cells are vigorous, are able to prevent the successful attack of any mildew except the *one* "biologic form" which has become specialised to overcome the resistance. When the vitality of the leaf, however, becomes affected by injury, this substance is destroyed, or becomes weakened, in the leaf-cells in the neighbourhood of the injury, so that the conidia of *other* "biologic forms" are now able to infect them.

The author suggests that injuries to leaves, caused in nature by hail, storms of wind, attacks of animals, etc., may produce the same effect as the artificial injuries described above in rendering the injured leaf susceptible to a fungus otherwise unable to infect it. Conidia produced on these injured places would be able to infect uninjured leaves, and would spread indefinitely. Such may be the explanation of a common phenomenon—the sudden appearance of disease caused by parasitic fungi on plants hitherto immune.

A case is described which, it is believed, gives evidence that the injuries produced by *Aphides* caused leaves previously "immune" to become susceptible.

In the concluding remarks, reference is made to the antagonistic forces concerned in the evolution of a "biologic form," viz., "specialising factors" and "generalising factors."

Attention is also drawn to the close parallel between (1) the behaviour of the fungus in the experiments in which the conidia were sown on the tissues of the leaf exposed by the cut; and (2) the biological facts obtaining in the class of parasitic fungi known as "wound parasites" (*Nectria*, *Peziza willkommii*, etc.), which are able to infect their hosts only through a wound.

"On the Origin of Parasitism in Fungi." By GEORGE MASSEE, Principal Assistant, Herbarium, Royal Gardens, Kew. Communicated by Sir WILLIAM T. THISELTON-DYER, K.C.M.G., C.I.E., F.R.S. Received January 11,—Read February 4, 1904.

(Abstract.)

Up to the present no definite explanation has been offered as to why a given parasitic fungus is often only capable of infecting one particular species of plant. This, however, is well known to be the case, for although the spores of fungus parasites germinate freely on the surface of any plant when moist, infection only takes place when the spores germinate on the particular species of plant on which the fungus is known to be parasitic. This apparently selective power on the part of the fungus I consider to be due to chemotaxis.

An extensive series of experiments were conducted with various species of fungi, including Saprophytes, facultative parasites, and obligate parasites, and the results are given in tabulated form in the full paper. The chemotactic properties of substances occurring normally in cell-sap were alone tested; among such may be enumerated saccharose, glucose, asparagin, malic acid, oxalic acid, and pectase. In those instances where the specific substance, or combination of substances, in the cell-sap assumed to be chemotactic could not be procured, the expressed juice of the plant was used.

These experiments proved that saprophytes and facultative parasites are positively chemotactic to saccharose, and this substance alone is sufficient in most instances to enable the germ-tubes of facultative parasites to penetrate the tissues of a plant, unless prevented by the presence of a more potent negatively chemotactic or repellent substance in the cell-sap.

As an illustration, *Botrytis cinerea*, which attacks a greater number of different plants than any other known parasite, cannot infect apples, although saccharose is present, on account of the presence of malic acid, which is negatively chemotactic to the germ-tubes of *Botrytis*.

In the case of obligate parasites the cell-sap of the host-plant proved to be the most marked positive chemotatic agent. Malic acid is the specific substance that attracts the germ-tubes of *Monilia fructigena* into the tissues of young apples; whereas the enzyme pectase performs the same function for the germ-tubes of *Cercospora cucumis*, an obligate parasite on the cucumber.

Immune specimens of plants belonging to species that are attacked by some obligate parasite owe their immunity to the absence of the substance chemotactic to the parasite.

Purely saprophytic fungi can be educated to become parasitic, by sowing the spores on living leaves that have been injected with a substance positively chemotactic to the germ-tubes of the fungus experimented with. By a similar method of procedure, a parasitic fungus can be induced to attack a different species of host-plant.

These experiments prove what has previously only been assumed, namely, that parasitism in fungi is an acquired habit.

A series of experiments prove that infection of plants by fungi occurs more especially during the night, or in dull, damp weather. This is due to the greater turgidity of the cells, and also to the presence of a larger amount of sugar and other chemotactic substances present in the cell-sap under those conditions.

"A New Method of Detecting Electrical Oscillations." By J. A. Ewing, LL.D., F.R.S., and L. H. Walter, M.A. Received January 11,—Read February 11, 1904.

The magnetic detector of Rutherford,* though now well known, appears to have created little interest until attention was directed to the subject by Marconi's adaptation of the method to his telephonic detector. Marconi's apparatus employs the change of hysteresis, which is produced in iron by the influence of electric oscillations, when these are caused to pass through a coil surrounding the iron, the change being made manifest by means of a telephone. In his view, the electric oscillations act by reducing the hysteresis.†

It occurred to us to exhibit the alteration in hysteresis by a different method, namely, by applying the principle which is used in an instrument invented some years ago by one of us for the mechanical measurement of hysteresis. In that instrument‡ the hysteresis is measured by the mechanical couple between a magnetic field and the iron, when either the iron or the magnet providing the field is caused to revolve. Thus, if the field revolves, the iron tends to be dragged after it, as a consequence of hysteresis in the reversals of its magnetism, and if the motion is prevented by a spring or other control, it assumes a deflected position. Suppose, now, the electric oscillations to act on it, any change of the hysteresis caused by them will be exhibited by a corresponding change in the deflection. We anticipated, in accordance with the generally accepted view that hysteresis is reduced by the oscillations, that their presence would be detected by a fall in the deflection.

With this expectation an experimental apparatus was arranged, consisting of an electro-magnet, capable of being rotated on a vertical axis by an electric motor. The magnet poles were bored out circular, and between them was suspended, by a phosphor-bronze strip, a ring made up of three thin, flat annuli of soft iron, clamped together, and provided at the foot with an axial pivot. The ring was free to turn inside of two bobbins wound with fine copper wire, the windings being at right angles to the plane of the ring. Through these copper windings, electrical oscillations, produced in the usual manner by means of a distinct spark-gap, were passed.

The first experiments resulted in a very small deflection from the position due to normal hysteresis, indicating, as was expected, a decrease of hysteresis when the oscillations arrived. The apparatus

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* 'Phil. Trans., A, vol. 189, p. 1, 1897.

† 'Roy. Soc. Proc.,' vol. 70, pp. 341—344, 1902.

‡ 'Journ. Inst. Electr. Engineers,' vol. 24, pp. 398—430, 1895.

was also tested with an alternating current of about 100 periods per second in place of the oscillations, with the effect that the normal hysteresis deflection was almost entirely wiped out.

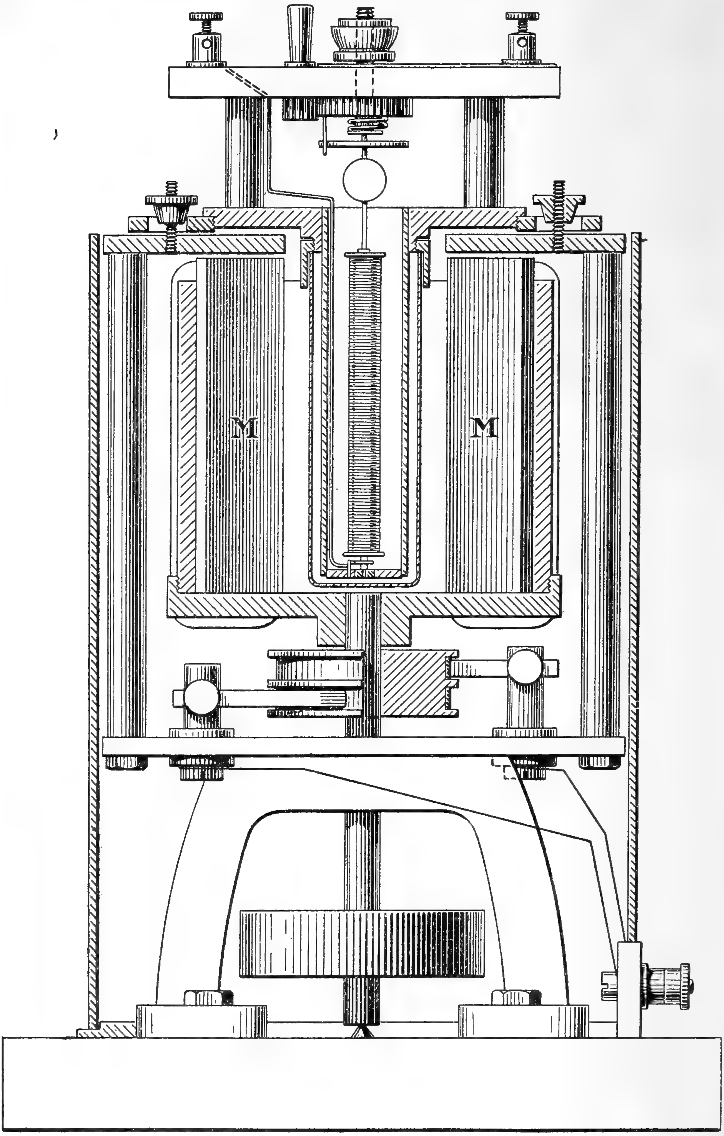
Various other forms were also tried with indifferent results, when it occurred to us that there would be advantages in passing the oscillations through the magnetic material itself, making it of magnetic wire. A small bobbin was therefore wound with insulated soft iron wire, and the ends soldered to the upper and lower halves of the spindle, which was itself divided at the centre, the upper half bearing the controlling spring, and the lower dipping into mercury, from which a connection led to the other terminal. On passing oscillations through this winding, a remarkable and unexpected result was obtained. The change of deflection was much more marked than in the former experiments, and was in the opposite sense, indicating an increase of hysteresis while oscillations were present. Afterwards, hard steel wire was substituted for the soft iron, and a very great increase in the effect was observed, still in the same direction—that of increase of hysteresis.

Owing to these encouraging results, it was decided to continue the experiments in this direction, abandoning the older form, in which a decrease of hysteresis was dealt with. The first bobbin constructed was about $\frac{5}{16}$ inch in external diameter, and had a vertical wire space of $\frac{1}{4}$ inch. The winding was a single No. 32-gauge iron wire, double cotton-covered, wound straight round from beginning to end. Later, No. 40 and No. 46 steel wires were employed, of which the latter gave the best results.

It was soon noticed that any method of increasing the oscillatory current in the wires, as by winding the bobbin with two wires having a slightly unequal number of turns, was of advantage in giving a larger deflection. Later a fine copper wire secondary, wound on the bobbin parallel to the magnetic wire, was tried, first with the ends insulated, and then with the ends soldered together. A marked increase in deflection was observed when the secondary was closed, showing that the magnetic nature of the wire itself was influential. Accordingly, a bobbin was then wound with insulated steel wire, doubled back on itself. This non-inductive winding gave by far the best results hitherto attained, and is now used, except when special results are required.

The instrument, though described as a detector of electrical oscillations, may be said to measure rather than detect, giving quantitative as well as qualitative results, and being capable of regulation from a sensibility of the same order as that of an average coherer down to practical insensibility to powerful sparks in the same room.

In the instrument, as shown in the figure, the electro-magnet takes the form of a ring capable of moving round a vertical axis, and is provided on the interior with two long wedge-shaped pole-pieces, M, M, the current to the winding being supplied through brushes



bearing against insulated rings below. The magnet is made to revolve by an electro-motor, the best speed being about five to eight revolutions per second, but the electro-magnet may be replaced by a permanent magnet system giving a similar field. A structure is built up, external to the magnet, to support the vessel containing the pivoted bobbin and its centreing arrangements. The bobbin itself is made of bone, and is about 2 inches long. It is provided with a steel spindle at each end bearing in a jewel hole, the two halves of the spindle being insulated from one another. The winding, which is, as far as possible, non-inductive, consists of about 500 turns of No. 46-gauge hard-drawn steel wire, insulated with silk. The bobbin is immersed in petroleum, or a mixture of petroleum with thicker mineral oil, which serves the double purpose of fortifying the insulation, and giving the damping effect necessary to steady the deflection due to the drag of the revolving magnet. Readings are taken by means of a spot of light, as with speaking mirror galvanometers, but a siphon-recording attachment has been fitted, and any form of contact for working a relay could be employed.

The detector, as before mentioned, gives quantitative readings, and, in some cases, the deflection may be too large to be easily read by the scale. For this purpose a variable shunt is provided, by which the deflection can be regulated.

For the purpose of wireless telegraphy, the instrument has the advantage of giving metrical effects. The benefit of this in facilitating tuning, and in other respects, need not be insisted upon.

From the physical point of view, the augmentation of hysteresis is interesting and unlooked for. It is probably to be ascribed to this, that the oscillatory circular magnetisation facilitates the longitudinal magnetising process, enabling the steel to take up a much larger magnetisation at each reversal than it would otherwise take, and thus indirectly augmenting the hysteresis to such an extent that the direct influence of the oscillations in reducing it is overpowered. The net result appears to be dependent on two antagonistic influences, and, in fine steel wire, under the conditions of our experiments, the influence making for increased hysteresis, as a result of the increased range of magnetic induction, is much the more powerful.

"Constant-Standard Silver Trial-Plates." By EDWARD MATTHEY, C.B., F.S.A., F.C.S., Assoc. Roy. Sch. Mines. Communicated by Sir WILLIAM CROOKES, F.R.S. Received January 11,—Read February 11, 1904.

Referring to my paper communicated to the Royal Society, February 16, 1894, and read March 15, 1894,* in which it was shown that moderately sized plates of sterling silver of a uniform standard could be obtained by casting from thin castings, my attention since then was drawn to the difficulty of casting larger quantities than those described in that paper, which were only of an average weight of 4 to 5 kilogrammes per plate, and to the desirability of obtaining a large plate, say of some 8 or 10 kilogrammes in weight, without difficulty, and I have therefore resumed my attention towards effecting this. It appears that considerable difficulties have been experienced with regard to obtaining large plates of constant standard.

In the Royal Mint Report of 1873 is a memorandum appended by Professor W. Chandler Roberts, which refers to a series of well-known experiments with regard to obtaining a constant alloy of 0·900 silver by Levol in the Paris Mint, he himself being at the time engaged in the preparation of a standard silver trial-plate. Professor Chandler Roberts states:—"From the foregoing remarks it will be evident *that it is impossible to cast a standard silver plate or bar of uniform composition*, and it was necessary therefore to resort to an artifice in order to obtain a standard trial-plate of the required dimensions."†

The means adopted by him were to cast 1000 ozs. of standard silver into a skillet-mould 30 cm. long, 25 cm. wide and 5 cm. broad, to plane off 4 mm. from its surface and to roll the planed skillet to 1·8 mm. thickness, a sheet being produced 1·5 m. long, 45 cm. wide. From this the portion was cut which showed constant results about 925, but which varied from 924·6—925·1; and the rest of the plate which varied from 924 (lowest) to 928·4 (highest) was abandoned. He shows all these results by diagrams accompanying his memoranda. The portion of available constant standard so cut out from the 1000 oz. sheet weighed 104 oz., about one-tenth of the whole plate. And this 104 oz. (= 3·230 kilogrammes) formed the mint trial plate from a mass of 1000 oz. (= 31·103 kilogrammes) specially cast for the purpose.

Notwithstanding that many experiments were subsequently made by Professor Roberts Austen to find a means of obtaining a constant standard trial-plate, in 1899 he was compelled to resort to what he calls the "cumbrous expedient" of 1873. His statement is:—"None

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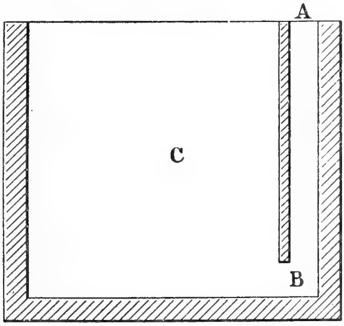
* 'Roy. Soc. Proc.,' vol. 55, 1894, p. 265.

† Fourth Annual Report, Deputy Master of Mint, 1873, pp. 44—46.

of the results were satisfactory, and eventually, after no less than 31 plates had been cast without success, recourse was had to the method adopted in 1873, of casting a large mass of metal and detaching a particular portion which proved by assay to be of approximately uniform standard."*

The casting of the standard silver into thin instead of into the thick moulds ordinarily employed having been attended with such excellent results,† I was induced to believe that it must be due to the more rapid cooling of the metal, by which liquation was arrested; so that if the standard silver were cast into moulds sufficiently cooled, liquation might be induced to disappear altogether. In order then to overcome the difficulty of obtaining the standard trial-plates in larger sizes, I commenced by casting quantities of not less than 8 kilogrammes into a mould cooled externally by ice, and also by freezing mixtures as low as 10° C., and by these means I obtained most encouraging results—results which confirmed me in the supposition that by cooling with rapidity there is less time for liquation, which appears to be the direct converse of what has been supposed hitherto, viz.: "that a uniformity of standard was best attained by slow and uniform cooling."‡ But although I thus obtained exceedingly good results (two of these are subjoined, see p. 126), I was not satisfied that this was the *best* way of producing a constant-standard plate.

I therefore adopted a different method of casting the standard silver. Instead of pouring the melted alloy into a mould from the top, I poured it into a mould by which the skillet was produced from the bottom, thus:—



Rough Section of Cast-iron Mould employed.

By pouring the alloy into the gate A the metal passes by B into the space C. And instead of cooling the mould by ice or freezing mixture I used the mould simply cold. I have obtained excellent results by

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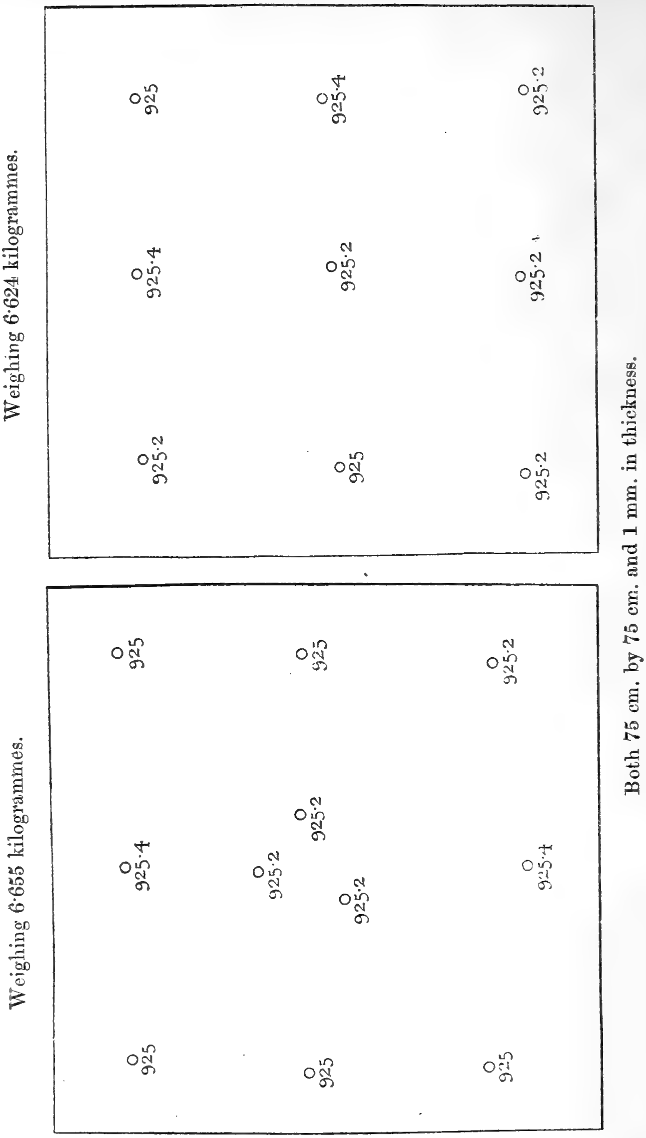
* Thirtieth Annual Report, Deputy Master of Mint, 1899, pp. 69, 70.

† *Vide* my paper of February 16, 1894, before referred to.

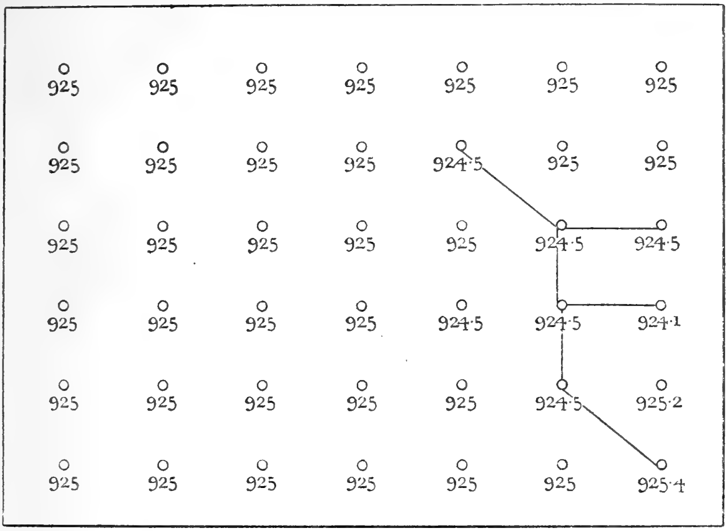
‡ *Vide* memorandum already referred to in Mint Report, 1873.



Both 75 cm. by 75 cm. and 1 mm. in thickness.

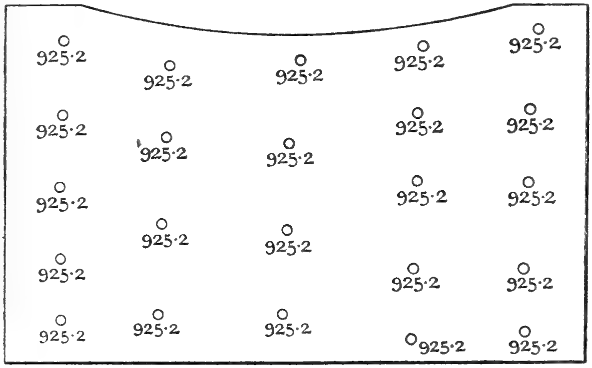
this method. Subjoined is one of the plates produced rolled to 1 mm. thick, measuring 90 cm. × 75 cm. and weighing 8·700 kilogrammes. Trimming the rough edges from the plate about 1 cm., the results came out as under, showing a constancy of 925 nearly all over the plate. I have drawn a line where the only wave of lower variation occurs.

Weighing 7·870 kilogrammes.



75 cm. by 90 cm. and 1 mm. in thickness.

Another cast simply into a cold mould, and rolled to a thickness of 1 mm., gave—



This only weighed 3·640 kilogrammes, is without any wave of variation, and is absolutely constant.

The hitherto accepted theory "that the molecular rearrangement is comparatively slight if the mass of metal is slowly and uniformly solidified"* is contradicted by the results I have obtained; and the results all bear out the fact that there is little or no difficulty in obtaining an 8 or 10-kilogramme plate of constant-standard silver, or even more if necessary.

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* Fourth Annual Report, Deputy Master of Mint, 1873, p. 46.

"Further Observations on the *Rôle* of the Blood Fluids in connection with Phagocytosis."

By A. E. WRIGHT, M.D., late Professor of Pathology, Army Medical School, Netley, Pathologist to St. Mary's Hospital, W., and STEWART R. DOUGLAS, M.R.C.S., Captain, Indian Medical Service.

Communicated by Sir J. BURDON SANDERSON, F.R.S. Received January 11,—Read February 25, 1904.

(From the Pathological Laboratory of St. Mary's Hospital, London, W.)

[PLATE 3.]

In a previous communication we showed that the phagocytosis which occurs when cultures of the Staphylococcus pyogenes are added to human blood, is directly dependent upon the presence of certain substances in the blood which exert a specific effect upon the bacteria. We suggested that the bacteriotropic substances here in question might appropriately be denoted by the term "opsonins."

In the present paper we propose to bring out certain further points in connection with the "opsonic power" of the blood.

RELATION OF THE OPSONIC POWER OF HUMAN BLOOD TO THE CAPACITY OF RESISTING INVASION BY THE STAPHYLOCOCCUS PYOGENES.

It has already been shown* by one of us that patients who are the subjects of acne, sycosis, or boils are characterised by a defective phagocytic power for the Staphylococcus pyogenes. We have recently been able to satisfy ourselves that this defective phagocytosis is dependent upon a defect of opsonic power.

It has also been shown by one of us that the cure of these bacterial infections, which can in almost every instance be achieved by the inoculation of appropriate quantities of sterilised staphylococcus cultures, is associated with the acquirement of an increased phagocytic power. We have now succeeded in establishing the fact—already adumbrated in our previous paper—that the increased phagocytosis which is associated with the achievement of the condition of immunisation here in question is dependent, not upon a modification of the white corpuscles, but upon a development of opsonins in the blood fluids.

The results of the subjoined experiment bring out this fact into clear relief.

Details of the Experiment.

Immunised Patient's Blood.—The patient, F. F., who had long been the subject of aggravated staphylococcic sycosis, had, after prolonged and

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* 'Lancet,' March 29, 1902.

ineffectual treatment with antiseptics, been subjected to three successive inoculations of a sterilised staphylococcus culture. Under these inoculations his clinical condition had ameliorated itself in an astonishing manner, and his phagocytic power, which had previous to the date of inoculation been less by half than that of the normal man who served as a control, had increased in a progressive manner after each inoculation.

A sample of blood was now (by the technique elsewhere described)* drawn off and mixed with $\frac{1}{10}$th of its volume of 10 per cent. citrate of soda. A second sample of blood was drawn off and allowed to clot in the ordinary way.

In the case of the first sample of blood the corpuscles were isolated from the plasma by repeated washing with physiological salt solution, and centrifugalisation. The corpuscles thus isolated are referred to below as "washed corpuscles."

In the case of the second sample of blood the serum was simply separated from the corpuscles in the ordinary way by centrifugalisation.

Control Blood from a Normal Man.—The blood which served as a control was obtained from a normal healthy man. It was drawn off in exactly the same manner and was treated in each case by exactly the same procedures as the blood obtained from the patient.

Bacterial Culture.—The bacterial culture employed in the experiments set forth below was obtained by suspending in physiological salt solution a portion of a 24 hours' growth of Staphylococcus albus on agar.

The quantities of serum, washed corpuscles, and staphylococcus culture which are specified below were then in each case taken up into a capillary tube, mixed on a glass slide, re-aspirated into the tube, and digested together at blood heat for 15 minutes. Films were then made and stained by Leishman's stain. Finally the number of ingested bacteria were enumerated in a series of polynuclear W.B.C. taken in order as they came.

The phagocytic index given below—and the same applies throughout this paper—represents in each case the average number of bacteria ingested by the individual P.W.B.C. The number of polynuclear white blood corpuscles which have furnished the index is in each case inserted in brackets :—

Experiment.

A.

Immunised patient's washed corpuscles	3 vols.
Immunised patient's serum	3 ,,
Suspension of staphylococcus culture	1 vol.

Phagocytic index (20 P.W.B.C.), 25·7.

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* 'Lancet,' January 23, 1904.

B.

Washed corpuscles from normal man	3 vols.
Serum from normal man	3 ,,
Suspension of staphylococcus culture	1 vol.

Phagocytic index (15 P.W.B.C.), 13.

C.

Immunised patient's washed corpuscles	3 vols.
Serum from normal man	3 ,,
Suspension of staphylococcus culture	1 vol.

Phagocytic index (15 P.W.B.C.), 13.

D.

Washed corpuscles from normal man	3 vols.
Serum from immunised patient	3 ,,
Suspension of staphylococcus culture	1 vol.

Phagocytic index (15 P.W.B.C.), 28·2.

Experiments on the Opsonic Power of Human Blood in its Relation to the Bacillus of Plague.

In these and all subsequent experiments, unless where otherwise specified, the technique employed was exactly the same as that employed in the experiments set forth above. It may further be premised that the bacterial suspensions employed were in each case suspensions of very young agar cultures—in most cases 24-hour cultures—in physiological salt solution. By the term "heated serum" is in each case to be understood serum which has been subjected to a temperature of 60° C. for 10 minutes or more.

Experiment 1.

A.

S. R. D.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of plague bacillus	1 vol.

Phagocytic index (20 P.W.B.C.), 3·0.

B.

S. R. D.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of plague bacillus	1 vol.

Phagocytic index (25 P.W.B.C.), 0·7.

Experiment 2.

A.

S. R. D.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of plague bacillus	1 vol.

Phagocytic index (20 P.W.B.C.), 13·1.

B.

S. R. D.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of plague bacillus	1 vol.

Phagocytic index (20 P.W.B.C.), 2·1.

Experiment 3.

A.

A. E. W.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of plague bacillus	1 vol.

Phagocytic index (21 P.W.B.C.), 19·6.

B.

A. E. W.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of plague bacillus	1 vol

Phagocytic index (54 P.W.B.C.), 8·4.

Experiment 4.

A.

B. H. S.'s unheated serum	2 vols.
A. E. W.'s washed corpuscles	2 ,,
Suspension of plague bacillus	1 vol.

Phagocytic index (43 P.W.B.C.), 5·3.

B.

B. H. S.'s heated serum	2 vols.
A. E. W.'s washed corpuscles	2 ,,
Suspension of plague bacillus	1 vol.

Phagocytic index (43 P.W.B.C.), 1·4.

It may incidentally be noted in connection with these experiments that while the plague bacilli which lay free in the films were in each case quite unaltered, many of those which had been ingested showed extremely characteristic involution forms* such as we have not seen since we worked with freshly isolated plague cultures in Bombay in connection with the Indian Plague Commission. So typical were the involution forms of the ingested plague bacilli, that we should not hesitate to employ the method of phagocytosis as an aid to diagnosis in the case of a doubtful plague culture.

Experiments on the Opsonic Power of Human Blood in Relation to Micrococcus Melitensis.

Experiment 1.

A.

S. R. D.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of *Micrococcus Melitensis*	1 ,,

Phagocytic index (10 P.W.B.C.), 26·9.

B.

S. R. D.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of *Micrococcus Melitensis*	1 ,,

Phagocytic index (10 P.W.B.C.), 9·2.

Experiment 2.

A.

A. E. W.'s unheated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of *Micrococcus Melitensis*	1 ,,

Phagocytic index (21 P.W.B.C.), 10·0.

B.

A. E. W.'s heated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of *Micrococcus Melitensis*	1 vol.

Phagocytic index (21 P.W.B.C.), 2·4.

Experiment 3.

A.

S. R. D.'s heated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of *Micrococcus Melitensis*	1 vol.

Phagocytic index (21 P.W.B.C.), 12·9.

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* It may be observed that our plague culture—like other plague cultures which have been cultivated on artificial nutrient media for a number of generations—has altogether lost the property of developing in a spontaneous manner the involution forms which are characteristic of freshly isolated plague cultures.

B.

S. R. D.'s heated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of *Micrococcus Melitensis*	1 vol.

Phagocytic index (21 P.W.B.C.), 0·9.

EXPERIMENTS ON THE OPSONIC POWER OF HUMAN BLOOD IN RELATION TO THE BACILLUS DYSENTERICUS (SHIGA).

Experiment 1.

A.

S. R. D.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of Shiga's bacillus	1 vol.

Phagocytic index (20 W.P.B.C.), 4·2.

B.

S. R. D.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of Shiga's bacillus	1 vol.

Phagocytic index (20 P.W.B.C.), 0·0.

Experiment 2.

A.

A. E. W.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of Shiga's bacillus	1 vol.

Phagocytic index (20 P.W.B.C.), 5·4.

B.

A. E. W.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of Shiga's bacillus	1 vol.

Phagocytic index (33 P.W.B.C.), 0·1.

Experiment 3.

A.

S. R. D.'s unheated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Suspension of Shiga's bacillus	1 vol.

Phagocytic index (20 P.W.B.C.), 3·6

B.

S. R. D.'s heated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Suspension of Shiga's bacillus	1 vol.

Phagocytic index (20 P.W.B.C.), 0·2.

A certain number of the bacilli (and these bacilli were found indifferently in the interior of the cells and free in the preparation) had, in the case of the experiments undertaken with unheated serum, undergone spherulation.

EXPERIMENTS ON THE OPSONIC POWER OF HUMAN BLOOD IN ITS RELATION TO THE BACILLUS COLI.

Experiment 1.

A.

B. H. S.'s unheated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the *Bacillus coli*	1 vol.

Phagocytic index (20 P.W.B.C.), 3·8.

B.

B. H. S.'s heated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the *Bacillus coli*	1 vol.

Phagocytic index (20 P.W.B.C.), 0·75.

Experiment 2.

A.

F. F.'s unheated serum	3 vols.
F. F.'s washed corpuscles	3 ,,
Suspension of the *Bacillus coli*	1 vol.

Phagocytic index (20 P.W.B.C.), 5.

B.

F. F.'s heated serum	3 vols.
F. F.'s washed corpuscles	3 ,,
Suspension of the *Bacillus coli*	1 vol.

Phagocytic index (21 P.W.B.C.), 0·76.

Experiments on the Opsonic Power of Human Blood in its Relation to the Pneumococcus of Fraenkel.

Experiment 1.

A.

S. R. D.'s unheated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Suspension of the pneumococcus of Fraenkel	1 vol.

Phagocytic index (15 P.W.B.C.), 16.

B.

S. R. D.'s heated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Suspension of Fraenkel's pneumococcus	1 vol.

Phagocytic index (40 P.W.B.C.), 1·1.

Experiment 2.

A.

A. E. W.'s unheated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Suspension of Fraenkel's pneumococcus	1 vol.

Phagocytic index (23 P.W.B.C.), 6.

B.

A. E. W.'s heated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Suspension of Fraenkel's pneumococcus	1 vol.

Phagocytic index (40 P.W.B.C.), 0·2.

Experiments on the Opsonic Power of Human Blood in its Relation to the Bacillus of Anthrax.

Experiment 1.

A.

S. R. D.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of *Bacillus anthracis*	1 vol.

Enumeration was here impossible, but there was everywhere evidence of phagocytosis. In the few cases where the leucocytes had not ingested bacteria, they were found to have extended themselves in a characteristic grasping manner along the bacterial threads (fig. 5).

B.

S. R. D.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of the *Bacillus anthracis*	1 vol.

Here there were practically no signs of phagocytosis. The cells were everywhere empty, and they had not drawn themselves into intimate contact with the anthrax threads (fig. 6).

Experiment 2.

A.

A. E. W.'s unheated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Broth culture of anthrax	1 vol.

Phagocytic index (36 P.W.B.C.), 2·4 (approximate only).

B.

A. E. W.'s heated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Broth culture of anthrax	1 vol.

Phagocytic index (100 P.W.B.C.), 0.

OPSONIC POWER OF HUMAN BLOOD IN ITS RELATION TO THE BACILLUS TYPHOSUS AND THE CHOLERA VIBRIO.

It is well known that human blood exerts a very considerable bactericidal power upon cultures of the *Bacillus typhosus* and of the cholera vibrio. The destructive effect in question manifests itself to microscopical observation in the form of very profound morphological changes which come under observation in cultures which have been digested with unheated serum. The bacteria in such cultures, after undergoing agglutination and spherulation, swell up and lose their chemical affinity for anilin dyes. Finally they are completely dissolved.

It is manifest that where disintegrative changes of this kind are occurring under the influence of the serum, opsonic effects will be more or less thrust into the background. These last will, in the case of phagocytic experiments conducted with unheated serum, be masked, on the one hand, by the fact that there will be fewer bacteria available for phagocytosis, and on the other hand by the fact that intracellular disintegration will, it may be presumed, be more rapid in the case where the serum has already exerted a disintegrative effect on the bacteria anterior to their ingestion.

Lastly, ingested bacteria which have lost their characteristic chemical affinity for their stain may readily escape enumeration.

All these points must be taken into consideration in connection with the subjoined experiments :—

Experiment 1.

A.

S. R. D.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of the cholera vibrio	2 ,,

Everywhere considerable phagocytosis. Complete spherulation of almost all the micro-organisms within and all the micro-organisms outside the cells. No indication of vacuolation round the ingested bacteria (fig. 3).

Phagocytic index (14 P.W.B.C.), 24 (*circ.*).

B.

S. R. D.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of the cholera vibrio	2 ,,

Everywhere considerable phagocytosis. No spherulation of the micro-organisms either within or without the leucocytes. Very marked vacuolation of the leucocytes round the ingested bacteria (fig. 4).

Phagocytic index (11 P.W.B.C.), 26·2 (*circ.*).

Experiment 2.

A.

A. E. W.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of the cholera vibrio	1 vol.

Complete spherulation of all the bacteria, whether within or without the cells.

Phagocytic index (21 P.W.B.C.), 8·1 (*circ.*).

B.

A. E. W.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of the cholera vibrio	1 vol.

No spherulation of the micro-organisms, either within or without the leucocytes.

Phagocytic index (13 P.W.B.C.), 0·8.

Experiment 3.

A.

S. R. D.'s unheated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Broth culture of the typhoid bacillus	2 ,,

Much phagocytosis. Complete spherulation of all the extracellular micro-organisms. Many of the bacilli in the interior of the leucocytes have completely preserved their original contours, others—probably the later ingested ones—are spherulated (fig. 1).

B.

S. R. D.'s heated serum	2 vols.
S. R. D.'s washed corpuscles	2 ,,
Broth culture of the typhoid bacillus	2 ,,

Much phagocytosis. All the micro-organisms, whether within or without the leucocytes, are morphologically unaltered and have preserved their staining properties unimpaired (fig. 2.).

Experiment 4.

A.

A. E. W.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Broth cultivation of the typhoid bacillus	2 ,,

Complete spherulation of all the extracellular bacteria which have escaped solution. In interior of leucocytes most of the bacteria have undergone spherulation, but in the centre of the corpuscles some—probably those which were soonest ingested—are morphologically unaltered and preserve their staining properties unaltered.

Phagocytic index, 100 (estimated).

B.

A. E. W.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Broth cultivation of the typhoid bacillus	1 vol.

No spherulation, either within or without the cells.

Phagocytic index (20 P.W.B.C.), 31·8 (*circ.*).

Experiment 5.

A.

S. R. D.'s unheated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of the typhoid bacillus	1 vol.

All the bacilli both within and without the cells have undergone spherulation.

Phagocytic index (11 P.W.B.C.), 13·6.

B.

S. R. D.'s heated serum	3 vols.
S. R. D.'s washed corpuscles	3 ,,
Suspension of the typhoid bacillus	1 vol.

No spherulation either within or without the leucocytes.

Phagocytic index (23 P.W.B.C.), 7·2.

Of incidental interest in connection with the above experiments is the demonstration which they afford, that the spherulation of the intracellular ingested micro-organisms, which has been often ascribed to the agency of the leucocytes, is in reality due to agency of the blood fluids.

Opsonic Power of Human Blood in its Relation to the Diphtheria Bacillus and the Xerosis Bacillus.

Experiment 1.

A.

A. E. W.'s unheated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of the diphtheria bacillus	3 ,,

Phagocytic index (27 P.W.B.C.), 0·7.

B.

A. E. W.'s heated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of the diphtheria bacillus	3 ,,

Phagocytic index (29 P.W.B.C.), 4·1.

Experiment 2.

A.

B. H. S.'s unheated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the diphtheria bacillus	2 ,,

Phagocytic index (20 P.W.B.C.), 8·0.

B.

B. H. S.'s heated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the diphtheria bacillus	2 ,,

Phagocytic index (20 P.W.B.C.), 10·9.

Experiment 3.

A.

B. H. S.'s unheated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the diphtheria bacillus	1 vol.

Phagocytic index (44 P.W.B.C.), 4·0.

B.

B. H. S.'s heated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the diphtheria bacillus	1 vol.

Phagocytic index (50 P.W.B.C.), 3·3.

Experiment 4.

A.

A. E. W.'s unheated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of the xerosis bacillus	1 vol.

Phagocytic index (40 P.W.B.C.), 2·8.

B.

A. E. W.'s heated serum	3 vols.
A. E. W.'s washed corpuscles	3 ,,
Suspension of the xerosis bacillus	1 vol.

Phagocytic index (25 P.W.B.C.), 3·2.

Experiment 5.

A.

B. H. S.'s unheated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the xerosis bacillus	1 vol.

Phagocytic index (30 P.W.B.C.), 6·3.

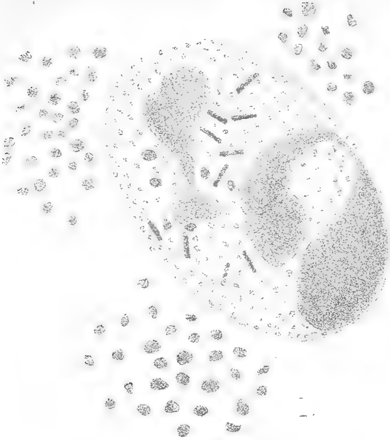
B.

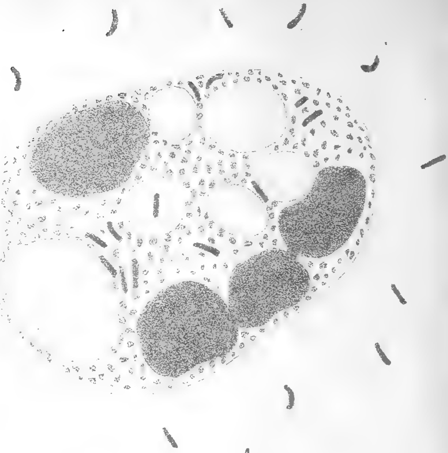
B. H. S.'s heated serum	3 vols.
B. H. S.'s washed corpuscles	3 ,,
Suspension of the xerosis bacillus	1 vol.

Phagocytic index (30 P.W.B.C.), 6.

Conclusions.

The experimental data which have been set forth above establish that the opsonic action of the blood fluids—to which attention was for the first time directed in our previous communication—is exerted not exclusively upon the *Staphylococcus pyogenes*, but also upon the *Bacillus*





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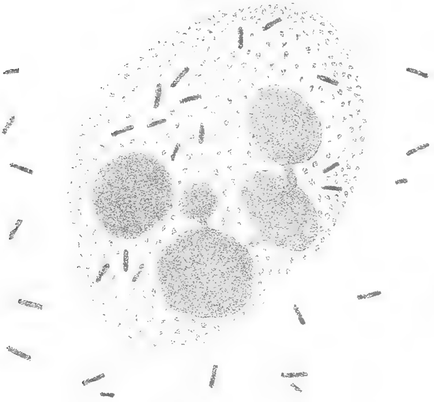

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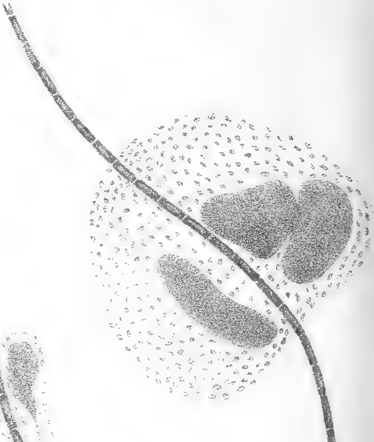

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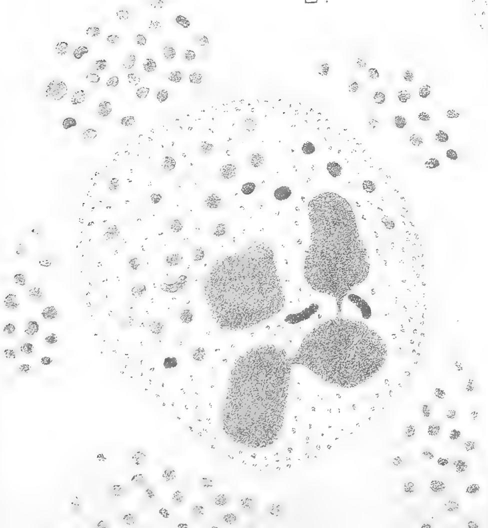

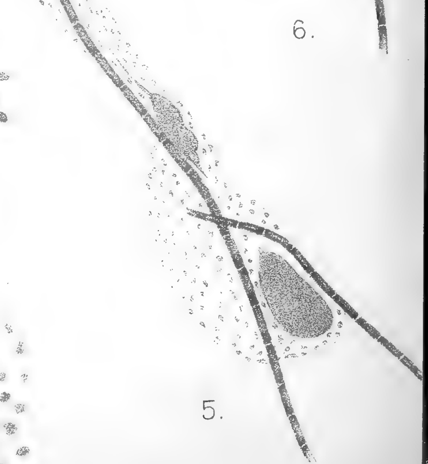

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pestis, the *Micrococcus melitensis*, the *Diplococcus pneumoniæ* of Fraenkel, the *Bacillus coli*, the *Bacillus dysenteriæ* (Shiga), the *Bacillus anthracis*, the *Bacillus typhosus*, and the *Vibrio choleræ Asiaticæ*.

So far as we have gone, the *Bacillus diphtheriæ* and its congener the *Bacillus xerosis* have proved to be the only pathogenetic bacteria which are insensible to this action of the blood fluids.

Taking these experimental data in conjunction with other facts which have been elicited by us, or as the case may be by one of us working in connection with Captain F. Windsor,* I.M.S., with regard to the bactericidal action exerted by human blood upon the various species of pathogenetic micro-organisms, we may classify these bacteria in the following categories:—

(1) *Bacteria which are eminently sensible to the bactericidal, bacteriolytic, and opsonic action of normal human blood fluids.*—The *Bacillus typhosus* and the *Vibrio choleræ Asiaticæ.*

(2) *Bacteria which are in some measure sensible to the bactericidal action of the normal human blood fluids, and which are eminently sensible to its opsonic action.*—The *Bacillus coli* and the *Bacillus dysenteriæ.*

(3) *Bacteria which are absolutely insensible to the bactericidal action of the normal human blood fluids, but are eminently sensible to the opsonic action of these fluids.*—The *Staphylococcus pyogenes*, the *Bacillus pestis*, the *Micrococcus melitensis*, the *Diplococcus pneumoniæ* of Fraenkel.

(4) *Bacteria which are insensible both to the bactericidal and to the opsonic action of human blood fluids.*—The *Bacillus diphtheriæ* and *Bacillus xerosis.*

It may be pointed out in conclusion that the demonstration furnished above, that successful immunisation against the staphylococcus pyogenes is dependent upon an elaboration of opsonins in the system of the inoculated patient, suggests that successful immunisation against plague and Malta fever, and we may add against streptococcal invasions, may be likewise dependent upon the elaboration of opsonins.

It will be manifest that if this is so, the determination of the opsonic power of the blood is calculated to render services also in connection with the testing of any therapeutic sera which may find an application in connection with the disease.

DESCRIPTION OF PLATE 3.

FIG. 1.—White Blood Corpuscles digested with *unheated* serum and culture of the *Bacillus typhosus* for 15 minutes at 37° C. Shows, in the case of the extra-cellular micro-organisms, complete spherulation and agglutination. Many of the micro-organisms in the interior of the phagocyte are unaltered with respect to their shape and staining reaction; others—presumably the later ingested micro-organisms—have undergone spherulation.

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* 'Journ. of Hygiene,' vol. 1.

FIG. 2.—White Blood Corpuscles digested with *heated* serum and culture of the *Bacillus typhosus* for 15 minutes at 37° C. Shows that the micro-organisms retain their shape, both within and without the phagocyte.

FIG. 3.—White Blood Corpuscles digested with *unheated* serum and culture of the Cholera Vibrio for 15 minutes at 37° C. Shows, in the case of the extra-cellular micro-organisms, complete agglutination and spherulation. Two of the micro-organisms in the interior of the phagocyte—presumably those first ingested—retain their characteristic shape.

FIG. 4.—White Blood Corpuscles digested with *heated* serum and culture of the Cholera Vibrio for 15 minutes at 37° C. Shows vacuolation of the phagocyte and no alteration in the micro-organisms, either within or without the phagocyte.

FIG. 5.—White Blood Corpuscles digested with *unheated* serum and culture of the *Bacillus anthracis* for 15 minutes at 37° C. Shows the phagocyte extending itself in such a manner as to invaginate the bacilli.

FIG. 6.—White Blood Corpuscles digested with *heated* serum and culture of the *Bacillus anthracis.* Shows an anthrax thread lying upon a phagocyte, which makes no attempt at phagocytosis.

"Sunspot Variation in Latitude, 1861—1902." By WILLIAM J. S. LOCKYER, M.A. (Camb.), Ph.D. (Gött.), F.R.A.S., Chief Assistant, Solar Physics Observatory. Communicated by Sir NORMAN LOCKYER, K.C.B., LL.D., F.R.S. Received January 16,—Read February 11, 1904.

[PLATES 4 AND 5.]

In a previous communication,* Sir Norman Lockyer and I gave the results of a discussion of prominence observations, and pointed out the necessity of dealing individually with small zones on the solar surface.

In that paper brief reference was made to the law of spot zones, as discovered by Carrington, and corroborated by Spörer, and it was further stated that more modern observations had established these *general* deductions of spot distribution. The words "general deductions" were purposely used, as it was then noticed that there were many anomalies that required explanation.

The object of the present paper is to draw attention to these anomalies, and to give the results that have been deduced from a minute examination of the changes of heliographic latitudes of sunspots from year to year. The present evidence indicates that the law of Spörer, although of great importance, represents only a very general idea of a complicated sunspot circulation.

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* "Solar Prominence and Spot Circulation, 1872—1901," 'Roy. Soc. Proc.,' vol. 71, p. 446.

From a valuable series of sunspot observations, made between the years 1853 and 1861, Richard Carrington* was the first to point out that spots had a general drift towards the Equator during a sunspot cycle, or, to use his own words, there was indicated "a great contraction of the limiting parallels between which spots were formed for two years previously to the minimum of 1856, and, soon after this epoch, the apparent commencement of two fresh belts of spots in high latitudes, north and south, which have in subsequent years shown a tendency to coalesce, and ultimately to contract, as before, to extinction." Spörer fortunately took up the work where Carrington left off, and his observations extended over the period 1861—1879. These were published in four different volumes,† and the conclusions at which he arrived practically corroborated those of Carrington. In the last of these publications, Spörer summed up all the observations for the period 1854—1879, and published curves, showing the relation between the sunspot frequency for these years and the variation of the mean heliographic latitude of the spots.

The law of zones, as definitely formulated by Spörer, is as follows‡ :—"Un peu avant le minimum, il n'y a de taches que près de l'equateur solaire, entre + 5° et − 5°. A partir du minimum, les taches, qui avaient depuis longtemps déserté les hautes latitudes, s'y montrent brusquement vers ± 30°. Puis elles se multiplient, un peu partout, à peu près entre ces limites, jusqu'au maximum, mais leur latitude moyenne diminue constamment jusqu'à l'époque du nouveau minimum."

As solar prominences appear on any part of the disc, it was sufficient, in order to trace their distribution, to divide the sun's surface into nine zones of 10 degrees each. Since, however, spots seldom occur above latitude 40°, the width of the zones had to be considerably diminished. For the present inquiry, it was finally decided to group the spots into belts 3 degrees wide, for even zones of 5 degrees in width were found to mask many important characteristics.

The necessity for such narrow zones will be seen from the accompanying figure (fig. 1), in which the yearly distribution of spots is shown for the years 1879—1883, taking zones of 10 degrees, 5 degrees, and 3 degrees in width respectively.

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* 'Observations of the Spots on the Sun,' made at Redhill by R. C. Carrington, F.R.S., 1863, p. 17.

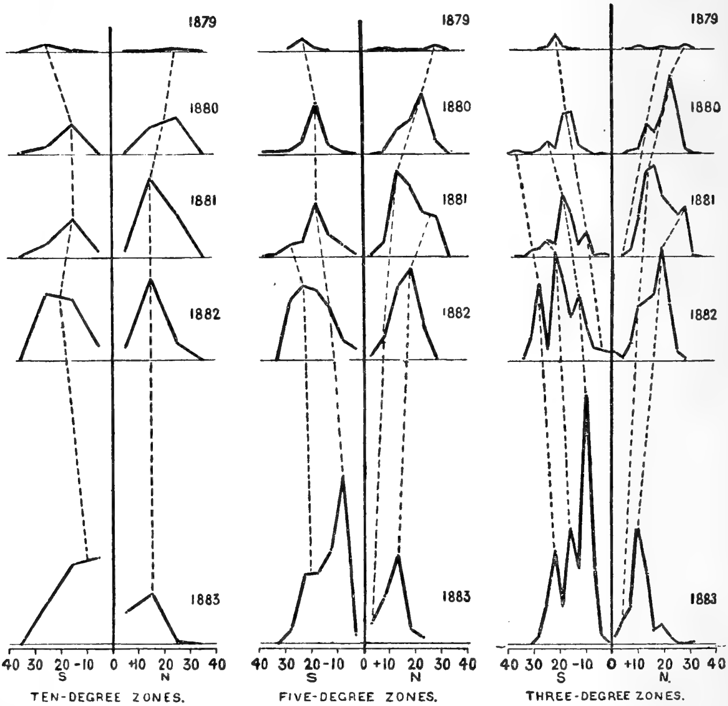
† 'Publication der Astronomischen Gesellschaft,' vol. 13 (Leipzig. 1874); 'Publication der Astronomischen Gesellschaft,' vol. 13. Fortsetzung (Leipzig, 1876); 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' No. 1, vol. 1, Part I; No. 5, vol. 2, Part I.

‡ 'Comptes Rendus,' vol. 108, p. 486.

In these curves each broad vertical line corresponds to the solar equator, and the scales to the right and left of each represent the north and south latitudes respectively. The heights of the curves above each horizontal zero line indicates the different amounts of spotted area, and the scales of these are so arranged that the curves are all proportional to the spotted area.

The curves themselves are formed by determining the mean spotted area for each zone, and plotting each value at the point representing the mean latitude of this zone; these points are then all joined together. Thus, in the case of the 0—10° zone, the mean spotted area is plotted at 5°, 10—20°, at 15°, etc. The other zone divisions are similarly treated, thus, 0—5° is plotted at 2·5°, 0—3° at 1·5°, etc.

Fig. 1.



DISTRIBUTION OF SUN'S SPOTTED AREA.

FROM 1879—1883.

In the 10° zone curves here shown there is only one maximum in each hemisphere for the years in question, and these, as indicated by the dotted curves which join them, do not progress gradually towards the equator, as would probably be the case according to Spörer's law. With 5° zones it is possible to detect the presence of two maxima in one or other of the hemispheres, all of which have a trend towards the equator in succeeding years. Still more detail is displayed in the 3° zones, and here is apparent a spot distribution and movement which is practically masked in the two preceding sets of curves.

The advisability of adopting 3° zones for the present investigation being thus apparent, the whole series of observations from the year 1861—1902 was treated in the above manner, the points plotted and the curves drawn as shown in the figure previously referred to.

This reduction was rendered comparatively easy by the fact that the Astronomer Royal has quite recently published* the values of the amount of sunspot area for each degree of latitude for each hemisphere from the year 1874—1902. For information previous to that date use was made of the detailed observations of the positions and areas of sunspots collected by Spörer in the publications already referred to, and curves for each year were drawn. For this period the curves employed were of a less degree of accuracy than those drawn from the Greenwich reduction, as the number of days of observation throughout a year was not so great.

Advantage was taken of the fact that Spörer's observations and those reduced at Greenwich overlapped during the four years 1874—1877, and a comparison of the curves from each series was rendered possible. The close similarity of these in each case showed that the reduction of Spörer's observations exhibited the chief features of the movements of centres of spot-activity as indicated by the Greenwich curves.

By thus employing Spörer's observations, curves for each of the 42 years from 1861—1902 inclusive were drawn in the manner described above, and these were placed vertically one under the other, like those shown in fig. 1 for the years 1879--1883.

In this way it was possible to trace the varying positions, as regards changes of latitude, of the centres of action, or maxima points of the curves, from year to year, just as was previously attempted in the case of the prominences. These centres of action were then connected by lines passing from one yearly curve to the next. It is worthy of remark that very little difficulty was met with in deciding the maxima points to be joined. There was always, throughout the whole period, a most distinct march of these points individually towards the equator, and the method of placing the curves one beneath the other rendered such movement at once obvious to the eye. There was only one instance where it seemed necessary that a march from lower to higher latitudes ought to be considered. This was in the southern hemisphere, in the years 1889 and 1890 (see Plate 5, Curve A). There

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* 'Monthly Notices R. A. S.,' vol. 63, pp. 452—461.

was a small indication of the presence of a spot centre of action in latitude 19° in the former year, while next year the position of the centre of action was in latitude 24°. Since this case was unique, it was considered advisable not to connect these points together, but to leave the centre of action in the year 1889 as an isolated point.

The diagram (fig. 1) not only exhibits some of the types of curves met with, but shows how the various centres of maximum spot-activity were joined up with each other, year by year, for the period of time over which the curves extend, namely, from 1879, the year following a sunspot minimum, to about a sunspot maximum in 1883.

Considering the curves relating to the sun's northern hemisphere, it will be seen that in 1879, the year following a sunspot minimum, when the spots were ending a cycle near the equator, two new outbreaks occurred in latitudes about 20° and 30°.

These two centres of activity moved towards the equator next year, and by 1881 the former had disappeared, while the other rapidly grew in intensity and reached latitude 15°. During this year a new outbreak in latitude 30° made its appearance, and this in the two following years had an equatorial trend.

A somewhat similar occurrence took place in the southern hemisphere, each of the centres of action moving rapidly towards the equator.

It is interesting to note the rapid growth and decay of these centres of action, an example of which is shown commencing in 1879 in latitude 28° in the northern hemisphere.

Attention may particularly be drawn to the three prominent maxima of the curves for the southern hemisphere in the years 1882 and 1883, which indicate that at this period there were three definite centres of spot action in existence.

In order to bring within a small compass the results of the above analysis for the whole period of investigation (the above-mentioned forty-two curves, although drawn close together on a small scale, cover a strip of paper 5 feet in length), a method was adopted similar to that employed in the case of the prominence reduction.*

In the accompanying plates the two sets of curves marked A indicate for each hemisphere the changes in the positions of these centres of spot activity from year to year plotted at equal intervals of a year. The striped portion is deduced from Spörer's observations, and the remainder from the Greenwich reductions. These lines have been proportionally thickened to indicate approximately the relative amount of spotted area at these centres of action, or, in other words, the heights of the maxima points on the yearly curves. These curves thus indicate for each year the positions, as regards latitude, of the particular zones

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* 'Roy. Soc. Proc.,' vol. 71, pl. 6.

in which the centres of spot activity occur, and give an idea of the movements of these centres during each sunspot cycle.

In this paper these curves have been called "spot-activity tracks," but it is important to point out that this term is not necessarily applied to the proper motion of any individual spot, but simply to the changes of position of the regions in which they are most numerous. As, therefore, the term "spot-activity tracks" represents the different positions of the regions of greatest spot-activity, so "prominence-activity tracks" may be employed to indicate the equivalent variations as regards the prominences which were shown in a previous paper.*

These "spot-activity tracks" have possibly a terrestrial equivalent in the variations from year to year of the positions of the "Zugstrassen," or cyclone tracks of Köppen, it having been found that cyclones in general, which move in the direction of the great mass of air carried by primary currents, have a strong tendency to pursue somewhat the same tracks according to the place of origin.

For the sake of comparison, curves B, C, and D in each plate have been added. Curves B show the variations of the mean heliographic latitude of the total spotted area for each hemisphere as determined and described in a previous paper.†

Curves C illustrate the distribution and changes of position of the centres of prominence activity. These curves are somewhat different to those previously published,‡ being so arranged that they form a continuous series from the year 1870. The small circles in the years 1870—1871 represent Respighi's observations, the curves from 1872—1881 those of Tacchini, and the remainder, up to the year 1902, Ricco and Mascari's observations. The dotted curves previous to 1870 are intended only to give a rough idea of the prominence variations based on a repetition of the observations of 1872—1885.§ The last curves, namely, those marked D in the plates, represent the variation from year to year of the total spotted area on each hemisphere of the sun, and special attention was drawn in a previous publication‖ to the great differences between the two hemispheres at the times of sunspot maxima. The vertical broken and continuous lines indicate the epochs of sunspot minima and maxima as determined by combining the amount of spotted area on both hemispheres of the sun.

Reverting now to the curves marked A, which form the special subject of the present paper, the following *general* deductions may be made:—

1. From sunspot minimum to minimum there are three, but generally

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* 'Roy. Soc. Proc.,' vol. 71, p. 446.

† 'Roy. Soc. Proc.,' vol. 71, p. 449.

‡ 'Roy. Soc. Proc.,' vol. 71, pl. 6 and 7.

§ 'Monthly Notices R. A. S.,' vol. 63, No. 8, p. 484.

‖ 'Roy. Soc. Proc.,' vol. 71, p. 246, footnote.

four, distinct "spot-activity tracks," or loci of movements of the centres of action of spot disturbance.

2. The first appearance of each of these "spot-activity tracks" occurs generally between a sunspot minimum and the following maximum. After about the epoch of maximum generally no new "spot-activity tracks" of large magnitude are commenced.

3. Their first appearance is mostly in higher latitudes than 20° in each hemisphere.

4. They are faintly indicated at first, become more prominent and distinct, and finally thin out and fade away.

5. They all fade away in regions close to the equator.

6. There seems to be a tendency for each successive "spot-activity track" to make its appearance in latitudes higher than the one preceding it.

7. At, or a little after, the time of sunspot maximum there is also a tendency for each "spot-activity track" to retain its latitude for a short time.

In the light of these curves it is interesting to analyse those formed by plotting the mean yearly heliographic latitude of spotted area for each hemisphere: these are given in the accompanying plates (Curves B). These latter curves represent the drift from year to year of the mean heliographic spot latitude, and illustrate Spörer's "Law of Zones." It will be noticed that each commences in high latitudes about the time of sunspot minimum, and gradually approaches the equator until the epoch of the following minimum, when a new cycle in high latitudes recommences.*

An important point to be noticed about this series of curves is that each is of a *wavy* nature. This peculiarity is clearly shown even in the plate illustrating Spörer's reduction† which covers the period 1854—1880. There the curves, representing the mean spotted area and the mean heliographic spot latitude for both hemispheres combined, were compared, and Spörer pointed out some anomalies between these mean curves and those formed by joining the actual points of observation. So conspicuous were these anomalies at one epoch (even after many of the peculiarities had been lost by combining the two hemispheres instead of treating them singly), that he drew attention to them in the following words‡: "Nun sieht man an den Beobachtungspuncten, welche bei der Breitencurve eingetragen sind, dass die aus specieller Rechnung hervorgehende Curve *ebenfalls eine*

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* These curves should overlap, but the unit of time, namely, the year, here employed, masks this feature.

† 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 2, Erstes Stück, No. 5, Tafel 32.

‡ 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 2, Erstes Stück, No. 5, p. 81.

Welle liefern würde, und zwar hätte diese ihre grösste Erhebung gegen Ende des Jahres 1875." As will be seen from a further quotation from the same page, Spörer distinctly noticed subsidiary increases of spotted area and a reversion of spots to higher latitudes, and this drew from him the conclusion that the epoch of the following sunspot minimum would be late.

He wrote (page 81), "die Ursache, welche eine Erhebung der Breitencurve bewirkte, d. h. welche veranlasste, *dass in höheren Breiten als vorher Flecke entstanden,* dadurch auch Veranlassung gewesen ist, dass wiederum Vermehrung der Flecke eintrat und damit auch das Flecken-Minimum für längere Zeit verzögert wurde."

Again, from the solar observations made at the Kalócsa Observatory during the years 1880—1884, both years inclusive, Dr. Braun* depicted in a graphic manner the progressive changes in the mean heliographic latitude of the spots during this period, and drew attention to the differences between the mean curve and that passing strictly through the points of observation (fig. 2). The mean curve he found

Fig. 2.

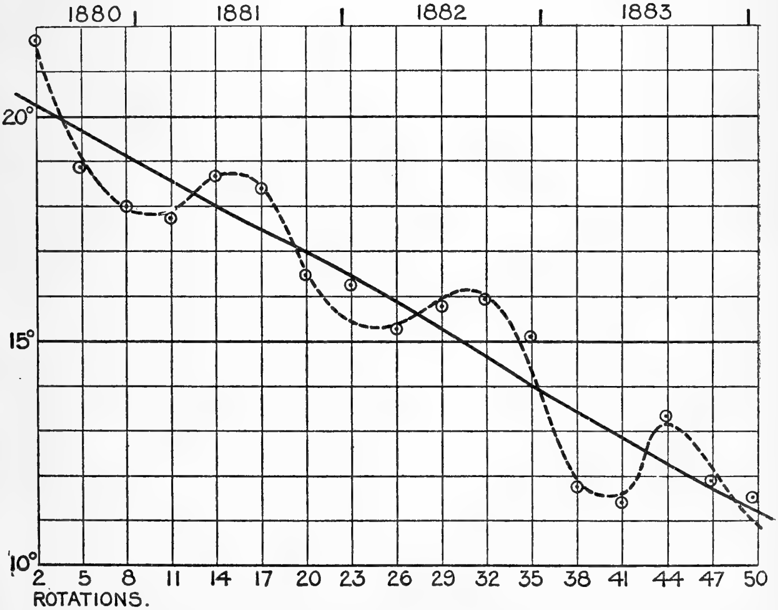

Diagram to illustrate for the years 1880—1883 the differences between the curve showing the decrease of the mean heliographic latitude for the entire spotted area of both hemispheres taken together (continuous curve), and that obtained by a less smoothed curve (dotted) passing through the points of the actual observations.

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* 'Berichte von dem Erzbischöflich Haynaldschen Observatorium zu Kalócsa in Ungarn,' von Carl Braun, S.J. Münster i. W., 1886.

to continually approach the equator, but superposed upon the line of uniform descent was a series of minor oscillations. In this connection he wrote:* "Die Spörer'sche Entdeckung der in jeder Periode auftretenden Annäherung der Flecken-Zone gegen den Aequator findet somit in unsern Beobachtungen eine unmisskennbare Bestätigung. Allerdings sind die Zeichnungen nicht durchaus von demselben Gehilfen ausgeführt worden, und dieser Umstand mag einigen Einfluss in Bezug auf diese Wahrnehmung haben . . . Sehr auffallend erscheint eine gewisse secundäre periodische Schwankung der mittleren Breite mit einer Periode von etwa 1 Jahr und einer Amplitude von reichlich 2 Graden . . . Diese mag wohl einem Zufall zuzuschrieben sein, welche nur während dieser vier Jahre obwaltete, vielleicht auch mit dem Wechsel der Zeichner in Zusammenhang steht. Doch wäre es immerhin von Interesse, dass diese Wahrnehmung durch die Discussion anderweitiger Beobachtungen geprüft und eventuell näher untersucht würde."

The present investigation seems to throw light on these peculiar changes of curvature shown by the mean heliographic latitude curves. These latter (Plates 4 and 5, Curves B) are individually really nothing more than the *integration* of the corresponding Curves A. Every change of curvature in Curves B is due to either the outburst of spots in another "spot-activity track" or by one "spot-activity track" becoming more intensified in relation to another, or lastly by the extinction of a "spot-activity track" as the equator has been reached as shown in the Curves A.

To illustrate this, let the curve for the mean heliographic spot latitude in the southern hemisphere (Plate 5, Curves B) beginning in the year 1879 be considered. This is practically the period referred to above by Dr. Braun.

At this time there is only one "spot-activity track" (latitude 22°) in existence, as shown in Plate 5, Curve A; so Curve B consequently commences in the same latitude. By the next year the "spot-activity track" (Curves A) has reached latitude 17°, and a new one has made its appearance in latitude 25°. Curve B, therefore, takes the mean position of about 20°, when allowance has been made for the difference of intensity of these two tracks.

In the following year 1881 both these "spot-activity tracks" have approached nearer the equator, but another has appeared in latitude 25°, so that the mean latitude for the whole hemisphere has only slightly changed.

By the year 1882 still another "spot-activity track" has come into existence in latitude 28°, while the first "spot-activity track" mentioned

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[*] *Ibid.*, p. 83.

above has vanished. The mean latitude for the whole hemisphere, as is indicated in Curve B for this epoch, is increased to latitude 20°. After this all three "spot-activity tracks" approach the equator and Curve B does the same, but owing to the relative changes in the amount of the spotted area in each of these "spot-activity tracks" as indicated by their thickness, the mean heliographic latitude curve suffers another change of curvature in 1885. In a similar way the various changes of curvature in all the other curves (Curve B) can be accounted for.

Particular attention has been drawn to the fact that about the times of sunspot maxima there is considerable spot activity in the highest spot latitudes, which according to Spörer's law would not be expected.

The following extract expressing the impressions of Messrs. De La Rue, Stewart and Loewy on this point is therefore of interest, since it shows that such activity at the maximum of 1871 was even remarked as long ago as 1872.* "A striking feature of last year's observations seems to have been the occurrence of groups in comparatively high latitudes, especially in the southern solar hemisphere; a group observed between March 21 and 23 had the almost unprecedented high latitude of 43°, while latterly, towards the end of the year, several groups in almost as high a latitude have repeatedly made their appearance."

The Wilna observers also drew attention to the high latitudes of some spots about this maximum period, 1869—1871, as can be gathered from the following extract.†

"Generally speaking, during the last three years, about the last maximum, the spots were most distant from the equator; five spots were observed near latitude 38°; three about latitude 40—43°; one at $51\frac{1}{2}$° latitude, . . ."

A word may finally be said as to the relationship between the curves representing the "spot-activity tracks" (Curves A), and those indicating the "prominence-activity tracks" (Curves C). As was pointed out in a previous communication‡ the general drift of the prominence activity is from low to high latitudes.

It is of interest here to note that from the time of a sunspot minimum when the "prominence-activity tracks" are approaching more rapidly high latitudes, up to about a sunspot maximum when they reach their highest positions, nearly all the "spot-activity tracks" come into existence. Further, the nearer the "prominence-activity tracks" approach the poles the higher in latitude do these "spot-activity tracks" also occur, and this is the case for each hemisphere of the sun separately.

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* 'Monthly Notices R. A. S.,' 1872, vol. 32, p. 225.

† 'Report of the Committee on Solar Physics,' 1882, p. 155.

‡ 'Roy Soc. Proc.,' vol. 71, p. 452.

What the actual connection between these two different systems of currents is, it is not possible yet to say, but these facts suggest a very close relationship.

In conclusion, I wish to express my thanks to Mr. T. F. Connolly, computer in the Solar Physics Observatory, for his assistance in making the reductions and drawing the numerous curves.

Conclusions.

The result of the investigation leads to the following conclusions:—

1. Spörer's law of spot zones is only approximately true, and gives only a very general idea of sunspot circulation.

2. Spörer's curves are the integrated result of two, three and sometimes four "spot-activity track" curves, each of the latter falling nearly continuously in latitude.

3. Spörer's, and many other previous reductions have indicated the peculiar "wavy" nature of the integrated curve, which peculiarity is here shown to be for the most part real and not due to errors of observation, etc.

4. Outbursts of spots in high latitudes are not restricted simply to the epochs at or about a sunspot minimum, but occur even up to the time of sunspot maximum.

5. The successive commencement of the "spot-activity tracks" in higher latitudes between a sunspot minimum and maximum seems to be closely related to the "prominence-activity tracks" at these periods.

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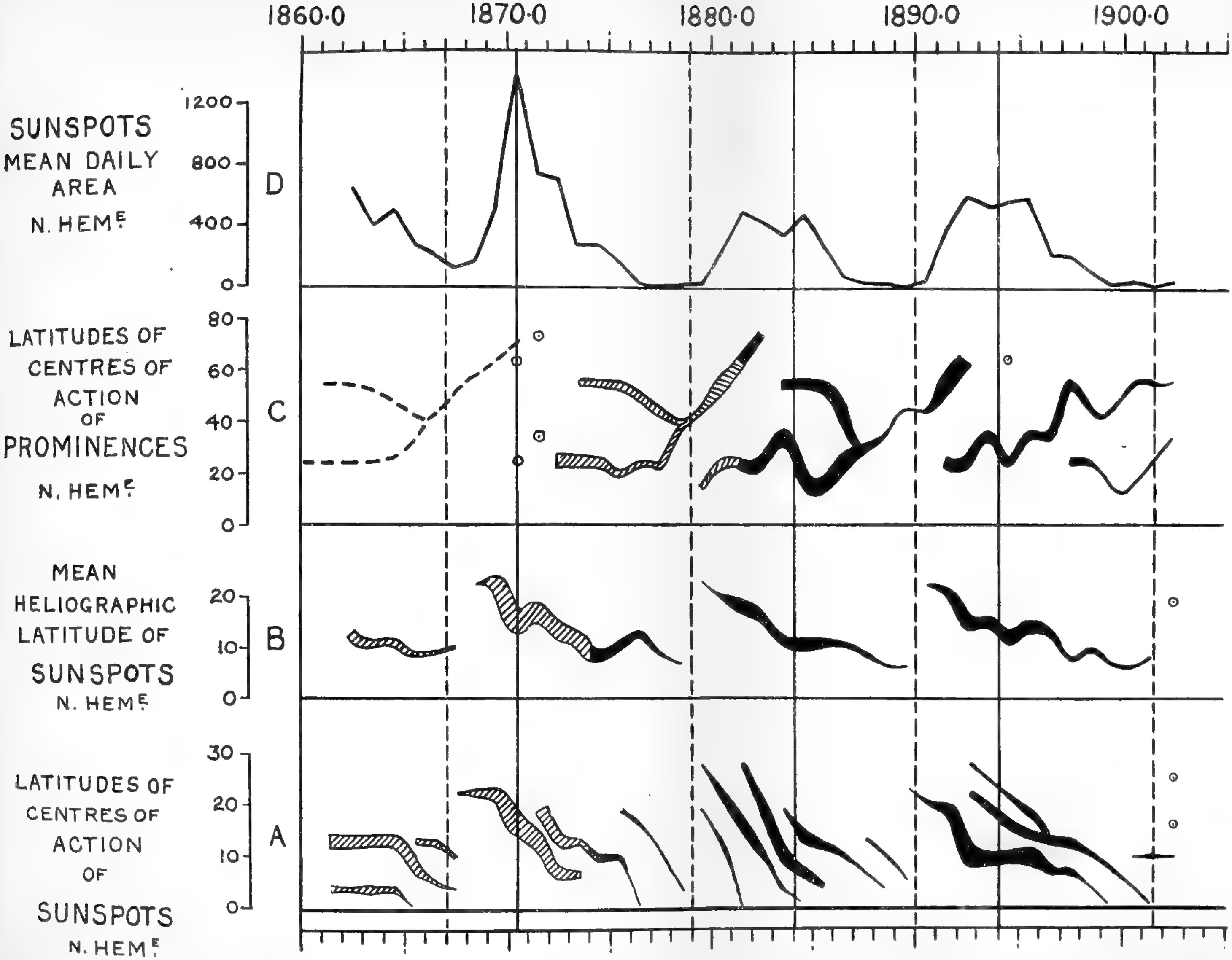

Curves showing the relation between the positions of the centres of action of Sunspots or "Spot-activity Tracks" (A), and Solar Prominences or "Prominence-activity Tracks" (C), the mean heliographic latitude of Sunspots north of the equator (B), and mean daily area of spots (D), for the northern hemisphere of the sun.

Note.—The continuous and broken vertical lines represent the epochs of sunspot maxima and minima as determined from the mean daily areas of the whole solar disc.

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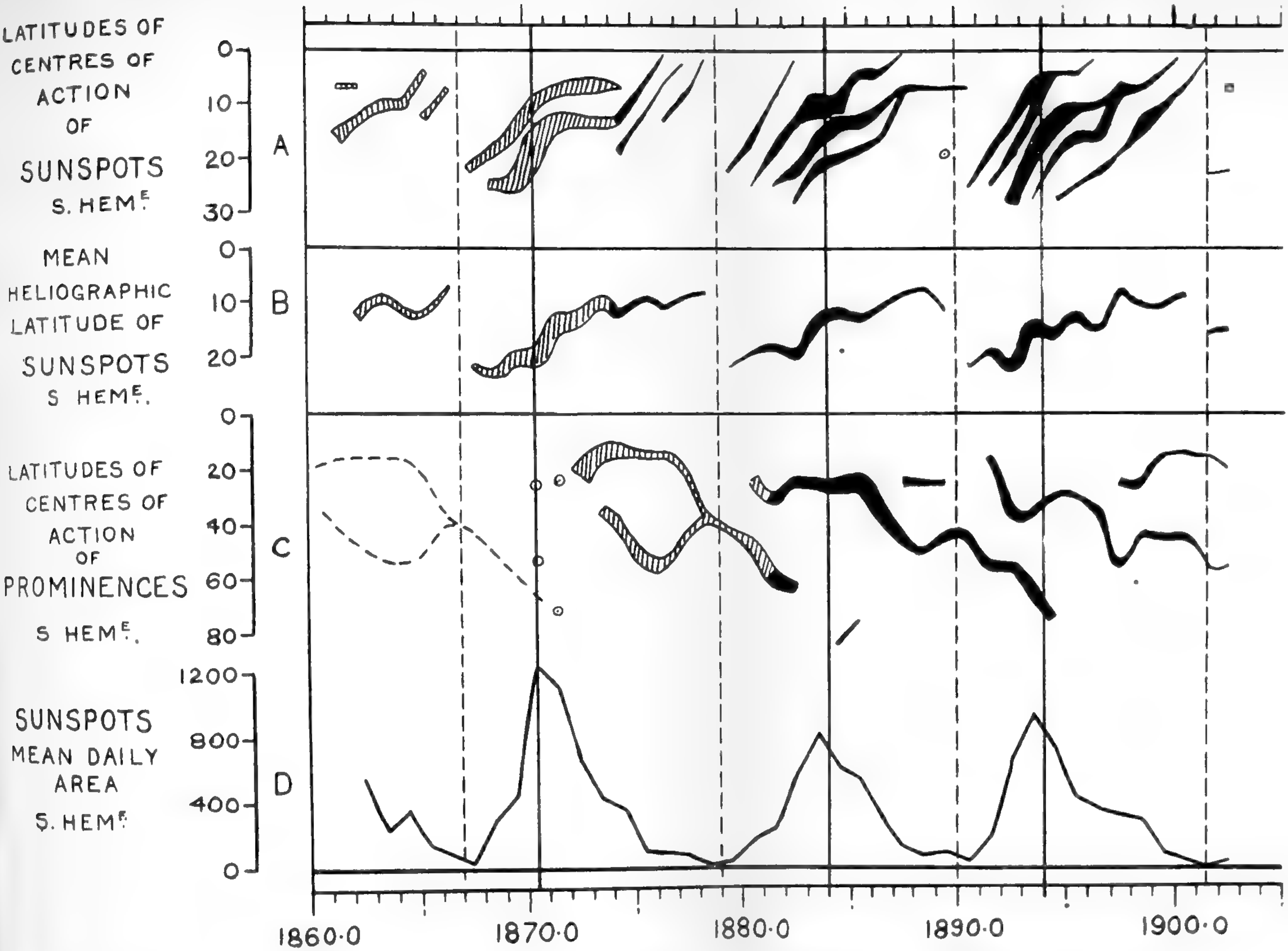

Similar curves to those on Plate 4, only in this case the southern hemisphere of the Sun is referred to. Vertical lines same as Plate 4.

"On the Compressibilities of Oxygen, Hydrogen, Nitrogen, and Carbonic Oxide between One Atmosphere and Half an Atmosphere of Pressure, and on the Atomic Weights of the Elements concerned.—Preliminary Notice." By LORD RAYLEIGH, O.M., F.R.S. Received February 3,—Read February 11, 1904.

The observations now referred to were conducted with an apparatus designed upon the same lines as that already described.* It must suffice to mention that the only important modification lay in the fact that the two single volumes, which, when employed together, constitute the double volume, were used separately and alternately, so as to eliminate in each set of measurements any question as to what the ratio of these volumes exactly is. It is hoped to give a full description of the method when it has been extended to the examination of other gases, such as nitrous oxide and carbonic anhydride. The temperatures ranged from 10°—15°, and care was taken that in each measurement the mean temperatures should be almost exactly the same for the single and for the double volume.

The results were reduced much as previously explained, and give for the values of B, which, according to Boyle's law, should be unity,

Oxygen	1·00040
Hydrogen	0·99976
Nitrogen	1·00017
Carbonic oxide	1·00028

B here denotes the quotient of the value of pv at the half atmosphere by the corresponding value at the whole atmosphere. That it would be less than unity in the case of hydrogen, and exceed unity for the other gases, is what would be anticipated from their behaviour at higher pressures.

If we measure p in atmospheres, and assume, as has usually been done, *e.g.*, by Regnault and Van der Waals, that at small pressures the equation of an isothermal is

$$pv = \mathrm{PV}(1+ap),$$

where PV is the value of the product in a state of infinite rarefaction, then

$$a = 2(1-\mathrm{B}).$$

Probably the chief interest of a knowledge of the coefficient a is

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* "On the Law of the Pressure of Gases between 75 and 150 Millimetres of Mercury," 'Phil. Trans.,' A, vol. 198, pp. 417—430, 1902.

the application to deduce a correction to the relative densities of gases as observed at atmospheric pressure, so as to determine what would be the relative densities in a state of great rarefaction, to which alone Avogadro's law is applicable.*

Taking oxygen as a standard, we see that the small correcting factor to be introduced in order to pass from the ratio of densities at one atmosphere to that at great rarefaction, is $(1+a)/(1+a_0)$, or $1+2(B_0-B)$, the suffix 0 relating to oxygen, that is, as follows:—

Hydrogen..............	1·00128
Nitrogen	1·00046
Carbonic oxide.........	1·00024

The double of the first number, viz., 2·0026, represents, according to Avogadro's law, the volume of hydrogen which combines with one volume of oxygen at atmospheric pressure to form water. Direct determinations by Scott gave 2·00245, and Morley, in his later work, found 2·0027, so that there is here a good agreement.

The following table gives the densities of the various gases, referred to oxygen = 16, at atmospheric pressure and at very small pressure, as deduced from my own observations.†

Gas.	Atmospheric pressure.	Very small pressure.
Hydrogen..............	1·0075	1·0088
Nitrogen	14·003	14·009
Carbonic oxide.........	14·000	14·003

From the researches of M. Leduc and Professor Morley, it is probable that the above numbers for hydrogen are a little, perhaps one thousandth part, too high.

The uncorrected number (14·003) for nitrogen has already been given,‡ and contrasted with the 14·05 obtained by Stas. This question deserves the attention of chemists. If Avogadro's law be strictly true, it seems impossible that the atomic weight of nitrogen can be 14·05.

From the molecular weight of CO, viz., 28·006, we deduce, as the atomic weight of carbon, 12·006.

It should be mentioned that D. Berthelot§ has, meanwhile, calculated very similar numbers, based upon the observations of Leduc.

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* The application to oxygen and hydrogen was made in my paper, "On the Relative Densities of Oxygen and Hydrogen," 'Roy. Soc. Proc.,' vol. 50, p. 448, 1892; 'Scientific Papers,' vol. 3, p. 525.

† 'Roy. Soc. Proc.,' vol. 53, p. 134, 1893; vol. 62, p. 204, 1897; 'Scientific Papers,' vol. 4, pp. 39, 352.

‡ Rayleigh and Ramsay, 'Phil. Trans.,' A, vol. 186, p. 187, 1895.

§ 'Comptes Rendus,' 1898.

"Theory of Amphoteric Electrolytes." By Professor JAMES WALKER, F.R.S., University College, Dundee. Received February 3,—Read February 18, 1904.

During the past few years considerable attention has been devoted to the behaviour of amphoteric electrolytes, *i.e.*, of substances capable of behaving as acids towards bases and as bases towards acids. The most exhaustive investigation of such substances is that by Winkelblech,* who determined by the customary hydrolytic methods the dissociation constants of a number of amino-acids, both with respect to their ionisation as acids and as bases.

The ionisation theory of these amphoteric electrolytes has not yet, however, been fully worked out, although the fundamental equilibrium equations have already been stated by Bredig,† and it is the object of this paper to present the theory from the standpoint of the law of mass action and Arrhenius's theory of electrolytic dissociation.

It is necessary first to deduce Ostwald's dilution law for simple electrolytes in the form in which we shall afterwards meet with it. When an acid, say acetic acid, is dissolved in water, the equilibrium between the ions present in the solution is expressed by means of two equations, one regulating the equilibrium between hydrion and the hydroxidion derived from the water, the other regulating the equilibrium of hydrion and the anion of the acid. Let the active masses (molecular concentrations) of the various substances involved be expressed as follows :

Hydrion H^+	Hydroxidion OH^-	Anion X^-	Unionised acid HX
a	b	c	u

then, from the law of mass action,

$$ab = K \;\ldots\ldots\; (1), \qquad ac = k_a u \;\ldots\ldots\; (2),$$

where K is the constant ionic product for water, which includes within it the constant active mass of water, and k_a is the dissociation constant of the acid. Now in order that the solution may be electrically neutral, the concentration of the positive ion must be equal to the sum of the concentrations of the negative ions, *i.e.*,

$$a = b + c \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

Summing (1) and (2) and substituting a for $b+c$, we obtain

$$a^2 = K + k_a u \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (4).$$

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* 'Zeit. f. physikal. Chem.,' vol. 36, p. 546, 1901.

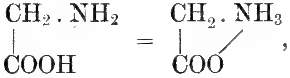
† 'Zeit. f. Elektrochemie,' vol. 6, p. 34, 1899.

For acids of moderate strength the value of a may be obtained from measurements of the electrical conductivity of the solutions, and it has been found that the results are then generally expressible by means of Ostwald's dilution formula,

$$a^2 = k_a u,$$

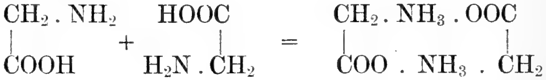
k_a being the dissociation constant of the acid. For all substances accessible to direct electrical measurement, the two expressions are in practice identical, for K at 25° has the value $1{\cdot}2 \times 10^{-14}$, whilst the product $k_a\, u$ has at least the value 10^{-11}. As far then as conductivity measurements are concerned, Ostwald's simple formula may be used instead of the strict theoretical formula.

When we consider the state of an amphoteric substance such as glycine, amino-acetic acid, $NH_2.CH_2.COOH$, in aqueous solution, it is apparent that if it acts both as acid and as base it must give rise to both hydrion H^+ and hydroxidion OH^-, the relative concentrations of which are regulated by equation (1), which holds good for all dilute aqueous solutions whatsoever. Further, if the substance is capable of behaving as acid and as base simultaneously, the acid portion must neutralise the basic portion and form a salt. There are two probable alternatives for the mode of neutralisation, first, the acid and the basic portion of one molecule may neutralise each other, thus



$$\begin{array}{l} CH_2\,.\,NH_2 \\ | \\ COOH \end{array} = \begin{array}{l} CH_2\,.\,NH_3 \\ |\;\diagup \\ COO \end{array},$$

or, second, the acid portion of one molecule may neutralise the basic portion of another, thus



$$\begin{array}{l} CH_2\,.\,NH_2 \\ | \\ COOH \end{array} + \begin{array}{r} HOOC \\ | \\ H_2N\,.\,CH_2 \end{array} = \begin{array}{lcr} CH_2\,.\,NH_3 & . & OOC \\ | & & | \\ COO\;.\;NH_3 & . & CH_2 \end{array}$$

As the former alternative is more simple than the latter, and is in accordance with known facts regarding the molecular weight of such substances in solution, we shall adhere to it in the following deductions.

Since we are unable by the methods here under discussion to distinguish between the unionised isomeric forms $NH_2.CH_2.COOH$ and $\overline{NH_3.CH_2.COO}$, and since by the law of mass action they must always exist is solution in invariable proportions, provided the temperature is constant, we may for present purposes treat them as being one and the same substance. Similarly with regard to the hydrated form $HO.NH_3.CH_2.COOH$ formed from either of the anhydrous forms by addition of water, the mass action law leads to the conclusion that the proportion of it relatively to the total unionised glycine in the solution must be constant.* From this hydrated form we may assume the ions to be derived. If the substance acts as base the ions produced are $NH_3.CH_2.COOH^+$ and OH^-; if it acts as acid the ions are H^+ and $OH.NH_3.CH_2.COO^-$, or the corresponding anhydrous ion $NH_2.CH_2.COO^-$. Ionisation of the internal salt would, of course, give rise to the same positive and negative ions of the glycine as those just mentioned. As a matter of fact, according to the dissociation theory, all of these ions must exist together in aqueous solution of glycine, and we shall now proceed to develope the appropriate equilibrium equations.

In general an amphoteric electrolyte H.X.OH will form the ions H^+, OH^-, $H.X^+$, and $X.OH^-$, the unionised portion being either the hydrous form H.X.OH or the anhydrous form X. Let the active masses of the various substances for equilibrium be represented as under :—

H^+	OH^-	XOH^-	HX^+	HXOH	X
a	b	c	d	e	f

For equilibrium between the various positive and negative ions in pairs we have

$$ab = K \ldots (5), \quad ac = k'_a e \ldots (6), \quad bd = k'_b e \ldots (7), \quad cd = Qf^2 \ldots (8).$$

Here we assume for the moment that the combination of the ions c and d gives rise to the anhydrous form X, in order to show from the ionic equations that e is proportional to f, as has already been deduced from a consideration of the unionised substances alone. Multiplying (6) and (7) together we obtain

$$abcd = k'_a k'_b e^2,$$

or, substituting from (5) and (8),

$$KQf^2 = k'_a k'_b e^2,$$

i.e., the ratio of e to f is constant, the magnitudes k'_a, k'_b, K and Q being all invariable. This result, that the relative proportions of the hydrous and anhydrous forms are independent of the dilution, has already been deduced in another way by Bredig.†

Seeing now that the total unionised substance is always proportional to e, we may rewrite equations (6) and (7) in the form

$$ac = k_a u \ \ldots\ldots \ (6a), \qquad bd = k_b u \ \ldots\ldots \ (7a),$$

in which u represents the active mass of the total unionised solute. The constants k_a and k_b are, of course, different from k'_a and k'_b, and represent the dissociation constants accessible to measurement from hydrolysis experiments or the like.

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* Compare Walker, 'Jour. Chem. Soc.,' vol. 83, p. 182 (1903).

† *Loc. cit.*

From (7a) and (5) we obtain

$$d = \frac{k_b}{K}ua \qquad (9).$$

From (6a) and (5) we obtain similarly

$$c = \frac{k_a}{K}ub \qquad (10).$$

Now the sum of the concentrations of the positive ions must be equal to the sum of the concentration of the negative ions, *i.e.*,

$$a+d = b+c,$$

or, substituting the values of c and d from (9) and (10),

$$a\left(1+\frac{k_b}{K}u\right) = b\left(1+\frac{k_a}{K}u\right) \qquad (11).$$

Multiplying both sides by a, and again making use of (5), we obtain

$$a^2 = \frac{K+k_a u}{1+\dfrac{k_b}{K}u} \qquad (12),$$

or

$$a = \sqrt{\frac{K+k_a u}{1+\dfrac{k_b}{K}u}}.$$

From equation (5) we obtain b in terms of a; the value of d is given by (9); and finally from (10) and (5) we obtain

$$c = k_a ua.$$

We are now able to express the concentrations of the various ions present in the aqueous solution of an amphoteric electrolyte if we know, as is in many cases easily possible, the concentration of the unionised substance, the dissociation constants of the substance acting as acid and base respectively, and the ionisation constant of water.

From the above formulæ it is evident in the first place that the electrical conductivity when treated in the ordinary way forms no measure of the acidity or even of the ionisation of the dissolved electrolyte, for besides hydrion there is the positive ion HX^+, the concentration of which may greatly exceed the concentration of the hydrion, and whose velocity can only be about one-fifth of the velocity of hydrion. The total conductivity is in fact the sum of four terms, each consisting of an ionic concentration multiplied into the corresponding ionic velocity. It may be seen from equation (9) that the concentrations of the two positive ions are equal when $u=K/k_b$. If the ionised proportion is small compared with the unionised, then u is approximately equal to $1/v$, where v is the number of litres in which one gramme-molecule is dissolved, and the concentrations of the positive ions will become equal when v is approximately equal to k_b/K. At greater dilutions a is greater than d; at less dilutions d is greater than a.

In order to see clearly how different an amphoteric electrolyte is in its conductivity relations from a simple electrolyte, whether acid, base, or salt, we may consider the case for which $k_a = k_b$, *i.e.*, where the substance is of the same strength as acid and as base. From (11) we deduce $a = b$, and since from (5) the product ab is constant for all dilute solutions, it follows that the concentration of hydrion and hydroxidion is equal to the concentration of these ions in pure water, *i.e.*, the substance is at all dilutions absolutely neutral. Its solutions, therefore, behave in this respect like those of a neutral salt, but differ from them in the effect of dilution on the molecular conductivity. From (9) and (10) namely, we find that $c = d$, and that $c + d$ is proportional to u, since a is here constant. The ionised proportion is thus a constant fraction of the total dissolved substance independently of the concentration, and consequently the molecular conductivity is independent of the dilution. Comparing different substances of this type with each other, the proportion ionised is seen to vary directly as k. If a substance at 25° had $k_a = k_b = 1{\cdot}2 \times 10^{-7}$, that is, if the acidic and basic constants were more than 100 times less than those of acetic acid or ammonia, the value of d derived from (9) would be $1{\cdot}1u$, or the substance would be ionised to the extent of 52 per cent. at all dilutions, and therefore a good electrolyte.

Comparing generally the expression $a^2 = \dfrac{K + k_a u}{1 + \dfrac{k_b}{K} u}$ deduced for an amphoteric electrolyte, with the expression $a^2 = K + k_a u$ deduced for a simple acid with the same acid constant, we see that the former is equal to the latter divided by $1 + \dfrac{k_b}{K} u$. When $k_b = 0$, that is, when the electrolyte has no basic character, the divisor becomes equal to unity, and the expression for a simple acid is obtained. When k_b/K and u have finite values, it is obvious that the amphoteric electrolyte cannot strictly obey Ostwald's dilution law. If, however, either k_b/K or u is very small, Ostwald's dilution law is approximately followed, for then the values of a from the simple and amphoteric formulæ become nearly equal, and the expression $d = \dfrac{k_b}{K} ua$ for the concentration of the other positive ion nearly vanishes. The smaller the basic dissociation constant, then, and the greater the dilution, the more likely is the amphoteric electrolyte to follow the dilution law characteristic of simple acids and bases.

Of the amphoteric substances measured by Winkelblech* all show a ratio k_b/K at least equal to 100. In order then that the expression $\frac{k_b}{K}u$ should become small in comparison with unity, the concentration of the unionised substance (which is roughly equal to the total concentration for the compounds he investigated) must be of the order 10^{-4}, that is, v must be of the order 10,000. Solutions of feeble electrolytes having this dilution are practically beyond our ordinary means of measurement of electrical conductivity, so that we may conclude that at customary dilutions the amphoteric electrolytes studied by Winkelblech cannot give a dissociation constant when the values of the conductivity are treated in the ordinary way. This conclusion is in accordance with Winkelblech's electrical measurements, from which he endeavoured in five instances to calculate a dissociation constant. The values he obtained at different dilutions varied for each substance from 20 to 50 per cent.

In order to exhibit the effect of the presence of even feebly marked basic character in an amphoteric acid, I have calculated the concentrations of the positive ions H^+ and HX^+ for substances possessing the acid constant $k_a = 10^{-5}$ and basic constants $k_b = 1{\cdot}2 \times 10^{-14}$, $1{\cdot}2 \times 10^{-13}$, $1{\cdot}2 \times 10^{-12}$, and $1{\cdot}2 \times 10^{-11}$ respectively, the corresponding ratios k_b/K at 25° being 1, 10, 100, and 1000. The calculation was made for the most part by approximation, it being assumed in the first instance that the total concentration was equal to the concentration of the unionised substance. A second approximation in which the value of u obtained from the first calculation was adopted usually sufficed. If a great many dilutions have to be calculated, the employment of graphical methods may effect a saving of time.

In the following table all values of a and d have been multiplied by 10^5. For comparison, the values for $k_b/K = 0$, *i.e.*, for a simple acid have been added.

$k_a = 10^{-5}$.

v.	$k_b/K = 0$.		$k_b/K = 1$.		$k_b/K = 10$.		$k_b/K = 100$.		$k_b/K = 1000$.	
	a.	*d.*	*a.*	*d.*	*a.*	*d.*	*a.*	*d.*	*a.*	*d.*
1	316	0	224	223	95·3	943	31·5	3050	9·99	9091
10	100	0	95	8·5	70·5	69·5	30·1	291	9·94	904
100	31·2	0	31·0	0·3	29·7	2·9	22·1	21·0	9·49	86
1000	9·5	0	9·5	0·0	9·4	0·1	9·06	0·8	6·79	6

The values of a for a given dilution fall off as k_b increases, and that

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* *Loc. cit.*, p. 587.

the more rapidly as the dilution is small. It will be noticed that d varies with the dilution much more rapidly than a, which for high values of k_b becomes nearly independent of the dilution. What is most deserving of attention is that although for $k_b/K = 1000$ the value of the acid constant k_a is still nearly a million times greater than the value of the basic constant k_b, the acidity of the amphoteric substance is greatly diminished at small dilutions, being for example, at $v = 10$ only one-tenth of that of a simple acid with the same constant. Although the concentration of the chief conducting ion of acids is thus greatly diminished, this diminution may be more than compensated by the comparatively great concentrations of the slower ion HX^+ which appear at the same low dilutions.

With regard to the negative ions OH^- and XOH^-, it may be seen from (5) that in the cases above considered b cannot exceed the value 10^{-10}, and may thus be neglected in comparison with the other ions. It follows that the concentration of the remaining negative ion c is equal to the sum of the concentrations of the positive ions, viz., $a+d$.

All calculations made from the conductivities of solutions of amphoteric electrolytes have hitherto proceeded on the assumption that the same method of treatment might be adopted as that applicable to simple electrolytes. This is, as we see, far from being the case, and we may now consider what manner of results amphoteric electrolytes with the above constants would yield if their conductivities were treated in this erroneous fashion. Perhaps this is rendered most clearly apparent by calculating what values the Ostwald dissociation "constant" would assume at different dilutions when deduced by the ordinary process from the conductivities.

The molecular conductivity μ_∞ corresponding to H^+, XOH^- at infinite dilution may be taken as from 350—370 at 25° when referred to reciprocal Siemens units. The molecular conductivity of HX^+, XOH^- under the same conditions would be 60—70. From every concentration of HX^+, then, we may obtain a concentration of H^+ having equal conducting power, by dividing d by a number varying from 5 to 6 according to the substance considered. If we add this quotient to the real value of a we obtain a false value α, which is assumed as the value of a in the simple calculation of the dilution constant. The subjoined table contains the values of the apparent Ostwald dilution constant k_o for the amphoteric electrolytes considered above when calculated in the customary way from the values $\alpha = a+d/5$ and $\alpha = a+d/6$. In each case the constant has been multiplied by 10^5.

For $k_b/K = 1$ it will be observed that a fairly good constant k_o may be got, the values for the greater dilutions approximating within the limits of experimental error to the true value k_a. For $k_b/K = 10$ the values of k_o are no longer even approximately constant, increasing rapidly with the dilution to attain a value at $v = 1000$ approaching

$k_o \times 10^5$ calculated from $\alpha = a + d/5$ and $\alpha = a + d/6$.

v.	$k_b/K = 0$.		$k_b/K = 1$.		$k_b/K = 10$.		$k_b/K = 100$.		$k_b/K = 1000$.	
	$a + d/5$.	$a + d/6$.	$a + d/5$.	$a + d/6$.	$a + d/5$.	$a + d/6$.	$a + d/5$.	$a + d/6$.	$a + d/5$.	$a + d/6$.
10	1·0	1·0	0·944	0·938	0·718	0·679	0·780	0·622	3·30	2·62
100	1·0	1·0	0·992	0·991	0·947	0·940	0·710	0·672	0·73	0·58
1000	1·0	1·0	0·993	0·993	0·991	0·988	0·936	0·930	0·69	0·65

the true value. For $k_b/K = 100$ we have in the ordinary range of dilutions a fall to a minimum for k_o, which is apparent when $\alpha = a + d/5$ is used, and also occurs between $v = 10$ and $v = 100$ when $\alpha = a + d/6$ is employed. In this case k_o at $v = 1000$ is about 7 per cent. beneath the true value of k_a. Finally, with $k_b/K = 1000$ the value of k_o at $v = 10$ greatly exceeds the true value, and falls very rapidly with the dilution to reach a minimum at high dilutions which will generally appear in the usual range investigated. Here k_o at $v = 1000$ is 30—35 per cent. short of the true value.

For other values of k_a than that used in the above calculations the change in the value of k_o with the dilution is similar, because, as may be deduced from the formulæ on pp. 157—158, the relative values of α for two electrolytes with the same k_b do not vary greatly with the dilution, being at all dilutions approximately proportional to the square roots of the acid constants as long as u does not differ sensibly from $1/v$.

Turning now to the experimental data, we find that Ostwald obtained for ortho-amino-benzoic acid, 1 : 2-$NH_2C_6H_4COOH$, values of k_o which steadily increased with increasing dilution,* confirmation of this result being subsequently furnished by Winkelblech.† Their numbers are given in the following table :—

v.	k (Ostwald).	k (Winkelblech).
64	$0·66 \times 10^{-5}$	$0·65 \times 10^{-5}$
128	0·74	0·74
256	0·84	0·84
512	0·92	0·91
1024	0·96	0·97

Ostwald accounted for the rise in the value of the constant by adopting a suggestion of Wislicenus, that double molecules might be formed according to the scheme referred to on p. 156, and that the breaking up of these double molecules at increasing dilutions into simple ionisable molecules would occasion a greater increase of ionisa-

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* Ostwald, 'Zeit. für physikal. Chem.,' vol. 3, p. 261 (1889).

† Winkelblech, *loc. cit.*, p. 564.

tion as dilution progressed than that corresponding to Ostwald's dilution law. This assumption, however, is unnecessary, the theoretical discussion already given being competent to explain the facts.

The increase in the value of k_o in the range of dilution examined is obviously of the same order as that calculated for $k_b/K = 100$. We should therefore expect to obtain from hydrolysis experiments conducted with the hydrochloride of the amino-acid a value for the ratio approximating to this number. Winkelblech found from the catalysis of methyl acetate $k_b/K = 112$ in excellent accordance with the theory. He also determined the same ratio from the conductivity of solutions of the hydrochloride and obtained the divergent value 164. The same discrepancy was observed with most of the other substances he examined. If we consider, however, that the catalysis method measures the concentration of one ion only, viz., hydrion, whilst the conductivity method is concerned with a complex equilibrium among at least five kinds of ions, we see that the former is likely to yield the more accurate results. This view receives confirmation from Winkelblech's own data. For the few substances with which there is agreement between the catalysis and conductivity methods, it is found that the latter gives concordant values for the ratio at all dilutions. Where, on the other hand, the two methods do not yield the same result, there are also wide divergences amongst the values of the ratio derived from the conductivities at different dilutions. The presumption, therefore, is that the catalysis method is more trustworthy than the other. It may be noted in general that the discrepancy is great for monobasic amphoteric acids when the acid constant is great.

Knowing the ratio k_b/K, the value of k_a, and the speeds of the various ions, it is possible to calculate the molecular conductivity, and from this the apparent Ostwald dilution constant k_o. If we take from the preceding table $0{\cdot}96 \times 10^{-5}$ as the value of k_o at $v = 1000$, we obtain $1{\cdot}02 \times 10^{-5}$ as the approximate value of k_a, since the table on p. 162 shows that k_a exceeds k_o by 6—7 per cent. With regard to the speeds of the ions we may adopt Winkelblech's value $\mu_\infty = 357$ as the sum of the velocities of hydrion and the anion. There is less certainty as to the sum of the velocities of kation and anion. The velocities attributed by Winkelblech to the kation in this and in similar instances are, in my opinion, considerably overestimated. For the amino-benzoic acids he does not give directly the experimental data from which he estimated the velocity of the kations, but presumably the values were obtained by the same method as that which he adopted for other substances, viz., by measurement of the conductivity of the hydrochloride in presence of excess of base.* Owing to the very considerable hydrolysis of such substances in aqueous solution, the results obtained for μ at the experimental dilutions

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* Compare Bredig, 'Zeit. für physikal. Chem.,' vol. 13, p. 214 (1894).

must be somewhat too high, and in consequence the speed of the kation is estimated at too high a figure. In order to proceed in a systematic way, I have added 6 in each case to the anion values found by Winkelblech for the amino-benzoic acids, and adopted the figures so obtained as the kation velocities in the succeeding calculations. The value of μ_∞ thus estimated is about 7 per cent. below that obtained by adopting Winkelblech's velocity for the kation. For the ortho-acid we have $\mu_\infty = 32 + 38 = 70$.

Calculating with these constants, we arrive at the values given in the following table :—

o-Amino-benzoic Acid, 1 : 2-$NH_2.C_6H_4.COOH$.

$k_b/K = 112$, $k_a = 1{\cdot}02 \times 10^{-5}$, $\mu_\infty H^+,XOH^- = 357$, $\mu_\infty HX^+,XOH^- = 70$.

v.	*a.*	*d.*	μ calc.	μ expt.	k_o calc.	k_o (O.).	k_o (W.).
64	$23{\cdot}9 \times 10^{-5}$	$40{\cdot}1 \times 10^{-5}$	7·26	7·21	6·6	6·6	6·5
128	20·3	17·0	10·8	10·8	7·4	7·4	7·4
256	16·3	6·7	16·1	16·2	8·3	8·4	8·4
512	12·3	2·5	23·3	23·6	8·9	9·1	9·2
1024	9·0	0·88	33·5	33·7	9·5	9·7	9·6

The experimental values of the molecular conductivity μ are the means of the concordant series of Ostwald and Winkelblech. It will be seen that the agreement between these and the calculated values is very close. For comparison the "constants" $k_o \times 10^6$ derived from the calculated and the experimental values of μ have been added.

The conductivities of solutions of para-amino-benzoic acid have been measured by the same observers, and Winkelblech determined the ratio k_b/K, which he found by the catalytic method to be 210. With this constant the value of k_a appears to be nearly 10 per cent. above the value of k_o at $v = 1000$. Adopting for the latter the mean of the numbers found by Ostwald and Winkelblech, viz. $1{\cdot}11 \times 10^{-5}$, we obtain $k_a = 1{\cdot}21 \times 10^{-5}$.

p-Amino-benzoic Acid, 1 : 4-$NH_2.C_6H_4.COOH$.

$k_b/K = 210$, $k_a = 1{\cdot}21 \times 10^{-5}$, $\mu_\infty H^+,XOH^- = 356$, $\mu_\infty HX^+,XOH^- = 68$.

v.	*a*	*d.*	μ (calc.)	μ (O.)	μ (W.)	k_o (calc.)	k (O.)	k_o (W.)
32	$22{\cdot}3 \times 10^{-5}$	$138{\cdot}7 \times 10^{-5}$	5·55	—	5·43	7·7	—	7·4
64	20·9	64·4	7·55	7·53	7·49	7·2	7·2	7·1
128	18·7	28·7	11·00	10·86	11·12	7·7	7·5	7·9
256	15·8	12·0	16·47	16·34	16·84	8·8	8·7	9·1
512	12·5	4·64	24·37	24·24	26·29	9·8	9·8	10·6
1024	9·4	1·70	35·40	35·01	36·86	10·7	10·5	11·7

The values of μ calculated from the theory agree very well with those found by Ostwald, somewhat less well with those of Winkelblech. The most interesting point about the "constant" k_o is that there is a minimum in both the calculated and experimental values at $v = 64$.

The experimental data for meta-amino-benzoic acid are not so satisfactory as those for the isomeric acids just considered. Ostwald and Winkelblech found widely divergent values of the conductivity. Since in the following calculations use is made of Winkelblech's constants, the comparison of conductivities can only be effected with his numbers. The value of k_b/K found by the hydrolysis method is in round numbers 1100. This involves an addition of over 30 per cent. to k_o at $v = 1000$ in order to arrive at an approximate value of k_a, which in this case comes to be $1 \cdot 4 \times 10^{-5}$.

m-Amino-benzoic Acid, 1 : 3-$NH_2.C_6H_4.COOH$.

$k_b/K = 1100$, $k_a = 1 \cdot 4 \times 10^{-5}$, $\mu_\infty H^+,XOH^- = 355$, $\mu_\infty HX^+,XOH^- = 66$.

v.	*a*.	*d*.	μ calc.	μ (W.).	k_o calc.	k_o (W.).
64	$10 \cdot 9 \times 10^{-5}$	$163 \cdot 0 \times 10^{-5}$	9·36	9·36	11·1	11·2
128	10·6	79·2	11·51	11·72	8·5	8·8
256	9·99	36·2	15·20	16·04	7·5	8·4
512	9·07	16·5	22·06	23·04	8·0	9·1
1024	7·73	6·88	32·77	35·24	9·2	10·7

Here it will be noted that the agreement is by no means so close as in the previous instances. This is probably connected with the fact that the meta-acid rapidly oxidises in contact with platinum electrodes, and assumes a dark brown colour. The effect of this oxidation would be most apparent in the dilute solutions. Notwithstanding the want of exact accordance, the run of the constants is similar, both the values calculated from the theory and the values given by Winkelblech exhibiting a minimum at $v = 256$.

These instances exhaust the data in Winkelblech's paper for which an exact comparison of the theory with experiment is possible. They cover a range for k_b/K of 110—1100, and in each case the peculiarities of the "constants" are faithfully reproduced by the theory. An experimental investigation of some other substances to which the theory is applicable is at present in progress, and I hope in a future paper to communicate the results obtained.

"The Electromotive Phenomena in Mammalian Non-medullated Nerve." By N. H. ALCOCK, M.D. Communicated by A. D. WALLER, M.D., F.R.S. Received December 15, 1903,—Read February 25, 1904.

(From the Physiological Laboratory of the University of London.)

Up to the present time there would appear to have been no published researches on isolated mammalian non-medullated nerve, and indeed, except for the paper of Brodie and Halliburton,* all our knowledge of these nerves has been either by inference from similar nerves in cold-blooded animals or derived incidentally from experiments undertaken for a different object.

When it appeared, therefore, that the technique for mammalian medullated nerve served equally well for the non-medullated,† it became a matter of interest to examine the phenomena displayed by the latter, and the very evident advantages of dealing with nerves of considerable size and comparative longevity, and the possession of a ready standard of comparison in the medullated nerves of the same animal, greatly assisted in obtaining an exact result.

Methods.

The splenic nerves were found to be very suitable objects for this purpose. They consist almost entirely of non-medullated fibres,‡ and in the horse, which is the animal which has been used for these experiments, the various bundles form a plexus around the splenic artery, which can be separated by careful dissection into its component parts, giving isolated pieces of nerve 1—1·5 mm. in diameter and from 6—8 cm. in length. These pieces will commonly retain their irritability for several hours if kept in 1·05 per cent. salt solution at 18° C., into which they are placed from a quarter to half an hour after the death of the animal. Waller's§ galvanographic method was employed as well as the capillary electrometer.||

In the experiments on the negative variation, the exciting current was derived from an accumulator of large capacity, in order to secure the greatest possible constancy, and the excitation was maximal, except in Experiment 716. The temperature at which the experiments were

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* Brodie and Halliburton, 'Journ. of Physiol.,' vol. 28, p. 181.

† 'Roy. Soc. Proc.,' February, 1902, p. 264.

‡ The proportion of non-medullated to medullated fibres varies in different animals; sections made from the nerves actually used showed that medullated fibres formed less than 0·5 per cent. of the total number.

§ Waller, "Signs of Life," 1903.

|| The analysis of the electrometer records will be considered at a future time.

conducted was 17—19° C., except where otherwise stated. The current of injury was balanced against an equal fraction of a volt, and the value read off. This compensation was maintained throughout the experiment. No current, therefore, flowed through the nerve when at rest.

The experiments were ordinarily carried out in duplicate, with both medullated and non-medullated nerves from the same animal.

The electromotive phenomena are considered in the present paper under two heads:—

1. Negative variation.
2. Electrotonic currents.

1. NEGATIVE VARIATION.

(*Experiments.*)

The electromotive phenomena in the nerves of the horse resemble those in the nerves of other mammalia in kind, but differ in degree, being of considerably less magnitude.* This is due, in part, to the large amount of connective tissue surrounding the nerves, and forming a derivation circuit. When this connective tissue is dissected off, the effect on the galvanometer is increased. Three experiments may be quoted on this point:—

Table I.

Experiment.		Nerve.	Current of injury.	Negative variation.	Notes.
			millivolts.	millivolts.	
720 (A)	*a*...	Median	2·7	0·172	
	b...	"	4·1	0·309	
747	*a*......	"	8·2	0·242	
	b......	"	8·5	0·359	
749	*a*......	Splenic	9·7	0·504	Whole nerve.
	b......	"	11·3	1·910	Single nerve-bundle.

The nerve was isolated in the usual way and the measurements made that are marked *a*, the connective tissue was then dissected off as far as possible, and the measurements *b* taken. Both injury current and negative variation are increased, the latter more than the former. But it will be noticed that even in the most favourable case the voltage of the negative variation is less in the medullated nerves of the horse

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* This has already been observed in the case of the current of injury, see Gotch, 'Schäfer's Text-book,' vol. 2, p. 520, and Biedermann, 'Elektrophysiologie,' 1895, p. 638.

than is usual in the mammalia. It is not certain how far this is due to the greater amount of connective tissue present between the individual nerve-fibres, or to some other cause.

Both current of injury and negative variation are considerably greater in the non-medullated nerves of the horse than in the medullated. The following table gives the result of several observations :—

Table II.

Nerve.	Current of injury.*		No. of experiments.	Negative variation.*		No. of experiments.
	Mean.	Max.		Mean.	Max.	
Median	3·86	5·3	5	0·380	0·83	4
Splenic	5·81	15·5	13	0·860	2·3	11

These are the values obtained with the galvanometer. They are probably too low, and for several reasons it is likely that the maximum values are more nearly correct than the mean. They are, however, strictly comparative, and it is seen the voltages in the splenic nerves are approximately three times the median, a result agreeing with the observations on cold-blooded animals.† The non-medullated nerves of

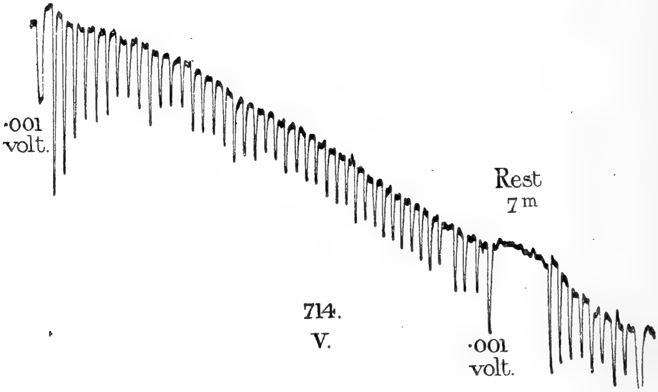

Fig. 1 (Exp. 714, V.).—Splenic of Horse. The vertical lines are the successive negative variations produced by tetanising currents (5000 Berne units, 2 volts in primary circuit) for 13 seconds, repeated once a minute.

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* Here and elsewhere in millivolts.

† Kühne u. Steiner, 'Unters. d. Physiol. Inst. d. Univ. Heidelberg,' vol. 3, p. 149; Sowton, 'Roy. Soc. Proc.,' vol. 66, p. 379.

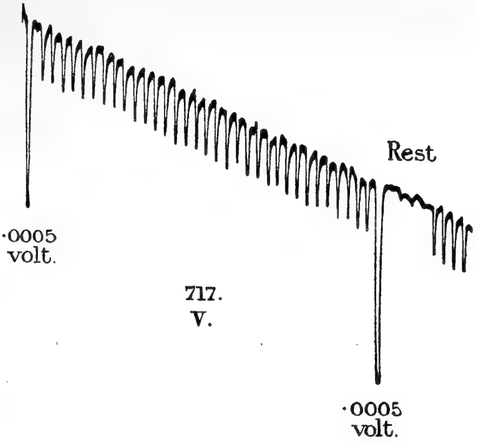

FIG. 2 (Exp. 717, V.).—Median of same animal as Fig. 1. All details as before.

the horse commonly retain their excitability for 8—10 hours *post-mortem*, the medullated for about half this time.

I have been unable so far to trace any quantitative relation between the current of injury and the negative variation as determined above.

Obtaining a non-medullated nerve, subjecting it to a series of repeated tetanisations, and recording the successive negative variations by the galvanographic method, it appears at once that the results are very different from the corresponding phenomena in medullated nerve. Figs. 1 and 2 are records from two nerves, the one splenic and the other median, from the same animal under identical conditions. The successive negative variations from the latter are approximately equal, those from the former fall off very rapidly, and this rapid decrease in the negative variation is characteristic of mammalian non-medullated nerve. The following experiments were made:—

Table III.—Splenic Nerves.

Experiment.	Neg. var. = *a*. Initial.	Neg. var. = *b*. Final.	Duration of experiment.	Diminution *b*/*a*.	Notes.
	millivolts.	millivolts.	mins.		
702. Horse I	0·95	0·33	11	0·35	Exc. lasting 10 seconds, once a minute.
704. ,, I	0·376 (1st) 0·397 (2nd)	0·219	25	0·582	Same nerve as Exp. 702.
705. ,, II	0·491 (1st) 0·576 (2nd)	0·109	22	0·222	
707. ,, III	0·317	0·131	26	0·413	
709. ,, III	0·560	0·396	30	0·710	
712. ,, IV	0·788 (1st) 0·847 (2nd) 0·878 (3rd) 0·792 (4th)	0·672 (23rd)	23	0·853	Same nerve as in Exp. 710 below, "staircase."
713. ,, IV	0·859	0·392	29	0·456	Same nerve as Exp. 710.
716. ,, V	0·593 (1st) 0·729 (2nd) 0·663 (3rd) 0·607 (4th)	0·215	50	0·362	Submaximal excitation, 500 units, "staircase."
Mean of eight experiments	0·617	0·307	27	0·498	

The following experiments were made under identical conditions with medullated and non-medullated nerves from the same animal:—

Table IV.

Experiment.	Nerve.	Neg. var. Initial = *a*.	Neg. var. Final = *b*.	Duration of experiment.	Diminution, *b*/*a*.	Notes.
710. Horse IV	Splenic	0·884 (1st) 1·020 (2nd) 0·951 (3rd)	0·600	31	0·679	"Staircase."
711. ,, ...	Median	0·219 (1st) 0·233 (2nd)	0·241	31	1·10	,,
714. Horse VI	Splenic	2·32	0·636 (33rd) 0·593 (42nd)	33 42	0·274 (0·256)	The bracketed figures refer to the continuation of the experiment beyond the comparison point (figs. 1 and 2).
717. ,, ...	Median	0·154	0·125	33	0·812	
720, B. Horse VII	Median	0·292	0·294	22	1·01	For the analysis of the latter portion of the experiments see Table V.
721. ,, ...	Splenic	1·60	0·500 (22nd) (0·372) (52nd)	22 52	0·313 (0·233)	
Mean of Expts. 710, 714, 721	Splenic	1·601	0·579	29	0·382	
Mean of Expts. 711, 717, 720, B.	Median	0·222	0·220	29	0·991	

These tables show very clearly the alteration in the negative variation by successive excitations. The decrease is most rapid at first, and after a little proceeds quite slowly, especially if the excitation is sub-maximal, so that if the result of the first few minutes is discarded, there remains a considerable period during which the diminution is small, and which can be used to test any desired procedure (*vide* figs. 1 and 6). The effect of rest is that the subsequent responses are greater for a time, but soon fall off (fig. 1), and the difference in this respect from the medullated nerves is very marked (fig. 2); in the latter the subsequent negative variations are almost invariably less after a pause.

This difference is very clearly seen in the electrometer photographs (Experiment 750, B, fig. 3, splenic of horse, and Experiment 751, fig. 4,

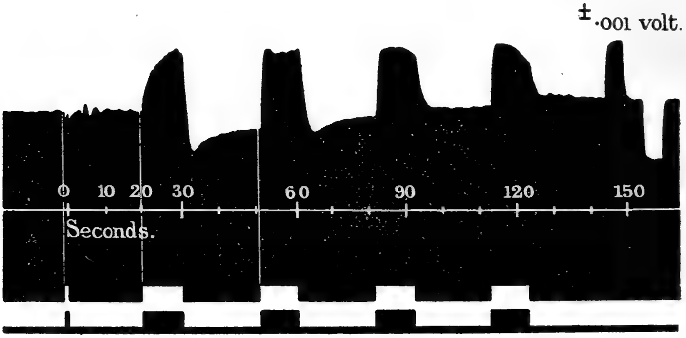

Fig. 3 (Exp. 750, B,).—Non-medullated Nerve, Splenic of Horse. Excitation for 10 seconds (Tet.), interval 20 seconds, as shown by the lower interrupted line.

Time—1 mm. = 2 seconds.

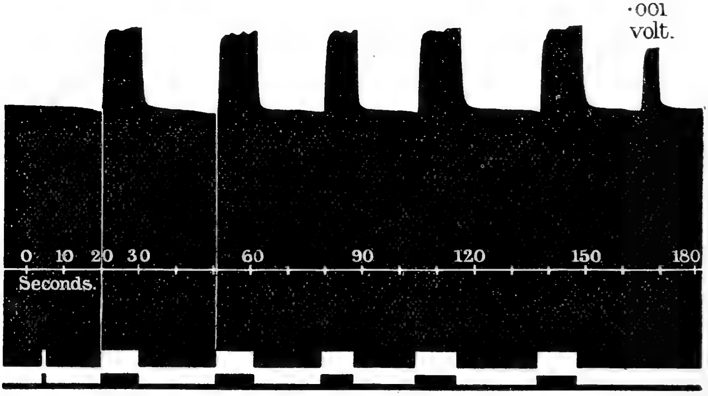

Fig. 4 (Exp. 751).—Medullated Nerve, Ulnar of Cat. Details as in Fig. 3.

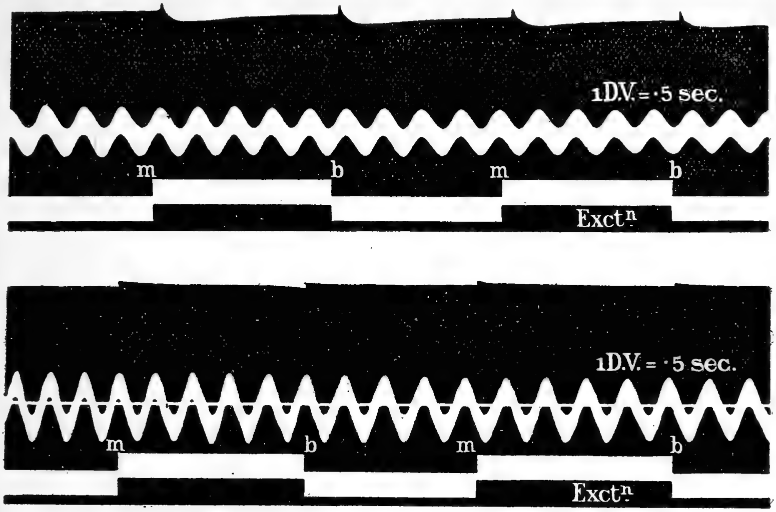

Fig. 5 (Exps. 750 B and 751).—Response of Non-medullated Nerve (upper) and Medullated (lower) to Single Shocks. Same nerves as figs. 3 and 4.

ulnar of cat). The full analysis of these will be considered in a subsequent paper, but these figures show (*inter alia*) that the electrical resistance plays an unimportant part in the production of the phenomena observed, and that the rate of transmission of the electrical effect is very much less in the non-medullated nerves.

As, therefore, the progressive diminution in the negative variation is characteristic of non-medullated nerve, both here and in cold-blooded animals,* further experiments were undertaken to ascertain whether this was due to events occurring along the whole length of the nerve, or to changes localised at the place of excitation. Two pairs of platinum wire electrodes were used for stimulation, placed respectively further and nearer the leading-off electrodes; the nerve was excited first at the "far" pair and then at the "near."

In Experiment 718 (fig. 6) the result of exciting through the near pair of electrodes was to increase the negative variation to very nearly the original amount (*a*). After 12 minutes of excitation at this point the current was again sent through the "far" electrodes, and the resultive negative variation (*c*) was much less than the original value. If the nerve had been simply resting, instead of being excited at a proximal place, the negative variation would have been increased (see fig. 1); *the effect, therefore, of passing a recently excited spot is diminution.*

In Experiment 719 the excitation was through three pairs of

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* Sowton, *loc. cit.*

Table V.

Experiment.	Far.			Near.			Notes (see below).
	Negative variation.		Time.	Negative var.		Time.	
	Initial.	Final.		Initial.	Final.		
			min.			min.	
716. Horse VI	a 0·694 (1st) 0·839 (2nd)	0·419	29	b 0·800	0·372	12	Fig. 6. a = far. b = near. c = far again.
	c 0·166	0·20	6				
719. " VI	a 0·111 (1st) 0·144 (2nd)	0·1109	24	b 0·400	0·138	18	a = 1st pair. b = 2nd pair. c = 3rd pair. c* = 3rd pair reversed.
				c 0·322	0·092	14	
				c* 0·092	0·115	3	
721. " VII	a 1·60	0·372 (0·496)	52 (17)	b 1·22	0·764	17	
720, B. " VII	a 0·292	0·294	22	b 0·355	0·378	20	Control with medullated nerve. a = far. b = near. c = far again.
	c 0·253	0·281	5				

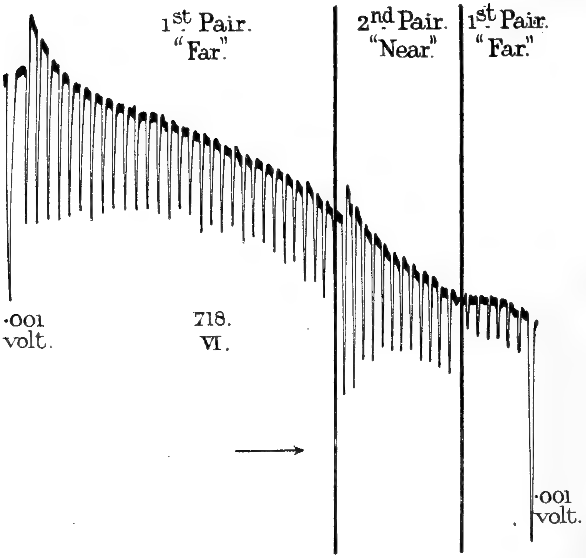

Fig. 6.—Splenic of Horse. Negative Variations. Result of exciting at (1) far, (2) near, and (3) far electrodes.

electrodes, and then the excitation was reversed; the alteration produced by reversal was very small.

Experiments 721 and 720 were with non-medullated and medullated nerves under the same conditions.

Considering the result of all these experiments, it is clear that the diminution of the negative variation in non-medullated nerves is due to changes occurring at the point where the nerve is excited. Using a constant stimulus this spot becomes less and less excitable, in the sense that the response becomes progressively smaller. The control experiments on medullated nerve show but slight traces of this effect,* either the medullary sheath prevents this loss of excitability in some way, or the two classes of nerves differ very widely in their reaction to stimuli. Speaking broadly, the evidence is in favour of the former hypothesis.

While the work of Sowton† and Garten‡ on cold-blooded nerves is in accord with my results on mammalian nerve, the paper of Brodie and Halliburton at first sight offers a contradiction. These experimenters excited the splenic nerves in the dog for many hours, and blocking the impulse by cold, observed that when the block was removed the splenic contractions followed as at first, apparently unaltered in amount. I have no doubt as to the correctness of their observations, and the results they obtained, differing from those

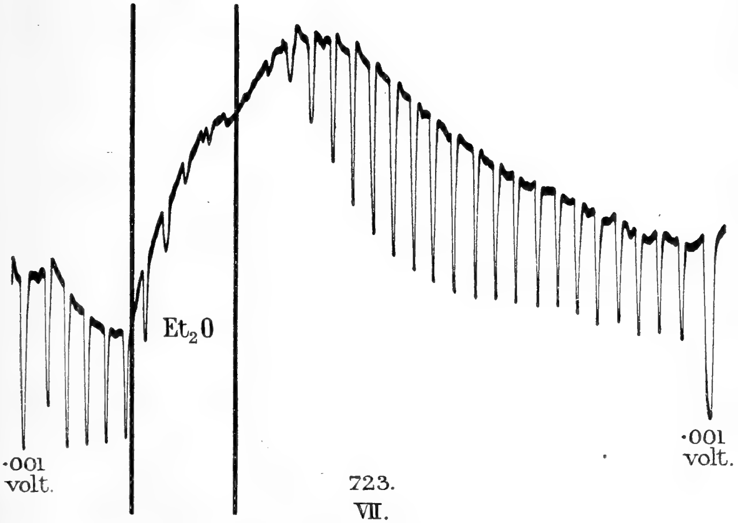

Fig. 7.—Splenic of Horse. Effect of Ether Vapour.

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* Dr. Waller has very kindly permitted me to measure a considerable number of his photographic records of frog's nerve, of 1896—1897; in all a small regular diminution is detectable, amounting to from 2·0 to 5·6 per cent., in experiments lasting 40 minutes.

† Sowton, *loc. cit.*

‡ Garten.

recorded above, may possibly be explained by supposing that the splenic contractions are not as delicate an index of the condition of the splenic nerves as the galvanometric response. Waller* has shown that with a gradually increasing stimulus the voltage of the negative variation reaches its maximum much later than the contraction of the attached muscle, and as in all the experiments here recorded a certain amount of the negative variation still persisted, it is possible that even this fraction—perhaps one-third of the initial value—indicated a sufficient intensity of nerve impulse to give a maximal splenic contraction.

If the medullary sheath has any such action as the hypothesis suggested above necessitates, the inquiry may be extended to see if any light can be thrown on the manner in which this sheath acts. The following experiments on electrotonic currents were, therefore, undertaken.

2. ELECTROTONIC CURRENTS.

Methods.

The nerve rested upon two pairs of non-polarisable electrodes. The distal pair led off to the galvanometer; the proximal were connected with the automatic reverser, used by Dr. Waller in 1897, which delivered in order—

(1) A current in the proximal direction (giving anelectrotonic currents in the nerve).

(2) Excitation by means of an induction coil.

(3) A current in the distal direction (katelectrotonic currents).

(4) Excitation as No. 2.

The result is seen in the photographic plate as a cycle consisting of one upward mark, anelectrotonus, and three downward, the middle one katelectrotonus, the two side negative variations.

Experiments.

The following pair of experiments was made under identical conditions :—

Table VI.

Experiment.	Nerve.	Anelectrotonus mean.	Neg. var. mean.	Katelectro. mean.	Inj. cur.	Notes.
728 VIII ..	Median (branch of)	0·702	0·356	0·595	0·20	Fig. 8
729A VIII	Splenic	0	0·540	0	0·80	Fig. 9

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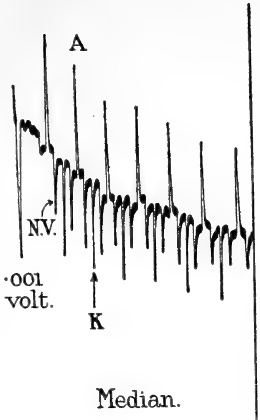
* Waller, 'Brain,' vol. 18, 1895, p. 210.



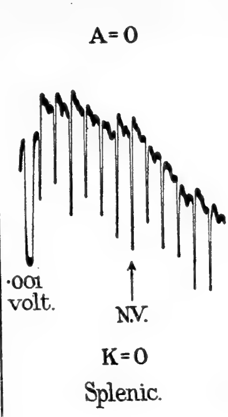


Fig. 8.—Exp. 728, VIII. Anelectrotonic and Katelectrotonic Currents, and Negative Variation on Medullated Nerve.

Fig. 9.—Exp. 729, A, VIII. The same on Non-medullated Nerve. An. and kat. are too small to be legible, and the negative variation only is seen.

The voltage of the polarising current was $\frac{2}{3}$ volt, the excitation 3000 units of the Berne coil, the distance between the centre of each electrode and the next was 11 mm.

In the medullated nerve the electrotonic currents exceeded the negative variation; in the non-medullated they were so small as to be imperceptible on the photographic plate. In order to see whether the electrotonic currents were completely absent, or merely very small, further experiments were then made, using a higher voltage in the polarising circuit.

These measurements are uncertain, both from the difficulty of measuring such very small currents, and from the fact that the cessation of anelectrotonus and the commencement of katelectrotonus excite the nerve, and the resulting negative variation in the latter case is added to the katelectrotonic current, so that except in special cases (as in Experiment 731) the readings of the latter are too high. It is also not absolutely certain that current escape has not some share in the result, though as all these effects are abolished by crushing the nerve, and as the an- and katelectrotonic currents are not equal in magnitude, and not exactly proportional to the polarising current, it is probable that this error, if it exists, is a small one.

Bearing these reservations in mind, certain conclusions may be drawn from these experiments. The electrotonic currents in the non-medullated nerves are evidently very small, about one-fortieth of the same currents in medullated nerves. Further, while the an- and katelectrotonic currents in the latter are nearly equal (0·702 millivolt

Table VII.

Experiment.	Polarising current (volts)		0·66.	1.	1·3.	1·6.	2.	2·7.	3·2.	4.	Neg. var.
729, B. VIII	Electrotonic currents (millivolts)	A K	? ?	0·035 0·088	0·044 0·11	0·053 0·11	0·066 0·080	0·088 0·080	0·11 0·088	0·17 0·12	} 0·651
	Polarising current (volts)		2.	3.	4.	4·8.	6.	Neg. var.		Fig. 10.	
730. VIII.........	Electrotonic currents (millivolts)	A K	? ?	0·045 0·041	0·090 0·045	0·12 0·041	0·16 ?	} 0·506			
	Polarising current (volts)		2.	4.			6.				
731. VIII	Electrotonic currents (millivolts)	A K	? A ? K	0·071 0·11 0·11 0·094 0·024 0·024 0·024	=0·096 (mean) =0·024 (mean)	.. ..	A = 0·12 K = 0·038	Later experiment with same nerve as 730, now giving no negative variation on excitation.			
	Polarising current (volts)		2.	4.	6.	8.	Neg. var.				
732. IX	Electrotonic currents (millivolts)		A = 0·02 K = 0·01	0·05 0·02	0·065 0·025	0·085 0·025	} 0·625				

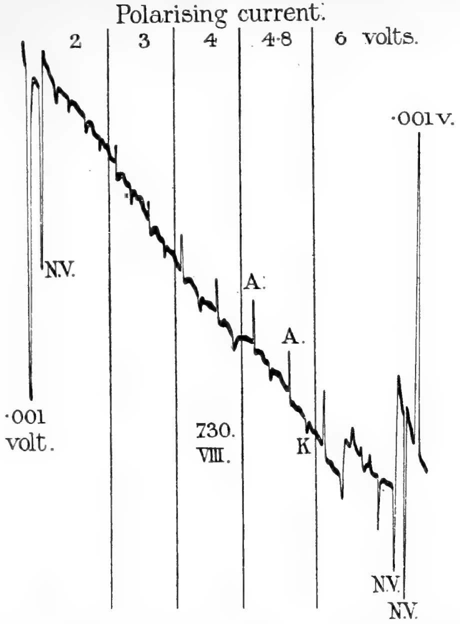


Fig. 10.—Exp. 730, VIII. A. and K. Currents only, with Increasing Strengths of Polarising Currents.

in A and 0·595 millivolt in K in Experiment 728), in the splenic nerves the anelectrotonic currents are perhaps four times greater than the katelectrotonic. This favours the view that these currents are not due to the presence of a small percentage of medullated fibres in these nerves, but are the actual expression of the fine sheaths present around the axis-cylinder.*

The result, therefore, of this series of experiments affords a very probable explanation of the diminution of the negative variation with successive excitations in the non-medullated nerve, and the relative absence of this diminution in the medullated. For in the latter the exciting current can diffuse up and down the nerve, in the former it is strictly limited to the spot where it is applied, and the current density at the excitable axis-cylinder must be many times greater in the non-medullated nerve. It is not surprising, therefore, that there should be a marked local effect, and whether this is termed "injury" or "fatigue" is rather a question of terminology than of fact.

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* Dr. W. M. Fletcher has recently examined the sheaths of medullated and non-medullated fibres between crossed nicols. He finds that in the former there is characteristically present an anisotropic cholesterin deposit; in the latter this is absent. The sheath round the non-medullated axis-cylinder is therefore of a totally different character to that in the medullated fibre. (Note communicated to the writer.) See also authors quoted by Brodie and Halliburton, *loc. cit.*

As the electrotonic currents are so small, it was not to be expected that there should be any marked alteration of excitability in the neighbourhood of the kathode or anode of a constant current. Three experiments were, however, made to serve as a check on the preceding:—

Table VIII.

Experiment.	Neg. var. (mean). Initial.	Neg. var. (mean). during anelec.	Neg. var. (mean) during katelec.	Neg. var. (mean). Final.	Notes.
743, X. Polarising current = 0·5 volt. Excitation = 1000.	0·488	0·468	0·452	0·460	
744, X. Polarising current = 2 volts.	0·492	0·453	0·444	0·434	Same nerve as 743.
745, X. Polarising current = 2 volts.	0·971	0·931	0·928	0·884	

The exciting electrodes were at a distance of 3 mm. from the polarising, and on their distal side. From an inspection of the figures it is clear that even taking the most favourable case of the last experiment the effect is a minimal one.

I have much pleasure in acknowledging the kind assistance and advice I have received from Dr. Waller in the prosecution of this research, and also in expressing my indebtedness to the Council of the Zoological Society and Dr. Chalmers Mitchell for permission to use the nerves of the horses which had been slaughtered for the carnivora.

Conclusions.

1. Non-medullated nerves exhibit a negative variation and current of injury of about three times the magnitude of the similar phenomena in the medullated nerves of the same animal.
2. The negative variation of non-medullated nerves undergoes a progressive diminution with repeated stimuli.
3. The immediate cause of this diminution is a localised change at the place of excitation.
4. The electrotonic currents of non-medullated nerves are very small, about one-fortieth of those in medullated nerves.
5. This latter fact affords an explanation of 2 and 3, as the exciting current, being confined to the place of application, has a greater current density and therefore a greater local effect.

"Note on the Formation of Solids at Low Temperatures, particularly with Regard to Solid Hydrogen." By MORRIS W. TRAVERS, D.Sc., Professor of Chemistry at University College, Bristol. Communicated by Sir W. RAMSAY, K.C.B., F.R.S. Received February 4,—Read February 18th, 1904.

In the year 1902 Dr. Jaquerod and I carried out some experiments on liquid and solid hydrogen with a view to determining its vapour pressure on the scales of the constant-volume helium and hydrogen thermometers. We found that hydrogen remained liquid down to 14°·2 (He scale), the lowest temperature to which we could reduce a large mass of the liquid by means of the pump at our disposal. When, however, a small quantity of liquid hydrogen, cooled to 14°·2 in a glass tube immersed in the liquid contained in the large vacuum vessel, was allowed to evaporate under reduced pressure, it solidified when the pressure fell to 49 or 50 mm. of mercury. This pressure corresponds to a temperature of 14°·1 on the helium scale. The presence of the solid was determined by mechanical means, and it was not possible to observe its appearance.*

Dewar gives the melting point of hydrogen at about 15° absolute, and the melting pressure at 55 mm. of mercury. He describes its appearance as that of "frozen foam," or as "clear transparent ice."†

It appeared to me worth while to carry out a few experiments to try to determine whether solid hydrogen formed definite crystal, or indeed whether the glassy substance was a true solid or merely a highly viscous fluid. My meaning will become clearer if I give an instance in which both such changes occur.

If an organic liquid, such as ethyl aceto-acetate, is cooled slowly to the temperature of liquid air, it is converted into crystalline solid, the formation of the crystals commencing when the liquid is cooled to about − 150° C., usually at several points on the side of the vessel, and spreading rapidly throughout the mass. If, on the other hand, the liquid is cooled very rapidly, a hard glassy substance is formed, and though crystals may begin to appear, they will only do so locally, as the velocity of crystallisation decreases rapidly as the viscosity of the liquid increases. The glassy substance is really a liquid of high viscosity; it is formed with perfect continuity from the normal liquid state, and should differ from the solid (crystalline) form in its physical properties. Such a substance might, for convenience, be called a pseudo-solid.

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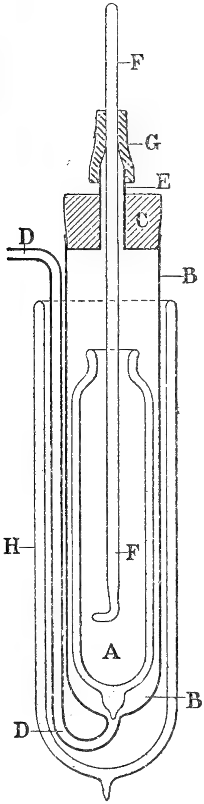
* 'Phil. Trans.,' A, vol. 200, p. 170.

† British Association, Presidential Address, 1902. See also paper on "Solid Hydrogen," 'Brit. Assoc. Report,' 1899, reprinted in 'Nature'; also 'Roy. Inst. Proc.,' 1900.

In investigating solid hydrogen the apparatus shown in the accompanying figure was employed. The liquid hydrogen was introduced into a small clear-glass vacuum-vessel 15 cm. long and 4 cm. in internal diameter. This vessel was placed inside a glass tube BB, which communicated with an exhaust pump through a tube DD sealed to it, and was closed by a rubber stopper C. A short glass tube E, 6 mm. in diameter, passed through the stopper, and through it passed the stirring rod FF. To allow of free rotating motion to the stirrer, and to make the apparatus gas-tight, a short piece of rubber tube G, was passed over the end of the tube E and was wired to F. The lower part of the apparatus was contained within the vacuum vessel H, which contained a small quantity of liquid air.



When the liquid hydrogen was made to boil *in vacuo*, its temperature fell, but the liquid did not appear to become more viscous. At length films of a colourless glassy substance formed at the surface, and broke away as the bubbles rose. After a short time the vessel became filled with these flakes, and while in this condition stirring, by giving the top of the rod F a rotatory motion, did not appear to indicate that the portion which remained liquid had undergone any considerable increase in viscosity. After a time the mass contained so much solid that it became pasty, and finally the whole of it appeared fairly homogeneous.

The solid evaporated fairly rapidly, so that after about 10 minutes only a hollow cylinder of it, about 3 cm. long and 2·5 cm. in diameter, remained. This had the appearance of a film of ice which had partly thawed, consisting of clear granules connected by thinner and less transparent portions of solid. No crystals were observed on either of the three occasions on which the experiments was carried out. An attempt was made to examine the solid in the field of a polariscope, but it was unsuccessful.

Though there is no direct evidence of the formation of crystalline hydrogen, my experiments lead me to the belief that solid hydrogen is a crystalline substance and not a pseudo-solid. The sharpness with which the solid hydrogen is formed, and the constancy of the apparent melting pressure, are distinct evidence in favour of this conclusion, though it must be allowed that the rate of change in viscosity, when the temperatures are measured on the Centigrade scale, will probably appear to be more rapid at low temperatures than at high temperatures.

The whole question of the formation of solids at very low temperatures is of great interest both from a physical and from a biological standpoint. It is quite possible that if living organisms were cooled only to temperatures at which physical changes such as crystallisation take place with measurable velocity, the process would be fatal, whereas if they once were cooled to the temperature of liquid air, no such change could take place within finite time, and the organism would survive.*

These experiments were made in connection with some investigations which were being carried out at University College, London, with the assistance of a grant from the Royal Society. As I am at the moment unable to continue the work, I have decided to publish this note.

"A Contribution to the Study of the Action of Indian Cobra Poison."†

By Captain R. H. Elliot, M.B., B.S. Lond., F.R.C.S. Eng., D.P.H. Camb., etc., of the Indian Medical Service (Madras). On special duty for Snake Venom Research under the orders of the Secretary of State for India. Communicated by Professor Sir Thomas R. Fraser, F.R.S. Received January 18, —Read February 25, 1904.

(Abstract.)

Previous Work on the Subject.

Brunton and Fayrer‡ discussed the pharmacology of Cobra venom at some length; they attributed the effects of the poison to its action on the cerebro-spinal nerve-centres, especially on the respiratory centre. They observed that Cobra venom had a direct action on cardiac muscle, and that it also affected the heart through the vagal system, but they did not lay much stress on circulatory failure. They surmised that the high and maintained blood pressure of a cobraised animal was due to arteriolar constriction, but did not attempt to explain how this was brought about. Amongst the many other points of interest they took up, was the influence of artificial respiration in cobraism.

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* Experimental results are given by Macfadyen, 'Roy. Soc. Proc.,' vol. 66, 1900, pp. 180, 339, 488; Swithinbank, 'Roy. Soc. Proc.,' vol. 68, 1901, p. 502.

† Owing to the kindness of Professor Sir Thomas Fraser and of Professor E. A. Schäfer, the writer was enabled to carry out this research in the Pharmacological and Physiological Laboratories of the University of Edinburgh. Towards the expense of this research, grants were received from the British Medical Association and from the Moray Fund for the Endowment of Research (Edinburgh).

‡ 'Roy. Soc. Proc.,' vols. 21, 22, and 23.

Cunningham in the 'Scientific Memoirs by Medical Officers of the Army in India'* urged the opposing theory that Cobra venom acted on respiration, through the blood and not through the nervous system.

Weir Mitchell, and Reichert† carried on Brunton and Fayrer's views. Their paper was mainly concerned with the venoms of other snakes than the Cobra. They thought two factors were at work on the rate of the heart, viz., an increased activity of the accelerator centres, quickening the beat, and a direct action on the heart slowing it. They attributed the primary fall in blood pressure to depression of the vaso-motor centres, but thought it might be partly cardiac. The rise they considered "capillary" and the final fall cardiac.

Bagotzi‡ laid great stress on the *rôle* played by nerve-end paralyses (especially phrenic), and disputed Brunton's views that respiration was attacked through the medullary centre. He did not find any action of the venom on the vagal mechanism. He surmised that death with a tightly contracted heart, the result of very large doses of venom, was due to a cardiac action.

C. J. Martin in the article on snake venom in 'Allbutt's System of Medicine,' considers that, in Cobra poisoning, the circulatory mechanism is not easily affected, and contrasts this with the state of affairs in viperine poisoning. He found that vagal stimulations stopped the heart up to near the end of life in Cobra poisoning.

Object of this Research.

This was to accurately ascertain the precise part played by the various important centres, nerves and organs in the production of death from cobraism.

Methods employed in the Research.

1. *Perfusion of the frog vessels was carried out with solutions of Cobra venom of various strengths.*—The central nervous system had been destroyed first in each case.

The strength-limitation of the action of the venom on the arterioles was carefully studied.

2. *Perfusion of frog hearts was carried out with solutions of Cobra venom of various strengths.*—The isolated hearts were perfused in Schäfer's plethysmograph, and blood mixture was employed as the vehicle for the poison. The strength-limitation of the action of Cobra venom was again determined here. Certain drugs which resemble this poison in their action on heart muscle, were also experimented with, *e.g.*, strophanthin and the sulphate of atropia. The risks apparently

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* 1895, Part IX, and 1898, Part XI.

† 'Smithsonian Contributions to Knowledge,' 1890.

‡ Virchow's 'Archiv für Path.,' vol. 122, p. 201.

attendant on the use of the latter drug in Cobra poisoning are pointed out.

3. *The study of the action of Cobra venom on the frog heart in situ* was next taken up, by means of a number of devices, which included the direct application of the poison to the medulla oblongata, which was exposed for the purpose.

4. *Perfusion of the mammalian heart was carried out with solutions of Cobra venom of various strengths.*—The isolated heart was perfused through its coronary vessels with a nutrient fluid, in which the venom was dissolved. Cats' and rabbits' hearts were used.

5. *By means of kymographic tracings, the blood pressure, respiratory movement, etc., of cobraised rabbits were recorded and studied.*—The activity of the vaso-motor mechanism was studied, in various stages of cobraism, by stimulations of the depressor and sciatic nerves, the vagi were cut, likewise at various stages, and their ends were also stimulated, in order to ascertain the part played in cobraism by the vagal inhibitory mechanism; injections of a solution of sulphate of atropine were also made, and the effects were observed. The author received much help in this section from Drs. Sillar and Prentice.

6. *A similar set of experiments to the last was carried out on dogs and cats, plethysmographic tracings of intestinal volume were also included here, in order to study the changes, if any, going on in the splanchnic area circulation.*

7. *The movements of the auricle and ventricle were studied in cobraised cats and dogs by removing the front of the chest parietes, and attaching the auricular and ventricular walls (by means of hooks and silk threads) to levers recording on a kymographic apparatus.*—The blood pressure in a large artery was recorded at the same time, and intestinal volume was also frequently taken by means of a plethysmograph. At various stages the vagi were divided or stimulated, and the results observed. The effect of giving further doses of Cobra venom with the vagi, intact or divided, was also studied. The condition of the vagal nerve-ends received close attention.

8. *By kymographic experiments the influence of artificial respiration on the centres, nerve-ends, etc., of cobraised animals was carefully studied.*—The experiments were varied in different ways.

9. *The direct action of Cobra venom on the respiratory centre of rabbits was tested by applying the poison to the exposed medulla oblongata.*—A stethograph recorded the respiratory movements, and the blood pressure was at the same time taken on the kymograph.

10. *Several series of experiments were undertaken to ascertain the part played by the phrenic and other nerve-ends* in producing the respiratory complications which are seen in cobraism.

Summary of Conclusions.

1. Cobra venom acts directly on the muscular tissue of the blood-vessels, or through their vaso-motor nerve-endings, constricting the arterioles, and thus raising the arterial blood pressure. It probably affects all organs alike. In the frog vessels the action can be traced down to dilutions of 1 : 10,000,000. In a Cobra-bitten man, the concentration of venom in the blood is probably at least thirty times as great as this.

2. Cobra venom also acts directly on the isolated frog ventricle, killing it in a position of firm systole, if the solution be concentrated, and stimulating it if a weaker strength be employed. The limit of the speedy lethal action on the isolated heart is reached at a concentration of about 1/500,000. The stimulating action can be traced down to a dilution of 1/10,000,000. This action of Cobra venom brings it into line with the glucosides of the strophanthin group. Its action is more rapid than that of strophanthin, and is certainly not inferior to it in strength. Atropine sulphate and Cobra venom, when acting in the same solution, intensify each other's action, and produce more summation of effect than one would have anticipated. This detracts from the value of the atropine salt in the treatment of cobraism, and makes it a dangerous remedy. The blood-pressure work has confirmed this view of the case.

3. Cobra venom powerfully affects the isolated mammalian heart, when solutions of it are perfused through the coronary circulation. The action appears to be a dual one, viz. (1) a direct action on the muscular fibre, or on the nerve endings, closely resembling that which is produced on the isolated frog ventricle; and (2) an action on the intracardiac vagal mechanism, which makes for inhibition. The result is that, in strong solutions, we find an irregular and extreme excitation of the heart, followed by early death in a position or systolic tone. If the concentration be less, the early stage of excitement yields to a prolonged phase, in which the tonic action of the poison on the heart is most pronounced: the beat is regular, steady, and strong. Cobra venom interferes with the circulation through the heart in a marked manner; this is probably due (1) to a constriction of the coronary vessels, brought about by the direct action of the venom on the vessel walls, and (2) to the condition of tonus into which the heart is tending to pass.

4. When given subcutaneously in low lethal doses, Cobra venom kills by paralysing the respiratory centre. Such a paralysis is under these circumstances gradually evolved, and in the early stages of the process there is often evidence of a phase of stimulation preceding the paretic phase.

There is a gradually increasing venosity of the blood, and in consequence thereof all the harmful results of slow asphyxiation are produced.

If life is prolonged beyond the usual term by artificial respiration, and possibly also if the dose of venom is a very low lethal one which takes many hours to kill, the phrenic and other motor nerve-ends may become paralysed, but this is certainly not an essential feature of death from lethal doses of Cobra venom, which kill within five hours. I hope to make a farther communication on this subject later.

The convulsions which precede death are purely asphyxial, and can be at once stopped by artificial aëration of the blood. Each such convulsion is followed by a phase of exhaustion of the respiratory mechanism, which is almost certainly central.

If the dose of Cobra venom administered be a large one, and especially if it be given intravenously, the respiratory centre is quickly and severely affected, and respiration may cease almost at once. This cessation of breathing may be permanent, if artificial respiration be not quickly started, but if the dose be a smaller one, the rhythmic activity of the centre reasserts itself. At first there may be a number of deep spasmodic gasps, and then the movements of respiration re-begin, very gently at the commencement, and gaining force as time goes on, till a normal rhythm is re-established, or even a stage of stimulation is manifested. Soon, however, the centre fails again, and all the phenomena of asphyxiation appear.

By applying Cobra venom directly to the exposed medulla oblongata of the rabbit, I have shown that the respiratory centre can be paralysed without the phrenic nerve-ends or the heart being appreciably affected.

If very large doses of venom are injected, death may take place by cardiac failure, before the respiratory mechanism has given way. We have here to do with the direct action of the venom on the heart muscle; the beats become rapid, and shortened, and the heart passes into a systolic phase, in which it dies tightly contracted.

5. Cobra venom, when given in low lethal doses subcutaneously, raises the general blood pressure. There may be a slight preliminary fall before the rise, but often this is wanting. In the absence of farther interference the blood pressure remains high till very near the end of life. In the asphyxial convulsions which herald death, a farther steep rise of blood pressure takes place; this is soon followed by a sudden and very rapid fall to death.

The high level of blood pressure is due to—

1. The direct action of the circulating venom on the muscular tissue of the arterioles, causing a constriction of these vessels, and thus opposing a barrier to the onward flow of the blood;

2. The increased force of the heart beat as the outcome of the direct stimulating action of the venom on its muscular tissue, and

3. The stimulation of the vaso-motor centre, as a result of the steadily increasing venosity of the blood.

The slight preliminary fall of blood pressure, which is sometimes seen, is due to cardiac inhibition, but this subject will be reserved for discussion when dealing in the next section with the action of large doses of the poison.

The late fall in the rate of the heart beat is due to cardia inhibition, the latter is due to several factors.

1. A gradually progressive asphyxiation is taking place throughout such an experiment; this affects the vagal centre in common with the rest of the nervous system; the result is a stimulation of the inhibitory mechanism, and a consequent slowing and weakening of the heart.

2. The direct stimulating action of the venom on the vagal inhibitory centre acts in the same direction as the asphyxiation of the centre.

3. There is distinct evidence that even when the influence of the vagal centres is removed, inhibition of the heart continues to progress, though in a lessened degree. The obvious inference is that the vagal nerve-ends are stimulated by the circulating venom, and probably also as a result of deficient aëration of the blood.

4. It is not improbable that a stage of exhaustion of the heart muscle follows the early stimulative action of the venom; and

5. Exhaustion of the heart is probably predisposed to by the strain put upon the organ, in having to work for a long period against an abnormally high blood pressure.

We are now in a position to explain the sudden rapid fall of the curves of heart-beat rate and of blood pressure, which usher in death at the close of one of these long experiments. An over-strained and weakened heart is suddenly and violently called upon to bear a farther burden, for respiration has ceased and the medullary centres are acutely asphyxiated. As a consequence there is a violent excitation of the cardio-inhibitory and vaso-motor mechanisms. The heart is slowed and at the same time has to work against a suddenly increased pressure, and it gives way. In fact we have the phenomena of asphyxiation in their entirety.

The vessels of the splanchnic area are affected *pari passu* with those of the body generally, and they in no wise act independently. The vaso-motor mechanism remains active throughout, and is, as we have seen, profoundly affected by changes in the venosity of the blood.

6. Cobra venom, when injected in large doses and especially when given intravenously, causes—

(1) a sudden fall of blood pressure;
(2) a subsequent rise, provided the dose has not been too large; and
(3) a final fall to zero.

The early fall is undoubtedly due to inhibition of the heart. It has been clearly shown that this is mainly brought about by the direct action of the poison on the vagal centres in the medulla oblongata, as it occurs before the accompanying failure of respiration has had time to act. Moreover, it is seen whilst artificial respiration is being actively carried on, and can be checked under these circumstances by division of the vagi.

On the other hand, there can be no doubt that asphyxiation of the vago-inhibitory centre intensifies and maintains the inhibition which direct influence of the venom on the vagal centre produces.

The spontaneous recovery of respiration, or the application of artificial respiration, has a powerful influence in mitigating the action of the venom on the vagal centre. In the same way artificial respiration, and to a less extent the spontaneous recovery of respiration, appear to act beneficially on the poisoned respiratory centre.

Even if the heart is cut adrift from all central vagal impulses, whether direct or indirect, by the division of the vagi, there yet remains evidence of a continued inhibition which must be attributed to the direct action of Cobra venom on the terminals of the vago-inhibitory mechanism. This action would appear to be a direct one, but there is every probability that it is indirect as well, in other words that it acts through asphyxiation of the vagal terminals, as well as by the poisoning of these parts by the circulating poison. There is, however, another factor which must not be lost sight of, viz., a direct exhaustion of the heart muscle as the result of irregular over-stimulation.

2. When the secondary rise of blood pressure, which follows the primary fall, occurs, it is due to the same factors which determine its occurrence when small doses have been injected. It remains to explain why it is sometimes absent, brief or ill marked. The explanation is simple; it is merely a question of cardiac failure. We have seen that the direct inhibitory action of the venom through the vagal centre is capable of overcoming the tendency which the blood circulating through the heart muscle has to throw that muscle into death in systolic tone. Were it not for these two rival forces to some extent equilibrating each other, Cobra poison would kill by its direct action on the heart muscle. When the doses are comparatively small, or when the vagi are cut or thrown out of gear by atropine, we find the tonic cardio-muscular influence of the venom in evidence, but when the dose of venom is a large, and especially when it is intravenously given (the vagi remaining intact), the inhibitory action overpowers the muscular excitation, and failure of the heart occurs. If the inhibition is sufficiently well marked, no amount of arteriolar spasm that occurs will compensate it, consequently the blood pressure falls.

When the dose of venom is a very large one, the direct muscular stimulation may be so intense as to overcome the maximum inhibitory impulse, and then the heart dies in systole with a quickened beat, and is found after death as hard as a contracted *post-partum* uterus. Under such circumstances, any increase in the force of the heart is temporary, for the beat is probably a very partial one, the heart passes through a stage of excitement into one of increasing systolic tonus, in which the contractions are very limited in extent.

Acknowledgments.

In conclusion, I desire to express my indebtedness to all who have so ungrudgingly helped me in my work. I owe my thanks to one and all of Sir Thomas Fraser's and Professor Schäfer's assistants, but especially to Drs. Sillar, Carmichael, and Hering, who were always willing to aid me in any way in their power. Messrs. Burnett, Jolly, and Locherby, who gave up much of their time to work regularly for me as my volunteer assistants, did excellent work throughout, and I most gratefully acknowledge that, but for their aid, the work could not have been done in the time.

The help given me by Sir Thomas Fraser and by Professor Schäfer I have already acknowledged. It is not possible for me to do justice to it, or to the unvarying kindness I met with from them both.

Lastly, but far from least, my acknowledgments are due to the Secretary of State for India, to the Government of India, and to the Government of Madras, for the opportunity that has been given me to carry out this work.

"A Study of the Radio-activity of certain Minerals and Mineral Waters."

By Hon. R. J. STRUTT, Fellow of Trinity College, Cambridge. Communicated by LORD RAYLEIGH, O.M., F.R.S. Received February 29,—Read March 10, 1904.

PART I.

A considerable number of minerals are known in varying degrees to be radio-active. Lists have been given by M. and Madame Curie,[*] and by Sir W. Crookes.[†] Except in the case of pitchblende, little has been done to determine the nature of the radio-active constituents; or to decide whether any hitherto unknown radio-active body is present.

To obtain complete information on these subjects, the only method available would be to completely analyse the mineral, and examine every precipitate and filtrate for radio-activity. This process is of course very tedious, and the results have to be interpreted with care, since traces of radio-active elements may often be carried down in the groups to which they do not properly belong, and thus cause confusion. A much easier method is to heat the crude mineral, and to examine the rate of decay of the emanation which it gives off. Each emanation has a characteristic time-constant of decay, and by determining this we can identify it.

The method is of course useless for testing the presence or absence of radio-active elements such as uranium,[‡] which do not give off a characteristic emanation. But the great facility with which it may be applied to a small quantity of material, and the definiteness of the results, are great merits.

In any case, when a material suspected to contain radium is obtainable in abundance, it is better to test for the presence of emanation than to look for activity in the solid. For but little of the solid material can be advantageously used in the test. Thick layers give no larger effect than thin ones, since the upper layers absorb the radiation from the lower. But the emanation can be extracted from any desired bulk of material, and the effect proportionately increased. If carbonic acid, or any other gas, is evolved at the same time in inconvenient quantities, it can be absorbed with a suitable reagent, and the emanation contained in it thereby concentrated.

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[*] 'Thèse présentée à la Faculté des Sciences,' Paris, p. 19.

[†] 'Roy. Soc. Proc.,' vol. 66, p. 411.

[‡] I have found a distinct, though feeble, emanation from re-crystallised uranium nitrate, having a rate of decay equal to that of the radium emanation. Whether this is really due to uranium, or to traces of radium, which the uranium still contains, must be left for the present an open question.

The present paper gives the results of an examination of certain radio-active materials by this method.

No new emanation has been recognised. The results have in all cases been attributable to thorium and radium.

If any emanation decidedly more permanent than that of radium existed in the evolved gas, the method could not fail to detect it. For in every case the activity of the gas was watched until it became comparable with the very small activity due to the walls of the vessel. If a more durable emanation had been present even in small quantities, the proportion of it present would have increased relatively to the radium emanation, and its presence would have become apparent towards the end, by a diminished rate of decay.

Small quantities of an emanation less durable than that of radium might have escaped detection. For they would have been masked by the much greater quantity of the latter.

By measuring the rate of leak due to the accumulated emanation from a weighed amount, the proportion of radium present may be estimated. A comparison with the leak due to the emanation of a known weight of radium must of course be made. For this purpose it would be best to weigh out, say, a milligramme of radium bromide, dissolve it in a litre of water, and evaporate a small measured quantity of the solution in a suitable tube. In this way the effect due to a standard quantity could be determined.

The method of experimenting was as follows:—

The powdered mineral was placed in a hard-glass combustion tube, drawn out and sealed at one end, connected to a mercury gas-holder at the other. The mineral was heated to redness, and the gaseous products collected in the gas-holder. When the evolution of gas had ceased, the point was broken off, and air drawn into the gas-holder, up to a standard volume.

For measuring the electrical effects, an electroscope was used. This was exhausted, and the gas extracted from the mineral, together with the air, which had been used to make up its volume to a sufficient amount, was admitted. After a few hours, enough for the deposited activity to attain its full value, the rate of leak was read. The day and hour was noted, and the gas was pumped out into a test-tube and stored over mercury. After a sufficient time had elapsed, it was again introduced into the apparatus by means of a syphon gas pipette* and the rate of leak again measured. In the meantime the apparatus had been available for making measurements with other gases.

In some cases the emanation was initially so strong that it could not be conveniently investigated. In such cases a portion of the gas was diluted with air for measuring the rate of decay at first. The

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* The methods of manipulation used in storing and transferring the gases without loss were those described in Dr. Travers' book, 'The Study of Gases.'

concentrated material was kept until, by lapse of time, it had become weak enough to be conveniently used. Its activity was followed until it had become too small for measurement.

With this preface the results for the various minerals tried may be given in the form of a table. The rates of leak are given in scale divisions per hour. When air alone filled the apparatus, the rate of leak was 2·25 sc. div. per hour. This was in each case subtracted.

Mineral.	Locality.	Quantity taken in grammes.	Rate of leak due to emanation (sc. div. per hour).	Rate of leak per 100 grammes.	Time in days taken by the emanation to fall to half its initial value.
Samarskite .	N. Carolina, U.S.A.	20	20,600	103,000	3·48
Fergusonite.	Norway ?	7	4,280	61,000	3·80
Pitchblende.	Cornwall..........	40	11,900	29,800	3·50
Malacone ..	Hitteroe, Norway ..	20	1,440	7,200	3·81
Monazite ...	Norway	51	2,060	4,000	3·50
,, ...	N. Carolina	82	37	45	3·81
,, ...	Brazil	54	11	24	3·80
Zircon.....	N. Carolina	60	24·6	41	4·05

All the minerals give radium emanation, though in very varying quantity.

These tests were not started quickly enough to give information as to the presence of a very quickly decaying emanation. This was tested for independently.

The mineral malacone is of peculiar interest, because it has been found to contain argon as well as helium.* Helium is formed by the degeneration of radium, and it is reasonable to assume that the other kindred gases have had a similar origin. It was hoped, therefore, that malacone might contain some new radio-active element. It is still possible that it does so, but, if so, this substance gives no emanation distinct from that of radium.

The meteorite of Augusta Co., Virginia, has also been found to contain argon and helium. But no emanation at all could be obtained from 20 grammes of it.

The minerals were all tested for thorium emanation by drawing air over them in the cold; the only one in the above list that gives it is the Norwegian monazite, and even this does not yield it very abundantly. A crystal of thorite, however, kindly lent me by Professor Lewis, was found to give torrents of thorium emanation.

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* Ramsay and Travers, 'Roy. Soc. Proc.,' vol. 64, p. 131.

Air drawn over it in the cold possesses strong discharging power. It was not permissible to heat the specimen, which might have injured it, so that the presence or absence of radium emanation in thorite could not be investigated.

There can be no doubt that the other specimens of monazite contained thorium, for they were given me by the late Mr. W. Shapleigh, who was connected with the thorium industry, and used these varieties of monazite for preparing thoria. They were, moreover, markedly radio-active, while the amount of radium emanation obtained from them was so small that their activity could not be mainly due to radium. They probably contain the thorium in what Rutherford and Soddy call the de-emanated condition. That is, the thorium emanation, though formed, is not able to escape.

It is a remarkable fact that these varieties of monazite, though they contain practically no radium, yield helium in fair quantity. There are several explanations possible. The radium originally present may have almost completely decayed into helium, and any other products which it may yield; or it may be that thorium, as well as radium, yields helium by its decomposition; or, lastly, the helium may not, in this instance, have been generated by radio-active changes at all.

It is interesting to know whether the minerals retain all the radium emanation which they generate when heat is not used to expel it. Two cases were examined. One hundred and fourteen grammes of powdered samarskite were kept for 3 weeks in a sealed glass tube. The air was pumped out and tested. It was found to contain about $\frac{1}{150}$ part of the emanation, which could have been extracted by heat.

A similar experiment with malacone showed that about one-fiftieth of its emanation was able to escape in the cold.

It appears, therefore, that these minerals retain nearly all their emanation. The same is probably true of the helium produced by the emanation. Samarskite which had been heated to redness was found to retain its emanation in the cold about as well as before.

Part II.

I happened to possess a small sample of a red deposit, coloured by iron, which is left by the water of the King's Spring, at Bath. It occurred to me that it might be worth while to test this for radio-activity. The result was to show that the deposit was markedly active. On leaving it in the testing vessel (which was closed airtight) for a few days, the activity was found to increase to several times its initial value. This shows that the deposit gives off an emanation freely, even without heat.

Experiments were then made to test the rate of decay of this emanation. It proved to be identical with the rate of decay of the emanation of radium.* The activity is wholly due to that element.

This deposit was collected inside the King's Well itself, where the hot water issues from the ground. Other deposits are left in the tanks and pipes. They are less active than that collected near the source.

Deposits from another of the hot springs at Bath, that known as the Old Royal Spring, have also been tested. These were found to be active also. In this case there was no opportunity of collecting the deposit at the well-head itself, but it was found that the deposit left in the channel near the source was more active than that in the tanks further from it.

It was interesting to determine whether the water itself contained any radium in solution. There could be little doubt that there must be traces left in solution, after the deposit had settled out. But, since the Bath water contains abundance of sulphates, and since radium sulphate is one of the most insoluble salts known, there could not be more than the merest traces present. The sulphate of barium is very much less soluble than that of strontium. And presumably the sulphate of radium is much less soluble still. Barium sulphate requires half a million times its weight of water to dissolve it; radium sulphate perhaps several hundred million times its own weight.

About 10 litres of the Bath water were evaporated to dryness. The resulting saline residue was sealed up in a hard-glass tube, and left for about a fortnight to generate a stock of emanation. On heating, a distinct emanation was obtained, giving several times the rate of leak that air did. A deposit, similar to that from the Bath water, but black in colour, can be collected from the source of the hot springs of Buxton. It has been analysed by Dr. J. C. Thresh,† and I am indebted to his kindness for a specimen of it. This deposit was found to contain radium also; the proportion present being not very different from what was found in the case of some of the Bath deposits.

The following table gives the quantitative data for these emanations from these deposits. The rates of leak are on the same scale as those in the preceding table.

It will be seen that the richest of the deposits is some thirty-six times more active than the salt obtained by evaporating the water.

Although the agreement in the rate of decay of the emanation

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* In the first experiment made, I obtained a small residual leak when the radium emanation had decayed. This was attributed to a new emanation, of greater durability. But I have failed to repeat the experiment, and am forced to conclude that the leak was due to a failure of the quartz insulation, owing to the presence of moisture. It is very difficult to understand how this can have happened, for the gas was passed through drying tubes. When the rate of leak was tested with air in the apparatus, it had always a perfectly definite and constant small value.

† 'Proc. Chem. Soc.,' January 17, 1882.

Material.	Quantity taken in grammes.	Rate of leak due to emanation (sc. div. per hour).	Rate of leak due to emanation from 100 grammes.	Time in days taken by the emanation to fall to half its initial value.
King's Spring, Bath—				
Deposit from inside of well.....	10	250	2,500	3·60
,, tank	12	78·2	650	..
Saline residue from water	18	12·4	69	..
Old Royal Spring, Bath—				..
Deposit from channel near well .	10	63·5	635	
,, bottom of tank ...	15	60	400	..
Hard deposit from sides of tank ..	25	43	173	3·58
Buxton deposit	26	356	1,370	3·81

seemed sufficient to prove that the activity was really due to radium, yet it was thought desirable to show that the chemical properties of the active constituent were in agreement with this conclusion. Two hundred grammes of the richest deposit were treated with dilute sulphuric acid. The activity was all in the insoluble residue, which was dirty white in colour, and amounted to about half of the entire quantity of deposit. The residue was boiled with strong sodium carbonate solution. This was washed away, and the mass extracted with hydrochloric acid. The hydrochloric acid solution gave a slight precipitate with sulphuric acid. This precipitate was collected, and found to be strongly active, so that there is every reason to conclude that the activity of the deposit is due to the presence of radium.

The presence of radium in the Bath water and deposits is of special interest because of the occurrence of helium in the gas which rises with the spring.* There can be little doubt that the helium owes its origin to the same store of radium that supplies the water.

It is interesting to estimate the quantity of radium annually delivered by the spring. Part of this is in the deposit. Part in the water. But the annual yield of deposit does not exceed a few hundred-weight at the most. And although it is much richer in radium than the dissolved salt, the quantity of the latter is so enormously greater, that the deposit may be neglected. According to the estimate of Sir A. C. Ramsay, the late Director of the Geological Survey, the salt annually delivered by the spring would be equivalent in volume to a column 9 feet in diameter, and 140 feet high. Taking the density to be twice that of water, this would weigh about 500,000 kilogrammes.

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* Rayleigh, 'Roy. Soc. Proc.,' vol. 60, p. 56.

Now the saline residue gives about $\frac{1}{1500}$ part of the quantity of emanation that samarskite gives. Let us assume that the latter contains one-millionth part of radium, which is, I think, an outside estimate. At that rate, the annual delivery of radium by the spring amounts to about one-third gramme. The volume of gas which the spring delivered is about 100 cubic feet per day.* About $\frac{1}{1000}$ part of this is helium, so that about 3 litres of helium is given off daily, or about 1,000 litres per annum. The proportion of helium to radium thus indicated is of the same order as in the radio-active minerals, though somewhat larger. This is in accordance with the view that the spring draws its supplies from the disintegration of such minerals.

In obtaining the various materials from the Bath springs, I have had the great advantage of Mr. Sydenham's help. His knowledge of everything connected with the springs has been of great assistance.

In addition to the Bath and Buxton waters I have examined several others. A sample of the Cheltenham saline water, and also a deposit left in the pipes, was kindly sent me by Mr. G. Ballinger. But no emanation could be obtained, either from the dissolved salts or from the deposit. The boiler crust from a domestic hot-water pipe, Terling, Essex, was examined, but the result was again negative.

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* Williamson 'B.A Reports,' 1865, p. 380.

"An Inquiry into the Nature of the Relationship between Sunspot Frequency and Terrestrial Magnetism." By C. Chree, Sc.D., LL.D., F.R.S. Received February 8,—Read March 3, 1904.

(Abstract.)

§ 1. The formula

$$R = a + bS \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

where R is some magnetic quantity such as the amplitude of the diurnal oscillation of the needle, a and b constants, and S sunspot frequency (after Wolf and Wolfer), was first applied by Wolf to the mean declination range throughout the year.

In a recent paper,* mainly devoted to other subjects, I applied it to the ranges, and the sum of the 24 hourly differences from the mean for the day, in the mean monthly and annual diurnal inequalities of declination, inclination, horizontal force, and vertical force at Kew. Some analogous results were also given for Wilhelmshaven, Potsdam, and Parc St. Maur.

The present paper is entirely devoted to the connection between sunspot frequency and terrestrial magnetism. It deals with data from Milan (1836—1901), Greenwich (1841—96), Pawlowsk and Katharinenburg (1890—1900), Batavia (1887—98), and Mauritius (1875—90). It aims at ascertaining wherein the results in my previous paper are peculiar to the station or period (chiefly 1890—1900) dealt with.

It investigates what differences may exist between the sunspot connection on ordinary days and on magnetically quiet days, and what differences arise when one applies (1) to the mean of the differences between the absolutely highest and lowest daily readings, instead of to the range of the mean diurnal inequality. It also considers various measures of the magnetically disturbed character of the year, and their relation to sunspot frequency.

§ 2. The inquiry into the influence of the period selected on the values of a and b in (1) is based on the above-mentioned Milan and Greenwich results, due respectively to Signor Rajna and Mr. Ellis. In both cases, unfortunately, there is a want of strict homogeneity in the earlier data. The Greenwich data suggest a slight progressive increase in b/a during the last 60 years in the case both of declination and horizontal force; but this is not confirmed by the Milan results. The values, however, obtained from b/a in the case of the declination range at Milan from the two periods 1837—50 and 1854—67 are, the one 28 per cent. below, the other 12 per cent. above the value obtained by Rajna for the period 1836—94. In more recent years there is less apparent irregularity in the magnetic and sunspot relation. This

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* 'Phil. Trans.,' A, vol. 202, p. 335.

suggests, of course, that the apparent considerable variations in the values of b/a just alluded to may be mainly due to observational imperfections, but uncertainty is not unlikely to remain on this point until the recurrence of a period of exceptionally high or low sunspot frequency.

§ 3. Table I gives some of the principal results obtained in the case of the range of the mean diurnal inequality for the year. The units are 1′ for angles, and 0·00001 C.G.S (or 1 γ) for force components.

Table I.

Place.	Data from—	Declination.			Horizontal force.			Vertical force.		
		a.	10^4b.	$10^4b/a$.	a.	10^3b.	$10^4b/a$.	a.	10^3b.	$10^4b/a$.
Pawlowsk	All days ..	5·74	400	70	20·7	211	102	8·1	265	326
„	Quiet days	6·17	424	69	20·6	195	95	5·9	27	46
Katharinenburg.	All days ..	5·29	342	65	16·8	182	109	8·6	117	137
Kew	Quiet days	6·10	433	71	18·1	194	107	14·3	81	56
Batavia	All days ..	2·47	179	73	38·7	274	71	30·1	156	52
Mauritius	„ ..	4·06	164	40	15·0	96	64	11·9	69	58

If we exclude Mauritius, where several anomalous features present themselves, we notice a remarkable uniformity in the values of b/a for declination in Table I. The extraordinarily large differences between the "all" day and "quiet" day (Wild's *normal* day) results at Pawlowsk for vertical force presents itself in every month of the year. Pawlowsk is a station where magnetic disturbances are particularly prominent, and the vertical force there seems particularly sensitive to them.

§ 4. For Greenwich only mean monthly diurnal inequalities were available. Table II gives the mean of the 12 monthly values of

Table II.

Place.	Period.	Data from—	Declination.			Horizontal force.		
			a.	10^4b.	$10^4b/a$.	a.	10^3b.	$10^4b/a$.
Pawlowsk	1890—1900	All days....	6·81	446	66	22·8	243	107
„	„	Quiet days..	6·52	442	68	22·2	208	94
Katharinenburg	„	All days....	6·18	355	58	19·2	195	101
Greenwich	1841—1896	„	7·29	377	52	26·4	190	72
„	1865—1896	„	7·07	396	56	23·6	215	91
„	1889—1896	„	6·71	418	62	23·7	218	92
„	„	Quiet days..	6·36	415	65	25·0	213	85
Kew...........	1890—1900	„ ..	6·49	410	63	21·5	191	89

a and *b*, and the corresponding value of *b*/*a* for Greenwich and some other stations. Here, as in Table I, the data relate to the mean diurnal inequality. The units are the same as in Table I.

§ 5. For comparison with Table I, I give in Table III results applicable to the mean value for the year of the absolute daily ranges (taken from individual days irrespective of the hours of occurrence of the maximum and minimum). The units are as before:—

Table III.

Place.	Declination.			Horizontal force.			Vertical force.		
	a.	10^4b.	$10^4b/a$.	*a*.	10^3b.	$10^4b/a$.	*a*.	10^3b.	$10^4b/a$.
Pawlowsk	11 ·3	1130	100	45 ·2	636	141	17 ·6	520	295
Katharinenburg.	8 ·00	652	82	30 ·7	366	119	14 ·6	248	171
Mauritius	5 ·53	255	46	30 ·4	186	61	16 ·2	84	52

The large increase in the values of both *a* and *b* as compared to Table I will be noted. Except at Mauritius, the values of *b*/*a* for declination and horizontal force are considerably greater in Table III than in Table I. There seems, in fact, a general tendency for *b*/*a* to increase as we pass from a quantity, such as the range of a diurnal inequality, which is comparatively independent of disturbances, to a quantity such as the mean absolute daily range, which is largely dependent on disturbances. Formula (1) becomes, however, less and less strictly applicable, the more disturbed the magnetic quantity to which it is applied. When we consider quantities such as the mean of the 12 monthly ranges (maximum and minimum for the month), or the annual range (maximum and minimum for the year), we find large differences between observed values and those calculated from (1). In all the stations considered, 1893, though a year of absolute sunspot maximum, was much less disturbed than 1892. At Pawlowsk and Katharinenburg 1893 might fairly be termed a quiet year, while 1892 and 1894 were largely and persistently disturbed.

In the case of ranges from mean diurnal inequalities for the year, the agreement between observed and calculated values is about equally good at Pawlowsk, Katharinenburg, Batavia, and Kew. In the case of declination, the mean difference between observed and calculated values is about 4 per cent. of the mean value of the range during the period dealt with. On the whole, the agreement is distinctly less good in the case of vertical force than in the case of declination, inclination or horizontal force.

When extracting data from the earlier of the two papers by Mr. Ellis* already mentioned, I found that he had there advanced evidence—though he seems hardly to have considered it conclusive—that the difference between the Greenwich declination and horizontal force ranges in years of many and few sunspots was not the same in different seasons of the year. Converted into exact mathematical language, this would imply the variability of b throughout the year. If I had been aware at the time of Mr. Ellis's remarks on this point I should have referred to them in my preliminary note† on this subject.

"The Optical Properties of Vitreous Silica." By J. W. GIFFORD and W. A. SHENSTONE, F.R.S.

Received January 12,—Read March 3, 1904.

The properties of vitreous silica suggest that it is not unlikely to play an important part in optical work. Its composition is definite, that is to say, it is not liable to those minute variations which make it impossible, we believe, to produce with certainty two meltings of glass which exhibit no sensible difference in their optical properties when tested by a first-rate spectrometer. Hardly any corrosive fumes, except those of fluorine and hydrogen fluoride, attack silica, and it is indifferent to most ordinary solvents. It is as transparent to ultra-violet radiations as quartz, but is not doubly refracting like that substance. And though it is a little difficult to prepare vitreous silica in large masses, this difficulty can be surmounted, and the supply of the substance is not limited like that of fluorite. In short, vitreous silica places at our disposal a really standard glass. The refractive index of the new glass is low, it approaches that of fluorite. Its dispersive power is sensibly greater than that of quartz.

The measurements given in this paper were made with a prism having faces 41 mm. high by 32 mm. wide, and angles of 60° approximately. The mass of silica from which this prism was cut was made under the supervision of one of us in conjunction with Mr. H. G. Lacell, to whom our thanks are due. As it was our object to produce a standard substance for optical work, no care was spared at this stage. In making the mass of silica the spectroscopic traces of lithium and the traces of sodium which occur in quartz were burnt out as completely as possible in the oxy-gas flame. The prism itself was built up from many hundreds of fine rods of vitreous silica, prepared specially for this purpose by a process which has been

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* 'Phil. Trans.,' for 1880.

† 'Roy. Soc. Proc.,' vol. 71, p. 221.

described previously.* These rods were applied when softened by heat to one end of a thicker rod of silica and gradually melted into the larger mass, the larger mass itself being maintained in a semi-molten state during the operation about the part where the addition was made by means of a powerful combination of four "mixed gas" oxy-gas flames. The process occupied many days, and in order to secure uniformity in the finished block, the whole was heated throughout after every interval in the process as thoroughly as possible before further additions were made. During the whole process the greatest care was taken to preserve the silica from dust and even from contact with the workman's hand. The complete absence of particles of foreign matter is of great importance, since they would be apt to cause the silica to devitrify during annealing.

The large mass of silica thus prepared was found to be not quite homogeneous when the prism cut from it was examined in the spectrometer. Therefore, it was afterwards securely sealed up in a case made of thick platinum foil to protect it from dust, and heated in an oxy-hydrogen furnace till pieces of platinum wire placed near it in the furnace began to melt. To allow time for the heat to penetrate the silica, which is not a good conductor, the process was prolonged so that the silica was kept at a temperature not far below the maximum for several hours. It was then gradually cooled down by slowly reducing the flame of the furnace during 5 or 6 hours, and finally left to fall to the ordinary temperature after all the openings of the furnace had been closed.

As it did not seem safe to assume that one prism of silica so prepared would be identical in its optical properties with another, a second prism was constructed in quite a different way. This second prism was a compound prism. It consisted of four flat prisms cemented together one above the other. This compound prism had angles of approximately 60°, and its faces were 56 mm. high by 38 mm. wide (see fig. below). It was made from four slabs of silica prepared by Messrs. Baird and Tatlock. The workman, a trustworthy man, was told that each slab must be reheated till plastic throughout after it was built up. He worked without any supervision from us, and the results show that a uniform material may be obtained from silica without any difficulty and without any other precautions than those which a careful workman may be trusted to take. The four slabs of silica were rough ground into four prisms, cemented together, and a single prism was cut from the mass and finished by Mr. Hilger.

When the compound silica prism was tested in the spectrometer, we found that its performance generally was not to be distinguished from that of the simple prism used for the measurements given in the table,

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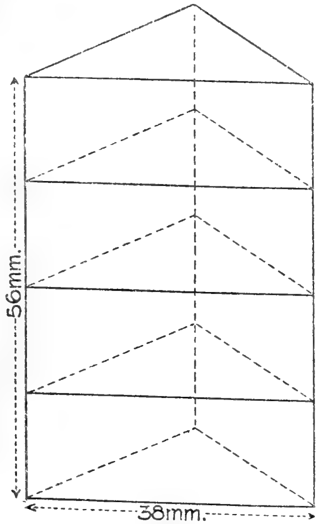
* 'Nature,' vol. 62, p. 20, and vol. 64, p. 45.

although the prism consisted of no less than four distinct masses of silica from four separate meltings.

We do not know whether a similar result could be obtained in the case of separate specimens of glass from *different* meltings. But we think not, for we found on a trial that a compound prism of



Compound Silica Prism, actual size.

Schott's borosilicate flint (No. 0·364), made from four pieces of glass taken from different parts of the *same melting*, gave a much less satisfactory spectrum than the compound prism made from *four separate meltings* of silica. When the two prisms were compared in the spectrometer, the D line given by the glass prism presented itself as a bundle of six lines,* whilst that given by the silica prism was normal, the D line being sensibly divided.

Measurements of Refractive Index.

Measurements of the refractive indices have been made with the same goniometer, by the same method, and for the same wave-lengths as those previously given in the case of fluorite, quartz and calcite,† and the same tests have been applied.

Errors due to the Method.—No one of the angles of the prisms differed from 60° by as much as 1′ of arc, and the error is therefore in the tenth place of decimals.‡

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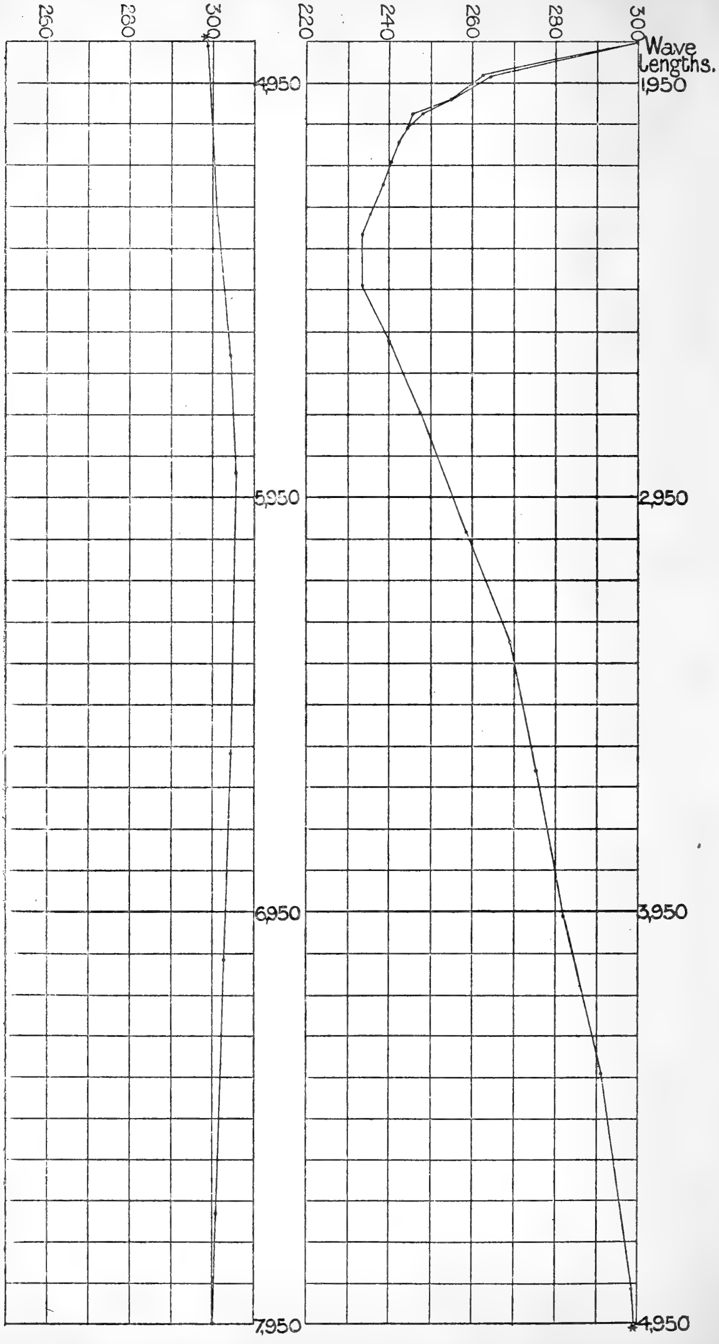
* The measurements of the mean D line for the components of the glass prism were:—Nos. 1 and 4 = 1·5757374, No. 2 = 1·5757179, No. 3 = 1·5757978.

† 'Roy. Soc. Proc.,' February 13, 1902.

‡ See note by Dr. R. T. Glazebrook, *loc. cit.*


260
280
300
4,950
5,950
6,950
7,950
220
240
260
280
300
Wave
Lengths.
1,950
2,950
3,950
4,950

Focal Curve.—The curve shown (p. 204), is that for a thin doublet of fluorite achromatised by vitreous silica (vitrified quartz). It will be seen that the focal length of the combination is almost independent of the wave-length. A list of focal lengths is also given (Appendix I).

Measure of Error.—The approximate estimate of the error from group deviations before referred to,* shows that of the 26 final deviations :—

In 6 of them the error is less than 0″·667,
„ 7 „ „ „ 1″·333,
„ 8 „ „ „ 2″·667,
„ 4 „ „ more „ 2″·667,
„ 1 only „ is as much as 9″·514,

the respective and corresponding variations of the index being—

0·0000022
0·0000045
0·0000089
0·0000286

As additional evidence of accuracy, it may be mentioned that the first measurement of line D was made with the larger prism in May, 1902, the index being..	1·4584772
The other prism mentioned above, built up of four separate small prisms of different meltings, cemented and ground all together, was produced in April, 1903, and gave an index of ...	1·4584830
The difference of the two readings being	0·0000058

A similar comparison of measurements from both prisms of the F line shows a difference of 0·0000103.

The coincidence of the four lines given by the four small prisms, of which the latter was built up was perfect, the definition being equal to that of the first prism. The D Lines were in both cases seen distinctly separated.

Probable Accuracy of the Indices.—The prisms were comparatively small and contained numerous bubbles.† For this reason the accuracy of the indices is not so great as of those of ordinary quartz already given.‡ But the indices of vitreous silica may be taken as correct to the fourth decimal place, and it is believed that in almost all cases the error does not exceed unity in the fifth place and in many does not exceed five in the sixth.

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[*] *Loc. cit.*

[†] This defect is not inherent in the material and will doubtless be eliminated as experience is acquired.

[‡] *Loc. cit.*

Interpolation.—In four cases alternative indices obtained by interpolation have been given. They are probably more correct for the wave-lengths taken and make a smoother curve.

The results of the observations are given in the following table of refractive indices. The partial and proportional dispersions of fluorite and vitreous silica are given in Appendix II.

Refractive Indices of Vitreous Silica.

Wave-length.	Index.	Wave-length.	Index.
7950 (Rb) ...	1·453398	2748·68 (Cd)	1·496131
A′ 7682·45 (K_α) ...	1·4538915	2573·12 „	1·503707
B′ 7065·59 (He)* ..	1·455180	2445·86 (Ag)	1·51096
C 6563·04 (H_α) ...	1·4564147	2312·95 (Cd)	1·519373
D 5893·17 (Na) ...	1·4584772	2265·13 „	1·523053
Λ 5607·1 (Pb) ...	1·459507	2194·4 „	1·529103
E 5270·11 (Fe) ...	1·4609945	2144·45 „ †....	1·533898
F 4861·49 (H_β) ...	1·463165	2098·8 (Zn)	1·538547
G′ 4340·66 (H_γ) ...	1·4668500	2062·0 „	1·54271
H′ 3961·68 (Al) ...	1·470542	2024·2 „ ‡....	1·54721
3610·66 (Cd) ...	1·475112	1988·1 (Al)	1·551990
3302·85 (Zn) ...	1·480610	1933·5 „ §....	1·55998
3034·21 (Sn) ...	1·486881	1852·2 „	1·5743

Temperature Refraction coefficient for D for 1° C. —0·00000346.||

Note.—The number of figures in each index indicate the estimated freedom from errors of observation. The following interpolated indices (see focal curves) are in all probability more correct for the wave-lengths given—

* 1·45516. † 1·53392. ‡ 1·54728. § 1·56003.

|| Communicated on March 3, 1904, in reply to a question by Dr. Watson.

In conclusion we desire to record the fact that though the cost of the experiments now described was not provided from the Government Grant Fund, yet this work has sprung from earlier work which was so assisted.

APPENDIX I.

Table giving the Focal Lengths in Metres of a Compound Lens of Fluorite and Vitreous Silica, achromatised for Wave-lengths 7950 and 1852.

Radii—R = 0·38733, S = 0·20351, R′ = S, S′ = ∞. R, S, R′, S′ refer to the surfaces of the two lenses.

Wave-length.		Focal length.	Wave-length.	Focal length.
7950		1·00000	2749	0·99250
7682	A′	1·00010	2573	0·99143
7066	B′	1·00045	2446	0·99054
6563	C	1·00070	2313	0 99055
5893	D	1·00086	2265	0·99078
5607		1·00059	2194	0·99120
5270	E	1·00017	2144	0·99151
4861	F	0·99983	2099	0·99174
4341	G′	0·99874	2062	0·99205
3962	H′	0·99743	2024	0·99258
3611		0·99653	1988	0·99360
3303		0·99558	1933	0·99490
3034		0·99409	1852	1·00000

APPENDIX II

Partial and proportional Dispersions of Fluorite and Vitreous Silica.

Substance.	C to F ($\delta\mu$).	$\mu_D - 1$.	$\frac{\mu_D - 1}{\delta\mu} = \nu$.	$\frac{\nu}{\nu'} = N$.	A′ to D.	D to F.	F to G′.
Fluorite	0·0045433	0·4338542	95·493*		0·0029048 0·63936	0·0032124 0·70706	0·0025594 0·56333
				1·406	0·03997	−0·01260	−0·01743
Vitreous Silica.........	0·0067503	0·4584772	67·920		0·0045857 0·67933	0·0016878 0·69446	0·0036850 0·54590
Quartz	0·0077697	0·5442558	70·048	..	0·0051954 0·66867	0·0054445 0·700735	0·0042755 0·55027

Note.—The last three columns give details as to the dispersions between the lines indicated. In each group of two lines connected with the given substance, the first line gives the differences in refractive indices, the second the ratio of these differences to the mean dispersion $\delta\mu$. The line between the groups in old-faced type shows the outstanding secondary dispersion when lenses of the two materials are combined.

* Given in error as 97·493 in the previous paper.

"Atmospherical Radio-activity in High Latitudes." By George C. Simpson, B.Sc., 1851 Exhibition Scholar, Owens College, Manchester. Communicated by Arthur Schuster, F.R.S.

Received February 3,—Read February 18th, 1904.

In 1901 Elster and Geitel first showed that a wire stretched out in the open air and charged to a high negative potential becomes radio-active; this observation has since been repeated by others, and the discoverers themselves have undertaken a series of daily measurements extending from December, 1901 to December, 1902. As the geographical distribution of the atmospheric radio-activity is at present entirely unknown, the observations which I have been able to make here in Karasjoh (Norway, 69° 20′ N., 25° 30′ E.), by the courtesy of the Commissioners for the 1851 Exhibition Scholarship, must be of considerable interest.

In my measurements I have used the method described by Elster and Geitel in the 'Physikalische Zeitschrift.'* A wire was stretched between an insulator in the open and another in my living room, the part exposed to the open air, 10 metres long, could be detached from the rest. The wire was charged to a negative potential by means of a small influence machine, built on the principle of a Kelvin replenisher and driven by a weight; by means of a variable high resistance, consisting of a strip of ebonite, one side of which had been rubbed with a black-lead pencil and so mounted in a tube that an earth-connected pad could move along it, the potential of the wire could be very easily regulated. In all my observations the potential was maintained at about 2250 volts, never rising above 2500 and never falling below 2000. The wire after being charged for 2 hours was removed to have its radio-activity determined; this was done by wrapping it round a cylinder of wire netting which fits inside the "protection cylinder" of an Elster and Geitel dissipation electrometer, which for this special purpose is closed at the bottom as well as the top; the rate at which the electrometer is discharged gives a measure of the induced radio-activity. Elster and Geitel have chosen the following as the arbitrary unit of the induced radio-activity. "The activity of the air is put equal to 1 when, after a 2 hours' exposure, a metre of the wire reduces the potential of the dissipation cylinder by 1 volt in 1 hour." The radio-activity when expressed in this unit is denoted by A in the accompanying tables.

The mean value of the radio-activity at Wolfenbüttel in mid-Germany, as determined by Elster and Geitel during the year 1902, was 18·6, the individual values varying between 64 and 4. A complete discussion of the effect of the different meteorological elements is given in the original paper.†

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* Vol. 3, p. 305, 1902.

† 'Physikalische Zeitschrift,' vol. 4, p. 526, 1903.

Table I.—Details of the Observations.

Date. 1903.	Observations made at 7.30 A.M.	Radio-activity (A), weather and potential gradient (P), between 9 and 12 A.M.	Observations made at 1.30 P.M.	Radio-activity (A), weather and potential gradient (P), between 3 and 6 P.M.	Observations made at 7.30 P.M.	Radio-activity (A), weather and potential gradient (P), between 9 and 11 P.M.
Nov. 23	B. 722·8 T. −3°·0 W. 0	**A = 48.** P. 101. Fine but cloudy.	B. 724·3 T. −1°·2 W. 1 S.	**A = 42.** P. 83. Little snow falling.	B. 724·6 T. −4°·2 W. 0	**A = 126.** P. 52. Little snow falling. Aurora visible through clouds.
" 24	B. 723·0 T. −6°·0 W. 1 N.	**A = 36.** P. 87. Had snow in the night. Clouds 8—6.	B. 725·0 T. −6·0 W. 2 S.	**A = 54.** P. 152. Clear, clouds 5—0.	B. 726·5 T. −8°·8 W. 0	**A = 42.** P. 307. *Clear*, cloudless. A little aurora light in N. until 10.30.
" 25	B. 727·7 T. −28°·0 W. 0	**A = 78.** P. 136. Cloudless, cold morning.	B. 728·6 T. −32°·3 W. 0	**A = 72.** P. 381. Cloudy, started to snow at end of observation.	B. 729·3 T. −25°·4 W. 0	**A = 150.** P. 288. Cloudless evening, but no aurora visible.
" 26	B. 731·5 T. −30°·0 W. 0.	**A = 114.** P. 278. Clear, cold, cloudless day.	B. 734·3 T. −34°·7 W. 0	**A = 150.** P. 387. Cloudless, faint aurora arch all the time.	B. 736·0 T. −37°·4 W. 0	**A = 138.** P. 331. Aurora and clouds.
" 27	B. 731·0 T. −30°·0 W. 0	**A = 78.** P. 474. Overcast, with thin clouds, which cleared away at 12 o'clock.	B. 732·0 T. −33°·0 W. 0	**A = 144.** P. 236. Clear, cold, cloudless. No aurora visible.	B. 733·6 T. −32°·0 W. 0	**A = 294.** P. 156. At 8 o'clock aurora was visible as diffused light in the north, from 8—11. it could only be seen by aid of the spectroscope.
" 28	B. 723·1 T. −33°·0 W. 0	**A = 18.** P. 276. Overcast, with snow falling in fine flakes. Wire became covered with fine coat of rime.	B. 731·0 T. −22°·4 W. 0	**A = 54.** P. 302. Fine, but completely overcast.	B. 730·2 T. −17°·8 W. 0	**A = 126.** P. 108. Snowing at first, but, during observation, it stopped, and the clouds broke a little, but the sky remained mainly overcast.

„ 30	B. 735·1 T. −19°·8 W. 0	**A** = **66.** P. 70. Very low clouds, or raised fog, snowed a little towards end.	B. 734·5 T. −15°·6 W. 0	**A** = **84.** P. 110. Sky about eight-tenths overcast.	B. 733·4 T. −19°·5 W. 1 N.	**A** = **228.** P. 147. Haze over the sky. No aurora visible.
Dec. 1	B. 737·0 T. −33°·0 W. 0	**A** = **60.** P. 312. Bright, cloudless, cold.	B. 739·3 T. −34°·0 W. 0	**A** = **102.** P. 279. Air filled with ice crystals.	B. 740·0 T. −35°·0 W. 0	**A** = **72.** P. 248. Air filled with ice crystals, and wire became slightly coated with frost.
„ 2	B. 741·7 T. −35°·2 W. 0	**A** = **144.** P. 278. Sky clear until about 7 o'clock in the morning, after which low clouds.	B. 742·5 T. −30°·0 W. 0	**A** = **36.** P. 264. Mist, wire became coated with a thin layer of frost.	B. 741·7 T. −27°·6 W. 0	**A** = **18.** P. 350. Misty, wire became thickly coated with rime.
„ 3	B. 741·0 T. −18°·5 W. 0	**A** = **54.** P. 142. 1st hour misty, 2nd hour clearer; wire obtained thin layer of frost.	B. 748·8 T. −19°·4 W. 0	**A** = **120.** P. 115. Overcast, thin coat of frost on wire.	B. 741·5 T. −24°·5 W. 0	**A** = **126.** P. 153. Air filled with ice crystals. Thin coat of frost on wire.
„ 4	B. 738·0 T. −14°·5 W. 1 E.	**A** = **84.** Overcast, with a little snow falling.	B. 736·3 T. −7°·0 W. 3 S.	**A** = **54.** Heavily overcast, with a high wind (3—4 S.E.).	B. 736·8 T. −1°·8 W. 1 S.E.	**A** = **24.** Negative P. Heavily overcast, with high S.E. wind.
„ 5	B. 739·4 T. 1°·7 W. 2 S.	**A** = **18.** Negative P. Dull, overcast, high wind	B. 742·2 T. −0°·2 W. 3 S.	**A** = **18.** Negative P. Overcast, but not so heavily as in the morning; high wind.	B. 742·2 T. −1°·0 W. 3 S.	**A** = **24.** Negative P.G. Overcast, high wind.
„ 7	B. 738·0 T. +1°·0 W. 1 S.E.	**A** = **144.** P. 46. Overcast, with a few breaks in the clouds.	B. 740·2 T. +0°·5 W. 1 E.	**A** = **78.** Clear, with aurora.	B. 742·6 T. −4°·4 W. 1 E.	**A** = **126.** P. 83. Overcast.
„ 8	B. 745·0 T. −3°·8 W. 1 S.W.	**A** = **36.** P. 91. Overcast.	B. 747·4 T. −4°·2 W. 2 S.	**A** = **48.** P. 71. Overcast.	B. 750·7 T. −8°·4 W. 1 E.	**A** = **136.** P. 91. During exposition clouds became much thinner, so that at end the moon and a few bright stars could be seen.

Table I.—*continued.*

Date. 1903.	Observations made at 7.30 A.M.	Radio-activity (A), weather and potential gradient (P), between 9 and 12 A.M.	Observations made at 1.30 P.M.	Radio-activity (A), weather and potential gradient (P), between 3 and 6 P.M.	Observations made at 7.30 P.M.	Radio-activity (A), weather and potential gradient (P), between 9 and 11 P.M.
Dec. 9	B. 754·0 T. −23°·8 W. 0	**A = 84.** P. 124. At first only high transparent clouds, but it soon became overcast and remained so.	B. 755·8 T. −17°·8 W. 1 S.	**A = 84.** P. 127. Heavily overcast, with a little snow.	B. 752·0 T. −10°·0 W. 0	**A = 174.** P. 96. Overcast, with a little snow.
,, 10	B. 751·0 T. +0°·8 W. 1 S.E.	**A = 138.** P. 33. Overcast, but at times breaks could be seen in the clouds and a very little rain fell.	B. 753·0 T. −2°·0 W. 0	**A = 90.** P. 87. Overcast, with a warm S. wind.	B. 753·2 T. 2°·5 W. 0	**A = 198.** P. 82. Overcast, with a warm S. wind.
,, 11	B. 753·6 T. 1°·5 W. 1 S.	**A = 168.** P. 48. Overcast, with a few clear patches.	B. 754·5 T. 1°·0 W. 2 S.	**A = 48.** P. −39. Heavily overcast.	B. 756·0 T. −1°·3 W. 1 E.	**A = 66.** P. − 45. Heavily overcast.
,, 12	B. 756·2 T. −7°·0 W. 1 E.	**A = 60.** P. 194. Clouds 7.	B. 754·6 T. −4°·5 W. 0	**A = 60.** P.G. 148. Overcast, but at times a few stars could be seen.	B. 754·4 T. −5°·0 W. 0	**A = 78.** P. 192. Clear, cloudless evening. A very little aurora light visible at commencement, but it soon disappeared.
,, 13	B. 755·8 T. −16°·8 W. 0		B. 753·4 T. −10°·0 W. 0		B. 752·0 T. −8°·8 W. 0	**A = 150.** P. 459. Wire exposed during the most brilliant aurora display we have yet had.

,, 14	B. 755·3 T. −12°·0 W. 0	**A** = **102.** P. 243. Overcast, but clouds are not quite so continuous, so that a little blue can be seen between.	B. 752·0 T. −9°·0 W. 0	**A** = **72.** P. 330. At first overcast, but clouds soon disappeared. No sign of aurora. Thin coat of frost on wire.	B. 756·0 T. −18°·0 W. 0	
,, 15	B. 748·8 T. −6°·7 W. 0	**A** = **78.** P. 150. Bright morning with thin high clouds.	B. 749·0 T. −11°·2 W. 0	**A** = **84.** P. 133. Overcast.	B. 743·6 T. −8°·2 W. 0	**A** = **102.** P. 265. Snowing from very thin clouds, so that at times bright stars were visible.
,, 16	B. 745·0 T. −8°·2 W. 0	**A** = **174.** P. 115. Overcast, with thin clouds.	B. 744·7 T. −8° W. 0	**A** = **138.** P. 83. Overcast, with a little snow towards end.	B. 744·7 T. −7°·5 W. 0	**A** = **126.** P. 163. At first overcast, but towards end sky cleared and a little aurora light became visible.
,, 17	B. 749·5 T. −18°·5 W. 0	**A** = **66.** P. 310. Clear cold morning. No clouds in 1st hour, thin ones in 2nd hour. Thin coat of frost on wire.	B. 753·2 T. −21°·0 W. 0	**A** = **384.** P. 168. Overcast, with thin clouds, bright stars visible every now and then towards end.	B. 754·5 T. −23°·0 W. 0	**A** = **432.** P. 106. Clear, cloudless evening. No aurora visible.
,, 18	B. 750·0 T. −17°·3 W. 0	**A** = **204.** P. 116. Cloudless the greater part of time, just a few (1—2) thin high clouds at beginning and end.	B. 747·2 T. −15°·0 W. 1 S.	**A** = **66.** P. 235. Overcast, with thin clouds, through which stars can be seen.	B. 743·5 T. −7°·0 W. 2 N.W.	The wind having risen to a gale, observations were impossible.
,, 19	B. 747·1 T. −9°·5 W. 2 N.W.	**A** = **36.** P. 81. Clouds vary between 3 and 6. Little snow at end.	B. 748·2 T. −11°·6 W. 1 S.	**A** = **42.** P. 324. Cloudless, no aurora.	B. 750·0 T. −18°·5 W. 0	**A** = **21.** P. 461. Mainly cloudless, but a few clouds appeared in the N. towards end. Thin coat of frost on wire.

The first few measurements which I made having shown that the radio-activity is very much greater than in lower latitudes, I determined to make a thorough investigation of it. On November 23, I started a series of measurements of the radio-activity, taking three observations a day; the first in the morning between the hours of 9 and 12, the second in the afternoon between 3 and 6, and the third in the evening between 9 and 11; the series continued uninterrupted for 4 weeks. The results are given in full in Table I, when the following symbols are used:

B = height of barometer in centimetres.
T = temperature in Centigrade degrees.
W = estimated velocity of wind, the direction being added.
P = potential gradient measured in volts per metre.

It will at once be seen that the numbers are very much higher than the corresponding ones for Germany, the mean for the month being 102, which is nearly six times as great as the German mean (for the year); and the highest value, 432, being nearly seven times greater than the German highest. To turn now to a closer investigation of the numbers, Table II shows that there is a marked daily period.

Table II.—Mean and Maximum Values of **A** for each Week.

	Morning **A.**		Afternoon **A.**		Evening **A.**		Mean of three.
	Mean.	Max.	Mean.	Max.	Mean.	Max.	
Nov. 23 to Nov. 28 ..	62	114	86	150	146	294	98
Nov. 30 to Dec. 5 ...	71	144	69	120	82	228	74
Dec. 7 to Dec. 12 ...	105	168	68	90	129	198	101
Dec. 13 to Dec. 19 ..	110	204	131	384	166	432	136
Mean for month ...	87		88		131		102

The maximum falls in the evening, there being little difference between the morning and afternoon means. Not only is the mean value greatest for the evening, but the absolute maximum falls in the evening each week, and on 13 out of 22 days on which three observations were taken the evening values were the greatest.

It would be of interest to find the relation, if any, between the radio-activity and the other electrical conditions of the atmosphere; but my observations of the dissipation and the ironisation of the air are not sufficiently complete for a rigid comparison, and so must be left for future investigation. Table III gives the relation between the radio-activity and potential gradient. The mean potential gradient for the period of each exposure having been taken with a self-registering electrometer, the values of the radio-activity for each interval of 50 volts meter have been put together and the mean taken.

Table III.—Radio-activity and Potential Gradient, measured in Volts per Metre.

	Negative pol. grad.	0—50.	50—100.	100—150.	150—200.
No. of observations.	6	3	13	17	8
A mean	**33**	**150**	**96**	**140**	**150**
A maximum.......	66	168	198	432	384

	200—250.	250—300.	300—350.	350—400.	400—450.
No. of observations.	4	6	8	2	3
A mean	**96**	**84**	**61**	**111**	**83**
A maximum	144	144	138	150	150

As can be seen, there is no direct relation between the radio-activity and the potential gradient.

To come now to the effect of the meteorological elements, Table IV shows that temperature and radio-activity are not closely related.

Table IV.—Radio-activity and Temperature.

Temperature °C. ..	−40° to −30°	−30° to −20°.	−20° to −10°.	−10° to 0°.	0° to 10°.
No. of observations.	12	10	17	25	8
A mean	**127**	**166**	**80**	**82**	**110**
A maximum	294	432	204	174	198

Nor does the barometric pressure appear to have any great influence.

Table V.—Radio-activity and Barometer.

Barometer	720—730.	730—740.	740—750.	750—760.
No. of observations ...	10	18	23	20
A maximum.........	**66**	**109**	**85**	**137**
A mean.............	180	294	204	432

The only meteorological element which appears to have a direct influence on the atmospherical radio-activity is the amount of cloud. To show this I have divided the observations into three classes: 1st Sky clear, or nearly so; 2nd Detached clouds; 3rd Sky completely overcast, and taken the mean for each class. The results are given in Table VI.

Table VI.—Radio-activity and Clouds.

Clouds.	Clear sky.	Detached clouds.	Completely overcast.
No. of observations ...	18	26	27
A mean.............	**130**	**107**	**76**
A maximum.........	432	384	198

There does not appear to be any close connection between the aurora and the radio-activity, the greatest value of the radio-activity having been obtained when no aurora was visible.

During the whole time these observations were being taken the sun did not rise above the horizon. The time used was mid-European, local mean time being 42 minutes ahead.

The place of observation is 140 metres above sea-level. The ground for a hundred miles round is hard frozen to a great depth and covered with a coating of snow the average depth of which is over 2 feet. Sudden changes in radio-activity are sometimes observed to take place, as, for example, on December 17, when with a rising barometer the activity rose for a few hours from the low value of 66 to the exceptionally high one of 384. There is some difficulty in reconciling these observations with Elster and Geitel's view that the activity is entirely due to a diffusion of a radio-active emanation from the soil.

"On the High Temperature Standards of the National Physical Laboratory: an Account of a Comparison of Platinum Thermometers and Thermo-junctions with the Gas Thermometer."

By J. A. HARKER, D.Sc., Fellow of Owens College, Manchester, Assistant at the National Physical Laboratory.

Communicated by R. T. GLAZEBROOK, F.R.S. Received January 20,—Read February 11, 1904.

(From the National Physical Laboratory.)

(Abstract.)

This paper contains an account of a continuation of the work of Dr. P. Chappuis and the author,* on a comparison of the scale of the gas thermometer with that of certain specially-constructed platinum thermometers, from temperatures below zero up to the boiling point of sulphur, and in one case to a point close to 600° C.

The results of this work substantially confirm the experiments of Callendar and Griffiths, and show that the indications of the platinum thermometer may be reduced to the normal scale by the aid of Callendar's difference formula :

$$d = \mathrm{T} - pt = \delta\left[\left(\frac{\mathrm{T}}{100}\right)^2 - \frac{\mathrm{T}}{100}\right],$$

where pt is the platinum temperature, T the temperature on the normal scale, and δ a constant which, for pure platinum, does not differ much from the value 1·5.

The temperatures chosen for the determination of δ are 0° C., 100° C., and the boiling point of sulphur.

In the present paper the work is extended to a temperature of 1000° C. Moreover, a number of standard thermo-junctions of platinum—platinum-rhodium were included in the comparisons.

The gas-thermometer employed for this work was presented to the laboratory by Sir Andrew Noble; it was obtained, along with materials for the electric furnaces and thermo-junctions, through the kindness of Dr. Holborn, of the Reichsanstalt. The bulbs used were of porcelain, glazed inside and out, and the gas used was pure dry nitrogen. The thermo-junctions, which were carefully compared by Dr. Holborn with the standards of the Reichsanstalt, at a number of fixed points up to 960° C., were again tested and compared together before use, with concordant results. A special potentiometer designed and made in the laboratory enabled the thermo-junction readings to be taken with great accuracy.

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* 'Phil. Trans.,' A, 1900.

The platinum thermometers employed were one of the three used by Harker and Chappuis in their earlier work, and a new one constructed in the laboratory as one of a number belonging to the British Association. The different instruments, after determination of their constants, were tested together in specially constructed electric resistance furnaces, heated from a special battery, in which temperatures from 400—1100° C. could be very steadily maintained for considerable periods. Special winding enabled a compensation to be made for the greater cooling effect at the ends of the furnaces, so that over a considerable length the temperature was exceedingly uniform.

The following table gives the results of one series of comparisons, and indicates the agreement obtained:—

Second Series of Comparisons. Compensated Furnace.

Gas Thermometer, Platinum Thermometer BA_2, Thermo-junction NPL_2.

No. of experiment.	Temperature.			G − Pt.	G − Th.	P − Th.
	Gas thermometer.	Junction.	Pt. thermometer.			
5	523 ·1	524 ·3	524 ·39	−1 ·3	−1 ·2	+0 ·1
10	568 ·5	569 ·5	569 ·35	−0 ·9	−1 ·0	−0 ·2
8	598 ·5	597 ·8	597 ·62	+0 ·9	+0 ·7	−0 ·2
1	599 ·4	599 ·0	598 ·80	+0 ·6	+0 ·4	−0 ·2
3	641 ·1	641 ·1	641 ·75	+0 ·6	+0 ·0	−0 ·6
4	682 ·4	683 ·0	682 ·54	−0 ·1	−0 ·6	−0 ·5
2	776 ·7	775 ·5	775 ·13	+1 ·6	+1 ·2	−0 ·4
9	820 ·0	818 ·4	818 ·31	+1 ·7	+1 ·6	−0 ·1
11	831 ·4	832 ·2	831 ·86	−0 ·5	−0 ·8	−0 ·3
12	866 ·7	868 ·4	868 ·28	−1 ·6	−1 ·7	−0 ·1
6	875 ·0	875 ·4	875 ·24	−0 ·2	−0 ·4	−0 ·2
7	959 ·8	956 ·0	955 ·47	+4 ·3	+3 ·8	−0 ·5
13	1005 ·0	1004 ·4	1004 ·37	+0 ·6	+0 ·6	−0 ·0

Thus the investigation shows that:

1. The readings of the platinum thermometers BA_2 and K_2, which may be taken as representative instruments, when reduced to the air scale by the use of Callendar's difference formula are, up to a temperature of 1000° C., in reasonably close agreement with the results obtained from the constant volume nitrogen thermometer, employing chemical nitrogen, and using the received value for the dilatation of the Berlin porcelain, of which the bulb is made.

2. The above platinum thermometers agree very closely with a set or thermo-junctions representing the temperature scale of the Reichsanstalt, and based on measurements with a gas thermometer having a bulb of platinum-iridium.

As the results of these experiments seem to justify very completely the use of Callendar's parabolic formula over a wide range, a table has been calculated by which the value of T may be obtained directly from the value of Pt for a range of temperature extending from -200 to $+1100°$ C., and for the value 1·5 of the constant δ.

A second short table extends this to all values of δ usually met with. It is hoped that this table may be of general use to others who are employing platinum thermometers.

The experiments were carried out at the National Physical Laboratory, and, in conclusion, I wish to thank those members of the staff who have assisted in it.

"The Spectra of Antarian Stars in Relation to the Fluted Spectrum of Titanium." By A. FOWLER, A.R.C.S., F.R.A.S., Assistant Professor of Physics at the Royal College of Science, South Kensington. Communicated by Professor H. L. CALLENDAR, F.R.S. Received February 18,—Read March 3, 1904.

[PLATE 6.]

The distinguishing feature of the spectra of the Antarian Stars* is the system of apparently dark flutings, sharp towards the violet and fading off towards the red end of the spectrum. The principal flutings are well seen in Antares, but they are more strongly developed in the spectra of α Herculis and o Ceti, in which stars additional details are also seen. These flutings have not hitherto received a definite chemical interpretation, and it has been uncertain, owing to the possibly misleading effects of contrast, whether the spectrum was to be regarded as one consisting wholly of absorption flutings fading towards the red, or as one partly consisting of emission flutings fading in the opposite direction.

The purpose of the present communication is to state the nature of the evidence which indicates that the spectrum is essentially an absorption spectrum, and that the chief substance concerned in the production of the flutings is titanium, or possibly a compound of that element with oxygen.

The first indication of this result was the striking general resemblance of the titanium flutings, as seen in photographs recently obtained, with the stellar flutings, both as to relative intensity and apparent position (Plate 6). The interspaces between the flutings, as they appear on a negative, in some cases also strongly recall the corresponding bright spaces in the stellar spectra.

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* Secchi's Type III; Vogel's Class IIIa.

The most extensive series of visual observations of the Antarian stars were made by Vogel* and Dunér† many years ago, and for the part of the spectrum extending from near D to the extreme red, no other measurements have yet been published. For the region more refrangible than D, however, wave-lengths derived from photographs are available, the most complete statements of these being due to Father Sidgreaves‡ and Mr. Stebbings.§ The individual results given by different observers vary considerably: visual observations are difficult, and, in the case of photographs taken with prismatic cameras, errors doubtless arise through the lack of suitable reference lines. There is also some difficulty in deciding where a fluting actually commences. The evidence in favour of a titanium origin for most of the flutings, however, depends on such a large number of coincidences that it is almost independent of a very precise knowledge of wave-lengths.

The flutings in question come out in the arc spectrum of titanium oxide, if the precaution be taken to provide a liberal supply of material and to use a very long arc, taking care also that the image of the "flame" is projected on the slit of the spectroscope. They are also seen in the arc spectrum of the chloride under similar conditions. Numerous lines accompany the flutings produced in this manner and some of the details are consequently masked or not recognized without careful study of the photographs.

So far the flutings have not been very successfully produced in the oxyhydrogen flame; they are visible in the flame spectrum of the fumes from the chloride, but their observation is difficult on account of the bright continuous spectrum.

The best representation of the flutings has been obtained by passing a spark, without jar, through the fumes of oxychloride which rise from the chloride of titanium on exposure to air. Under these circumstances the lines which appear are not numerous, and some of the secondary flutings which are masked by lines in the spectrum of the flame of the arc are readily detected, in spite of the continuous spectrum which is also present. The few lines which do appear in this spectrum are probably low temperature lines which may be found of special importance in the cooler stars.

Photographs have been taken over the region C to K, the instrument employed being one built up on the Littrow principle, having one prism of 60°,‖ and a 2-inch objective of 40 inches focal length, giving a linear dispersion from D to K of 5 inches. Wave-lengths were

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* 'Beobachtungen zu Bothkamp,' vol. 1, p. 20, etc.

† 'Sur les Etoiles à Spectres de la Troisième Classe'; "K. Sven. Vet.-Akad. Hand.," vol. 21, No. 2, 1884.

‡ 'Monthly Notices, R.A.S.,' vol. 58, p. 344; vol. 59, p. 509.

§ 'Lick Observatory Circular,' No. 41, May, 1903.

‖ Lent by the Government Grant Committee.

determined in the usual manner by micrometric measurements of the photographs, using reference lines of titanium and iron, and calculating by the Cornu-Hartmann formula; though only provisional, they are probably not greatly in error.

It is instructive first to make a comparison between the more conspicuous flutings and those recorded visually in the stars by Vogel and Dunér. Details of the measurements are given in Table I, but reference should also be made to Plate 6, in which Dunér's drawing of the spectrum of α Herculis, as seen with a spectroscope of small dispersion, is compared with a negative of the titanium flutings, as they appear in the "arc" spectrum of titanium oxide.

Table I.—Comparison of Titanium Flutings with Visual Observations of the Spectra of Antarian Stars.

Titanium flutings.		Antarian flutings (more refrangible edge).	
Wave-length.	Visual intensity.	Wave-length.*	Dunér's number.
7055	10	..	Out of range.
..	..	6493	1
6162·5	10	6164	2
..	..	5862	3
5604·5	8	5596	4
5447·0	10	5453	5
5241·0	5	5243	6
5167·5	10	5169	7
4955·1	8	4960	8
4761·6	7	4769	9
4584·3	5	4608	10

It will be seen be seen that eight of the ten bands recorded by Vogel and Dunér agree within the possible limits of error with the flutings of titanium, and it is to be noted also that the only one of the principal titanium flutings which is not represented in the stellar spectrum is out of range in the extreme red. The origin of the two outstanding bands at 5862 and 6493 has not yet been ascertained. There are traces of titanium flutings near their positions, but they seem inadequate to account for two such distinct bands as those drawn

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* The wave-lengths given are the means of Vogel's and Dunér's measurements, corrected to Rowland's scale (Scheiner's 'Astronomical Spectroscopy,' p. 301). For bands 5, 7, 8, 9, 10, the means of the wave-lengths derived from photographs by Lockyer, Pickering, Sidgreaves and Stebbings are respectively 5448, 5165, 4954 4761, and 4584 (see Table II).

Table II.—Comparison of Titanium Flutings with Photographic Spectra of Antarian Stars.

Flutings of Ti (Fowler).*		Flutings in stars (Stebbings).		Flutings in *o* Ceti (Sidgreaves).		Remarks.
λ.	Intensity.	λ mean of five stars.†	*o* Ceti.	λ.	Intensity.	
			4313 ·4			
4352 ·9	3	..	4352 ·7	4352	7	
4395 ·2	3	..	4395 ·0	4395	5	Masked by lines in arc spectrum.
4422 ·0	2	4421 ·5	4421 ·5	4421	4	” ” ”
4437 ·0	3	..	..	..	..	Masked in arc spectrum. Sidgreaves notes a "wide line" at 4436.
4461 ·9	4	..	4462 ·1	4460	6	Head well defined.
4505 ·0	4	..	4504 ·8	4504	6	Head not very well defined. Not recognised in arc spectrum.
4548 ·4	3	..	4547 ·9	4546	5	Head apparently strengthened by a line at 4548 ·9.
4584 ·3	6	4584 ·4	4584 ·1	4583	10	Beginning of Dunér's band 10, λ 4585 Lockyer,‡ 4586 Pickering.§ Head has the appearance of a double line.
4626 ·4	6	4626 ·3	4625 ·8	4625	8	Head well defined, and has the appearance of a double line.
4667 ·8	5	4667 ·6	4668 ·6	4669	9	” ” ” ” ” ”
4714 ·5	3	4714 ·1	4713 ·2	4714	4	Head not sharply defined.
4738 ·0	2	4737 ·1	..	4736	3	Possibly only an ill-defined line which appears in "chloride" spark, but not in the arc.
4761 ·6	7	4761 ·0	4759 ·0	4758	10	Beginning of Dunér's band 9, λ 4763 Lockyer, 4762 Pickering. Head sharply defined, and has the appearance of a double line.
4805 ·6	7	4804 ·6	4803 ·2	4803	6	Head appears as a double line.
4842 ·0	1	..	4841 ·8	4842	8	A feeble brightening seen only in best photographs; it is not the Ti line λ 4841 ·0.
4848 ·6	5	4848 ·4	..	..	..	Head appears as a double line.
4894 ·0	2	..	..	4892	3	Head indefinite.

4955 ·1	8	4954 ·5	4953 ·1	4951	10	Beginning of Dunér's band 8, λ 4958 Lockyer, 4954 Pickering. Head sharply defined ; a line at 4957 ·8 possibly forming part of a double head.
5002 ·8	4	..	..	4998	9	Head rather indefinite ; not clearly distinguished in arc. Lines at 4981 · 9, 4991 · 2, 4999 ·7, 5007 ·4, 5014 ·4.
5050 ·5	2	..	..	5046	6	Head rather indefinite ; not clearly distinguished in arc.
				5074	3	
				5098	4	
				5135	3	
5167 ·5	10	5165 ·8	5165 ·9	5162	10	Beginning of Dunér's band 7, λ 5165 Lockyer, 5168 Pickering. Head very sharply defined and perhaps double with 2nd head at 5169 ·8.
5241 ·0	5	..	..	5237	8	Beginning of Dunér's band 6. Begins with a line which is relatively stronger than in the arc spectrum.
5308 ·0	3	..	5307 ·2	5306	7	
5356 ·6	4	..	5357 ·7	5356	6	
5407 ·0	1	..	..	5406	2	
			5438 ·3			
5447 ·0	10	5446 ·8	5446 ·8	5447	10	Beginning of Dunér's band 5, λ 5455 Lockyer, 5445 Pickering. Head very sharply defined.
5497 ·5	5	5496 ·8	5496 ·9	5498	5	Head well defined.
5604 ·5	8	..	..	5597	8	Beginning of Dunér's band 4. There are strong lines near 5598, 5630, 5662, 5708, which, with small dispersion, might appear to be the heads of the adjacent flutings. Among other details within this group, Sidgreaves notes " bands " at 5603 and 5642, and a " wide line " at 5667.
5636 ·3	6					
5668 · 4	5	..	..	5660	7	
5713 ·9	3	..	..	5709	3	
5760 ·9	5	..	..	5756	8	Head well defined } Among the details in this region Sidgreaves notes " bands " at 5761, 5808, and 5845.
5811 ·0	5	..	..	5804	8	,, ,,
5847 ·2	3	..	..	5840	8	,, ,,

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* As seen in spark without jar through fumes from titanium chloride ; they are also seen in the arc unless otherwise stated.

† α Herculis, ρ Persei, β Pegasi, α Orionis, α Ceti.

‡ 'Phil. Trans.,' A, vol. 186, 1893, p. 702.

§ 'Annals Harv. Coll. Obs.,' vol. 28 Part 1, p. 8.

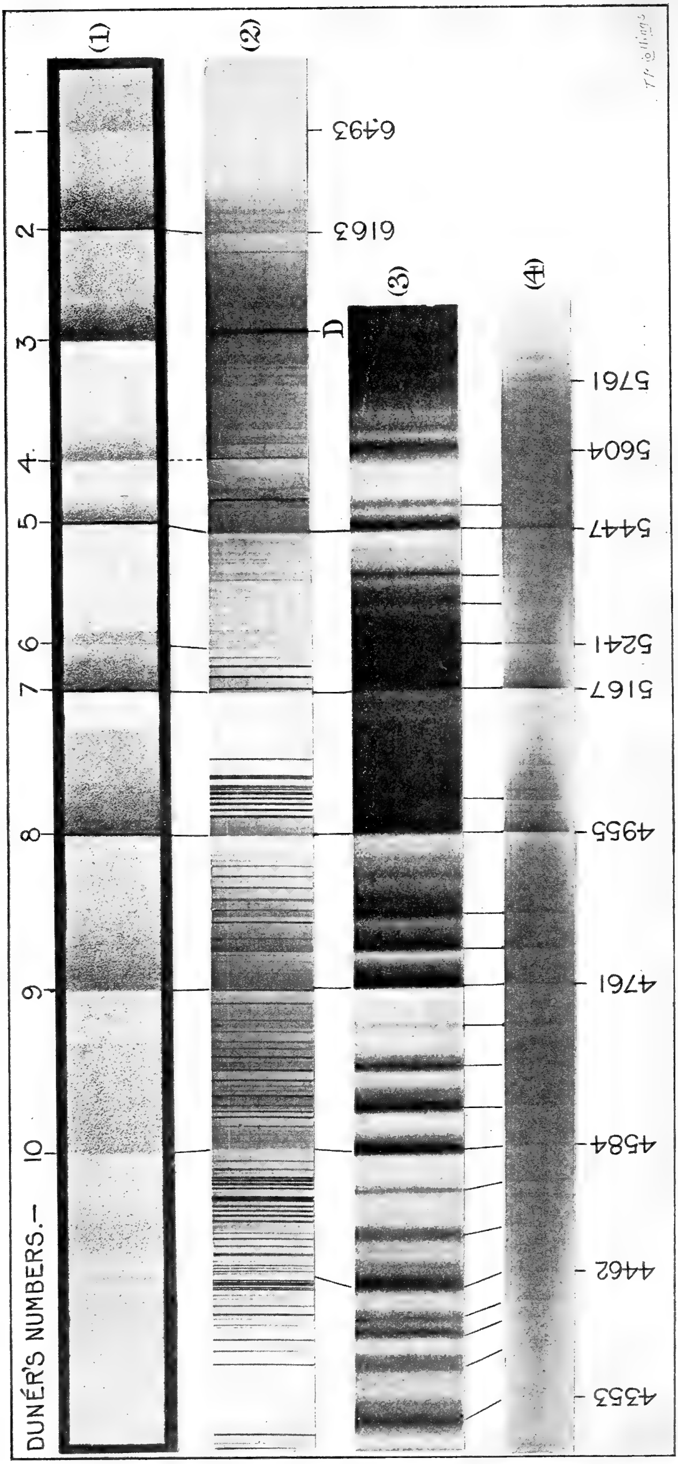
by Dunér. The association of vanadium with titanium in the spectra of sun spots suggested that they might be due to the former element, but this does not appear to be the case. The strongest fluting of vanadium is near 5472, and there is no certain evidence to show the presence of this fluting in the Antarian type of spectrum.

The evidence for titanium in the case of the remaining flutings, however, is enormously strengthened by a discussion of their structure and by extending the comparison further into the violet. Photographs of the stellar spectra, especially those of *o* Ceti and *α* Herculis, show that some of the principal flutings are composite, Dunér's band 10, for example, containing, according to Sidgreaves, four distinct flutings separated by intervals of about 44 tenth-metres, each of which is weaker than the one which precedes it on the more refrangible side. A precisely similar structure is found in the case of the titanium flutings, and a comparison of wave-lengths indicates that the various components occupy the same positions as those in the stars, so far as the available measurements permit the test to be applied. For this comparison (Table II) the wave-lengths derived from photographs by Father Sidgreaves and Mr. Stebbings are utilised. The relation may also be gathered by inspection of the reproductions of the photographs given in Plate 6, that of *o* Ceti having been very kindly placed at my disposal by Father Sidgreaves. Not all the details of the negatives, however, can be brought out in the reproductions, and the relative dispersions are not exactly the same.

It will be seen from the table that the details of the titanium flutings are reproduced with remarkable fidelity in the stellar spectra, and more especially in *o* Ceti. In the latter spectrum the number of flutings recorded is slightly greater than in the case of titanium, but it is by no means certain that every detail of the titanium spectrum has yet been photographed. It is possible also that some of the features described as flutings in the stellar spectrum may be groups of lines, and in at least one instance (4437) a fluting has been classed as a "wide line."

The points of difference are very slight, and are mostly in the less refrangible part of the spectrum, where the reductions of the stellar spectra present the greatest difficulty. There is a peculiar displacement of the fluting 4848 to λ 4842 in the spectrum of *o* Ceti, which may possibly be due to the superposition of a fluting or group of lines of undetermined origin; or, it may be that the feeble maximum observed at 4842 in titanium is strong enough in this spectrum to account for the apparent shift. There is also some uncertainty in connection with the complicated groups of flutings and lines extending from 5598 to D, which need further investigation in the stellar spectra with instruments of greater dispersion.

The general agreement is nevertheless such as to leave no reason-



(1) α Herculis (Dunér). (2) Titanium in flame of arc.
(3) o Ceti (Stonyhurst), (4) Titanium in spark without jar.

able doubt that titanium is the main factor in the production of the dark flutings which characterise the Antarian group of stars.

This explanation of the dark flutings suggests that the appearance of bright flutings in the Antarian spectrum arises chiefly from effects of contrast. It does not, of course, exclude the possibility of the presence of bright flutings, such as might be indicated by local brightenings which are not exactly in coincidence with the edges of dark flutings.

Whether the absorption flutings are produced by the vapour of titanium or by that of the oxide has not yet been completely determined. As already pointed out, the flutings may be obtained either from the oxide or the chloride, but as the latter so readily unites with oxygen on exposure to air, it furnishes no evidence against the supposition that the flutings are due to the oxide.

The author has pleasure in acknowledging the very able assistance which has been rendered in the experimental work involved in this investigation by Mr. F. W. Jordan, B.Sc., Teacher in Training in the Department of Astronomical Physics, Royal College of Science.

DESCRIPTION OF PLATE 6.

1. Visible spectrum of α Herculis, as drawn by Dunér.
2. Flutings of titanium, as they appear in the spectrum of the "flame of the arc," when charged with titanium oxide.
3. Photographic spectrum of *o* Ceti, from a photograph taken at the Stonyhurst College Observatory, November 29, 1897.
4. Flutings of titanium, as they appear when a spark, without jar, is passed through the fumes which rise from titanium chloride on exposure to air.

(*Note.*—The coincidences cannot be very exactly shown in this manner, on account of the differences of dispersion of the three instruments with which the spectra were recorded.)

"The Specific Heats of Metals and the Relation of Specific Heat to Atomic Weight.—Part III." By W. A. Tilden, D.Sc., F.R.S., Professor of Chemistry in the Royal College of Science, London. Received March 9,—Read March 17, 1904.

(Abstract.)

The object of the experiments, of which an account is given in this paper, was to determine whether the atomic heats of the elements entering into combination are preserved in the compound at all temperatures, previous results obtained by the author and others having shown that the specific heats of metals of small atomic weight, such as aluminium, increase very rapidly with rise of temperature.

As it is not possible to determine the specific heat of sulphur throughout a long range of temperature, tellurium was chosen for experiment. Compounds of tin, silver and nickel with tellurium were prepared, and two alloys of silver and aluminium. The average specific heats of all these elements, except tin, were determined over various intervals from the boiling point of liquid oxygen to nearly 500° C. in the case of the less fusible elements, a range of about 680° C. From these mean specific heats the true specific heats at intervals of 100 Centigrade degrees absolute temperature were calculated, and from the specific heats the atomic heats were deduced. The mean specific heats of the compounds, formed by their union, were also determined, and from these data the moelcular heats of the compounds calculated. On comparing the sum of the atomic heats of the elements present with the molecular heat of the compound at the successive temperatures, it was found that there is throughout a close concordance. The order of difference may be shown by one example—

Nickel Telluride, NiTe.

Temperature, absolute.	Sum of atomic heat of Ni and Te.	Molecular heat of NiTe.
100°...........	9·20	8·38
200°...........	11·08	11·35
300°...........	12·22	12·41
400°...........	13·00	12·92
500°...........	13·49	13·15
600°...........	13·85	13·28
700°...........	14·11	13·35

The results of these experiments show that Neumann's law is approximately true, not only at temperatures from 0° to 100° C., but at all temperatures. They thus support the view that the specific heat of

a solid is determined by the nature of the atoms composing the physical molecules, and is not a measure of the thermal work done in expansion.

The paper concludes with a discussion of the relations of specific heat to atomic weight in the solid, liquid and gaseous states.

"Further Researches on the Temperature Classification of Stars." By Sir NORMAN LOCKYER, K.C.B., LL.D., F.R.S.

Received January 30,—Read February 18, 1904.

[PLATES 7—9.]

CONTENTS.

1. *Historical Review.*

In my first Bakerian Lecture in 1873 I dealt with the question of the spectra of stars, and pointed out that the facts accumulated up to that time by Rutherford and others led to the view that in the reversing layers of the sun and stars various degrees of dissociation are at work.

I also suggested that the stellar evidence indicated that one of the results of dissociation temperatures could be to "prevent the coming together of atoms which at the temperature of the earth, and at all artificial temperatures yet obtained here, compose the metals, the metalloids, and compounds."*

In a subsequent communication to the Paris Academy I wrote,† "Il semble que plus une étoile est chaude plus son spectre est simple, et que les éléments métalliques se font voir dans l'ordre de leurs poids atomiques."

This last generalisation rested upon the great preponderance of hydrogen in certain stars, which I classed as hottest on the ground that the blue end of the spectrum was open. Of the spectrum of helium,

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* 'Phil. Trans.,' vol. 164, p. 479.

† 'Comptes Rendus,' vol. 77, 1873, p. 1357.

which I had first observed and named in 1869, we then knew nothing either on the earth or in the stars; the solar line D_3 being its only representative. The question of the relative temperatures of stars became of great importance in relation to the questions thus raised, but it was not till 1892 that I was able to approach it by means of photography. The interval was spent chiefly in researches bearing upon solar and terrestrial changes in spectra when differences of thermal and electric energies were obvious.

In a paper on stellar spectra in relation to temperature, communicated to the Royal Society in 1902,* I gave an account of an attempt at a temperature classification of stars, utilising the fact that an extension of spectra into the ultra-violet is produced by increased temperature, and further that a lower temperature in an atmosphere above a photosphere would increase the absorption in the blue end. The classification arrived at was based on photographs obtained with instruments having prisms and lenses made of glass which has a strong absorbing effect on the ultra-violet rays.

The general results of the discussion was the conclusion that the stars so far considered might be divided into two series, one of ascending, the other of descending temperature. Further, that the classification proposed was justified both by the relative extensions of the spectra into the ultra-violet and by the temperature sequence of the few typical lines then available for study.

By 1899, laboratory work on the spectra of different substances under different conditions, and the discovery of a terrestrial source of helium by Ramsay, which enabled me to investigate the complete spectrum, had so far facilitated the study of the typical lines in the various stellar spectra, that I felt myself justified in attempting to classify the stars in relation to the chemical sequence revealed by the presence of gaseous and metallic lines, using especially the lines of helium and hydrogen and the "enhanced" and arc lines of the metals. In this way I hoped to be able to test the classification of 1892 based on the relative lengths of spectra.

An account of this research was published in the Proceedings of the Royal Society (vol. 65, pp. 186—191, 1899), and ultimately the complete results obtained were included in a "Catalogue of 470 Brighter Stars Classified According to their Chemistry."†

In this catalogue the stars were arranged in sixteen groups along a temperature curve with its apex in the central portion. On the assumption that the chemical changes were due to temperature, including in that term the possible results of electrical energy, the general arrangement of the stellar groups in the order both of ascending and descending temperatures was indicated, the group

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* 'Phil. Trans.,' A, vol. 184, p. 688.

† 'Publications of the Committee on Solar Physics,' London, 1902.

containing the hottest stars, on the dissociation hypothesis, being placed at the top of the curve. It will be convenient to reproduce this table here.

10		Argonian	
9		Alnitamian	
		Protohydrogen Stars	
8	Crucian	Cleveite-gas Stars	Achernian
7	Taurian		Algolian
6	Rigelian	Proto-metallic Stars	Markabian
5	Cygnian		—
4	—		Sirian
3	Polarian		Procyonian
2	Aldebarian	Metallic Stars	Arcturian
1	Antarian	Stars with fluted spectra	Piscian

There was abundant chemical evidence to show that the mean temperature of the stars occupying the same height on both branches of the curve was not very different, so that we have ten horizons, or stages, of mean temperature indicated in passing from the complete fluted spectra of Antarian-Piscian stars to the simplified line spectra of the γ Argus type.

So far as we could judge from the photographs then available, the chemical changes gave a sequence identical with that desired for the length of ultra-violet spectra in 1892, so that the latter classification was fully justified by the test which had been applied to it.

2. *Aims and Conditions of the Present Research.*

As before mentioned in the work of 1892, the relative lengths of the ultra-violet spectra were determined from photographs secured with instruments having glass optical parts. It was possible therefore to secure still another test by obtaining a new set of photographs using calcite and quartz in place of glass, to enable the far ultra-violet to be obtained and studied. Nor was this all, it seemed of the first importance to utilise not only the length of spectrum in the blue, but the relative brightness of the different parts. What happens regarding the relative brilliancy of the different parts of the spectrum, as well as the extension into the ultra-violet by increased temperature, was very clearly stated by Sir George Stokes[*] in 1876, in the following words, "When a solid body such as a platinum wire, traversed by a voltaic current, is heated to incandescence, we know that as the temperature increases, not only does the radiation of each particular refrangibility absolutely increase, but the proportion of the radiations of the different refrangibilities is changed, the proportion of the higher to the lower increasing with the temperature."

This question was also investigated by Melloni and Crova; and in recent years exact determinations of the law of increase have been made by Lummer, Paschen, and others. Melloni showed[†] experimentally that the maximum radiation moved towards the more refrangible end of the spectrum as the temperature increased. Crova made use of this fact in determining the temperatures of various incandescent light sources, and was one of the first to suggest[‡] that the method was applicable to the determination of the temperatures of the sun and stars.

3. *The Observational Conditions.*

The kind of spectroscope to be used and method of observation to be followed were indicated by the following considerations.

In order to utilise the effect of temperature changes to the full it was necessary to record the red end of the spectrum as well as the ultra-violet, as only in this way could the *relative* changes in intensity be recorded, *hence it was desirable to employ only a small dispersion.*

In addition to the natural differences photographed in the ultra-violet, artificial differences due to the absorbing effect of our atmosphere—which, even when clearest, is more or less opaque to the ultra-violet radiations—might be introduced, therefore it was considered advisable, in order to eliminate the effects of atmospheric absorption, to obtain the spectra of any two stars to be compared *whilst they were at approximately the same altitude.* Further, to avoid the many pit-falls to which those who compare photographs taken on plates of unequal sensitiveness, and differently exposed and developed, are liable, it was obviously important that any spectra to be compared should be obtained on the same plate in order to secure identical plate sensitiveness and development.

Again, in order to secure similar optical treatment it was arranged to photograph both spectra near to the optical axis of the camera; thus they are near together in the centre of the plate.

I am sorry to say this work has been considerably delayed by the long time taken in preparing a new camera and optical parts suitable for the research, as above defined, and latterly by a long spell of bad observing weather.

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[*] 'Roy. Soc. Proc.,' vol. 24, p. 353, 1876.

[†] Taylor's 'Scientific Memoirs,' vol. 1, p. 56.

[‡] 'Comptes Rendus,' vol. 87, p. 981.

4. *Description of the Instrument used.*

With regard to the new apparatus, I may state that an objective prism camera (fig. 1), was devised having a 2-inch 30° calcite prism mounted in front of a $2\frac{1}{2}$-inch quartz lens of 18 inches focal length.

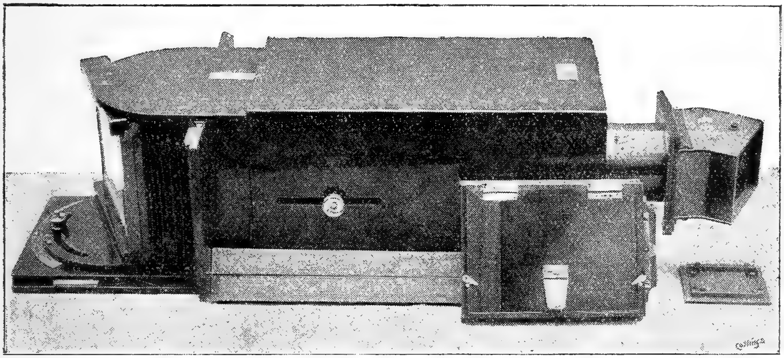

FIG. 1.—Quartz-Calcite Prismatic Camera.

The prism is so cut that its first face is perpendicular to the optic axis of the crystal, and it is arranged that the incident rays are normal to this face. All the rays, therefore, pass through the prism parallel to the optic axis and in this way there is no double refraction.

The whole length of the spectrum is brought into focus in a plane inclined at 42° to the optical axis. The apparatus is attached to the side of an equatorially mounted 6-inch Dallmeyer doublet camera with an angle in declination between the two optical axes equal to the angle of deviation of the calcite prism, so that the Dallmeyer is used as a finder.

Edward's snap-shot isochromatic plates have alone been used, and as they are not sufficiently sensitive for wave-lengths between λ 486 and λ 550 (approx.), there is a break in each spectrum about this region followed by a further portion of the spectrum having its centre about "D." The length of spectrum obtained is such that H_β to H_ϵ is 0·355 inch (9·0 mm.) and from H_ϵ to H_γ 0·165 inch (4·2 mm.).

5. *Method of Work.*

As shown in Table I, there are ten stages or steps of temperature to be investigated, and pairs of stars were selected in order to show the differences between adjacent stages or else the difference between stages widely apart.

Stages 1 and 10 are not available in consequence of the photographic faintness of the Antarian and Piscian stars, and the fact that the only star as yet known representing the Argonian group is situated in the Southern Hemisphere. The stages already investigated are shown by brackets in the following table.

Table I.

10.		Argonian.								
9.		Alnitamian.				┐		┐		┐
8.	Crucian.		Achernian.			│		│	┐	│
7.	Taurian.		Algolian.			│ 3.		│	│	│
6.	Rigelian.		Markabian.		┐	┘	┐	│ 5.	│	│ 7.
5.	Cygnian.		———		│ 2.		│	│	│ 6.	│
4.	———		Sirian.	┐	┘		│ 4.	│	│	│
3.	Polarian.		Procyonian.	│ 1.			│	┘	│	│
2.	Aldebarian.		Arcturian.	┘			┘		┘	┘
1.	Antarian.		Piscian.							

Thus an Arcturian star (second stage), was compared with a Sirian (fourth stage), and so on. In the case of some stars, *e.g.*, Capella, this was repeated several times, each photograph showing a different comparison.

In securing the photographs an attempt was always made to obtain the pair of spectra with the region between H_β and H_γ of the same intensity in each. This condition is very difficult to fulfil in actual practice owing to the difference of magnitude of the two stars, their difference in declination, and hence in clock rate, and lastly the very important actual differences between the actinism of the two bodies in this region of the spectrum due to their different temperature conditions.

It was also arranged to obtain, whenever possible, the spectra of two stars near together, so that the chances of introducing atmospheric interference due to the different conditions possibly present in various parts of the sky might be reduced to a minimum. In every case where the observer had reason to suspect any change in the atmospheric conditions obtaining during the two exposures, which sometimes extended over a period of 1½ hours, the result has not been included in the discussion.

Thus we have a series of comparison photographs from which all variable conditions except the natural variations in radiation have, as far as possible, been eliminated.

6. *Description of the Photographs.*

Many of the photographs taken are too faint to reproduce well enough to exhibit completely the striking differences shown between the pairs of spectra. I shall therefore content myself by giving a detailed description of each negative (Table II), only reproducing those photographs which are sufficiently dense to plainly show the phenomena under discussion.

Table II.

No. of negative.	Date.	Star.	Stage of temperature.	Type.	Altitude.	Description.
From Stage 2 to Stage 4.						
19	7.5.03	α Lyræ	4	Sir.	56°	Faintly red, fairly bright to H_γ.
		α Boötis	2	Arct.	57	Intense red, scarcely extends beyond K.
4	29.12.02	β Ursæ Maj.	4	Sir.	59	Red scarcely visible, maximum about λ 400.
		α Ursæ Maj.	2	Arct.	58	Fairly bright red, maximum about λ 460.
14	17.2.03	α Geminorum	4	Sir.	42	Red scarcely visible, maximum about λ 418.
		α Aurigæ	2	Arct.	39	Fairly bright red, maximum about λ 455.
37	21.1.04	α Geminorum	4	Sir.	68	Weak red, maximum about λ 420.
		α Aurigæ	2	Arct.	68	Very dense red; blue maximum commences at F, centre at λ 460, weak beyond λ 388.
From Stage 4 to Stage 6.						
11	28.1.03	β Orionis	6	Rig.	21	Red not so intense, and ultra-violet much more extended than in Sirius.
		α Canis Maj.	4	Sir.	22	Dense red, centre of maximum about λ 422.

Table II—*continued.*

No. of negative.	Date.	Star.	Stage of temperature.	Type.	Altitude.	Description.
From Stage 6 to Stage 9.						
35		κ Orionis	9	Alnit.	20°	Although spectrum of Rigel is generally much stronger, that of Alnitam extends as far into the ultra-violet.
		β Orionis	6	Rig.	24	
Various Intervals.						
34	10.12.03	γ Ursæ Maj.	6	Mark.	78	Faint red, maximum about λ 416, well sustained up to H_θ.
		α Ursæ Maj.	2	Arct.	76	Fairly bright red, maximum at λ 460, rapidly falling off beyond λ 430.
36	14.1.04	η Ursæ Maj.	8	Cruc.	51	Faint red, maximum at about λ 418, bright extension well beyond the end of the Capella spectrum.
		α Aurigæ	2	Arct.	52	Very bright red, maximum at about λ 455, faint beyond K.
23	14.11.03	ε Orionis	9	Alnit.	38	Faint red, centre of maximum at about H_δ. Bright extension far beyond the hydrogen series.
		α Canis Min.	3	Proc.	41	Very bright red, maximum about H_γ, and falls quickly beyond H_κ.
Extreme Interval.						
33	10.12.03	ε Orionis	9	Alnit.	36	Very faint red, centre of maximum about H_δ, *i.e.*, near the more refrangible end of the Aldebarian spectrum.
		α Tauri	2	Aldeb.	36	Very strong red, centre of maximum about λ 465, end of spectrum about H_δ.

7. *Discussion of Photographs.*

The following is a detailed discussion of the photographs described in Table II.

Stage 2 *to Stage* 4.

No 19. This photograph of Vega (fourth stage), and Arcturus (second stage), is very striking in the relative intensities of the two spectra. The red portion of the Arcturian spectrum is considerably more intense and forms the one end of a maximum which—except in the green region where the plate is not very sensitive—extends from D to about λ 454. The part of the spectrum more refrangible than that rapidly becomes less intense, until at and beyond K it is very faint. Matters are very different in the spectrum of Vega. Here the maximum radiation occurs about a third of the distance from H_δ to H_γ (λ 422) and the spectrum extends without any great falling off in intensity to H_ν, beyond that it is weaker but continues without any great decrease to twice the distance on the more refrangible side of H_ν than the latter is from K. From that point to the end the spectrum gradually declines. Whilst the maximum radiation in this spectrum has moved wholesale towards the ultra-violet, the red is relatively only about half the density of the red in the Arcturian spectrum.

No. 4. The general appearance of these two spectra leads to the conclusion that that of β Ursæ Majoris (fourth stage) is much stronger than that of α Ursæ Majoris (second stage).

A more careful examination, however, shows that whilst the detached red part of the former is only seen with difficulty, the same portion of the latter is comparatively prominent.

The maximum intensity in α is situated at about λ 450 and does not vary a great deal between there and K. After the latter point is reached, however, the fall is rather sudden, and the spectrum soon dies out. In β the maximum intensity is between H and H_ϵ, and the ultra-violet up to H_κ is fairly strong. Beyond H_κ the intensity of the spectrum drops rather suddenly, and then continues with a gradual decrease for some distance.

No. 14. There is no great difference in the lengths of these two spectra, the one of α Geminorum (fourth stage), the other α Aurigæ (second stage), but the red portion of the second stage spectrum is most decidedly more intense than that of the fourth stage, the latter, in fact, being scarcely visible at all. In α Aurigæ the maximum intensity is at about λ 454, but in α Geminorum it must be placed at or near to H_δ (λ 410).

No. 37. In the taking of this negative the spectrum of Capella (second stage) received the advantage of exposure and is consequently much stronger in the H_γ—H_β region than that of Castor (fourth stage), and yet the latter extends as far into the ultra-violet as the former.

Furthermore, the intensity of the blue part of the spectrum of Capella rises to its maximum immediately at H_β and commences to decline towards the violet at H_γ, whereas the maximum region of Castor does not commence at once after leaving the green gap, and attains its centre at about λ 422.

It will be observed that whether we take Arcturus or Capella to represent Stage 2, the spectra of stars of higher stages have relatively longer ultra-violet and reduced red radiation. It has to be noted, however, that there are indications that Stage 2 will, as a result of further work, have to be divided, for Capella is certainly hotter than Arcturus as determined in the manner now under discussion.

Stage 4 *to Stage* 6.

No. 11. On examination of this negative it is seen that the detached red portion of the spectrum of Sirius (fourth stage) is decidedly more intense than the same portion of the Rigelian spectrum (sixth stage). In the ultra-violet, however, we find that although both stars are fairly high on the temperature curve, and, therefore, both spectra extend far into the ultra-violet, the extension of the spectrum of Rigel is more intense and greater than that of Sirius.

Stage 6 *to Stage* 9.

No. 35. By reason of its greater exposure the spectrum of Rigel (sixth stage) is generally much stronger than that of κ Orionis (ninth stage), and especially is this so in the red portions of the two spectra. This inequality notwithstanding, the spectrum of κ extends practically as far into the ultra-violet as that of Rigel.

Various Intervals.

No. 34. In α Ursæ (second stage) the red part of the spectrum is comparatively very bright, nearly as bright as the region between G and F. In γ Ursæ (sixth stage) this red portion is barely visible. Again, in the α Ursæ spectrum the maximum occurs at λ 460, and then the intensity gradually declines to K, beyond which it is very faint. The maximum intensity of the γ Ursæ spectrum is situated at about λ 422, and it extends without becoming greatly impaired to H_θ.

No. 36. In this comparison we have a very striking case. The spectrum of Capella (second stage) is compared with that of η Ursæ Majoris (eighth stage); Capella has been over-exposed, so that the red portion is abnormally intense, η Ursæ Majoris received the correct exposure, and the red part of the spectrum is rather faint.

Notwithstanding this difference the spectrum of η Ursæ extends further into the ultra-violet than does that of Capella, and not only does it extend further, but the maximum intensity is extended much further into the ultra-violet than is that of Capella, which drops off rapidly beyond K.

No. 23. In this comparison the red part of the Procyon (third stage) spectrum is much brighter than that of Alnitam (ninth stage). The intensity in the longer portion of the spectrum of Procyon rises at once to its maximum at H_β, has its centre of maximum at about λ 460, and at H_γ commences to diminish towards the violet. In the spectrum of Alnitam, however, the maximum is delayed until the region about λ 426 is reached, and is then sustained up to H_ζ, finally extending to the ultra-violet with a marked superiority, comparatively, over that of the Procyon spectrum.

Extreme Interval.

No. 33. This comparison of two type stars respectively situated near the extremities of the temperature curve is naturally one of the most striking pieces of evidence in support of this method of temperature classification. The spectrum of Alnitam (ninth stage) is nowhere so intense as the red and blue parts of the Aldebaran (second stage) spectrum, and yet it extends more than twice as far towards the ultra-violet, from H_β, as the hydrogen series, whilst the more refrangible limit of the Aldebaran spectrum only extends to K. The maximum intensity of the blue portion of the Alnitam spectrum occurs much nearer the ultra-violet than that of the Aldebaran spectrum, the latter attaining its maximum at about λ 465.

8. *Conclusions.*

It may be pointed out that the temperature classification, confirmed by this research, does not agree with that published by Sir William and Lady Huggins who, in their "Atlas of Representative Stellar Spectra" containing "a Discussion of the Evolutional Order of the Stars," place the solar stars on a higher temperature level than the white stars.

A reduction of intensity in the continuous spectrum beyond the hydrogen series to which attention has been called by more than one observer, Schumann* among others, does not affect the results which I have stated. Another paper dealing with this and similar points is in course of preparation.

The result of the research may be stated as follows :—

Taking the stars assumed to be hottest in the chemical classification, we find that in all cases the relative length of the spectrum is reduced,

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* 'Smithsonian Contributions to Knowledge,' No. 1413, 1903, p. 23.

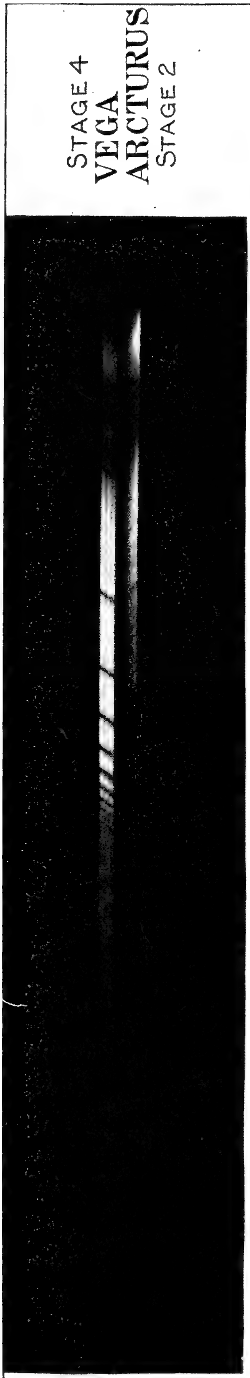
and the relative intensity of the red is increased, as a lower temperature is reached. That is to say that where two spectra having their intensities about the region H_β — H_γ equal are compared, we find that in the cooler stars, according to the chemical classification, the emissions in the red preponderate, whilst in the hotter star the ultra-violet is more extended and intense.

My best thanks are due to Messrs. Rolston and Goodson, who took the various photographs to which I have referred, the former also helping me in the preparation of this paper, and to Mr. Wilkie for preparing the enlargements of the negatives.

9. *Description of Plates.*

No.	No. of negative.	Stars.	Stage.	Type.
		Plate 7.		
1	19	Vega	4	Sir.
		Arcturus	2	Arct.
2	14	Castor	4	Sir.
		Capella	2	Arct.
3	37	Castor	4	Sir.
		Capella	2	Arct.
		Plate 8.		
4	11	Rigel	6	Rig.
		Sirius	4	Sir.
5	35	κ Orionis......	9	Alnit.
		Rigel	6	Rig.
6	34	γ Ursæ Maj. ..	6	Mark.
		a Ursæ Maj. ..	2	Arct.
		Plate 9.		
7	36	η Ursæ Maj. ..	8	Cruc.
		Capella.	2	Arct.
8	23	Alnitam	9	Alnit.
		Procyon	3	Proc.
9	33	Alnitam	9	Alnit.
		Aldebaran	2	Aldeb.

In producing these plates the original negatives have been enlarged about $3\frac{1}{2}$ times.


STAGE 4
VEGA
ARCTURUS
STAGE 2

1


STAGE 4
CASTOR
CAPELLA
STAGE 2

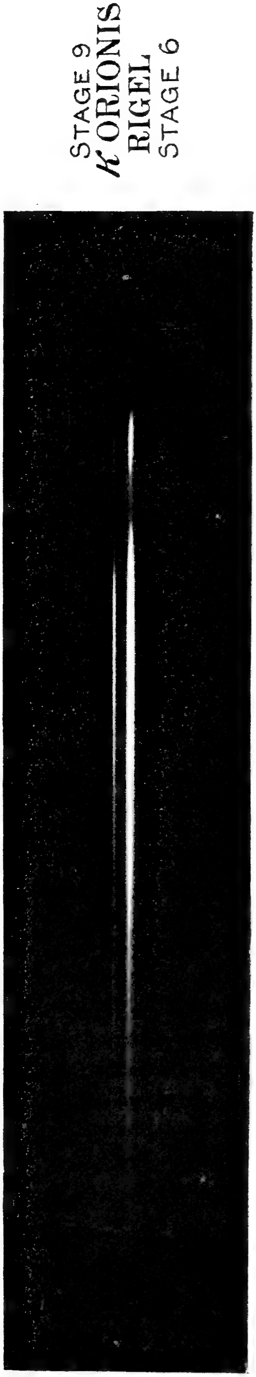
2


STAGE 4
CASTOR
CAPELLA
STAGE 2

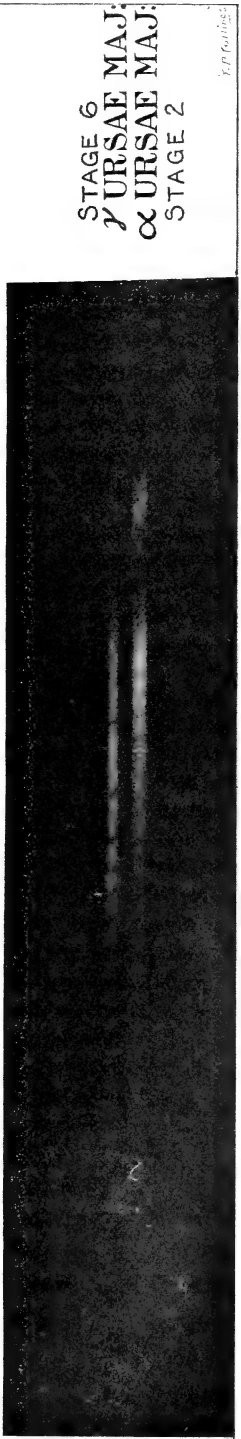
3


STAGE 6
RIGEL
SIRIUS
STAGE 4

4


STAGE 9
κ ORIONIS
RIGEL
STAGE 6

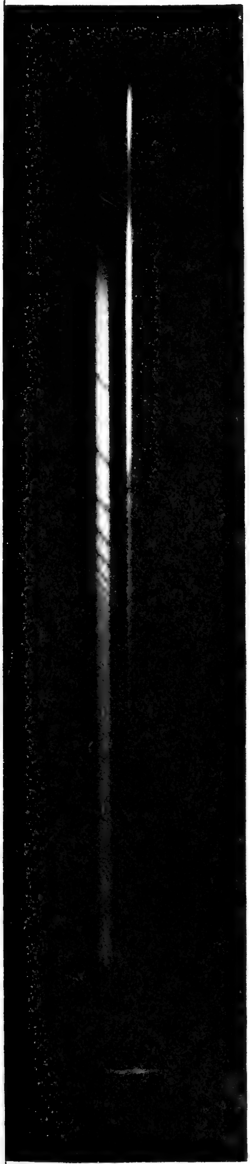
5


STAGE 6
γ URSAE MAJ:
α URSAE MAJ:
STAGE 2

6

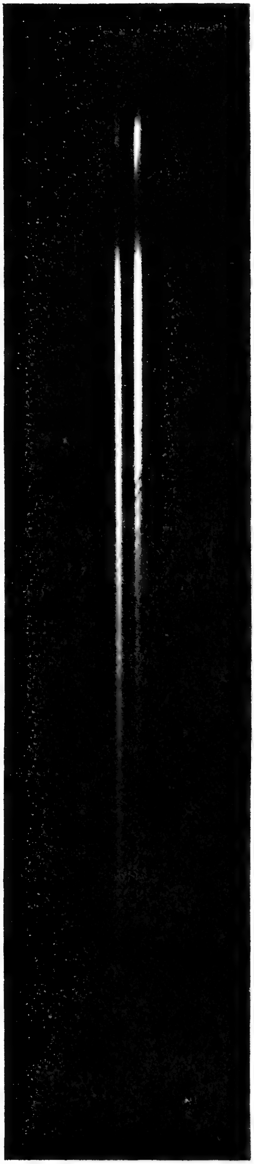
T. P. Collings

STAGE 8
η URSAE MAJ:
CAPELLA
STAGE 2



7

STAGE 9
ALNITAM
PROCYON
STAGE 3



8

STAGE 9
ALNITAM
ALDEBARAN
STAGE 2

9

"On the Construction of some Mercury Standards of Resistance, with a Determination of the Temperature Coefficient of Resistance of Mercury." By F. E. SMITH, A.R.C.Sc., Assistant at the National Physical Laboratory. Communicated by R. T. GLAZEBROOK, F.R.S. Received March 10,—Read March 17, 1904.

(From the National Physical Laboratory.)

(Abstract.)

This paper contains an account of the construction and measurement of eleven mercury standards of resistance at the National Physical Laboratory, Teddington.

It is divided into three parts: (*a*) theory of construction; (*b*) determination of the mechanical constants: (*c*) measurement of resistance, and deductions.

(*a*) *Theory of Construction.*

The definition of the international ohm necessitates the determination of a length and a mass only, the resistance of a uniform column of mercury of length, L. cm., and of mass, W. gr., being

$$\frac{L^2}{W} \cdot \frac{14 \cdot 4521}{106 \cdot 3^2} \text{ international ohms.}$$

The difficulties of accurately evaluating L^2/W are discussed in the paper. The method finally adopted for the determination of this ratio differs from those which have been previously employed. A quantity proportional to L^2/W was first of all determined for different, but practically similar portions of a selected tube. The tube was then broken into three parts, the positions of fracture being carefully chosen, so that a mercury column occupying the central portion should have a nominal resistance of 1 ohm. The ratio L^2/W for this central portion was calculable from the previous observations. It is shown that this method admits of measurements of a high degree of accuracy, while all the operations involved are of a simple nature.

The effect of liquid and gaseous films in contact with the glass is briefly discussed. If the same cycle of operations is always adhered to, it is shown that such films produce no disturbing effects.

The fact that the axis of a tube may be undulatory in character, is proved to be a very serious obstacle to an accurate computation of the resistance of a mercury column. Thus, it is quite possible for the radius of curvature at any part of the axis to be as small as 40 cm., and for the maximum displacement of the curve from a straight axis to be 0·015 mm. only, and yet, under such circumstances, the computed resistance is 0·0036 per cent. greater than that calculated by the usual

formula. If such an axis were projected and examined, the curve could not be distinguished by the eye from a straight line.

The correction for conical form was obtained by a method very similar to those previously employed by other observers.

(*b*) *Determination of the Mechanical Constants.*

The lengths of the mercury columns, and of the tubes, were measured in a water bath comparator by Mr. B. F. E. Keeling, the temperature of the mercury and of the glass being accurately known to 0°·02 C. The mass of the mercury was determined with a probable error of about 0·0005 per cent. These determinations enabled the mean cross-sections of different parts of the tube to be evaluated. From the results, the mean cross-section of the standard portion was deduced, and, finally, that length of a uniform mercury column of similar cross-section that would have a resistance of 1 international ohm. The consistency of the results obtained from different fillings of the tubes, each thread of mercury occupying a slightly different position, may best be judged from the results of actual observations. The following values are for the first three standards constructed. The horizontal lines in the table represent separate fillings of the three tubes:—

Standard.	M.	P.	T.
Length of the mercury column possessing a uniform cross-section equal to the mean of that of the standard, and having a resistance of one international ohm.	59·0336 cm.	63·5493 cm.	57·7801 cm.
	59·0326 ,,	63·5485 ,,	57·7808 ,,
	59·0328 ,,	63·5494 ,,	57·7805 ,,
	59·0337 ,,	63·5484 ,,	

The results with the remaining eight tubes are equally good.

(*c*) *Measurement of Resistance, and Deductions.*

The standards were erected in three fashions. In the first of these, two other glass tubes were connected to the ends of each standard by means of a special connector. The ends of the tubes thus brought into contact were of the same cross-section, and the shapes of the sections were also identical. An adjustment was provided for, which insured the absence of internal irregularities at the junctions. Thin pieces of platinum foil, interposed at the points of union, acted as potential leads, and enabled the resistance to be measured. The value thus obtained was for a column of mercury completely filling the tube, and terminated by planes at the ends of the tube. This mode of erection answers capitally with care, but many precautions have to be taken.

The second and third modes of fitting up the standards introduced "end corrections" to the resistance. The ends of the tubes passed into comparatively large vessels containing mercury, into which two leads were introduced.

For the measurement of resistance, three methods were employed; the Kelvin double bridge, the potentiometer, and the Carey Foster bridge. The last of these methods is shown to be subject to a considerable error when mercury standards are measured.

The following table contains the results of the resistance measure-

Standard.	Res. in ohms (Kelvin double bridge).	Res. in ohms (potentiometer).	Mean value of res.	Difference from mean parts in 100,000.	
M	1·001162	1·001167	1·001164	-0_2	$+0_3$
	1·001164	1·001162		$\pm 0_0$	-0_2
	1·001166	1·001165		$+0_2$	$+0_1$
P	1·000467	1·000468	1·000470	-0_3	-0_2
	1·000468	1·000472		-0_2	$+0_2$
	1·000472	1·000471		$+0_2$	$+0_1$
T	1·000277	1·000273	1·000278	-0_1	-0_5
	1·000281	1·000280		$+0_3$	$+0_2$
	1·000276	1·000281		-0_2	$+0_3$
U	1·000215	1·000213	1·000217	-0_2	-0_4
	1·000216	1·000216		-0_1	-0_1
	1·000222	1·000218		$+0_5$	$+0_1$
V	1·001462	1·001455	1·001462	$\pm 0_0$	-0_7
	1·001468	1·001462		$+0_6$	$\pm 0_0$
	1·001463	1·001465		$+0_1$	$+0_3$
W	1·000156	1·000161	1·000153	$+0_3$	$+0_8$
	1·000152	1·000151		-0_1	-0_2
	1·000146	1·000151		-0_7	-0_2
X	1·001147	1·001147	1·001151	-0_4	-0_4
	1·001154	1·001151		$+0_3$	$\pm 0_0$
	1·001155	1·001152		$+0_4$	$+0_1$
Y	1·000356	1·000354	1·000350	$+0_6$	$+0_4$
	1·000347	1·000350		-0_3	$\pm 0_0$
	1·000348	1·000346		-0_2	-0_4
Z	1·001387	1·001391	1·001389	-0_2	$+0_2$
	1·001389	1·001384		$\pm 0_0$	-0_5
	1·001392	1·001393		$+0_3$	$+0_4$
G	1·001130	1·001135	1·001135	-0_5	$\pm 0_0$
	1·001138	1·001135		$+0_3$	$\pm 0_0$
	1·001131	1·001132		-0_4	-0_3
S	1·001053	1·001054	1·001057	-0_4	-0_3
	1·001055	1·001061		-0_2	$+0_4$
	1·001061	1·001057		$+0_4$	$\pm 0_0$

ments when the mode of erection introduced the "end correction," but the fittings were not adapted for the measurement of resistance on the Carey Foster bridge.

Each horizontal line indicates a separate filling.

The unit of resistance employed in the evaluation of the standards is that derived from the coils belonging to the British Association, and assumed as equal to 10^9 C.G.S. units.

The difference between the observed resistance, employing this unit, and the calculated resistance in international ohms, is shown in the following table :—

Standard.	Mode of Erection I.			
	Observed resistance in ohms.	Theoretical resistance in international ohms.	Observed — theoretical value.	Difference from mean.
M	$1 \cdot 00005_2$	$0 \cdot 99994_6$	$0 \cdot 00010_6$	$+0 \cdot 00002_1$
P	$0 \cdot 99938_4$	$0 \cdot 99930_4$	$0 \cdot 00008_0$	$-\quad 0_5$
T	$0 \cdot 99912_1$	$0 \cdot 99904_6$	$0 \cdot 00007_5$	$-\quad 1_0$
U	$0 \cdot 99912_9$	$0 \cdot 99904_6$	$0 \cdot 00008_3$	$-\quad 0_2$
V	$1 \cdot 00043_3$	$1 \cdot 00036_1$	$0 \cdot 00007_2$	$-\quad 1_3$
W	$0 \cdot 99915_2$	$0 \cdot 99905_5$	$0 \cdot 00009_7$	$+\quad 1_2$
X	$1 \cdot 00006_3$	$0 \cdot 99998_0$	$0 \cdot 00008_3$	$-\quad 0_2$
Y	$0 \cdot 99926_3$	$0 \cdot 99915_5$	$0 \cdot 00010_8$	$+\quad 2_3$
Z	$1 \cdot 00032_5$	$1 \cdot 00024_8$	$0 \cdot 00007_7$	$-\quad 0_8$
G	$1 \cdot 00034_2$	$1 \cdot 00027$	$0 \cdot 00007$	$-\quad 1$
S	$1 \cdot 00025_4$	$1 \cdot 00016$	$0 \cdot 00009$	$+\quad 1$
		Mean	$0 \cdot 00008_5$	

Another series of observations shows that the resistance of the unit employed as 1 international ohm at the Reichsanstalt is greater than the resistance of the international unit derived from the eleven mercury standards constructed at the National Physical Laboratory by

$$0 \cdot 00002_0 \text{ ohm.}$$

A comparison with the results obtained by Dr. Glazebrook in 1888 for the specific resistance of mercury indicates that the unit of resistance, as used at the Cavendish Laboratory, Cambridge, in 1888, may still be recovered.

From this it follows that—

$$\left.\begin{array}{l}\text{Resistance of 1 international}\\ \quad\text{ohm (as derived at the}\\ \quad\text{N.P.L.)}\end{array}\right\} - \left\{\begin{array}{l}\text{Res. of unit employed and}\\ \quad\text{assumed as equal to } 10^9\\ \quad\text{C.G.S. units}\end{array}\right\}$$

$$= 0 \cdot 00008_5 \text{ ohm.}$$

The temperature coefficients of resistance of—

(1) Mercury in Jena 16‴ glass,
(2) Mercury in verre dur glass,
(3) A constant volume of mercury,

have also been determined for a range of temperature 0° C. to 22° C. The results are as follows :—

(1) Mercury in 16‴ glass—

$$R_T = R_0 [1 + 0{\cdot}00088018T + 0{\cdot}00000105793T^2].$$

(2) Mercury in verre dur glass—

$$R_T = R_0 [1 + 0{\cdot}00088036T + 0{\cdot}00000103094T^2].$$

(3) A constant volume of mercury—

Deduced from (1),

$$R_T = R_0 [1 + 0{\cdot}00088788T + 0{\cdot}0000010564T^2]$$

Deduced from (2),

$$R_T = R_0 [1 + 0{\cdot}00088776T + 0{\cdot}0000010376T^2],$$

T being the temperature on the hydrogen scale.

The whole of the observations have been carried out at the National Physical Laboratory. To the director, the author has to express his great indebtedness in all departments of the work. To his colleague, Mr. B. F. E. Keeling, the success of the linear measurements is entirely due.

"On Electric Resistance Thermometry at the Temperature of Boiling Hydrogen."* By Professor JAMES DEWAR, M.A., LL.D., F.R.S. Received February 25,—Read March 10, 1904.

[PLATE 10.]

In my paper entitled "The Boiling Point of Liquid Hydrogen determined by Hydrogen and Helium Gas Thermometers," communicated to the Society in 1901, I detailed means taken to determine the temperature in gas thermometer degrees at which liquid hydrogen boils. The experiments clearly proved that the constant-volume hydrogen gas thermometer is reliable under varying conditions, down to a temperature some degrees below the boiling point of hydrogen, 20°·5 absolute, on the Centigrade scale.

In the course of making low temperature determinations, a number of thermometers of different kinds were employed, some being constant-volume gas thermometers, others in the form of thermoelectric junctions, and a set depending on change of electric resistance with temperature. The group of thermometers depending upon the assumed relation between electromotive force and temperature broke down about the temperature of liquid oxygen, while those based upon the ordinary law correlating change of electric resistance with temperature failed for all the metals and alloys somewhere above the boiling point of liquid hydrogen.

In the first experiments made to ascertain the boiling point of liquid hydrogen under a pressure of 30 to 60 mm. a platinum resistance thermometer was used, with the result that only 1° of reduction of temperature in the boiling point was recorded instead of some 5° indicated by theory. This result suggested that in all probability the rate of change of resistance with the pressure became gradually smaller under very low temperatures.

In the Bakerian Lecture I adverted shortly to some results obtained in low temperature resistance thermometry, and gave a table† of numerical values deduced from experimental observations for some of the more prominent metal resistance thermometers. In the present communication the experimental records of eight additional electric resistance thermometers are given, and the results of the observations on all the resistance thermometers used during the investigations are collected and compared.

Two facts seem to result from this inquiry, viz.: (1) That the resistance of an unalloyed metal continually diminishes with temperature and in each case appears to approach to a definite asymptotic

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* In continuation of Art. 3 of the Bakerian Lecture, 'Roy. Soc. Proc.,' vol. 68, p. 360.

† 'Roy. Soc. Proc.,' vol. 68, p. 363.

value below which no further lowering of the temperature seems to reduce it; and (2) that the parabolic connection between temperature and resistance is no longer tenable at very low temperatures.

Of the different thermometers constructed on the electric resistance principle, fifteen were serviceable throughout the investigations; the others broke or failed from various causes. The metals employed were platinum, gold, silver, copper, palladium, iron, nickel, and two alloys, platinum-rhodium, and German silver. The metals were supplied by Messrs. Johnson and Matthey, and the late Sir W. C. Roberts-Austen of the Mint. Every endeavour was made to attain the highest purity in the samples. In the Bakerian Lecture a table* was given containing the constants of seven of these thermometers, and in the annexed Table I similar results are given for the remaining eight.

The observed resistances, after all corrections were made, were reduced both by Callendar's and by Dickson's methods, and the close accord of their results is apparent. When these temperatures are compared with the results given by the hydrogen thermometer, they confirm the general inferences above referred to. In the Callendar method the α was determined from the resistances at 100° C. and 0° C., and the δ was then obtained from the resistance and corresponding observed temperature of liquid oxygen boiling under atmospheric pressure. The same data were used in the Dickson formula, except in a few cases where it was thought worth while to employ the method of least squares, in which cases these data were included.

In Table I the suffix number attached to the name of each metal indicates a particular thermometer in my notes of the observations, and may be taken as a rough index of the chronological order in which the observations were made.

Platinum, gold, silver, and copper show a remarkable agreement between the two methods of reduction. The later specimens of the metals of each of these groups are purer than the earlier ones. In the platinum and gold groups we notice that the Centigrade temperature Ω, at which the resistance would vanish, rises with the purity. This is still seen in copper, but something of the reverse appears in the case of silver (perhaps this may be explained later). However, the general rule is again apparent in palladium. With regard to Callendar's coefficients, the temperature coefficient α increases with the purity of the metal, while the difference coefficient δ diminishes; nevertheless some uncertainty is apparent in the platinum thermometer. Alloys appear to have much smaller values of α and much larger values of δ. On the other hand the magnetic metals have greater α's and very much increased δ's.

A marked peculiarity in the magnetic metals is that the difference coefficient δ is negative, that is, the temperatures in metal-degrees

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* *Loc. cit.*

Table I.—Electric Resistance Thermometry at the Boiling Point of Liquid Hydrogen.

Metals.	Platinum (P_7).	Gold (Au_{33}).	Silver (Ag_{34}).	Copper (Cu_{37}).	Palladium (Pd_{35}).	Palladium (Pd_{46}).	Nickel (Ni_{41}).	German silver (Arg_{32}).
R_1	7·3519	2·856	4·447	1·623	4·108	9·801	5·567	7·049
R_0	5·3280	2·099	3·209	1·144	3·255	7·261	3·449	6·859
Resistance at CO_2	—	—	—	—	—	—	—	6·702
,, ,, liq. O	1·3984	0·729	0·895	0·231	1·485	2·022	0·718	6·466
,, ,, liq. N	—	—	—	—	—	1·642	—	6·444
,, ,, liq. O, *x*	—	—	—	—	—	—	—	6·407
,, ,, liq. H	0·129	0·234	0·122	0·020	0·893	0·439	0·294	6·319
,, ,, liq. H, *x*	0·114	0·219	—	—	—	0·382	—	—
α	0·003799	0·003607	0·003858	0·004187	0·002621	0·003498	0·006141	0·000277
$\Omega' = -\frac{1}{\alpha}$	−263·25	−277·28	−259·21	−238·83	−381·77	−285·87	−162·83	−361·00
T. observed of liq. O	−182°·6 C.	−182°·4 C.	−182°·34 C.	−182°·85 C.	−182°·34 C.	−182°·41 C.	−182°·45 C.	−182°·4 C.
δ	2·2402	−0·27568	0·88769	1·5004	4·8872	4·6298	−10·3836	4·7447
Calculated temp. C.—								
At CO_2	—	—	—	—	—	—	—	−76°·25 −78°·94
,, liq. O	−182°·600 −182°·67	−182°·40 −182°·5	−182°·345 −182°·34	−182°·846 −182°·850	−182°·343 −182°·340	−182°·410 −182°·410	−182°·455 −182°·60	−182°·400 −184°·35
,, liq. N	—	—	—	—	—	−194°·66 −194°·39	—	−191°·85 −193°·47
,, liq. O, *x*	—	—	—	—	—	—	—	−207°·60 −208°·54
,, liq. H	−238°·76 −238°·44	−248°·76 −248°·87	−242°·01 −241°·92	−223°·78 −223°·66	−237°·68 −235°·75	−232°·73 −231°·21	−224°·75 −216°·37	−244°·30 −243°·00
,, liq. H, *x*	−239°·42 −239°·09	−250°·78 −250°·88	—	—	—	−234°·51 −232°·91	—	—
Ω	−244°·40 −244°·02	−280°·22 −280°·37	−251°·37 −251°·26	−227°·64 −227°·50	−316°·99 −309°·12	−246°·36 −244°·22	−260°·09 −240°·65	−1867°·20 −2325°·21
Ratio $\frac{R_0}{\text{Res. at B.P. of H}}$	41·30	8·97	26·30	57·20	3·645	16·54	11·73	1·085

R_1, R_0, are the resistances at 100° C. and 0° C. respectively.
x means that the liquid is boiling under exhaustion measured by about 30 mm. of mercury.
Ω' is the temperature, in degrees of the metal in question, at which the resistance would vanish.
Ω is the temperature Centigrade at which the resistance would vanish, either on the Callendar or on the Dickson method.
Calculated temperatures—the upper by Callendar's method; the lower by Dickson's.
Callendar's α and δ are calculated from the given values of R_1, R_0, the resistance of the metal in liquid O, and the temperature observed in liquid O.

The same data are used to calculate the Dickson formula, except in the cases of P_1, whose formula is given in 'Phil. Mag.,' June, 1898; P_7, whose formula is given in 'Roy. Soc. Proc.,' vol. 64, p. 228, and Pt-Rh_{20}, Arg_{32}, and Cu_{47}, which were calculated by least squares. [For other seven metals, see Table, 'Roy. Soc. Proc.,' vol. 68, p. 363.]

Note.—In the table (vol. 68, p. 363) given in the Bakerian Lecture, the heading of the third column should be "Platinum" P_{27}, not "Potassium" P_{27}.

are algebraically higher than in Centigrade degrees, a peculiarity which is shared with them by gold. It is also remarkable that in the cases of all the purest metals examined, their resistances calculated by either method of reduction vanish at temperatures above $-273°$ C. (Au_{33} was not so pure as Au_{40}, which was electrolytic gold.)

As measurers of temperature gold and silver seem to be the best. One prominent characteristic associated with them is, that their δ's are the smallest. Clearly those metals (if there are any) are accurate temperature measurers for which δ vanishes, so that we expect those to be best in which this constant is least. There is a further characteristic displayed by the best metals as shown in Table I, which may be explained thus: both methods of reduction rely on the parabola, and the farther away the representative arc of the parabola is from the vertex of the curve, the more nearly straight does this arc become and the smaller will δ be. Now in both metals referred to, especially in the purer specimens, this characteristic is most marked compared with the other metals employed.*

It is worthy of note that for these pure platinums the average value of δ is very nearly 2·5, while Callendar's platinums, also pure, gave 1·5—1·6. Is the parabola determined by the resistance at 444°·53 C., 100° C. and 0° C., different from that determined by the resistances at 100° C., 0° C., and $-182°{\cdot}5$ C.? In the sequel I shall show that these must be different, and, in fact, that we must look for an entirely different hypothesis to correlate resistance and temperature.

As a matter of interest, in the last line of Table I, I have noted the ratio in which the resistance of each metal at 0° C. is reduced on cooling it to the boiling point of hydrogen. This seems to be a quantity showing no connection with other properties of metals.

So far we have looked at the results rather from the point of view of metals as thermometers. But a much more important question is, What is the relation between resistance and temperature in metals?

We are entitled to consider the temperatures at which liquid oxygen and hydrogen boil under atmospheric pressure as being known to within one- or two-tenths of a degree, namely, $-182°{\cdot}5$ C. and $-252°{\cdot}5$ C. Further observations made with the constant-volume hydrogen gas thermometer lead to the conclusion that hydrogen

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* In the Callendar parabola $(A-T)^2 = P(B-R)$, the values of A for Au_{40} and Ag_{43} are $-27{,}053°$ and 14,520° (the only other one greater than 10,000° being $-18{,}087°$ for Au_{33}), and the corresponding values of B are $-598°$ and 177° (the only other one greater than 100 being 135° for P_{27}). Similarly in the Dickson parabola $(a+R)^2 = p(b+T)$ the values of a for Au_{40} and Ag_{43} are $-1080°$ and 339° (the only other two greater than 100 being 251° for P_{27} and $-129°$ for Au_{33}), and the corresponding values of b are $-11{,}841°$ and 7319° (the only others greater than 2000° being $-8400°$ for Au_{33} and 2881 for Ag_{34}). This characteristic comes out equally strongly when the curves are reduced to a common resistance of (say) 1000 ohms at 0° C.

freezes about 5° below its boiling point. In the present experiments I have been able to get eight observations in liquid hydrogen boiling under pressures varying from 5—50 mm., and it will not lead us appreciably astray to take the temperatures of these observations as (say) 4° below the boiling point. If the law connecting resistance with temperature be parabolic, the very gentle curvature at the boiling point of hydrogen will allow us to consider the rate of drop in resistance per degree of temperature for the 4° below the boiling point of hydrogen as roughly the same as that between the boiling points of oxygen and hydrogen (70°), so that the ratio of these two drops on this supposition should be about 4 : 70, or say one-eighteenth. These ratios are given in Table II.

Table II.

P_7.	P_{27}.	$Pt-Rh_{29}$.	Au_{33}.	Au_{40}.	Ag_{43}.	Pd_{46}.	Cu_{47}.
$\frac{0{\cdot}015}{1{\cdot}269}$	$\frac{0{\cdot}121}{6{\cdot}836}$	$\frac{0{\cdot}06}{3{\cdot}21}$	$\frac{0{\cdot}015}{0{\cdot}495}$	$\frac{0{\cdot}083}{2{\cdot}999}$	$\frac{0{\cdot}018}{1{\cdot}425}$	$\frac{0{\cdot}057}{1{\cdot}583}$	$\frac{0{\cdot}006}{1{\cdot}512}$
or $\frac{1}{84}$	$\frac{1}{57}$	$\frac{1}{53}$	$\frac{1}{33}$	$\frac{1}{36}$	$\frac{1}{79}$	$\frac{1}{28}$	$\frac{1}{252}$

Now these ratios are all much smaller than one-eighteenth, hence we infer that the curves have taken a more or less quick turn in the neighbourhood of the boiling point of hydrogen, or perhaps above it.

On Plate 10 the observed resistances are displayed graphically. For convenience I make seven groups, namely platinum, gold, silver, copper, palladium, magnetic metals, and alloys; and in order to bring the characteristics of these groups into comparison, each metal is supposed to have the resistance of 30 ohms at the freezing point. This number was chosen to suit the scale—the intention being that, roughly, the "plot" of resistance and temperature should be a line equally inclined to the two axes of resistance and temperature. This has been accomplished by taking a centimetre to represent 20° C. in temperature, and 2 ohms in resistance. The diagram for each group has the reading at 0° C. placed $2\frac{1}{2}$ cm. higher than that of the group below it, in order to obviate confusion among so many approximately coincident lines. Attention paid to this will enable each group of curves to be clearly seen and compared with the others.

The first noticeable peculiarity is the close coincidence of the two silver curves. For them, Callendar's α's, the Ω's, and the ratios of the resistances at 0° to that at the boiling point of hydrogen are almost the same, although the δ's differ much. In like manner the Dickson constants for Ag_{43} are all in the same ratio (about 5 : 2) with those for Ag_{34}.

Next, the curves for P_7, Au_{40}, the two silver, and Pd_{46}, are very approximately parallel. Of the two palladiums, Pd_{35} would appear to have contained so much impurity as to have behaved almost like an alloy. The two alloys take up quite independent positions compared either with the purer metals, or with themselves, tending more to parallelism with the axis of temperature. In this connection I may mention that I constructed and used once or twice a carbon thermometer; in its case the resistance diminished as the temperature rose (a result already known), and its "plot" departed still farther from the pure metals than the alloys do, its α being $-\cdot 08048$.

The magnetic metals present the most striking curves, being at first sight quite unlike any of the others. But on closer inspection we shall find that this is not so, and in fact they give the clue to the general connection between resistance and temperature in metals. The magnetic metals and gold were found to have *negative* values of δ. Now, if we examine the curves of the other metals, they will all be found concave towards the axis of temperature, for the arcs extending from the boiling point of water, through the freezing point, down to the boiling point of oxygen; while below the boiling point of oxygen these curves are convex to this axis. On the other hand, gold and the magnetic metals are already convex to this axis from the boiling point of water to the lowest temperature reached.

This leads me back to the research made by Professor Fleming and myself in 1896* on the electric resistance of mercury, in which we were able to observe the resistance of the metal from far below its melting point, and considerably above it when in the molten state. The curve connecting the resistance of mercury with temperature, throughout this range, including the change of state, was somewhat like the disused old English ʃ, the temperature being measured horizontally to the right, and the resistance vertically upwards. In the present instance, though in different circumstances, this same curve reappears.

For platinum, silver, copper, palladium, and the alloys these experiments include a part of the curve (fig. 1) starting from (say) P, passing through I, the point of inflexion, and through Q down towards the absolute zero; whereas, in the case of gold and the magnetic metals, the corresponding part of the curve begins below I, (say) at Q, and proceeds thence towards the absolute zero. Professor Callendar, from former experiments of Professor Fleming and myself, had noticed this behaviour in the case of platinum.†

The eight observations below the boiling point of hydrogen are shown in Plate 10.

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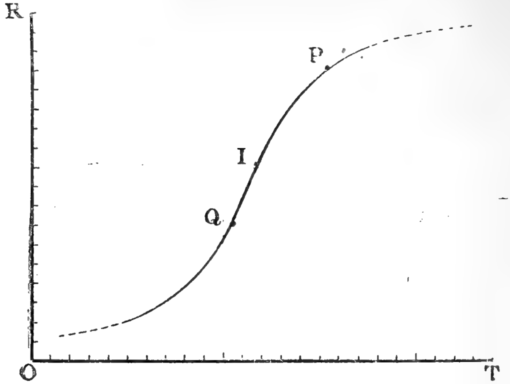
* "On the Electric Resistivity of Pure Mercury at the Temperature of Liquid Air," 'Roy. Soc. Proc.,' vol. 60, p. 76.

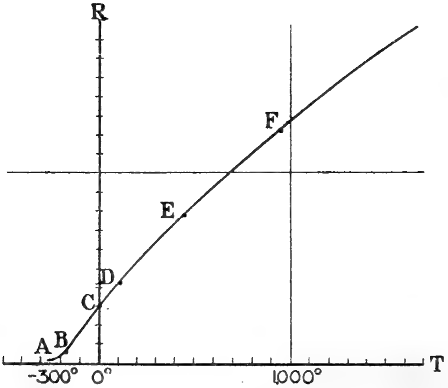
† 'Phil. Mag.,' vol. 47, pp. 218, 222.

Fig. 1.

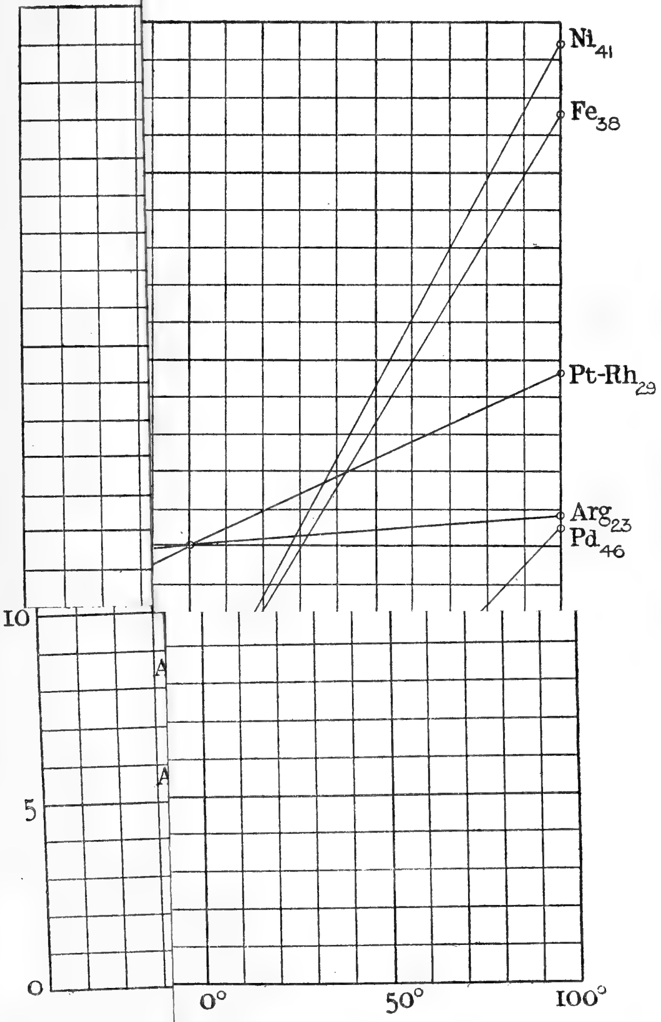


It is clear that in no case can anything parabolic connect resistances and temperatures ranging from the boiling point of water to that of hydrogen. Just as we seek for a circle of curvature at any point of a curve, so in the present case we may, at a point, or over a short range of the *f*, seek for an approximate parabola; but any such parabola will be of no, or little, use for extrapolation. I have mentioned that the value of δ for my platinum (Callendar) parabola is about 2·5, whereas observers at high temperatures finds its value about 1·5 for pure platinum. Such differences have been found by others also, but they do not seem to have attracted attention. On looking at fig. 2, we shall find the discrepancy easily accounted for. The portion ABCD of the curve represents roughly (on a much reduced scale, in order to show the curvature more clearly) the curve of P_{27}, given in Plate 1. The points B, D, E, represent the boiling points of oxygen, water, and sulphur; and C, F, represent the freezing points of water and silver.

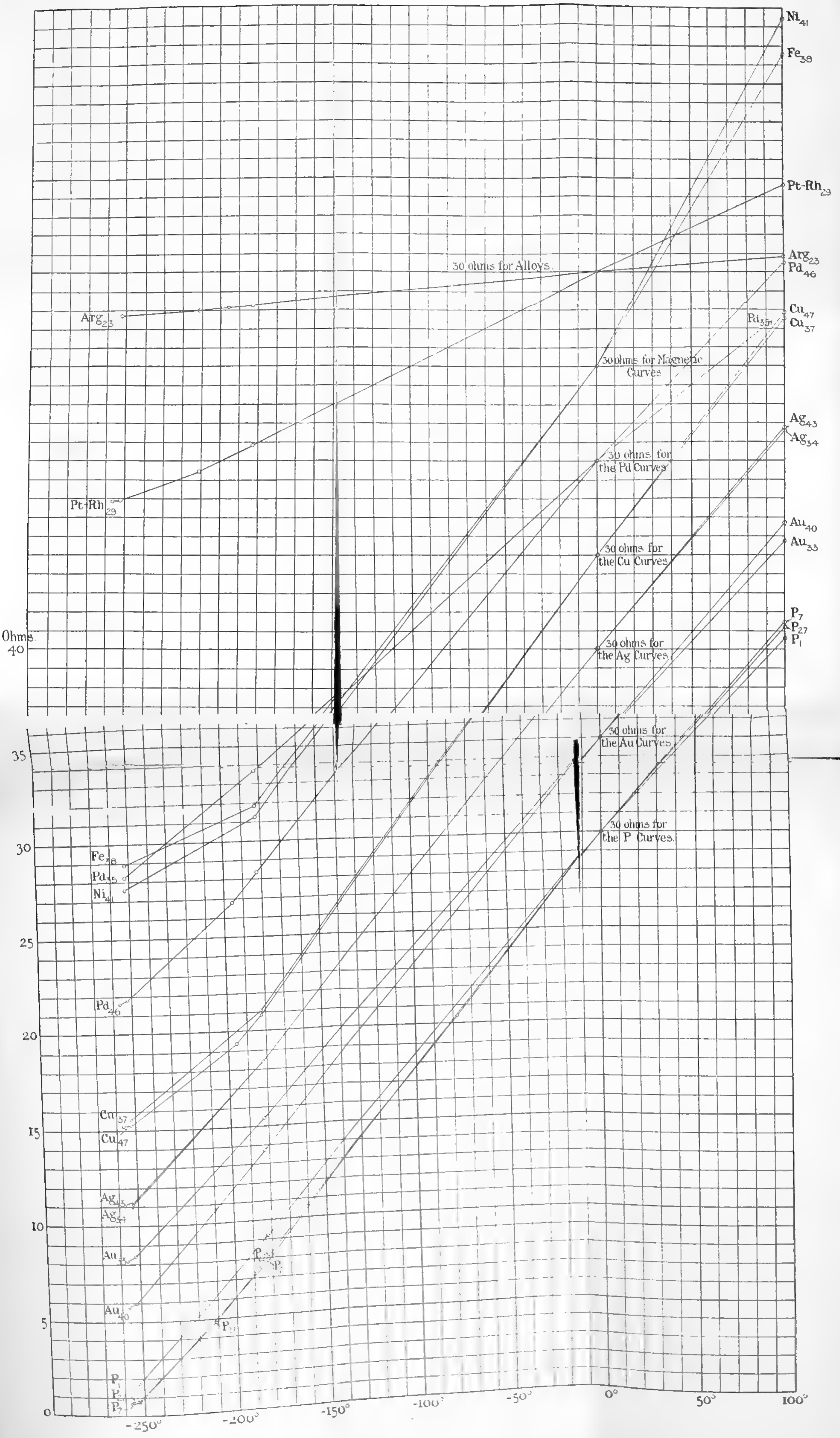
Fig. 2.



Dew *Soc. Proc., vol.* 73, *Plate* 10.



Dewar. *Roy. Soc. Proc., vol. 73, Plate 10.*



If parabolas with vertical axes (Callendar) be described through the points (1) B, C, D; (2) C, D, E; (3) C, D, F, they will be different, since the curve of observation is not itself parabolic. In the case of these parabolas, where the resistances at C and D are common to all, the product of δ and the parameter (P) of the corresponding parabola is constant; hence the smaller δ becomes, the more open—or less curved—will be the parabola along the arc we have to deal with. This result is in accordance with the curve in fig. 2, and explains why, as I have already pointed out, the two parabolas for high and low temperatures are not only different, but also may differ from each other by any amount, within certain limits depending on the nature of the unknown curve of temperature and resistance.

I am greatly indebted to Mr. J. D. H. Dickson, M.A., Fellow of Peterhouse, for help in the calculation and reduction of the observations.

"Physical Constants at Low Temperatures. (1)—The Densities of Solid Oxygen, Nitrogen, Hydrogen, etc." By Professor JAMES DEWAR, M.A., LL.D., D.Sc., F.R.S. Received March 9,—Read March 17, 1904.

1. The following experiments on the solid densities of oxygen, nitrogen and hydrogen were carried out as part of a former investigation dealing with gaseous densities at low temperatures.*

The method adopted was to measure the volumes of the gases sucked into a cooled space of known capacity, when the temperature was such as in the first place to induce liquefaction and finally solidification. For such experiments to be successful the rate of liquefaction and the cooling must be under thorough control, otherwise the cooled space may not get completely filled with solid. Further, the volume of gas condensed ought to be as large as possible, in any case about 20 litres, in order to diminish errors inseparable from the mode of manipulation. The inertia of the bell-jars of the large gas-holders causes some variation in the pressure; and errors of their calibration, and want of uniformity of temperature in the mass of gas, are all important factors. As my object was to ascertain experimentally the limiting density in the solid state, the elimination of these variations was not so important as it would have been for the study of fluid density.

The dry purified gas was contained in a gas-holder connected by a pipe with a glass bulb of 20 or 30 c.c. capacity, sealed to a narrow tube some 10 cm. long, with a glass stop-cock at the end. The

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* "The Specific Volumes of Oxygen and Nitrogen Vapour at the Boiling Point of Oxygen," 'Roy. Soc. Proc.,' vol. 69.

glass bulb was immersed in liquid oxygen, nitrogen, or hydrogen contained in a vacuum-jacketed vessel with arrangements for lowering the temperature of the liquids by exhaustion. The narrow tube just above the glass bulb had a mark engraved upon it, and the volume up to this point, which is to be filled with liquid or solid after cooling, was antecedently carefully calibrated. The glass stop-cock on the projecting part of the glass tube outside the vacuum vessel enabled the rate of the gas supply to be under complete control. The real difficulties were those of manipulation.

The temperatures employed were the boiling point of oxygen, 90°·5; the boiling point of nitrogen, 77°·5; the melting point of nitrogen, 62°·5; the boiling point of hydrogen, 20°·5; hydrogen boiling under 76 mm. of pressure taken as 14°·7; and hydrogen (solid) under 10 mm. of pressure taken as 13°·1.

Allowance for the contraction of the bulb was made by taking 0·0000250 as the coefficient of contraction (cubical) of the glass. It is possible that in going to very low temperatures (below − 200° C.), this coefficient ought to be less, such as 0·0000225. An estimate made of the difference between the two would come within the range of errors, consequently the former value which was adopted in the earlier investigations was retained.

The weight of 1 litre of oxygen at 0° C. under 760 mm. pressure was taken as 1·430 grammes, of nitrogen as 1·2564 grammes, and of hydrogen as 0·0899 gramme.

The observations and results are given in the following tables:—

Table I.—Oxygen.

No.	Description.	T′.	V.	T.	*p*.	*d*.
		°		°	mm.	
1	At B.P. of O ..	−182·5	19·631	14·4	758·5	1·1181
2	At B.P. of N ..	−195·5	20·536	14·4	758·5	1·1700
3	At M.P. of N ..	−210·5	21·743	14·55	758·5	1·2386
4	At B.P. of H ..	−252·5	25·003 (solid)	14·6	758·5	1·4256

Where T′ = the temperature Centigrade of the condensed gas in the flask at the time of observation.

V = volume of gas in litres at temperature T° C. and pressure p. mm.

d = density of the condensed gas at T′° C.

The volume of the flask at 15° C. was 23·9212 c.c.

Coefficient of expansion of glass, taken as 0·0000250.

Weight of 1 litre of oxygen at 0° C. and 760 mm., taken as 1·430 grammes.

Table II.—Nitrogen.

No.	Description.	T′.	V.	T.	*p*.	*d*.
					mm.	
1	At B.P. of N ..	−195°·5	15·192	16°·1	747	0·8042
2	At M.P. of N ..	−210·5	16·592	15·95	747	0·8792
3	At B.P. of N ..	−252·5	18·911 (solid)	14·65	761	1·0265

The same notation is used as in Table I.

The volume of the flask at 14°·2 C. was 22·1454 c.c.

Coefficient of expansion of glass, 0·0000250.

Weight of 1 litre of nitrogen at 0° C. and 760 mm., 1·2564.

Table III.—Hydrogen.

No.	Description.	T′.	V.	T.	*p*.	*d*.
					mm.	
1	At B.P. of H ..	−252°·5	18·051	14°·6	760	0·0700¼
2	H under 76 mm.	−258·29	19·447	14·6	760	0·0754½
3	H under 10 mm.	−259·9	19·597 (solid)	14·3	762	0·0763

The same notation is used as in Table I.

The volume of the flask at 14°·2 C. was 22·1454 c.c.

Coefficient of expansion of glass, 0·0000250.

Weight of 1 litre of hydrogen at 0° C. and 760 mm., taken as 0·0899.

2. It is of advantage to estimate the effects of an error, or of any reading that may have been taken roughly. Taking the notation already employed, and putting w for the weight of 1 litre of the gas at 0° C. and 760 mm., then the weight of gas used is

$$\mathrm{W} = \frac{\mathrm{V}p273}{760\,(273+\mathrm{T})}\,w;$$

and if V_0 be the volume of the flask up to the mark at some ordinary temperature T_0° C., and V′ its volume at T′ the temperature Centigrade of the observations, then

$$\mathrm{V}' = \mathrm{V}_0\,\frac{1+\beta\mathrm{T}'}{1+\beta\mathrm{T}_0},$$

where β is the cubical coefficient of expansion of the glass. Hence, the density of the substance at the time of observation is

$$d = \frac{W}{V} = \frac{Vp273w}{760\,(273+T)} \times \frac{1+\beta T_0}{V_0\,(1+\beta T')},$$

or, with sufficient accuracy for our investigation,

$$d = \frac{273w}{760V_0} \times \frac{Vp\,(1+\beta\overline{T_0-T'})}{273+T} \quad \text{.................} \quad (1).$$

An important error might be expected to arise from an inaccurate knowledge of T′ the temperature of the cooled gas. An alteration of a degree in the value of T′ would alter the value of d by

$$-\frac{273w}{760V_0} \times \frac{Vp}{273+T}\,\beta,$$

or very approximately, a rise of a degree in the estimate of T′ above its true value would diminish d by $\beta\, d$—in the present case by $\frac{1}{40000}$ part.

Similarly an error of a degree in the estimate of the temperature T_0, at which the volume of the flask was measured, would have the like small effect, but in the opposite direction.

On the other hand errors in the measurements of either V or V_0 or of p or T would give directly proportional errors in the value of d. The values of T were easily read to one-tenth of a degree, so that an error in this quantity would have an effect less than $\frac{1}{3000}$ on the value of d. Similarly the pressure was read to $\frac{1}{2}$ mm., leaving an error of less than $\frac{1}{1500}$ on the density. The volume V showed a tendency to error in the earlier portion of each group of experiments, but this was eliminated as the experiments progressed and especially where the results were more important, namely, in the region of the solidified gases. The greatest care was taken to insure that the whole volume of the bulb was occupied by the solid. The gas was allowed to enter in successive portions, each of which was liquefied and solidified previous to any further admission, so as to insure the absence of any vacua due to contraction.

3. The results for oxygen seem low, the boiling-point density being 1·118, whereas former results gave 1·138. On plotting the densities as ordinates to the temperatures as abscissæ the observations lie very closely on a straight line which (by least squares), is

$$d = 1{\cdot}5154 - 0{\cdot}004420t \quad \text{.....................} \quad (2),$$

t being absolute temperature.

Such a line as (2) must in any case be only a chord of the curve of liquid densities, and the nearer two observations are to the absolute zero, the more nearly will the chord joining them partake of the nature of a tangent to this curve at the absolute zero. Now, at so low a temperature as $20^\circ{\cdot}5$ for oxygen we may consider its gaseous density to be practically negligible. Hence one point on Matthias's rectilineal diameter for oxygen will be, at $20^\circ{\cdot}5$, a density equal to 0·7128, the half of that given in No. 4, Table I. In former papers I found the density of liquid oxygen at its boiling point to be 1·138, and of gaseous oxygen at the same point to be 0·00440; half the sum of these is 0·5712, which gives another point on Matthias's diameter at 90·5°. Thus Matthias's diameter for oxygen is

$$d = 0{\cdot}7543 - 0{\cdot}002023t \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3),$$

and taking the critical temperature as 155° absolute, we get the critical density to be 0·4407, agreeing notably with the usually accepted value 0·44. The inference from this is that the density of solid oxygen at the boiling point of hydrogen is 1·4256.

The results for nitrogen, taken at three temperatures, do not warrant the deduction of a linear relation between d and t, especially as on plotting the observations the concavity of the liquid density curve though slight is quite apparent. However, there are two observations at temperatures so low that the corresponding gaseous densities may be neglected, thus enabling us to construct a Matthias diameter. At the boiling point of hydrogen, the ordinate of the Matthias line is therefore very nearly $\frac{1}{2}$ (1·0265) = 0·5133; similarly at the melting point of nitrogen the ordinate is $\frac{1}{2}$ (0·8792) = 0·4396. Hence the Matthias diameter is

$$d = 0{\cdot}5492 - 0{\cdot}00175t \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4),$$

which for the critical temperature 127° gives the critical density as 0·3269. This agrees very well with the value deduced by Matthias* from Wroblewski's liquid densities, namely 0·333, though it is somewhat higher than the value 0·299 which he deduced from the theory of corresponding states.†

Only three observations have been obtained for hydrogen, which again lie nearly on a straight line, but nevertheless present a very slight concavity to the axis of temperature. If we treat the two lowest densities as we have done with nitrogen, we get for the Matthias diameter the line,

$$d = 0{\cdot}04136 - 0{\cdot}000247t \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5),$$

whence the annexed table (p. 256) of critical densities according to the temperatures chosen for the critical temperatures. Berthelot gives an estimate for the critical density as 0·033, and quotes Wroblewski's critical temperature as 33°, two results closely in accord with the numbers in this table. We are, therefore, justified in considering

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* 'Mem. Soc. Roy. des. Sci. de Liége,' 3rd Series, 1899.

† 'Le point critique des corps purs,' p. 176.

tc.	*dc.*
28°	0·03444
29	0·03420
30	0·03395
31	0·03370
32	0·03346
33	0·03321
34	0·03296

these hydrogen densities as very closely in agreement with facts, the density at the boiling point coinciding nearly with my former determinations.

4. Assuming that vapour densities at very low temperatures may be neglected in comparison with corresponding liquid densities, the Matthias diameter enables us to approximate to the molecular volumes of the condensed gases at the absolute zero. For if $d' = a - bt$ be the equation of Matthias's diameter, then $d = 2a - 2bt$ is very approximately the tangent of the liquid density curve near the absolute zero, and therefore $1/2a$ is the specific volume at absolute zero.

Hence from the above equations for Matthias's diameter, if V_0 = the molecular volume at absolute zero, we have $V_0 = 21·21$ for oxygen, $V_0 = 25·49$ for nitrogen, and $V_0 = 24·18$ for hydrogen. The oxygen and nitrogen molecular volumes at absolute zero probably err by defect; but the hydrogen result must be taken as very near the true value.

We may compare these values with the results of theoretical investigation. Guldberg* gives for the molecular volume at zero of oxygen 21·5, and of nitrogen 23·6; and Berthelot's† values for the same gases respectively are 20·8 and 25·0. From Baly and Donnan's ‡ equations for oxygen, nitrogen, carbonic oxide and argon, deduced from observations within the range of temperature 69°—90° absolute, we find the following values for the molecular volumes at absolute zero: oxygen 20·30; nitrogen 24·04; carbonic oxide 24·54; argon 20·34.

Again, the Waterston-Avenarius formula connecting temperature and fluid volume, namely,

$$v = c - d \log (A - t) \quad \ldots\ldots\ldots\ldots \quad (6),$$

where A is the critical temperature, gives the following equations, from Baly and Donnan's results,

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* 'Zeit. f. Phys. Chem.,' 1895, vol. 16, p. 7.

† 'Comptes Rendus,' March, 1900.

‡ 'Journ. Chem. Soc.,' July, 1902, pp. 911—914.

Oxygen .... $v = 1{\cdot}9305 - 0{\cdot}5838 \log (154 - t)$,
$V_0 = 20{\cdot}90, \quad c/d = 3{\cdot}3.$

Nitrogen .... $v = 2{\cdot}5659 - 0{\cdot}7868 \log (127 - t)$,
$V_0 = 25{\cdot}80, \quad c/d = 3{\cdot}27.$

Carbonic oxide ...... $v = 2{\cdot}6181 - 0{\cdot}7948 \log (133 - t)$,
$V_0 = 26{\cdot}04, \quad c/d = 3{\cdot}29.$

Argon................. $v = 1{\cdot}6331 - 0{\cdot}5026 \log (155 - t)$,
$V_0 = 21{\cdot}30, \quad c/d = 3{\cdot}70.$

This same formula has been adopted by Mallet and Friderich,* subject to the modification that A is a unique temperature to be determined for each substance from experiment. On applying it to twenty-five substances, studied by Sydney Young, they find that A is somewhat higher than the critical temperature and that c/d is always very nearly equal to 3·78. Baly and Donnan's observations give rise to these Waterston-Mallet equations:—

Oxygen .... $v = 3{\cdot}88413 - 1{\cdot}3604 \log (253 - t)$,
$V_0 = 19{\cdot}68, \quad c/d = 2{\cdot}86.$

Nitrogen .... $v = 3{\cdot}1563 - 1{\cdot}0560 \log (143{\cdot}67 - t)$,
$V_0 = 24{\cdot}58, \quad c/d = 2{\cdot}99.$

Carbonic oxide ... $v = 4{\cdot}60090 - 1{\cdot}64207 \log (191{\cdot}1 - t)$,
$V_0 = 23{\cdot}94, \quad c/d = 2{\cdot}80.$

Argon .. .. $v = 0{\cdot}98285 - 0{\cdot}19263 \log (112{\cdot}4 - t)$,
$V_0 = 23{\cdot}52, \quad c/d = 5{\cdot}10.$

With assumed values for A, in the neighbourhood of the range of temperature in which we look for the critical temperature, two Waterston-Mallet formulæ were constructed for hydrogen based on the results of Table III, namely:—

$$v = 23{\cdot}22 - 7{\cdot}536 \log (36{\cdot}5 - t), \quad V_0 = 22{\cdot}90,$$
$$v = 26{\cdot}83 - 9{\cdot}592 \log (41{\cdot}5 - t), \quad V_0 = 22{\cdot}62.$$

Assuming that for liquids Van der Waals's equation may be written

$$\frac{Rt}{v-b} = \frac{a}{v^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (7),$$

an assumption which has been employed by G. N. Lewis† and others, the results of Table III give for hydrogen, with this equation, $b = 11{\cdot}56$ and hence $V_0 = 23{\cdot}12$.

For comparison these values may be arranged in tabular form thus:—

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* 'Arch. Sci. Phys. et Nat.,' July, 1902.

† 'Amer. Acad. Arts and Sci.,' 1900, vol. 35, pp. 1—27.

	O.	N.	H.	CO.	Arg.
Present experiments	21·21	25·49	24·18	..	..
Guldberg	21·50	23·6	..	..	..
Berthelot	20·8	25·0	..	..	..
Baly, Linear	20·30	24·04	..	24·54	20·34
Baly, Waterston	20·90	25·80	..	26·04	21·30
Baly, W.-Mallet	19·68	24·58	..	23·94	23·52
W.-Mallet, for H.	..	..	22·90	..	..
W.-Mallet, for H.	..	..	22·62	..	..
Lewis	..	..	23·12	..	..

The two Waterston-Mallet results for hydrogen are of importance as showing that even great variations in the value of A affect but little the zero molecular volume. It is further to be remarked that the hydrogen results claim precedence over the others because they have been obtained from observations extending down to some 14° absolute. Moreover, the value 24·18 for hydrogen has been got directly from experiment, whilst the other values have been obtained from semi-theoretical equations whose validity is subject to some doubt.

5. With these values for the molecular volumes at absolute zero of oxygen, nitrogen and hydrogen, namely 21·21, 25·49 and 24·18 respectively, and knowing that the molecular volume of ice at absolute zero is 19·21 and of carbonic acid is 25·7 as deduced from a study of the coefficients of expansion at low temperatures,* we can find the contraction or expansion on the assumed hypothetical production of these compounds from their elements at the zero of temperature.

Thus 100 volumes of mixed solid hydrogen and oxygen become after combination 55·22 volume of ice, or there is a contraction of 45 per cent. This is of the same order of magnitude as for N_2O and Li_2O, whose contraction from solid oxygen, solid nitrogen and metal amounts to 60 per cent. On the other hand the production of carbonic acid from diamond ($V_0 = 6·82$) or graphite ($V_0 = 10·44$) and solid oxygen gives in the former case a slight expansion of $4\frac{1}{3}$ per cent., but in the latter case a slight contraction of 3 per cent.

The case of carbonic oxide ($V_0 = 24·5$) is interesting; produced from diamond and oxygen it expands by $74\frac{3}{4}$ per cent., and from graphite and oxygen it expands by $54\frac{3}{4}$ per cent., but on the addition of another atom of oxygen the resulting product, carbonic acid, undergoes a contraction of 27 per cent.

6. Berthelot† made a minute examination of the values of $d(pv)/dp$ in reduced co-ordinates for eight substances whose critical temperatures

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* "Coefficients of the Cubical Expansion of Ice, Hydrated Salts, Soluble Carbonic Acid, and other Substances at Low Temperature," 'Roy. Soc. Proc.,' vol. 73.

† "Sur les Thermomètres à gaz. Trav. et Mém. Poids et Mesures," vol. 13.

range from that of carbonic acid to that of hydrogen; and came to the conclusion that the co-volume is one quarter of the critical volume, at least for low pressures.

Guldberg* from a careful graphical discussion of Young's results arrived at the conclusion that the critical volume is 3·55 times the volume at absolute zero.

An independent examination of the reduced curve for Young's fluorbenzene showed that the reduced volume for a reduced pressure between 0·05 and 0·10 is very approximately 0·4, in other words, that the critical volume is two and a half times the co-volume. And it is to be noted that the variations of the reduced volume in this neighbourhood are very slight compared with the change of the reduced pressure. Further, this range of reduced pressure will cover the extent of the present experiments whatever may eventually prove to be the value of the real critical pressure. My old experiments gave as a maximum a critical pressure of a little over 15 atmospheres.

Hence taking Van der Waals's $b = 12$ for hydrogen, these results give respectively 48 c.c., 42·6 c.c. and 30 c.c. as the critical volume, with the corresponding critical densities, 0·0208, 0·0235, and 0·033.

What further considerations can be brought to determine between these results? Van der Waals gives, for corresponding states of two bodies, the equation†

$$\frac{d}{d'} = \frac{\mathrm{M}}{\mathrm{M}'}\frac{p_c}{t_c}\frac{t'_c}{p'_c},$$

where d is density and M is molecular weight. Now in the liquid states for moderate pressure, the density changes but slightly, hence we may assume with very little error that the boiling-point densities belong to corresponding states. Comparing, therefore, hydrogen with oxygen, we get:—

$$\frac{0{\cdot}070}{1{\cdot}138} = \frac{2}{32}\frac{p_c}{t_c}\frac{155}{50} \text{ or } \frac{t_c}{p_c} = 3{\cdot}15\,;$$

and comparing hydrogen with nitrogen we have,

$$\frac{0{\cdot}07}{0{\cdot}79} = \frac{2}{28}\frac{p_c}{t_c}\frac{127}{35} \text{ or } \frac{t_c}{p_c} = 2{\cdot}93,$$

hence, taking the mean, we have for hydrogen $t_c/p_c = 3{\cdot}04$. Now, Van der Waals's theory makes $p_cv_c/t_c = \frac{3}{8}\mathrm{R}$, but experiment seems to require p_cv_c/t_c to be equal to $\frac{1}{3{\cdot}77}\mathrm{R}$, and when p is measured in atmospheres, $\mathrm{R} = 41{\cdot}0183$ for hydrogen, for which, therefore,

$$\frac{p_cv_c}{t_c} = \frac{41{\cdot}0183}{3{\cdot}77} = 10{\cdot}8801.$$

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* *Loc. cit.*

† "Continuity, &c." (English translation), p. 491.

This gives $v_c = \frac{10\cdot8801}{3\cdot0} = 35\cdot08$ and the critical density as 0·0285, a result midway between that got from Guldberg's ratio and that derived from Young's reduced fluor-benzene.

Further experimental results can be brought to bear on the question. I have recently measured directly the heat of vaporisation of liquid hydrogen at the boiling point and find it to be not more than 125 calories per gramme. Assuming the Rankine equation $\log_{10} p = \mathrm{A} - \mathrm{B}/t$, the molecular heat of vaporisation is $2\mathrm{B} \log_e 10$. Hence we get $\mathrm{B} = 54\cdot34783$; and determining A from the values of p and t at the boiling point we have in atmospheres,

$$\log_{10} p = 54\cdot34783 \left(\frac{1}{20\cdot5} - \frac{1}{t}\right).$$

Now, at the critical point we have found $t_c = 3\cdot04\ p_c$, hence combining these results we have finally,

$$t_c = 33\cdot88^\circ, \text{ and thence } p_c = 11\cdot145 \text{ atmospheres.}$$

Before leaving these results we may write the Rankine equation in Van der Waals's form, namely,

$$-\log \epsilon = \frac{54\cdot34783}{t_c}\ \frac{1-m}{m},$$

where ϵ and m are the reduced pressure and temperature, and Van der Waals's f is $54\cdot34783/t_c$, or with the above result, 1·604, about half the usual value of f. In like manner Trouton's constant, namely, the ratio of the molecular latent heat to the absolute temperature of the boiling point, is 125/20·5 or 6·096, again only about half the usual value.

In this connection we may refer to Olzewski's experimental observation of the temperature, 192° absolute, at which the Joule-Thomson effect vanishes when the expansion is from a great pressure, in this instance between 110 and 117 atmospheres. With the usual Van der Waals's notation we may express the connection between this inversion temperature and the critical temperature in either of the forms,

$$t = \frac{27}{4}\left(\frac{v-b}{v}\right)^2 . \ t_c \quad \dots\dots\dots\dots\dots\dots \quad (9),$$

or

$$t = 3\left(1 + \tfrac{1}{2}\sqrt{1 - \frac{p}{9p_c}}\right)^2 . \ t_c \quad \dots\dots\dots\dots \quad (10),$$

the former equation shows that for small pressures and consequent values of v so great that b may be neglected in comparison with v, the critical temperature is 4/27 of the temperature of inversion. But when the pressure is great, the ratio b/v cannot be neglected and the critical temperature is a greater multiple of the temperature of inversion. The second of these equations shows that the initial pressure must not exceed nine times the critical pressure. Assuming a critical pressure of 16 atmospheres for hydrogen and taking $p = 117$ atmospheres and $t = 192°$, this equation gives the critical temperature as 42° absolute. Again, for $p_c = 15$ atmospheres, $t_c = 46°$ and for $t_c = 32°$, $p_c = 41$ atmospheres.

Results derived from a discussion of similar equations depending on Clausius's formula, Berthelot's "modified" Van der Waals's, or Reinganum's formula, are still farther from the value we expect.

In part explanation of this failure it is to be noted that these formulæ are but the best theoretical approximations we have at hand, and while they are useful within short ranges, we can hardly expect the same accuracy over a temperature range of five or six times the critical temperature.

Again, for a very large number of bodies the product of α, the co-efficient of expansion at the boiling point, and the critical temperature is constant and about 0·6 to 0·7.

Thus for oxygen from equation (2), we have $\alpha t_c = 0{\cdot}61$. For nitrogen we get $\alpha t_c = \dfrac{0{\cdot}0750}{0{\cdot}8042 \times 15} \times 127 = 0{\cdot}79$, but for hydrogen we have $\dfrac{0{\cdot}0054}{0{\cdot}07 \times 5{\cdot}8} \times 34 = 0{\cdot}45$ and even if we take the critical temperature as high as 42, αt_c only reaches 0·56.

7. There are, therefore, as far as we can see at present, and as far as theoretical considerations can aid us, great departures shown by hydrogen from what are fairly general results. Van der Waals's f and Trouton's constant are each only about half the usual values; and we have now found, from the consideration of the point of inversion of the Joule-Thomson effect, and of the product αt_c, variations greater than the average from the values we should have expected. Further experiment will be necessary before these discrepancies can be cleared up.

"On a Criterion which may serve to Test various Theories of Inheritance." By KARL PEARSON, F.R.S., University College, London. Received March 4,—Read March 17, 1904.

(1) One of the most difficult problems in the treatment of heredity is that of obtaining a satisfactory criterion which will enable us to distinguish the truth or falsehood of various hypotheses. As a rule, all the criteria used have been based upon a determination of the type of offspring due to parents of selected types. Unfortunately such a method of approaching the problem of heredity fails wholly to reach some of the most important modern theories, for the reason that these theories start from the assumption that the type of the offspring is not any, or at least any precise and simple function of the parental types. The type is said to be a factor which at present can only be determined by direct observation or by experimental crossing.

Mr. Galton in his 'Natural Inheritance,' it is true, used the term "midparent" to denote an individual compounded, in a simple way, from the two parental types, and giving offspring of a definite type. In generalising, however, Mr. Galton's conceptions in my "Law of Ancestral Heredity,"* I purposely placed before myself the aim of reducing the theory to a purely statistical theory, and discarded entirely the conception that the type of offspring was settled by the parental types. The generalised midparent of any generation became a compound of the *deviations* from type of the ancestry of that generation, and no assumption was made as to any inheritance of absolute type; the theory became purely a statistical theory of the distribution in various generations of the deviations from type. At a somewhat later date the Mendelians gave up the conception that the type of the offspring was known from the parental types. The actual effect of crossing two individuals was compared to the formation of a chemical compound, the character of which could not *à priori* be predicted from the known nature of the components. It was a matter to be determined by observation or experiment only. With this wider view the original Mendelian theory of "dominant" and "recessive" characters has disappeared, and that theory has thus far approximated itself to the "Ancestral Law."

In a second paper communicated to the Royal Society† entitled the "Law of Reversion," I endeavoured to work out a general theory of alternative inheritance, on the hypothesis that a certain number of the offspring were for any character like one or other parent or like some one or other ancestor, the proportions of offspring like ancestral types diminishing in number with the distance of descent. This

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* 'Roy. Soc. Proc.,' vol. 62, pp. 387 and 388.

† 'Roy. Soc. Proc.,' vol. 66, p. 142 *et seq.*

theory was developed with special reference to certain characters in man and hound, which were said to be alternative, *i.e.*, the offspring, if the parental types were different, took after one or other parent. Quite recently Dr. Franz Boas* has published a very suggestive paper on "Heredity in Head Form." He propounds a theory that the cephalic index in man is a case of alternative inheritance, and that the offspring take after one or other parent. His theory is less general than my theory of 1899, because he excludes from consideration the reversion to grandparents or higher ancestry. It is more general than mine, in that he assumes imperfect and not perfect correlation between the groups of offspring and the individual parents whom they respectively follow. This I consider a distinct gain. But the neglect of ancestry, other than the immediate parents, renders the application of his theory to so-called Mendelian phenomena absolutely impossible. Thus, when a white mouse is crossed with a grey mouse the hybrid generation can hardly be considered as made up of two groups taking respectively after white and grey parents. In the following, or segregating generation, it is possible to consider the groups as a result of reversion to grandparental or higher ancestral types; it is not possible to deal with them on Dr. Boas's more limited theory. Hence, I think he errs in terming his theory a generalised form of Mendel's Law. It is a theory of alternative inheritance, and no such theory which stops at resemblance to the paternal and maternal types can describe the fundamental phenemenon of segregation in the second generation. We must deal with reversion to higher ancestors, whether such reversion be physiologically brought about by the purity of the gamete or by any other process.

A general theory of alternative inheritance may cover Mendelian phenomena; a theory of the individual dominance of either parent in one or another group of offspring, a theory of what I have defined as intermittent unit prepotency, cannot do so.† Still, Dr. Boas considers that he has evidence for his theory in the inheritance of cephalic index in man, and it seems to me that his paper indicates the manner in which it may be possible to still further generalise my results of 1899. It is clear, however, that we badly need some criterion to distinguish between these competing theories in the case of measurements of the inheritance of any given character. Since none of the three theories referred to is essentially based on the determination of the type of the offspring from the parental types,‡ we are thrown back

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* 'The American Anthropologist' (N.S.), vol. 5, pp. 530—538.

† 'Biometrika,' vol. 2, p. 389.

‡ Dr. Boas, I think, has not fully understood my theory of the midparent. He repeatedly speaks of the *mid* parental value of a character and of the offspring clustering round this value on the theory of "Galton and Pearson." There is absolutely no antagonism between my theory and the stature of Americo-European half-bloods exceeding both parental types. My midparent is based solely on

on a consideration of the variability of the offspring due to parents of given types. Luckily the three theories give us totally different values of the variability of an array of offspring due to parents of given types, and we have in this question of variability a crucial test of the applicability of one or other of the theories to the inheritance of a given character.

In order to bring this point out I must briefly consider the variability of arrays of offspring under the three theories.

(2) Variability of an array of offspring on the pure statistical theory developed as the "Law of Ancestral Heredity."

If σ_c be the standard deviation of the offspring, say of one sex, ρ_{fc} their correlation with father, ρ_{mc} with mother and ρ_{fm} the coefficient of assortative mating, then

$$\Sigma = \sigma_c \sqrt{\left(\frac{1 - \rho_{fc}^2 - \rho_{mc}^2 - \rho_{fm}^2 + 2\rho_{fc}\,\rho_{mc}\,\rho_{fm}}{1 - \rho_{fm}^2}\right)}$$

is, whatever be the nature of the frequency distribution provided the regression be linear, the mean of the standard deviations of all the arrays due to parents of given types. If the characters be distributed according to the normal law of deviation, then Σ will not only be the mean of all the array standard deviations, but the actual standard deviation of each array. If, therefore, the character selected be in each generation distributed according to the normal law, we should expect to find that if we take all pairs of given types, the offspring due to such pairs will have a variability given with reasonable closeness by the above result. If we deal with all the offspring due to fathers, say, of a given type, the mean standard deviation of the arrays will be $\sigma_c \sqrt{(1 - \rho_{fc}^2)}$, and in so far as the distributions are approximately normal the standard deviations of all arrays will be the same.* Hence arises the importance, when we use Σ as the variability of the offspring, of showing that the regression for the given character is linear, and that the frequency is not widely divergent from a normal distribution. These points were dealt with by Mr. Galton in his very first investigation of the subject. He actually considered in the case of stature whether some of the offspring followed the father and some the mother, and showed that Σ did not vary sensibly from array to array.† Subject, therefore, to a demonstration for each character that the frequency is approximately normal and the regression linear, we see that the purely statistical theory of

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deviations from type ('Roy. Soc. Proc.,' vol. 62, p. 387), and the offspring type itself may be wholly different from both parental types, exceeding or falling short of them.

* The property that Σ is the mean of the standard deviation of all the arrays was first stated by Yule, 'Roy. Soc. Proc.,' vol. 60, p. 477, for the case of linear regression.

† 'Natural Inheritance,' pp. 89—90, and Table 10, p. 207. This investigation seems to have escaped Dr. Boas, see *loc. cit.*, p. 530.

heredity summed up in the "Law of Ancestral Heredity" would assert that the offspring of all parents of a given type would have a constant variability, whatever that type might be.

(3) *Mendel's Theory.*—If we take as a fair sample of this theory the generalised Mendelian theory, discussed by me in a recent communication to the Society, and now published in the 'Phil. Trans.,' we find that this constancy of the standard deviation of the array is no longer true. It only becomes true if the number of Mendelian couplets on which the character depends is indefinitely great. In other cases, while $\sigma_c \sqrt{(1-\rho_{fc}^2)}$ is still the mean of the standard deviations of the arrays, the actual value of the standard deviation alters sensibly and continuously as we cross the correlation table, always tending to increase in one direction and decrease in the other. Clearly we have, as I have pointed out in the paper referred to, an excellent criterion here between the two theories.*

(4) Lastly, let us turn to the theory of individual parental dominance. I will give the analysis for this case, extending and generalising Dr. Boas's formulæ. I suppose the total offspring n of a pair of parents to be divided into two groups n_1 and n_2 in number. In the first groups with a mean m_1 the fathers are considered as predominant without the mothers being supposed at present entirely without influence; in the second group with mean m_2, the mothers are supposed to have the predominating influence. We may speak of these two groups, for convenience only, as "father's offspring" and "mother's offspring." Let σ_{c_1} and σ_{c_2} be the standard deviations of "father's offspring" and "mother's offspring" for a given character z; let σ_f, σ_m, σ_c be the standard deviations of the fathers for the same character, of the mothers, and of the offspring as a whole. The mean m of the offspring as a whole will be given by $m = (n_1 m_1 + n_2 m_2)/n$. Further let r_{1f}, r_{2f} be the paternal offspring correlations for "father's offspring" and "mother's offspring," and r_{1m}, r_{2m} the maternal offspring correlations for the same two groups respectively; r_{fm} shall be the coefficient of assortative mating between parents, x, y, z are the characters in father, mother, and child, x and y being measured from the parental means and z from some other origin. Then, if S_1 stands for a summaration of all the offspring of the first and S_2 of the second class, we have

$$n\sigma_c^2 = n_1\{\sigma_{c_1}^2 + (m_1 - m)^2\} + n_2\{\sigma_{c_2}^2 + (m_2 - m)^2\},$$

or

$$\sigma_c^2 = (n_1\sigma_{c_1}^2 + n_2\sigma_{c_2}^2)/n + n_1 n_2 (m_1 - m_2)^2/n \ldots\ldots\ldots\ldots \quad \text{(i).}$$

Let ρ_{fc} and ρ_{mc} be the total paternal and maternal correlations; we have

$$\begin{aligned}\rho_{fc} &= [S_1\{x(z-m)\} + S_2\{x(z-m)\}]/(n\sigma_f\sigma_c) \\ &= (n_1\sigma_f\sigma_{c_1}r_{1f} + n_2\sigma_f\sigma_{c_2}r_{2f})/(n\sigma_f\sigma_c),\end{aligned}$$

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* 'Phil. Trans.,' A, vol. 203, p. 66.

or

$$\rho_{fc} = \frac{n_1}{n}\frac{\sigma_{c_1}}{\sigma_c} r_{1f} + \frac{n_2}{n}\frac{\sigma_{c_2}}{\sigma_c} r_{2f} \;\ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(ii)}.$$

Similarly,

$$\rho_{mc} = \frac{n_1}{n}\frac{\sigma_{c_1}}{\sigma_c} r_{1m} + \frac{n_2}{n}\frac{\sigma_{c_2}}{\sigma_c} r_{2m} \;\ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(iii)}.$$

Here we have supposed that although the fathers of "father's offspring" and of "mother's offspring" will, when weighted with their offspring, be unequal in number (*i.e.* n_1 and n_2), yet their variabilities are the same, and similarly for the mothers. This is equivalent to supposing no correlation between the dominant effect of a parent and his or her deviation from type. Otherwise we cannot put σ_f in the S_1 sum the same as σ_f in the S_2 sum.

We are now able to write down the general regression equation of bi-parental inheritance, *i.e.*,

$$z_p - m = \frac{\rho_{fc} - \rho_{fm}\,\rho_{mc}}{1 - \rho^2_{mf}}\,\frac{\sigma_c}{\sigma_f}\,x + \frac{\rho_{mc} - \rho_{fm}\,\rho_{fc}}{1 - \rho^2_{fm}}\,\frac{\sigma_c}{\sigma_m}\,y,$$

where z_p is the probable value of the character in offspring of parents of characters x and y. Hence, if we remember that $r_{fm} = \rho_{fm}$, we have on substituting from (ii) and (iii):—

$$z_p = m + \left(\frac{n_1}{n}\frac{\sigma_{c_1}}{\sigma_f} R_{1f} + \frac{n_2}{n}\frac{\sigma_{c_2}}{\sigma_f} R_{2f}\right) x + \left(\frac{n_1}{n}\frac{\sigma_{c_1}}{\sigma_m} R_{1m} + \frac{n_2}{n}\frac{\sigma_{c_2}}{\sigma_m} R_{2m}\right) y \ldots \quad \text{(iv)},$$

where R_{1f}, R_{1m} are the bi-parental co-efficients $(r_{1f} - r_{mf}\, r_{1m})/(1 - r_{m\,f}^{\,2})$ and $(r_{1m} - r_{mf}\, r_{1f})/(1 - r_{m\,f}^{\,2})$, of the "father's offspring" and R_{2f}, R_{2m} similar quantities for the "mother's offspring."

Now, fixing our attention for a moment on "father's offspring," we should expect parents of characters x and y to produce an array of father's offspring with a mean:

$$\mu_1 = m_1 + R_{1f}\frac{\sigma_{c_1}}{\sigma_f}\,x + R_{1m}\frac{\sigma_{c_1}}{\sigma_m}\,y \;\ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(v)},$$

and with a standard deviation s_1 given by

$$s_1^2 = \sigma_{c_1}^2\,(1 - r_{1f}^2 - r_{1m}^2 - r_{mf}^2 + 2 r_{1f} r_{1m} r_{mf})/(1 - r_{m\,f}^{\,2}) \;\ldots \quad \text{(vi)}.$$

Similarly the arrays of "mother's offspring" for parents of the same characters would have a mean:

$$\mu_2 = m_2 + R_{2f}\frac{\sigma_{c_2}}{\sigma_f}\,x + R_{2m}\frac{\sigma_{c_2}}{\sigma_m}\,y \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(vii)},$$

and a standard deviation s_2 given by

$$s_2^2 = \sigma_{c_2}^2\,(1 - r_{2f}^2 - r_{2m}^2 - r_{mf}^2 + 2 r_{2f} r_{2m} r_{mf})/(1 - r_{mf}^2) \ldots \quad \text{(viii)}.$$

Now, if ν_1 and ν_2 be the numbers of children in the array of $x\ y$ parents belonging to either group, and $\nu = \nu_1 + \nu_2$, we shall have for the standard deviation of the total offspring of $x\ y$ parents:

$$\nu\Sigma_{xy}^2 = \nu_1\{s_1^2 + (\mu_1 - z_p)^2\} + \nu_2\{s_2^2 + (\mu_2 - z_p)^2\} \quad \ldots\ldots \quad \text{(ix)},$$

where z_p is given by (iv) and is the mean of the whole array.

If the relative influences of mother and father depended upon their characters, we could go no further with (ix) until this had been determined. If, however, we suppose this influence on the average to be not sensibly dependent on the characters x and y, we may write $n_1/n = \nu_1/\nu$ and $n_2/n = \nu_2/\nu$. Substitute from (iv), (v) and (vii), we find after some reductions:

$$\Sigma_{xy}^2 = \frac{n_1 s_1^2 + n_2 s_2^2}{n} + \frac{n_1 n_2}{n^2}\left\{(m_1 - m_2) + \left(R_{1f}\frac{\sigma_{c_1}}{\sigma_f} - R_{2f}\frac{\sigma_{c_2}}{\sigma_f}\right)x + \left(R_{1m}\frac{\sigma_{c_1}}{\sigma_m} - R_{2m}\frac{\sigma_{c_2}}{\sigma_m}\right)y\right\}^2 \quad \ldots\ldots \quad \text{(x)}.$$

To verify this equation I transferred to the mean, summed for every possible array, *i.e.*, for all values of x and y, and divided by the total number of arrays. The left-hand side should be σ_c^2, the right-hand side became, after some considerable reductions, identical with the right-hand side of (i), as it should be.

We have in equation (x) accordingly, the variability of an array of all offspring on the hypothesis that the children may be divided into two groups, differently related to the two parents. We see at once that the variability of an array will depend on the actual values of the parental characters, unless:

$$R_{1f}\sigma_{c_1}/\sigma_f = R_{2f}\sigma_{c_2}/\sigma_f,$$

and

$$R_{1m}\sigma_{c_1}/\sigma_m = R_{2m}\sigma_{c_2}/\sigma_m.$$

But this is asserting that the bi-parental regression co-efficients for the two groups are the same, or, as we may put it, that there is no distinction between the parental influences in the two groups. This is the case usually assumed under the "Law of Ancestral Heredity" with its constant variability within the limits of random sampling for the arrays. In every other case the arrays alter in variability with the magnitude of the parental character.

Let us look at the matter from another light and sum Σ^2_{xy} for every value of y only, or we can obtain the same result by direct investigation, namely, we find if Σ_x be the standard deviation of an array of offspring due to fathers of character x,

$$\Sigma_x^2 = \frac{n_1\sigma_{c_1}^2(1 - r_{1f}^2) + n_2\sigma_{c_2}^2(1 - r_{2f}^2)}{n} + \frac{n_1 n_2}{n^2}\left\{m_1 - m_2 + \left(r_{1f}\frac{\sigma_{c_1}}{\sigma_f} - r_{2f}\frac{\sigma_{c_2}}{\sigma_f}\right)x\right\}^2 \quad \ldots\ldots \quad \text{(xi)}.$$

Or, we see that unless

$$r_{1f}\sigma_{c_1}/\sigma_f = r_{2f}\sigma_{c_2}/\sigma_f,$$

i.e., the paternal influence be the same in both groups, or there be no question of individual dominance of the parents in "father's offspring" and "mother's offspring" respectively, the arrays of offspring due to different classes of fathers will not be equally variable. It is clear that if the standard deviation of the array be plotted to the size of the father's character, the resulting curve will be a hyperbola with real axis vertical, and in the two directions across the correlation table taken from the parental value

$$x = (m_1 - m_2)\Big/\left(r_{2f}\frac{\sigma_{c_2}}{\sigma_f} - r_{1f}\frac{\sigma_{c_1}}{\sigma_f}\right)$$

the variability of the arrays will rapidly increase from a minimum.

As there can hardly be a sensible distinction between m_1 and m_2, for it would mean bimodality in all the characters dealt with, which is contrary to experience, we may say that the variability of the offspring arrays increases in both directions with the deviation of the father (or mother) from the mean.

If we put $m_1 = m_2$ we have:

$$\Sigma_x^2 = \frac{n_1\sigma_{c_1}^2(1-r_{1f}^2) + n_2\sigma_{c_2}^2(1-r_{2f}^2)}{n} + \frac{n_1 n_2}{n^2}\left(r_{1f}\frac{\sigma_{c_1}}{\sigma_f} - r_{2f}\frac{\sigma_{c_2}}{\sigma_f}\right)^2 x^2 \quad \ldots \quad \text{(xii)}.$$

which shows clearly how the variability of the array increases hyperbolically with the deviation of the father from the mean.

(5) Now it is as well to take one or two numerical cases to appreciate the degree of curvature of this hyperbola, for if it were a very flat hyperbola, it possibly could not be readily distinguished from a horizontal straight line (Ancestral Law) or from a parabola (Mendel's Law). I take the hypothesis suggested by Dr. Boas, *i.e.*, a negligible influence of the father on "mother's offspring," and a negligible influence of the mother on "father's offspring."

We have $r_{2f} = r_{1m} = 0$. Further, if we suppose the two groups of offspring to be equal in number and equally variable, $n_1 = n_2$ and $\sigma_{c_1} = \sigma_{c_2} = \sigma_c$. From (ii) it follows that

$$\rho_{fc} = \frac{n_1}{n}\frac{\sigma_{c_1}}{\sigma_c} r_{1f} = \tfrac{1}{2} r_{1f}.$$

Therefore from (xii) it follows that

$$\Sigma_x^2 = \sigma_c^2\left\{(1 - 2\rho^2_{fc}) + \rho_{fc}^2\frac{x^2}{\sigma_f^2}\right\}.$$

Accordingly, we have:

$$\begin{aligned} x &= 0, & \Sigma_x^2 &= \sigma_c^2(1-2\rho_{fc}^2),\\ x &= \sigma_f, & \Sigma_x^2 &= \sigma_c^2(1-\rho_{fc}^2),\\ x &= \sqrt{2}\sigma_f & \Sigma_x^2 &= \sigma_c^2,\\ x &= 2\sigma_f, & \Sigma_x^2 &= \sigma_c^2(1+2\rho_{fc}^2), \end{aligned}$$

whence we have the following table:—

Table of Σ_x/σ_c.

	ρ_{fc}.		
x.	0·3.	0·4.	0·5.
0.	0·91	0·82	0·71
σ_f.	0·95	0·96	0·87
$\sqrt{2}\sigma_f$.	1·00	1·00	1·00
$2\sigma_f$.	1·09	1·15	1·22

Now parental correlations in my experience of extensive masses of good data are rarely as low as 0·3, generally over 0·4, or nearer even 0·5. But even with 0·3 we see that there ought to be about 20 per cent. increase in the standard deviation of an array as we pass from the mean father to a father with a deviation equal to twice the paternal deviation, *i.e.*, to an array which will be given with at least a moderate number of cases in any parental correlation table. As we approach a parental correlation of 0·4—0·5, this increase of the standard deviation amounts to 40—72 per cent., and should be still more conspicuous. We conclude, therefore, that if the parents are respectively dominant in two separate groups of offspring, then when we plot the standard deviations of the arrays of offspring to the deviations of the parent from the mean, we ought to get a very sensibly hyperbolic curve. Or, if we plot the squares of the one to those of the other, we ought to get a sloping straight line of very sensible slope, 0·1—0·25 about, instead of the horizontal line of the "Ancestral Law."

If we suppose the same conditions to apply to a bi-parental array, *i.e.*, $m_1 = m_2$, $n_1 = n_2 = \frac{1}{2}n$, $\sigma_{c_1} = \sigma_{c_2} = \sigma_c$, $r_{2f} = r_{1m} = 0$, we find that (x) reduces to

$$\Sigma_{xy}^2 = \sigma_c^2\left\{\left(1-\frac{1}{2}\frac{r_{1f}^2+r_{2m}^2}{1-r_{mf}^2}\right)+\left(\frac{\frac{1}{2}r_{1f}}{1-r_{mf}^2}\frac{x}{\sigma_f}-\frac{\frac{1}{2}r_{2m}}{1-r_{mf}^2}\frac{y}{\sigma_m}\right)^2\right\}.$$

Or, using (ii) and (iii), which give $\rho_{fc} = \frac{1}{2}r_{1f}$, $\rho_{mc} = \frac{1}{2}r_{2m}$:

$$\Sigma_{xy}^2 = \sigma_c^2\left\{\left(1-2\frac{\rho_{fc}^2+\rho_{mc}^2}{1-\rho_{mf}^2}\right)+\left(\frac{\rho_{fc}}{1-\rho_{mf}^2}\frac{x}{\sigma_f}-\frac{\rho_{mc}}{1-o_{mf}^2}\frac{y}{\sigma_m}\right)^2\right\} \quad \text{(xiii)}.$$

Let us apply this to the case of stature of parents and sons in man. Here I have deduced from my "Family Data Records"*

$$\rho_{fc} = 0{\cdot}5140, \ \rho_{mc} = 0{\cdot}4938, \ \rho_{mf} = 0{\cdot}2804 \ldots\ldots\ldots \quad \text{(xiv)}.$$

We find:

$$\Sigma_{xy}{}^2/\sigma_c{}^2 = -0{\cdot}1028 + \left(0{\cdot}5579\frac{x}{\sigma_f} - 0{\cdot}5359\frac{y}{\sigma_m}\right)^2 \ldots\ldots \quad \text{(xv)}.$$

Hence we have the impossible value $32\sigma_c\sqrt{-1}\,\sigma_c$ for the variability of the arrays of offspring due to mean parents. Generally we deduce—

Values of Σ_{xy}/σ_c for Arrays.

Father: x/σ_f.	Mother: y/σ_m.	Array: Σ_{xy}/σ_c.
0	0	(0·32 $\sqrt{(-1)}$?)
+1	0	0·46
+1	−1	1·05
+1	−2	1·60
+2	−2	2·19

We are dealing here with measurements on upwards of 1000 families; the probable errors, therefore, of ρ_{fc} and ρ_{mc} hardly allow of our supposing the first term on the right in (xiv) to be zero. But if we do make this hazardous assumption, we see that as we pass from pairs of mean parents to fathers of 6′ 1″ and mothers of 4′ 10″ we should pass from an array of offspring of no variability to one *twice* as variable as the general population. Further, for any given male there would exist a female relatively only very slightly taller than he is, who would have offspring with him of sensibly no variability. In view of this result we may safely assert that the hypothesis of "father's offspring" and "mother's offspring" cannot apply to stature in man under any conditions in the least approximating to Dr. Boas's assumptions in the case of cephalic index.

(6) Our conclusions may be summed up as follows:—The variability of the array of offspring due to a group of parents of definite character can be satisfactorily used as a criterion between various theories of inheritance. In particular if the variability of the array be plotted to the character of the parent—

(*a*) On the hypothesis of the "Ancestral Law" a horizontal straight line is the resulting curve.

(*b*) On the generalised Mendelian theory the result is a parabola with horizontal axis.

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[*] 'Biometrika,' vol. 2, pp. 373 and 378.

(*c*) On the generalised theory of alternative inheritance which divides the offspring into two groups more intimately associated with one or other parent, the resulting curve is a hyperbola with vertical real axis.

(7) I have applied the criterion here developed to my measurements on father and son in more than 1000 families. The three characters stature, span and forearm are dealt with. The correlation tables for the paternal inheritance of these characters will be found in a recent memoir by Dr. Lee and myself on the "Inheritance of the Physical Characters in Man."* We excluded all arrays with less than eight individuals in them, deeming it absolutely untrustworthy to find a mean and standard deviation from fewer than eight cases. The standard deviation of each array for a given paternal character was found by Dr. Alice Lee. These were then plotted to the paternal character by Mr. W. L. Atcherley, and results are shown in the accompanying Diagram 1. The zigzag polygons in each case give the plotted variabilities of the arrays, the vertical numbers being the total on which the variability is based. The horizontal line AA gives the mean value, $\sigma_c \sqrt{(1-\rho_{fc}{}^2)}$, of the standard deviations of the arrays according to the statistical theory. The broken lines *ccc* and *c′c′c′* give $\sigma_c \sqrt{(1-\rho^2{}_{fc})} \pm$ twice the probable error of the deviation of an array from $\sigma_c \sqrt{(1-\rho^2{}_{fc})}$. Thus if $\Sigma = \sigma_c \sqrt{(1-\rho^2{}_{fc})}$ and $\Sigma_x =$ the standard deviation of an array of m individuals out of a total of n, we have plotted up and down from Σ the quantity

$$2 \times 0{\cdot}65449 \sqrt{\left(\frac{\Sigma^2}{2n} + \frac{\Sigma_x{}^2}{2m}\right)}.$$

Now unless a difference is at least *twice* its probably error we certainly cannot assert it to be significant. Now, if the diagram be examined, it will be seen that almost without exception the zigzag polygons fall well within the non-significant areas bounded by *ccc* and *c′c′c′*. There are, indeed, three exceptions, but all three occur in arrays with less than twenty individuals, or arrays where some eccentricity of individual or measurement might easily make itself felt. But there is really no need to appeal even to this explanation, we have thirty-nine observations on arrays, and three of these only exceed the double of their probable error; this is actually less than *half* the excesses we might have expected on the theory of probability.

There is clearly absolutely nothing in the observed results opposed to that constancy of variability in the arrays which is suggested in the usual treatment of the "Law of Ancestral Heredity."

(8) I next look at the theory of alternative inheritance, or at the hypothesis that some children follow the one, some the other parent. An examination of the diagrams show that there is not the least

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* 'Biometrika,' vol. 2, pp. 415—417.

approach to a hyperbolic distribution of the array variabilities. The hyperbola would have its axis vertical and vertex downwards. In the case of stature I give the hyperbola that would result from a special

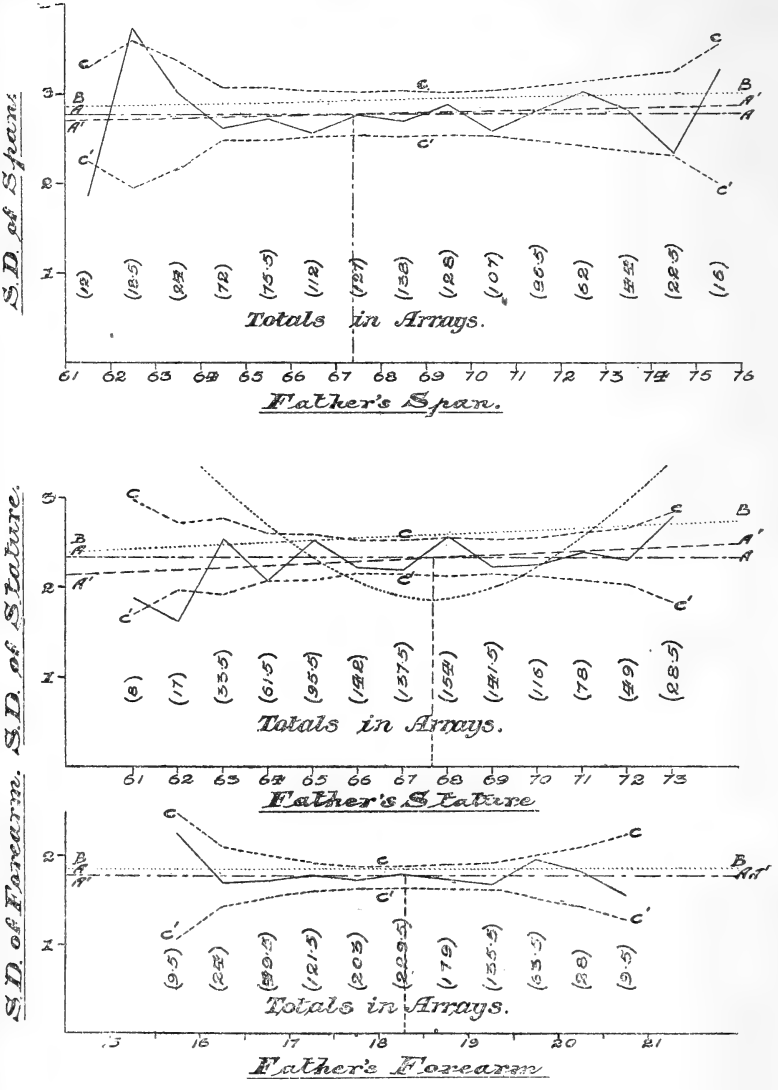

Diagram I.—Arrays of sons.

case, namely, Dr. Boas's hypothesis of equal numbers of offspring with equal variability and of like mean following each parent. The equation to this hyperbola may be found from (xi), by putting

$\sigma_{c_1} = \sigma_{c_2} = \sigma_c = 2''\cdot710$, $r_{1f} = 2\rho_{fc} = 1\cdot0280$, $n_1 = n_2 = \frac{1}{2}n$, and $r_{2f} = 0$. We have

$$(\Sigma_x/\sigma_c)^2 = 0\cdot4716 + 0\cdot2642\,(x/\sigma_f)^2,$$

where $\sigma_f = 2''\cdot568$.

It will be seen at once that the variability of the array is too small for the mean father and far too large for the exceptional father. I think we may safely conclude that for man in the case of the three characters under investigation no such theory of alternative inheritance applies.

(9) I now turn to the third or Mendelian hypothesis. Are the variability distributions better represented by parabolas of horizontal axes than by horizontal straight lines? We have a series of points in each case and the question is: What is the best fitting parabola? The equation to the Mendelian parabola, as given in my memoir already referred to,* is in the notation of the present paper

$$\frac{\Sigma_x{}^2}{\sigma_c{}^2} = \frac{8}{9} + \frac{4}{9\sqrt{(3q)}}\frac{x}{\sigma_f} \quad\text{......................} \quad \text{(xvi)},$$

where q is the number of Mendelian couplets, and x is measured from the mean.† Writing $\Sigma_x{}^2/\sigma_c{}^2 = \xi$, $x/\sigma_f = \eta$, this is of the form, a and b being numerics:

$$\xi = a + b\eta.$$

Applying the method of least squares, since a is an absolute constant, we find for the best value of b, μ_x being the total in the x array:

$$b = \frac{\sigma_f}{\sigma_c{}^2}\frac{\mathrm{S}\{\mu_x(\Sigma_x{}^2 - \frac{8}{9}\sigma_c{}^2)\,x\}}{\mathrm{S}\,(\mu_x\,x^2)},$$

or, q the number of Mendelian couplets is to be found from

$$\frac{1}{\sqrt{(q)}} = \frac{9\sqrt{(3)}}{4}\frac{\sigma_f}{\sigma_c{}^2}\frac{\mathrm{S}\{\mu_x(\Sigma_x{}^2 - \frac{8}{9}\sigma_c{}^2)\,x\}}{\mathrm{S}\,(\mu_x\,x^2)} \quad\text{.........}\quad \text{(xvii)}.$$

Working this out for the case of stature first, I found, after some rather laborious arithmetic, that $q = 48$. Thus the best fitting Mendelian parabola needs no less than 48 couplets. We may write the parabola in the form

$$\frac{\Sigma_x{}^2}{\sigma_c{}^2} = \tfrac{8}{9}\left(1 + \tfrac{1}{24}\frac{x}{\sigma_f}\right).$$

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* 'Phil. Trans.,' A, vol. 203, pp. 66–67.

† To reduce to the result of the above memoir we have $\Sigma_x = \epsilon\sigma_s$, $\sigma_c = \sigma_f = \epsilon\sqrt{(\frac{3}{16})n}$ $x = \epsilon(s - \frac{1}{4}n)$, $q = n$, where ϵ is an undetermined constant depending on the relation between actual scale and number of Mendelian couplets.

or, since x does not exceed 2 to 3 σ_f we have for the part of the parabola involved very nearly the straight line

$$\Sigma_x/\sigma_c = \sqrt{\tfrac{8}{9}}\left(1 + \tfrac{1}{48}\frac{x}{\sigma_f}\right) \quad\ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(xviii)}.$$

This, substituting the values of σ_c and σ_f, is a straight line of slope 0·021. Now the best fitting line to the observations, A′A′, has a slope of 0·022, or we conclude that the Mendelian parabola is when the number of couplets is as large as in the present case sensibly parallel to the line which best represents the variability of the arrays plotted to the parents' character. The Mendelian line B′B′ of Diagram I is not as good a fit as the line AA′, because the theory constrains it to pass through the point given by $\sigma_s = \sqrt{(8/9)}\,\sigma_c$, and not through the actual mean point $\sigma_c\sqrt{(1 - r_{fc}^2)}$. This is owing to the fact that the Mendelian theory gives r_{fc} constant and equal to 1/3. Hence, we see that the Mendelian theory will not, as a rule, give as good a fit to the observations as the best fitting line, when the number of couplets is large as in this case and the correlation differs from 1/3. The parabola thus sensibly coincides in direction with the best fitting straight line, but is raised above it in position.

I give the best fitting straight lines for the three characters we have been considering

For Span—

$$y - 2''\cdot762 = 0\cdot011\,(x - 67''\cdot396).$$

Probable error of the slope 0·011, equals 0·013.

For Stature—

$$y - 2''\cdot344 = 0\cdot022\,(x - 67''\cdot686).$$

Probable error of the slope 0·022, equals 0·013.

For Forearm—

$$y - 1''\cdot773 = -0\cdot003\,(x - 18''\cdot279).$$

Probable error of the slope −0·003, equals 0·009.

Thus, of the three slopes all differ by less than twice and two of them by less than once their probable error from zero. We may accordingly conclude that neither in the best fitting straight lines, nor consequently in the Mendelian parabolas for the measurable range, is there any sensible deviation from horizontality. In fact we have 48 couplets in the case of stature, and roundly 150 for span* and 2000 for forearm. With such numbers the Mendelian theory cannot on the problem of variability of arrays give any other sensible answer for the range available for investigation than the constant variability

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* If the Mendelian theory discussed were correct, it would be difficult to grasp how the forearm-inheritance could be determined by far *more* couplets than the span is, or why one slope should be negative and the other positive.

of the "Ancestral Law," but it does not equal the latter law in its description of the facts, because it shifts the mean variability of the array out of its proper place owing to its absolute rigidity in the value of parental correlation. I think we may say that for the three characters here considered the "Law of Ancestral Heredity" is the only one of the three theories which clearly describes the facts, and the facts at no point differ significantly from its statements.

(10) The theory of alternative inheritance, which differentiates the offspring into "father's offspring" and "mother's offspring," permits of being tested in another manner, namely, by considering the variability of the array of brothers who have a brother of given character x. Let us first look at the problem generally. A man of extreme character value will have a group of brethren, say, "father's offspring," who allowing for regression are like himself, but the other group of his brethren will be far less like himself, and on the average of all mothers near the population mean. Thus, the array of brethren of a man of extreme character will be made up of two components, one tending to be like himself, the other like the general population. Hence on the whole the array of brothers corresponding to men of a given type must become more and more variable, the more marked the deviation of the given type. I will now give the results of the analysis. Let ρ be the general correlation of brothers, m their mean, and σ their standard deviation. Let r_{11} be the correlation of brothers who are "father's offspring," m_1 their mean, and σ their standard deviation; let r_{22}, m_2 and σ_2 be the corresponding constants for the brethren who are "mother's offspring." Let ν_1 and ν_2 be the number of brethren of each class. Then if an individual have a deviation x from the mean of all brethren, he will if a "father's offspring" have a co-fraternity of standard deviation $\sigma_1 \sqrt{(1 - r_{11}^2)}$ and at distance from the mean of offspring given by

$$\begin{aligned}\mu_{11} &= m_1 + r_{11} \frac{\sigma_1}{\sigma_1} (x + m - m_1) - m - \rho x \\ &= (r_{11} - \rho) x + (m_1 - m)(1 - r_{11}).\end{aligned}$$

He will have $\nu_1 - 1$ of such brethren, but he will also have ν_2 brethren of mother's offspring type with a standard deviation $\sigma_2 \sqrt{(1 - r_{12}^2)}$ and at distance from mean of offspring given by

$$\begin{aligned}\mu_{12} &= m_2 + r_{12} \frac{\sigma_2}{\sigma_1} (x + m - m_2) - m - \rho x \\ &= \left(r_{12} \frac{\sigma_2}{\sigma_1} - \rho\right) x + (m_2 - m)(1 - r_{12}).\end{aligned}$$

Similarly, if the brother of deviation x were a "mother's offspring," his brethren would be made up of $\nu_2 - 1$ of standard deviation $\sigma_2 \sqrt{(1 - r_{22}^2)}$ and mean μ_{22}, and of ν_1 brethren of standard deviation $\sigma_1 \sqrt{(1 - r_{12}^2)}$ and mean μ_{21} where

$$\mu_{22} = (r_{22} - \rho)\,x + (m_2 - m)\,(1 - r_{22})$$

$$\mu_{21} = \left(r_{12}\,\frac{\sigma_1}{\sigma_2} - \rho\right) x + (m_1 - m)\,(1 - r_{12}).$$

Hence, generally for the variability of the brethren of a given brother of deviation x, we have

$$\begin{aligned}\Sigma_x{}^2 = {} & \sigma_1{}^2 \left\{ \frac{\nu_1 - 1}{2\,(\nu_1 + \nu_2 - 1)}\,(1 - r_{11}{}^2) + \frac{\nu_1}{2\,(\nu_1 + \nu_2 - 1)}\,(1 - r_{12}{}^2) \right\} \\ & + \sigma_2{}^2 \left\{ \frac{\nu_2 - 1}{2\,(\nu_1 + \nu_2 + 1)}\,(1 - r_{22}{}^2) + \frac{\nu_2}{2\,(\nu_1 + \nu_2 - 1)}\,(1 - r_{12}{}^2) \right\} \\ & + \mu_{11}{}^2\,\frac{\nu_1 - 1}{2\,(\nu_1 + \nu_2 - 1)} + \mu^2{}_{22}\,\frac{\nu_2 - 1}{2\,(\nu_1 + \nu_2 - 1)} + \mu_{12}{}^2\,\frac{\nu_2}{2\,(\nu_1 + \nu_2 - 1)} \\ & + \mu_{21}{}^2\,\frac{\nu_1}{2\,(\nu_1 + \nu_2 - 1)} \quad \ldots\ldots \quad \text{(xix).}\end{aligned}$$

Clearly this gives a hyperbola for Σ_x in terms of x, with its real axis perpendicular to x.

Similarly we have :

$$\begin{aligned}\rho\sigma^2 = {} & \frac{\nu_1\,(\nu_1 - 1)}{(\nu_1 + \nu_2)\,(\nu_1 + \nu_2 - 1)}\,\{r_{11}\sigma_1{}^2 + (m_1 - m)^2\} \\ & + \frac{\nu_2\,(\nu_2 - 1)}{(\nu_1 + \nu_2)\,(\nu_1 + \nu_2 - 1)}\,\{r_{22}\sigma_2{}^2 + (m_2 - m)^2\} \\ & + \frac{2\nu_1\nu_2}{(\nu_1 + \nu_2)\,(\nu_1 + \nu_2 - 1)}\,\{r_{12}\sigma_1\sigma_2 + (m_1 - m)\,(m_2 - m)\} \quad \ldots\ldots \quad \text{(xx),}\end{aligned}$$

and further

$$\sigma^2 = \frac{\nu_1}{\nu_1 + \nu_2}\,\{\sigma_1{}^2 + (m_1 - m)^2\} + \frac{\nu_2}{\nu_1 + \nu_2}\,\{\sigma_2{}^2 + (m_2 - m)^2\},$$

and

$$m = \frac{\nu_1 m_1 + \nu_2 m_2}{m_1 + m_2}.$$

In any actual case we may reasonably suppose our pairs of brothers to be a random sample from families with indefinitely great numbers,* or put ν_1 and ν_2 infinite in the ratio of n_1 to n_2. Hence :

$$m = (m_1 n_1 + m_2 n_2)/n\,, \qquad \sigma^2 = (n_1\sigma_1{}^2 + n_2\sigma_2{}^2)/n + \frac{n_1 n_2}{n_2}(m_1 - m_2)^2\,,$$

$$\rho\sigma^2 = \frac{n_1{}^2}{n^2}\,r_{11}\sigma_1{}^2 + \frac{n_2{}^2}{n^2}\,r_{22}\sigma_2{}^2 + \frac{2n_1 n_2}{n^2}\,r_{12}\sigma_1\sigma_2 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(xxi),}$$

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* 'Phil. Trans.,' A, vol. 203, p. 77.

$$\Sigma_x^2 = \sigma_1^2 \left\{ \tfrac{1}{2}\frac{n_1}{n}(1 - r_{11}^2) + \frac{n_1}{2n}(1 - r_{12}^2) \right\}$$

$$+ \sigma_2^2 \left\{ \tfrac{1}{2}\frac{n_2}{n}(1 - r_{22}^2) + \frac{n_2}{2n}(1 - r_{12}^2) \right\}$$

$$+ \frac{n_1}{2n}(\mu_{11}^2 + \mu_{21}^2) + \frac{n_2}{2n}(\mu_{22}^2 + \mu_{12}^2) \quad \text{............} \quad \text{(xxii).}$$

To simplify still further, assume no distinction in the totals of "mothers' offspring" and "fathers' offspring," or take $m_1 = m_2 = m$, $\sigma_1 = \sigma_2 = \sigma$, $n_1 = n_2 = \frac{1}{2}n$. Hence

$$\rho = \tfrac{1}{4}(r_{11} + r_{22}) + \tfrac{1}{2}r_{12},$$

$$\Sigma_x^2 = \sigma^2 \{1 - \tfrac{1}{4}(r_{11}^2 + r_{22}^2) - \tfrac{1}{2}(1 - r_{12}^2)\}$$

$$+ \tfrac{1}{4}\{(r_{11} - \rho)^2 + (r_{22} - \rho)^2 + 2(r_{12} - \rho)^2\} x^2.$$

Hence we see that if the variability of the arrays of brethren is to be constant, it is absolutely necessary that $r_{11} = r_{22} = r_{12} = \rho$, or the degree of likeness between brothers whether they belong to "mothers' offspring" or "fathers' offspring" must be identical. If this be not true, the variability of the array of brethren of a brother of given character must obey a hyperbolic law, being least for a brother of mean character.

If we adopt Dr. Boas's theory of complete alternate inheritance we have

$$r_{11} = r_{22} \quad \text{and} \quad r_{12} = 0.$$

Whence

$$r_{11} = 2\rho,$$

and

$$\Sigma_x^2 = \sigma^2(1 - 2\rho^2) + \rho^2 x^2 \quad \text{..................} \quad \text{(xxiii).}$$

This is a very easy result to test. On the whole, however, it is better to ask the general question: Are the variability of arrays of brethren hyperbolically distributed?

(11.) I propose to answer this by appeal to my data for nearly 2000 pairs of brothers measured for their cephalic index. Selecting the arrays of 20 brothers and upwards the results plotted in Diagram II. were reached. Here $\rho = 0{\cdot}4861$ and $\sigma = 3{\cdot}314$ and the mean cephalic index $= 78{\cdot}92$. Hence the "Law of Ancestral Heredity" gives for the mean value of Σ_x, $\Sigma = 2{\cdot}896$. On Dr. Boas's hypothesis

$$\Sigma_x^2 = 5{\cdot}7924 + 0{\cdot}2363x^2.$$

These results are shown in the same manner as in the first diagram, the broken lines marking the limits of twice the probable error of Σ and the actually observed Σ_x.

We may, I think, safely conclude from this result that the horizontal or mean line of the "Ancestral Law" gives a better result than the hyperbola. As a matter of fact in the small arrays at the extremes we get as before very erratic results; almost any value of the standard deviation may be reached, not only because the probable errors are so large, but because the appearance of any single abnormality or of any slip due to measurement or classification becomes so very disturbing in these small arrays. Even then we only find four cases in seventeen which deviate by more than twice the probable error from the mean line. In this case the mean error of the "Ancestral Law" for the seventeen arrays is 0·31, and of Dr. Boas's hyperbola 0·49.*

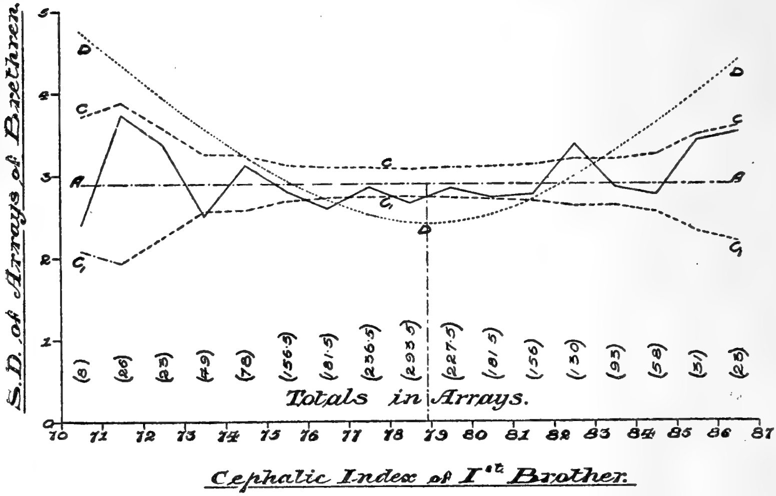

Diagram II.

(12.) It may be asked how Dr. Boas has reached the conclusion that in the case of cephalic index the "Ancestral Law" does not apply and that the children break up into "mothers' offspring" and "fathers' offspring"? He has used the measurements of Dr. Maurice Fishberg of New York, on forty-eight families of "East European Jews." Now, I think it may be reasonably questioned whether a population defined as "East European Jews" can be considered as homogeneous. I mean by this: Would any such category breed true to itself? Is it to be looked upon as a "race" in the sense used by me† of a popula-

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* If the large values often found for the variability of the extreme arrays of a correlation table have real significance, which I doubt, I personally am inclined to think it is due to deviation from true linear regression, the regression curve being a cubic curve with its inflexional portion representing the regression line of the ordinary range.

† 'Biometrika,' vol. 2, p. 511, and compare with the stable Mendelian population discussed in my paper, 'Phil. Trans.,' A, vol. 203, pp. 58—60.

tion which has been isolated and intra-bred for some generations? If not, it would very possibly exhibit "segregation" in the second generation; thus if there were any Aryan or other non-Jewish blood in the immediate ancestry, this might be induced by a segregation tendency increasing the variability in the offspring of such pairs. Further apparent segregation would arise in the case of any unfaithfulness, and this might be correlated with physical dissimilarity. Shortly, we want a good deal further information as to the nature of these particular American East European Jews. But quite apart from all this, the forty-eight families had only 158 children or an average of less than 3·3. Now, some families must have had four and five children and Dr. Boas gives the standard deviation of all the separate families, or he must have found a standard deviation here and there on the basis of at least two and possibly even fewer individuals![*] I do not think standard deviations so found can be of any value at all. Further, and most important, he has not determined the standard deviation about the mean of the individual family, which he ought to have done, but about the theoretical average mean of all families having a father and mother of the given characters. Now the *individual* family has all its ancestors in common beside the father and mother and hence its mean is *not* in our previous notation:

$$m + \mathrm{R}_{fc}\frac{\sigma_c}{\sigma_f}x + \mathrm{R}_{mc}\frac{\sigma_c}{\sigma_m}y,$$

the value used by Dr. Boas, but the much longer expression given by the general formula for "Ancestral Heredity."[†] It is only when we take the average results for *all* families of parents x and y, that the other ancestral terms will disappear, and we reach the above result His only method of reaching an approximation to the mean of an *individual* family, would be to actually find it from the family itself, and then investigate the standard deviation from this mean.[‡] This he has not done, and I think we must doubt, even if he had done it, the validity of means and variabilities based upon two to four individuals. Still his memoir is very suggestive, and it seems to me that the investigation of a large series of head measurements in parents, and, if possible, adult children, say, in 1000 families forming a homogeneous population, would be of great value.

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[*] I have shown in 'Biometrika,' vol. 1, p. 399, that the values of the standard deviation found from two individuals will, on the average, only be about 0·563 of its true value. Similar large reductions occur if it be found from three to four individuals, and so on. This is quite apart from the large probable errors introduced by paucity of numbers.

[†] 'Roy. Soc. Proc.,' vol. 62, p. 394.

[‡] There would, I take it, certainly be correlation between $x-y$ and the omitted ancestral terms in the family mean.

(13.) The present paper has, I think, brought out the following points :—

(i) A variability criterion between contending theories of inheritance is possible, and is easily applied.

(ii) Variability plotted to parental character will give (*a*) a horizontal straight line on the Theory of Ancestral Heredity, (*b*) a parabola with axis along the axis of parental character on Mendelian Principles, (*c*) a hyperbola with its real axis perpendicular to the axis of parental character on the Theory of Alternate Inheritance which is summed up in the idea of parentally differentiated groups within the family.

(iii) As far as my own measurements on stature, span, forearm, and cephalic index in man go, there is nothing to support the view that the variation curve of (ii) is effectively represented by either a parabola or a hyperbola. Within the limits of the probable errors of random sampling it appears to be a horizontal straight line.

Further applications of the criterion will no doubt be soon forthcoming, but it is essential they should be made with a full understanding of what the various theories amount to, and how they must be applied to observations. In particular it is very needful that we should distinguish between the mean of an individual family as determined from all its ancestry, and the mean of an array of offspring of given parentage-type—but with all varieties of earlier ancestry—determined from that parentage only.

"Some Uses of Cylindrical Lens-Systems, including Rotation of Images."

By George J. Burch, M.A., D.Sc. Oxon., F.R.S., Lecturer in Physics, University College, Reading. Received February 29,—Read March 10, 1904.

It was, I believe, first shown by the late Sir George Stokes that if two similar cylindrical lenses are placed in contact, with their axes of curvature at right angles, the combination acts as a spherical lens, and that if the axes are not at right angles the system is equivalent to a cylindrical and a spherical lens combined, the spherical element disappearing when the axes are parallel.

I have recently had occasion to employ a modification of this arrangement which has proved of some practical utility as well as theoretical interest.

When, as in Sir George Stokes's experiment, the two lenses are placed in contact, the combination acts as a spherical lens in every respect, that is to say, the formula $\frac{1}{u}+\frac{1}{v}=\frac{1}{f}$ holds in regard to it.

But when the two lenses are not in contact there is only one pair of conjugate points at which a real object will give a real image. Reference to the ordinary focometer formula will show that this must be the case. Let l be the distance between an object and a screen, and let a convex lens of focal length $f < \frac{1}{4}l$ be moved along the line joining them. Then there must be two, and only two, positions of the lens at which it will form images on the screen, one at a distance u from the object and v from the screen, and the other at a distance v from the object and u from the screen where

$$u+v = l, \quad u-v = a, \quad \text{and} \quad \frac{l^2-a^2}{4l} = \quad .$$

In either of these two positions a cylindrical lens of equal power will, if its axis be vertical, form sharp images of vertical lines, or of horizontal lines if its axis be horizontal. If therefore we place one cylindrical lens with its axis vertical at a distance u from the object and a second of equal power with its axis of curvature horizontal at a distance u from the screen, there will be produced on the screen a sharp image magnified vertically v/u times, and horizontally u/v times.

This method may be employed for comparing by photography curves plotted to different scales, or for increasing or diminishing the ordinates of a curve or record the scale of which is unsuitable. Figs. 1 and 2 are an example of this latter use.

Fig. 1 is a record taken with the capillary electrometer of the discharge of the electrical organ of Malapterurus, the two sides of the organ being connected with the outer coatings of a pair of condensers, the inner coatings of which were connected with the electrometer.

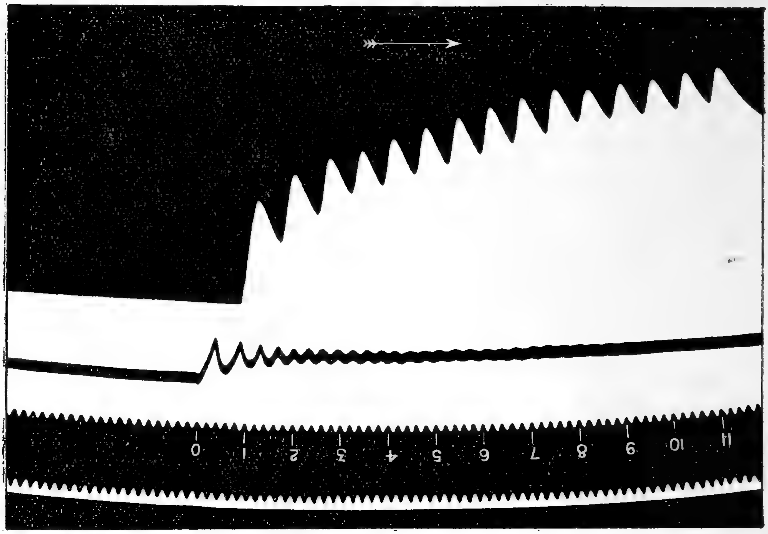

Fig. 1.

A full description of the experiment is given in the papers by Gotch and myself, from which this illustration is taken.*

In order to find the E.M.F. indicated by such a record it is necessary to measure the subtangent—or, in this case, the polar subnormal to the curve at various points according to the method described in my papers on the "Capillary Electrometer."† The method is, however, inapplicable if the angles are too steep.

It occurred to me that if the ordinates could be diminished and the abscissæ magnified by photography the angles might be brought within the measurable range. Accordingly I fixed the original of fig. 1 at 56·25 cm. from a photographic plate, with a cylindrical lens of +12·5 cm. with its axis horizontal at a distance of 17·5 cm., and a second with its axis vertical at a distance of 43·75 cm. The result is shown in fig. 2. The definition is remarkably good in the negative,

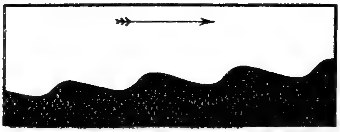

Fig. 2.

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* 'Phil. Trans.,' B, vol. 187, p. 347; 'Proc. Roy. Soc.,' vol. 65, p. 434.

† 'Phil. Trans.,' A, vol. 183, p. 81; 'Proc. Roy. Soc.,' vols. 48, 59, 60, p. 388, and vol. 70, p. 175.

but the field of view is small. The abscissæ are magnified two and a half times and the ordinates diminished in the same proportion, so that the total effect is the same as if the original photograph had been taken on a plate moving 25/4 times as fast, and the resulting curve can be easily analysed.

It should, however, be noted that fig. 1, being a curve with polar co-ordinates, is not properly adapted for this process, which requires, to be accurate, curves with rectangular co-ordinates. It serves, nevertheless, to illustrate the method, and as the portion photographed occupies not more than an angle of 2° of arc, the error involved is hardly noticeable although great enough to render it not worth while to spend time over a minute analysis of the modified curve.

It is not necessary that both lenses should be of the same focal length. If they are, the image is magnified as much in one direction as it is diminished in the other. If it is desired to preserve the dimensions in one direction unaltered—as, for instance, in comparing the time relations of two electrical responses of different intensity—*e.g.*, nerve under the influence of CO_2, and in a normal condition—one lens may be put midway between the object and the screen.

If m be the magnification required in the other direction, the focal length necessary for the other cylindrical lens is $f = \frac{m}{1+m} . l$ and its distance from the object is $\frac{l}{1+m}$.

In practice it is more convenient to employ a pair of lenses of rather more than the required power when close together, and to adjust the distance between them until the right magnification is obtained.

The image thus modified by the cylindrical system is projected on to the curve with which the comparison is to be made, and examined with a lens.

The principal difficulty is the smallness of the field of view, which is elliptical in shape, the major axis being limited by the diameter of the lenses and the ratio between the axes being as $1 : m^2$. Unfortunately the use of lenses of larger aperture introduces errors of chromatic and spherical aberration which spoil the definition.

Owing to the fact that each cylindrical lens has to be separately focussed, and that no image is formed until both are correctly adjusted, not only as to distance but also as to the direction of the axes of curvature, focussing is a matter of some difficulty. I have found the following method answer very well :—

Two lines, one vertical and the other horizontal, are ruled, across the middle of a glass plate—a spare negative answers very well, and the lines can be ruled with a needle on the film. This is placed in the object holder. The first cylindrical lens is inserted in its cell and the horizontal line focussed with it sharply on the screen. The first lens being removed, the second is inserted in the other cell and adjusted to focus the vertical line.

The first lens is then replaced in position and rotated till both lines are sharply focussed. When three lenses are used, the two employed to obtain magnification must be contained in a sliding tube furnished with a pin working in a slot to prevent rotation.

In the preceding pages the axes of curvature of the two components of the cylindrical lens-systems are, in all cases, at right angles to each other. Equally interesting properties are possessed by those systems in which they are parallel to each other.

CASE I.—*Two equal convex cylindrical lenses set with their axes of curvature parallel, and at such a distance apart that their principal foci coincide as in a telescope.*

A clear and undistorted view of distant objects is obtained, but on rotating the object the image appears to rotate with equal angular velocity in the opposite direction, and on rotating the tube containing the lenses, the image appears to rotate with twice the angular velocity in the same direction.

The reason is obvious. Suppose the axes of curvature are vertical, then there is no vertical deviation of the image-forming rays, and so far as its vertical components are concerned the virtual image coincides with the real object. In the horizontal plane there is deviation. The rays from the cylindrical objective cross as in a telescope before reaching the cylindrical eyepiece. If, therefore, we focus the eyepiece so that the final virtual image is at the same distance from the eye as the real object, a clearly defined image will be produced, erect, but enantiomorphic, as if reflected in a plane mirror. If the object is viewed through two cylindrical telescopes in tandem, then if either is rotated, the image rotates in the opposite direction, but if both are rotated together, the image remains stationary as in the case of two erecting prisms in tandem.

Two causes may disturb the sharpness of the definition, namely, deviation from exact parallelism of the axis, and error in adjusting the distances between the lenses, so that the horizontal components of the virtual image are not focussed at the same distance as the vertical.

If the two lenses are not of equal focal length the resulting images are clearly defined, but not symmetrical, being magnified or diminished in one direction and of natural size in the other.

CASE II.—*Two cylindrical lenses with their axes of curvature parallel, the distance between them being greater than the sum of their focal lengths.*

Such a system acts as a compound microscope, giving well defined images of objects situated at a *certain distance* from the objective.

The same phenomena of rotation of the image on rotating the lens system are observable in this case as in the cylindrical telescope, but with this difference: the images, though sharply defined, are not symmetrical, the object being magnified at right angles to the axis of curvature, and not magnified parallel to it.

The focal distance at which alone good definition is obtainable is that at which the virtual image is the same distance from the eye as the object. But to an astigmatic eye there are two distances at which the instrument gives perfect definition, the direction of the axis of curvature coinciding with the astigmatic axis of the eye in one case and being at right angles to it in the other.

The cylindrical telescope cannot by altering the focussing be made to project an image on a screen like an ordinary telescope. To do that we must employ two cylindrical telescopes with their axes of curvature at right angles. They need not both be of the same power, and, curiously enough, one may be situated inside the other without interfering with its action.

Polarised Light.

Another peculiarity of the cylindrical telescope is rather remarkable and extremely valuable. As would be expected, rotation of the tube of a cylindrical telescope, though it rotates the image, is absolutely without effect as regards the polarisation of the ray. There is, so far as I know, no other means of rotating an image without altering it in this respect, the ordinary erecting prism, whether silvered or unsilvered, introducing a difference of phase. I have found the cylindrical telescope extremely useful in some experiments where this was important.

Note added March 12.

If the axis of a telescope of which the magnifying power is m is moved sideways through an angle θ, the displacement of the image is $(1 \pm m)\theta$, taking the upper sign if the image is inverted, and the lower sign if it is erect.

In the telescope formed of two equal cylindrical lenses, parallel to the axis of curvature $m = -1$, and at right angles to it $m = +1$. Accordingly, if the axis of such a telescope is moved through an angle θ in the plane of the axis of curvature, the displacement of the image is zero. If it is moved in a plane at right angles to this the displacement is 2θ. And if the plane of movement makes an angle of 45° with the axis of curvature, the image appears to move at right angles to it. The effect of rotating the telescope about its axis while moving it in azimuth is very striking.

A cylindrical telescope may be made with one lens as follows:—A plane mirror is set at right angles to the optic axis in the principal focus of a cylindrical lens. Between the lens and the eye a plate of unsilvered glass is placed at such an angle as to reflect the rays from some distant object through the lens on to the mirror, whence they are reflected once more through the lens into the eye. The unsilvered reflection is not necessary if the eye is held at some considerable distance from the lens. In such an arrangement the image is not enantiomorphic, the effect of the cylindrical lens-system being neutralised by that of the mirror, but the image rotates when the lens is rotated exactly as in an ordinary cylindrical telescope, and the plane of polarisation is not affected by the rotation.

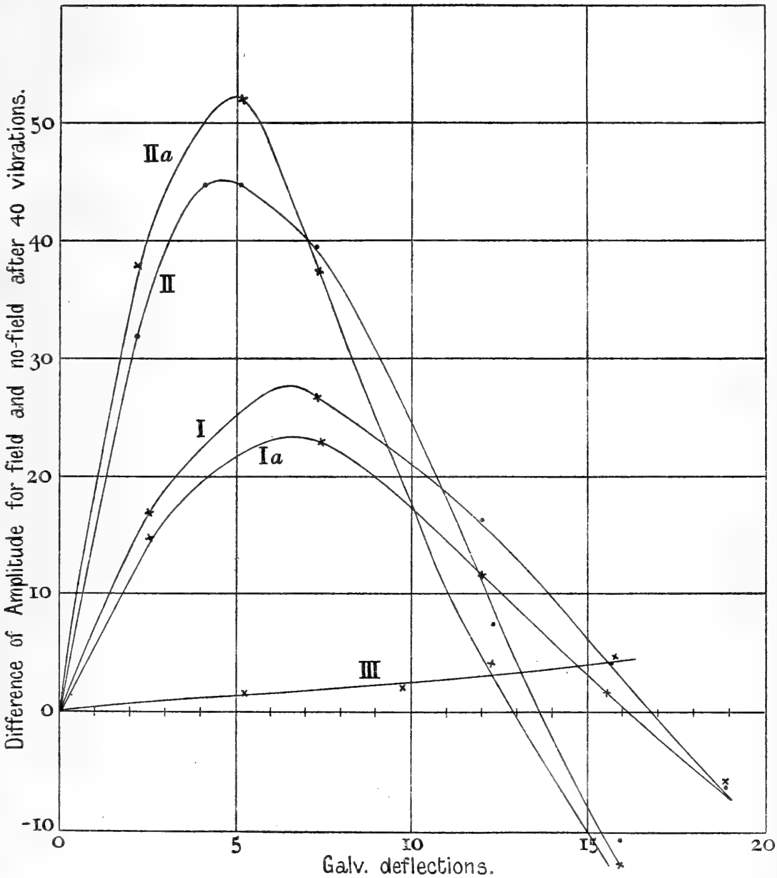
"On the Effect of a Magnetic Field on the Rate of Subsidence of Torsional Oscillations in Wires of Nickel and Iron, and the Changes Produced by Drawing and Annealing." By Professor ANDREW GRAY, F.R.S., and ALEXANDER WOOD, B.Sc. Received March 12,—Read March 17, 1904.

On May 15th, 1902, we communicated to the Royal Society a paper entitled "On the Effect of a Longitudinal Magnetic Field on the Internal Viscosity of Nickel and Iron, as shown by Change of the Rate of Subsidence of Torsional Oscillations." We described in that paper the results of experiments on the rates of subsidence of torsional oscillations in nickel and iron wires in fields of different strengths, and showed that the effect of the field, or, more properly, of the magnetisation of the wire, is to increase the rate of subsidence in nickel and to diminish it in iron. In nickel, it was pointed out, this effect rose to a maximum at a certain field, from 100—180 C.G.S., according to the initial amplitude, and thereafter diminished as the field was increased; while in iron the effect was in the main all produced at a field of about 160 C.G.S. or rather less, and increased only slightly with further increase of field intensity.

The experiments described in the present paper are referred to at the end of the former one as in progress, and some account of their results is given; and we propose now to describe them a little more in detail.

Experiments on the Effect of Drawing Down and of Annealing a Nickel Wire.—A piece of the nickel wire formerly experimented on was tested for subsidence in the manner already described, and then drawn down, by being passed through a draw plate, from the diameter 1·4 mm. to 0·775 mm. The results are illustrated by fig. 1. Take first curve I of that figure. It has for ordinates the differences between the amplitudes at a given stage in the course of the subsidence with various fields, and the amplitudes at the same stage in the subsidence with zero field. The stage chosen was in each case the instant after the completion of the fortieth vibration after the attainment of the initial amplitude indicated on the curve. The abscissæ are galvanometer deflections and are proportional to the fields: each unit of deflection means a field of 15·54 C.G.S. units.

FIG. 1.



Curve I shows the effects of the various fields on the wire as it was received from the makers. The wire was then hardened by being drawn down as described; and it was then found that no effect of magnetisation on subsidence was perceptible.

The nickel wire was next annealed by heating it to a bright red heat and then plunging it into cold water; when it was found to be softer than it was originally. For the original wire, for example, the modulus of rigidity was 7·97 × 10 C.G.S., for the wire as drawn down it was 8·06 × 10 C.G.S., and for the annealed wire it was 7·81 × 10 C.G.S. Curve II is the curve for the annealed wire corresponding to curve I, that is, showing the differences after forty oscillations of the amplitudes of oscillation of the wire under the various fields and the amplitude with zero field. The latter amplitude it is to be remembered was the greater.

After annealing and re-drawing had been performed in succession, the wire gave curve III, and its rigidity modulus was 8·318 × 10 C.G.S.

These three curves make it clear that the effect of the magnetic field on the internal viscosity of a nickel wire depends very largely on the hardness of the metal, as, of course, we should expect.

The progressive modification of the groups of molecular magnets by the magnetic field is opposed by the greater resistance introduced by the hardening, and the changes due to the field are not produced. In the annealed condition the wire has its groups much more at liberty to take up a new arrangement, and this is shown also, of course, by the smaller magnetic susceptibility in the hardened condition.

Curves I_a, II_a, give the results as in I and II, but after twenty oscillations. It will be observed that while II_a is above II up to and beyond the field of maximum difference of amplitude, and is then below II, curve I_a is below II throughout its entire course.

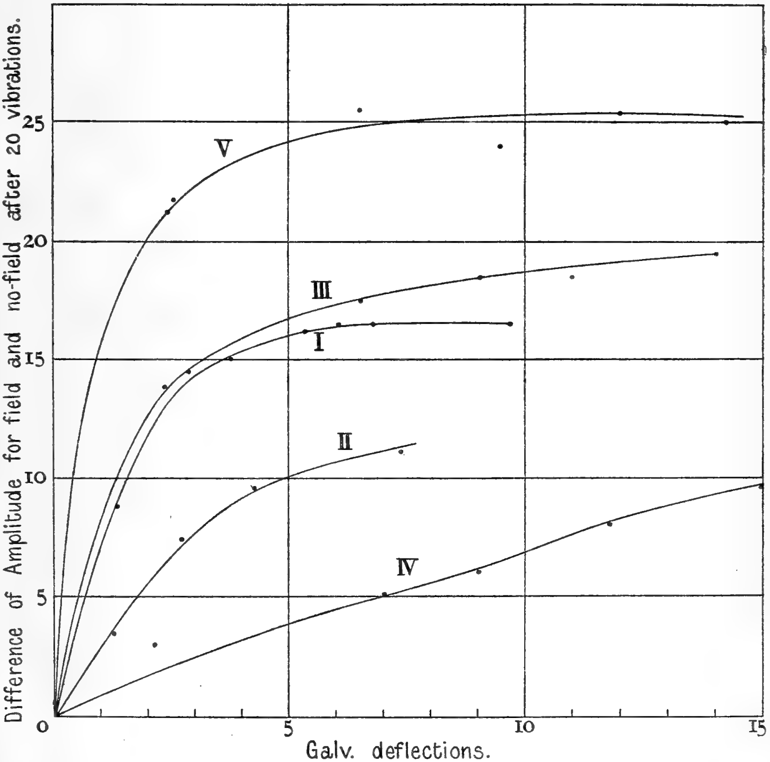
Effect of Drawing Down on the Iron Wire.—This is shown in fig. 2, in which the ordinates and abscissæ represent the same quantities as do the ordinates and abscissæ of the curves in fig. 1, after twenty oscillations in each case. The amplitude for iron, it is to be remembered, is greater with field than with no field. The steep rise of curve I, and the horizontality of the remaining part, show the fact noted in the former paper, that the effect of the magnetisation in iron is attained with low fields, and that higher then produce little further effect.

After the results shown in curve I had been obtained the wire was drawn through two holes of the draw plate, and after this treatment gave curve II. This curve is lower and rises less sharply than I, showing that the internal viscosity of the wire was not so much affected by the magnetic field as previously.

The drawn wire was then annealed, and gave curve III, which does not differ much from I. Further drawing down resulted in curve IV, which is practically a straight line, that is, the difference is now nearly proportional to the field. Re-annealing of the now much thinner wire gave curve V, which shows a very distinctly greater effect of magnetisation on viscosity than ever before. The repeated drawing and annealing process thus seems to result in the finally annealed wire in a collocation of molecules much more easily affected by a magnetic field.

Effect of Permanent and Temporary Strain in Nickel Wire.—This is illustrated on fig. 3, which is constructed on the same principle as figs. 1 and 2. Curve I describes the behaviour of the wire in its original state. After this curve had been obtained for the different fields, a weight (that of a large vibrator) of 11,202 grammes was hung on the wire and left for two days. At the end of that time curve II was obtained, showing that the permanent strain had increased the differences of amplitude remaining after the forty oscillations. The weight was left on for another 2 days, and curve III was observed. Two repetitions of the process gave curves IV and V, which practically coincide with III, and are not drawn.

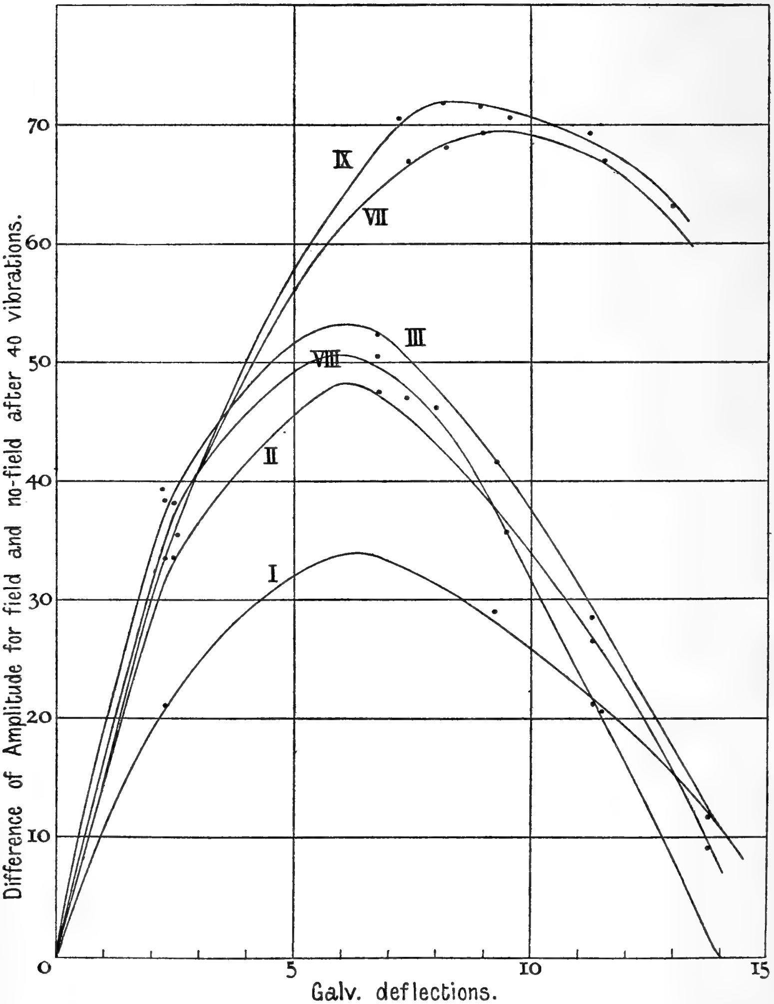
Fig. 2.



It is remarkable that permanent strain should have in nickel practically the same effect as annealing in increasing the effect of field on the rate of subsidence, which effect it is to be remembered is in nickel an increase.*

The wire was now allowed to rest for a few days without any weight

Fig. 3.



upon it; but it was found that the curve then obtained coincided with curves III, IV, and V, so that the rest seemed to produce no effect.

The heavy vibrator was now hung on instead of the light one, which had been used in all the previous experiments with both wires.

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* See the former paper, *loc. cit.*

Curve VII was obtained. The light vibrator and the heavy one were then used again in succession and gave curves VIII and IX. Probably the difference between curves VIII and IX was due to the additional longitudinal strain (in this case temporary) given to the wire by the additional load. The subsidences were taken for all the curves after forty vibrations no matter which vibrator was used.

The difference between curves VII and IX, it will be noticed, is a twofold one; a much greater maximum of effect and a shifting of the maximum to a greater field.

It seems not impossible that the main differences between nickel and iron disclosed by the experiments described in the previous paper, and the effects now discussed, may be explained by supposing that the groups of magnetic molecules in nickel lie in layers across the wire, separated by a matrix of conducting material which becomes hardened by drawing, and prevents the progressive changes in the groups from the initial condition of closed chains, which is brought about by magnetic force. On the other hand, the supposition would be that in iron the molecular magnets are in longitudinal groups with non-magnetic material between. Thus, in the vibrating wire in the first case, the conducting material moving in the field of the molecules would give rise to dissipation of energy, and there would be a greater rate of subsidence in the field than without it.

In the other case it is conceivable that the changes of the longitudinal and initially more or less nearly closed chains might result in such a modification of field as to result in less dissipation of energy by induction currents due to relative motion of the conducting substance and the molecular magnets, for the conducting matter between the longitudinal rows of elementary magnets may move in a feebler field after the magnetisation than before, owing to the breaking-up of the closed chains.

The effect of permanent and temporary longitudinal strain in nickel seems contrary to what we should have expected. It is known that longitudinal strain on nickel diminishes its longitudinal magnetisation, and judging from this we should have rather expected the contrary effect to that which we have observed. There is however, no doubt as to the result, which is borne out by many sets of observations. The cause must be matter for further consideration and experiment.

"A Radial Area-Scale." By R. W. K. Edwards, M.A. Communicated by Professor A. G. Greenhill, F.R.S. Received January 28,—Read March 3, 1904.

This is a contrivance for finding the area of a plane figure by means of a transparency. The design in the transparency consists of a number of radiating lines. Each of these lines is graduated.

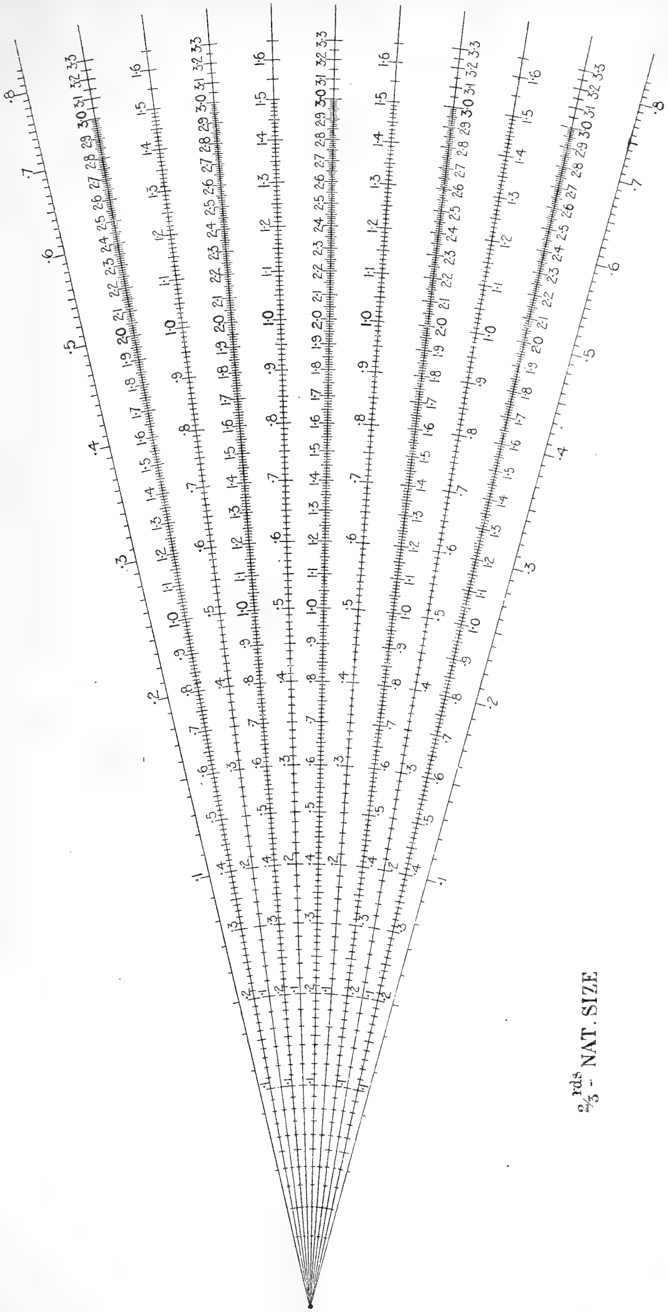
There are various patterns of this design, and their respective claims to convenience and accuracy form a wide field for discussion. In the accompanying transparency (reproduced in the figure), which is fairly simple and effective, there are eleven straight lines radiating from a point at equal angles. The way in which the transparency is used is as follows :—

The figure whose area is to be found is placed under the transparency, in close contact with it, so that its contour lies *just* between the two outside lines of the transparency, *i.e.*, so that each outside line touches the contour, or passes through a cusp or angular point, or contains some rectilineal portion of the contour. Each of the radiating lines thus becomes a tangent or transversal, or contains a side of the figure. The graduations of the right-hand points of intersection of these transversals are read and added together; then the graduations of the left-hand points of intersection are read and added together. The second sum is subtracted from the first; and the result records the number of square inches in the figure.

It will be seen that if each of the outside lines touches, or passes through an angular point of the figure, there will be eighteen graduations to be read—those on the outside lines cancelling each other. If one of the outside lines contains a rectilineal portion of the contour, there will be twenty graduations to read; if both outside lines do so, twenty-two graduations must be recorded.

If a quicker use of the area-scale, with less chance of accuracy, is desired, the figure to be quadrated may be placed so as to lie just between the first and ninth, or the first and seventh, or the first and fifth lines, in which cases fourteen, or ten, or six graduations will be read respectively.

If the figure is too large to be included between the outside lines, it may be divided into two parts by a straight line drawn across it, or into three parts by a pair of straight lines inclined to one another at the same angle as the outside lines, which may be done by means of the cardboard slip accompanying the diagram. The second of these methods of dividing up the figure may be also employed when it is desired to obviate the inaccuracy that may result from the two outside lines otherwise being tangent to the figure.



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Principle of the Area-Scale.

Suppose the equation of a curve to be

$$r^2 = a + b\theta + c\theta^2 + d\theta^3,$$

the area between the curve, the initial line, and the radius vector making angle 2θ with the initial line, is $\frac{1}{2}\int_0^{2\theta} r^2\, d\theta$; *i.e.*,

$$a\theta + 2b\theta^2 + \tfrac{8}{3}c\theta^3 + 4d\theta^4, \text{ i e., S, say.}$$

Now if r_0, r_2 be the outside radii of the sector so quadrated, and r_1 be the radius vector bisecting the angle between them, it can be seen that

$$\begin{aligned} r_0^2 &= a, \\ r_1^2 &= a + b\theta + c\theta^2 + d\theta^3, \\ r_2^2 &= a + 2b\theta + 4c\theta^2 + 8d\theta^3; \end{aligned}$$

whence, assuming $(Ar_0^2 + Br_1^2 + Cr_2^2)\,\theta = S$, and solving three of the four simultaneous equations for A, B, C, we get values $A = \frac{1}{6}$, $B = \frac{2}{3}$, $C = \frac{1}{6}$, which also satisfy the fourth.

We have then

$$S = \tfrac{1}{6}\theta\,(r_0^2 + 4r_1^2 + r_2^2).$$

The design on this particular transparency is made on the assumption that each of the ten separate portions of the curve between the first and third, the third and fifth, . . . the ninth and eleventh radiating lines, approximates to some member of the above family of curves.

Anyone acquainted with the discussion of areas, in Cartesian co-ordinates, in terms of a series of equidistant ordinates, and their common distance apart, given in Bertrand's *Calcul Intégral,* Section 363, will see an analogy in the foregoing expression of the area of a sector in terms of the squares of three equally inclined radii and their common inclination. Other designs of the transparency can be made on the assumption of larger portions of the curve approximating to members of families of curves in which r^2 is equal to a rational algebraical function of θ in ascending powers of θ, of degree higher than the third. On the whole it is easier, and possibly more effective, to use the method employed above, in the same way as "Simpson's Rule" uses the corresponding theorem in Cartesian co-ordinates. There is no reason to suppose that "Simpson's Rule" gives a less accurate approximation in the generality of cases than "Weddle's Rule"* or the numerous rules deducible in terms of the co-efficients calculated by Cotes.

An investigation of the family of curves

$$r^2 = a + b\theta + c\theta^2 + d\theta^3$$

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* See Boole's 'Finite Differences.'

shows that they form a large variety of spirals. An important point to notice is that if, as usual, ϕ denotes the inclination of a radius to the tangent at its extremity, we have tan ϕ, *i.e.*, $rd\theta/dr$, equal to

$$\frac{2(a+b\theta+c\theta^2+d\theta^3)}{b+2c\theta+3d\theta^2},$$

which, in general, cannot be zero unless at the origin, when the curve passes through the origin. This means that error is likely to be perceptible when the curve quadrated is such that any tangent to it passes through the point of radiation; *e.g.*, when the outside lines are tangents to the curve; or when the curve is re-entrant in such a way that any one of the radiating lines gives three or four readings. The design may be expected to give the best results, therefore, when arranged so that the outside lines past through cusps or angular points of the curve, and so that no radiating line crosses it more than twice. For curves in which there are no sharp points, it is best, therefore, to divide into two or three areas.

There is no reason why the presence of points of contrary flexure should be supposed to vitiate the results. For the equation for θ giving the positions at which such points occur in the stated family of curves, will be found to be, in general, of the sixth degree.

Calculable deviations from strict accuracy may be expected in the case of nearly all the well-known regular curves quadrated by means of the area-scale. If, as in the present pattern, the angle between the outside lines is half a radian, these deviations will be found to be insignificant, except in the cases of oval curves touching the outside lines and not treated as suggested on p. 292.

"On the Compressibility of Solids." By J. Y. Buchanan, F.R.S.

Received February 11,—Read February 25, 1904.

The solids dealt with in this research are the metals platinum, gold, copper, aluminium, and magnesium. Their absolute linear compressibilities were directly determined at pressures of from 200—300 atmospheres at temperatures between 7° and 11° C. The determinations were made by the same method, and with the same instrument which I used for the determination of the compressibility of glass in 1880.* As nearly a quarter of a century has passed since then it will be expedient to recall the principal features of the instrument, and of the method.

The idea of it occurred to me on the evening of March 23, 1875, the day on which the "Challenger" made her deepest sounding, namely, 4475 fathoms (8055 metres), and I was able to put it in practice 6 days later, on March 29, when, however, the depth was only 2450 fathoms (4410 metres). The observations which I was making during the voyage on the compressibility of water, sea-water, and mercury, were of little value without a knowledge of the compressibility of the envelope which contained them. It was a matter to which I had given much thought. I had studied all the methods which had been used up to that date, but they had all turned out to be faulty.

The idea of utilising the linear compressibility of glass in order to arrive at its cubic compressibility had occurred to me, as it had, no doubt, occurred to many others, before. The difficulty lay in giving the idea experimental expression. It was clear that the instrument would fall to be classed as a piezometer, and would have to be a self-registering one, because what takes place in the depths of the sea is removed from observation. All my piezometers contained a liquid, and this I had recognised to be fatal to absolute measurements. The problem had, therefore, come to be: to design a piezometer which should contain no liquid; and it was the solution of this problem which occurred to me on the evening of March 23, 1875.

The form which the instrument took was very simple. In my laboratory outfit I had included some lengths of tubing suitable for the stems of piezometers, of which I had to make a number during the voyage. In order to be able to use the indices of broken deep-sea thermometers, the tubes had the same internal diameter as the stems of these instruments, about 1 mm. On the outside of the tubes a scale of millimetres was etched. I took the greatest available length of this tube, namely 60 cms. I then drew out a wire of the same glass and passed it into the tube until it appeared at the other end of

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* 'Roy. Soc. Edin. Trans.,' vol. 39, p. 589.

the tube. This end of the tube was then sealed up, and the end of the glass wire was fused into it, so that, while free throughout its whole length, longitudinal motion was prevented. The length of the glass wire was 57 cm., so that there was an empty space in the tube of 3 cm. above it. The magnetic index of a broken deep-sea thermometer was re-haired and passed into the tube above the glass wire. The open end of the tube was then sealed up. The result was a piezometer consisting of nothing but glass. In principle it was precisely the same as any of the other piezometers. The indices of these give the difference between the compression produced by the pressure on the contents and on the envelope. In the case of the other piezometers, which contained liquids, the balance was on the side of the contents. In the all-glass piezometers the contents, besides being of the same material as the envelope, were completely protected from pressure, and the whole of the change of length measured fell to the envelope. It has, therefore, a feature which is possessed by no other instrument; with it the absolute compressibility of a solid is determined by one measurement.

Before the instrument was attached to the sounding line, the index was brought down by means of a magnet to rest on the end of the internal glass wire, exactly in the same way as if it had been the mercury column in a *maximum* and *minimum* thermometer. The instrument was then sent to the bottom, or to whatever depth might be decided on.

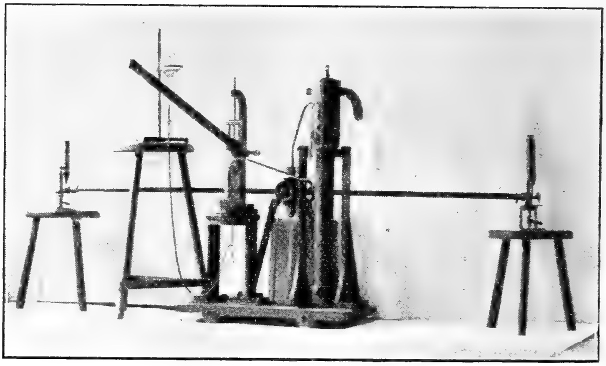
During the descent the temperature of the glass, both inside and outside, fell with that of the water through which it passed, but as the contraction produced was the same on the wire and on the tube, there was no differential effect to be recorded by the index. On the other hand, the increasing pressure, as the instrument descended, affected only the outside tube, which it shortened. In contracting, it was obliged to pass the index, which was kept in its place by the internal wire. When the instrument was being hove up, the reverse process took place; the tube lengthened, and lifted the index clear of the internal wire by an amount equal to the lengthening of that portion of the tube. As the whole clearance produced by the expansion from the greatest depth did not exceed 1 mm., its amount had to be estimated by the eye with the assistance of a magnifying glass.

The experiment made on March 29, 1875, was quite successful, and it gave 3·74 as the cubic compressibility per million per atmosphere, of the glass of which the tube was made. The exact figure found in 1880 for glass from the same source was 2·92. A number of observations were made with the instrument, both on the sounding line and in the compression apparatus with which the ship was supplied, and figures from 3—5 per million were found. These were sufficient to give the order of the constant which was sought, but it was impossible with the appliances at hand to measure such small distances with sufficient accuracy to enable a definite value to be determined.

On the return of the ship I embodied the principle in an instrument of precision, which I had constructed in the early part of 1880, and I used it in the month of June of that year for the exact determination of the compressibility of the glass which had been used in the construction of my "Challenger" piezometers.

It is this instrument, and without any alteration, which I have used for the purpose of the present research.

Fig. 1.



With the assistance of fig. 1, its features, and the distribution of its parts, will be apparent without any lengthy description. It consists of three parts: the force pump on the left, the receiver for the reception of piezometers or other bodies on the right, and behind these, the block with tubes projecting on either side to receive the rod or wire of the solid, the compressibility of which is to be determined. Every part of the instrument is made of steel. The part which most concerns the present research is the steel block, in the rear, with tubular prolongations. When the rod or wire to be experimented with has been introduced, the ends of the tubes are closed with thick glass tubes, which are kept in their places by open steel caps. Each of these tubes is commanded by a microscope with micrometer eye-piece. In 1880, when the instrument was housed in a room with a stone floor, these microscopes stood on three-legged stools, as shown in the figure. As the room with the stone floor was no longer available, I had to instal the instrument close to the windows of the laboratory, which has a wooden floor, and fix metal brackets in the wall to carry the microscopes. In both cases the micrometers, which measure the expansion or contraction of the body under examination, are independent of the instrument which holds it.

The *manometer*, which indicates the pressure in the instrument, is seen under the steel block which carries the tubes. It is simply a mercurial thermometer with a very thick bulb. The scale on it is an arbitrary one, and its value as a measure of pressure is fixed by observing its reading when the principal piezometer which I used during the voyage of the "Challenger" was in the receiver. This piezometer, known as C No. 1, contained distilled water, and from very many carefully executed experiments at depths from 800 fathoms (1440 metres) up to 2500 fathoms (4500 metres), made in the South Pacific where the oceanic conditions were most favourable, the apparent compression of distilled water in this particular instrument at the temperature ruling in these depths, which averages in round figures 2° C., and when exposed to measured columns of sea-water, of known quality as regards density, was accurately known. The indications of the manometer are, therefore, equivalent to those of piezometer C No. 1, the standardisation of which was effected under an open-air water column. The observations made with C No. 1 on board the "Challenger," which form the basis of the scale of pressures, are collected in Table I. They are expressed in terms of the apparent compressibility of distilled water deduced from them.

In the table the vertical lines represent apparent compressibility in volumes per million per atmosphere, rising by steps of 1 per million from 45—55, so that all the values of the compressibility falling between say 45 and 46, or 49 and 50 are arranged in one column. Above each entry of apparent compressibility will be found the depth in metres to which the instrument was sent, and the temperature (° C.) of the sea-water at that depth. The depth is expressed in metres because it so happens that the average density of the water in this part of the South Pacific, allowance being made for the vertical distribution of temperature, compression, and salinity, is such that a vertical column of it 10 metres high exercises very exactly the same pressure as 760 mm. of mercury. So that the depth in metres, divided by 10, gives the pressure in ordinary atmospheres. At great depths a very slight correction has to be made; the nature of this will be apparent from the following table, in which, for different depths in metres, D, the pressure P in atmospheres is given :—

D......	1400	2000	3000	4000	5000	6000
P......	139·96	200·14	300·82	401·98	503·62	605·75

Owing to the preponderance of water of low temperature and of very uniform salinity in a vertical column of water in any part of the open ocean, the pressure exercised by it per thousand metres does not differ appreciably from 100 atmospheres.

Inspection of Table I shows at once in which column the true value of the apparent compressibility is most likely to be found. It is the one which includes values between 49 and 50. Outside of this column it is only the adjacent one containing values between 48 and 49 which enters into competition. The mean of all the values in these two columns is 49·16, and this figure forms the basis of the measurement of pressure in this investigation, and it is used in interpreting the pressure-value of the readings of Piezometer C. No. 1, when being compared with those of the manometer used for the ordinary measurements of the pressure in the apparatus.

The change of apparent compressibility of water with change of temperature for the small range of temperatures with which we are concerned was found in 1880 to be at the rate of 0·33 per degree (Celsius), and this figure is used in the present research.

Micrometers.—The same microscopes and micrometers, which served in 1880, were again used in this research. Their value was determined by reference to a stage micrometer, ruled into hundredths and thousandths of an inch. This was then verified at the National Physical Laboratory. The changes of length measured by the micrometers are therefore given in terms of the standard inch; and, it may be added, the values attached to the readings of the micrometers in 1880 were exactly the same as those now found by reference to the standard of the National Physical Laboratory.

In the microscope which was always placed on the left hand, one division was equivalent, on the stage, to 0·0004219 inch. In the one on the right hand one division was equivalent to 0·0004167 inch on the stage.

As the contractions or expansions are given directly in terms of the inch, the total length of the rod is given in inches also. In order to bring the ends into a suitable position for observation with the microscopes the length of the rod or wire had to be not less than 75 or greater than 75·5 inches. The actual lengths were measured exactly in each case. The average was 75·32 inches (1·913 metres).

To facilitate the observation of the ends through the thick glass tube a piece of microscopic covering glass was moistened with a drop of water and laid horizontally on the tube, producing the same effect as if a flat surface had been ground and polished on it.

The effect observed and measured is the lengthening of the rod when the pressure is relieved. As the compressibility of solids is very small, the highest pressures have been used which were found to be compatible with the reasonable persistence of the glass terminals; the usual pressure was in the neighbourhood of 200 atmospheres. Very few of the glass terminals stood over 300 atmospheres. The pressures actually chosen were as nearly as possible those at which the manometer had been compared with the "Challenger" piezometer.

The body under observation is in the form either of a rod or a wire. If it is in the form of a rod then it is fitted with wire ends of sufficiently small calibre to enable them to enter the glass terminals.

Table I
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Table I.—The Apparent Compressibility of Water in Glass, as observed in Piezometer C. No. 1, on board H.M.S. "Challenger," in the South Pacific, in October, November, and December, 1875. The observations are arranged in order of apparent compressibility, which is expressed in volumes per million per atmosphere. Above each entry of the apparent compressibility is, on the left, the depth, in metres, of the water at the particular station, and, on the right, its temperature, in Celsius's degrees.

	Apparent compressibility per million per atmosphere.									
Apparent compressibility.	45–46	46–47	47–48	48–49	49–50	50–51	51–52	52–53	53–54	54
	2475 1°·9 45·0	1800 2°·6 46·8	1890 2°·8 47·1	4140 1°·6 48·7	1620 2°·4 49·6	1800 2°·4 50·6	2700 1°·9 51·0		1800 3°·1 53·4	2880 1°·8 55·5
		2385 2°·2 46·8	3195 1°·9 47·3	3330 1°·6 48·1	1620 2°·7 49·5	1800 2°·4 50·8	2700 2°·3 51·7			
			4005 2°·0 47·8	4050 1°·4 48·0	2880 1°·8 49·8	1800 2°·3 50·1				
			2610 2°·0 47·0	2700 2°·0 48·9	2520 2°·2 49·4					
				1620 3°·0 48·8	3645 1°·3 49·7					
				2700 2°·0 48·4	1440 3°·2 49·65					
				2700 2°·0 48·15	4085 1°·4 49·2					
					2700 1°·8 49·75					
					3285 1°·8 49·1					
					3195 1°·95 49·0					
					1800 2°·7 49·8					
					3240 2°·2 49·7					
					1800 2°·8 49·0					
Mean values ..	2475 1°·9 45·0	2090 2°·4 46·8	2925 2°·18 47·3	3035 1°·95 48·56	2540 2°·17 49·48	1800 2°·4 50·5	2700 2°·1 51·35		1800 3°·1 53·4	2880 1°·8 55·5
				2700 49·16 2°·1						
Number of observations	1	2	4	7	13	3	2	0	1	1
				20						

During an experiment with a rod it contracts while the pressure is being raised, and expands again when the pressure is relieved. The steel tube which holds it, however, acts in the opposite sense, it expands while the pressure rises and contracts while it falls. If the two surfaces were perfectly smooth, one half of the change of length would be measured at the one end and the other half at the other end. As the surfaces are not perfectly smooth, this does not usually occur. Moreover the steel tubes are prolongations of the central steel block which holds them. The block is bored with holes at right angles to each other in the three principal directions. Consequently for a distance of about an inch and a half in passing through the block the rod is not supported at all. With the exception of this small portion, however, the rod is supported throughout the whole of its length by the steel tube. Now, although it is thus nominally supported equally throughout the whole of its length, we know that in reality this is pretty certain not to be the case. At some place, either in the right arm or in the left arm of the apparatus, the rod is sure to bear more heavily than in any other part. The contraction under pressure and the expansion under relief of pressure will then apparently take place as from this point as origin. Supposing this point itself to be motionless, it is evident that the change of length measured at the two ends will be in the same proportion to each other as would be the arcs which they would describe if the rod were a lever oscillating on the point as a fulcrum. As there is no support at all at the centre, this point must lie on one side or on the other of it and the motions of the ends must be unequal. But the fixed point of the tubular receiver is the central block; therefore any point in, let us say, the right-hand tube will, when pressure is being raised, move to the right, and, on relief of pressure, retreat by an equal amount to the left. Consequently when we observe and measure the change of position of, for instance, the right-hand extremity of the rod, when the pressure is relieved, that change of position is composed of two motions, the expansion of the part of the rod which lies between the right-hand extremity and the point in it whose motion with respect to the steel carrying tube is *nil*, along with the proper motion of that point. Similarly, when we measure the change of position of the left-hand end, it also is composed of two parts, the expansion of the part of the rod which lies between the left-hand extremity and the same point in the length of the rod where its motion with respect to the steel tube is *nil*, along with the proper motion of that point. But at the left-hand end the motion of expansion is to the left, and at the right-hand end it is to the right, while the proper motion of the position of the common point on the rod and on the tube is always in one direction, and in this case, to the left. Therefore the distance measured in the right-hand microscope is the expansion of the portion of the rod which lies to the right of the point on it which is motionless relatively to the tube *minus* the proper motion of this point: and the distance measured at the left-hand end is the expansion of the remainder of the rod *plus* the proper motion of the common point. Consequently the algebraic sum of the two motions measured is the expansion of the rod under the relief of pressure.

When the substance is used in the form of a rod, as, for instance, in the case of glass, its ends are drawn out into wires, such that they can enter and be visible in the glass terminals. What we really measure then is the change of length under change of pressure of the axial glass wire in the rod, which may be looked on as a *fascine* of a very large number of similar but somewhat shorter wires. The sole function of these other wires is to maintain the wire that falls under observation in an axial position. It is obvious that this function can be performed with equal efficiency by wires of any other material, and that the conditions are in no way altered if these are fused into a tube of which the wire to be measured may be regarded as the core. Consequently by my method the linear compressibility of a solid can be determined as well on a wire as on a rod; and there is no limit to the thinness of the wire, so long as it can be handled, and be perceived in the microscope.

These two conditions are, in a way, antagonistic, because for the microscope the finest possible point is desirable, while for the handling of the wire a sensible thickness is essential. Only in the case of glass can a good working compromise be effected, because the wire which enters the glass terminal can be drawn out at the end to the finest possible hair, and the end of the hair can be fused into the minutest possible sphere, which can then be observed in the microscope with the sharpness with which a barometer can be read with a good telescope.

When the substance under observation is in the form of a wire, it lies in a glass tube which fits the bore of the steel tube as closely as possible. Its bore is a very little larger than that of the glass terminals, or about 1 mm. This tube acts as a bearer, and its length is as nearly as possible equal to the distance which separates the inside ends of the glass terminals when in position. When the pressure in the apparatus is raised, both the wire and the glass tube which carries it are shortened, while the steel tube which carries both of them is lengthened, and when the pressure is relieved the reverse takes place. The glass tube behaves exactly like the glass rod, that is, it is liable to a slight motion of translation. Similarly, the wire, which is carried by the glass tube, generally expands and contracts under pressure at a less rate than does the glass, producing again a slight apparent motion of translation. But again, as in the case of the rod, the algebraic sum of the observed motions is the expansion or contraction of the wire.

There is an advantage in having a very slight leak in the apparatus. The routine of an observation is then that the observer in charge of the pump and the manometer gets the pressure up somewhat higher than that desired; he then settles himself with the relieving lever in his hand and calls out as the mercury in the manometer in falling passes each division. The observers at the microscopes read their micrometers at the same moment. When the pressure has fallen a little below the desired pressure, the pressure is very carefully relieved, and the readings of the micrometers and of the manometer are taken at atmospheric pressure. The algebraic sum of the movements of the two ends on the micrometers gives the linear expansion of the body which has taken place, and the difference of the two readings of the manometer gives, when interpreted by the help of Piezometer C. No. 1, the difference of pressure which has caused the expansion. The micrometer measurements are then reduced separately to their absolute values in terms of the inch. The algebraic sum then gives the linear expansion in terms of the inch. It is then divided by the length of the rod or wire in inches and by the pressure in atmospheres; the resulting quotient is the linear compressibility of the metal or other substance. Multiplying this by three, we obtain the cubic compressibility of the substance, if truly isotropic.

It will be evident that, to work with this instrument, three observers are necessary, namely, one for each microscope, and one to raise and relieve the pressure and observe the manometer. I was fortunate in being assisted during this investigation by Mr. Andrew King, who was formerly my regular assistant, and is now of the Heriot-Watt College, Edinburgh, and by Mr. J. Reid, Demonstrator in the chemical laboratory of that institution. These gentlemen gave up their Christmas vacation for this work, and I owe them a deep debt of gratitude for the willingness and the efficiency of their help. The metals experimented with have been used in the form of wire, and the size chosen was No. 22 of the standard wire gauge (S.W.G.). In the case of aluminium, however, the size was No. 20. The dimensions corresponding to these numbers are given in the following table:—

No. of wire.	Diameter of wire.		Sectional area of wire.	Length of 1 c.c.
S.W.G.	inch.	mm.	sq. mm.	metre.
20	0·036	0·914	0·656	1·524
22	0·028	0·711	0·397	2·519

The degree in which the actual wires corresponded with the tabular specification was checked by weighing measured lengths of them.

The weight of 1 metre of each wire was as follows:—Platinum, 8·156 grammes; gold, 7·320 grammes; copper, 3·375 grammes; aluminium (No. 20), 1·642 grammes; and magnesium, 0·552 gramme. Neglecting the magnesium which, being pressed and not drawn, is very uneven in its calibre, these figures show that the actual wires were very slightly smaller than they should be by the gauge. Thus, in the case of the platinum wire 1 c.c. occupies 2·636 metres (lineal) instead of 2·519 metres as by the table.

The platinum and gold wires were pure specimens obtained from Messrs. Johnson and Matthey in the year 1880. The copper was "high conductivity" copper, and it as well as the aluminium and magnesium wires were of the best quality obtainable at the present day. The platinum and gold wires were heated to redness over a Bunsen lamp before use, so that they were thoroughly annealed. The aluminium wire was also heated, though to a much lower temperature, so as to soften it. The other metals were used in the state in which they were supplied. All the wires were straightened, but not stretched, before use.

The temperature of the wires during the operations was always that of the laboratory, and every care was used to keep it as uniform as possible, and it was as nearly as possible that of the air outside. Working in the middle of winter and in a comparatively high latitude, I hoped to be able to do so in conditions which, as regards temperature, would be similar to those which obtain in the depths of the sea, but the extraordinary mildness of the weather this year made it impossible, and the temperatures fell, mostly between 9° and 11° C.

The results of the investigation are set forth in detail in Tables II to VI, and they are summarised in Table VII.

Table II.—Platinum. Date, January 9, 1904. Temperature, 7° C.
Wire No. 22 S.W.G. Length, 75·35 inches.

No.	Pressure, P.	Changes of length.			Compression per million, $10^6 \frac{s}{75\cdot35} = S$.	Linear compressibility, $S/P = \lambda$.
		Right arm, r.	Left arm, l.	Sum, $r + l = S$.		
	atm.	in.	in.	in.		
1	204	0·003750	−0·000844	0·002906	38·57	0·188
2	204	0·003750	−0·000970	0·002780	36·88	0·180
3	204	0·003750	−0·000928	0·002822	37·45	0·184
4	300	0·005292	−0·001181	0·004111	54·56	0·182
						0·1835

Table III.—Gold. Date, January 10, 1904. Temperature, 10°·6 C.
Wire No. 22 S.W.G. Length, 75·4 inches.

No.	Pressure, P.	Changes of length.			Compression per million, $10^6 \frac{s}{75{\cdot}4} = \text{S}$.	Linear compressibility, S/P = λ.
		Right arm, *r*.	Left arm, *l*.	Sum, $r + l = \text{S}$.		
	atm.	in.	in.	in.		
1	231 ·5	0 ·005208	−0 ·000591	0 ·004617	61 ·21	0 ·264
2	230 ·0	0 ·005208	−0 ·000970	0 ·004238	56 ·20	0 ·244
3	230 ·0	0 ·005417	−0 ·000844	0 ·004573	60 ·64	0 ·264
4	204 ·0	0 ·005000	−0 ·000590	0 ·004410	58 ·48	0 ·287
5	247 ·0	0 ·005834	−0 ·001181	0 ·004653	61 ·70	0 ·254
6	230 ·0	0 ·005208	−0 ·001094	0 ·004114	54 ·55	0 ·237
7	238 ·5	0 ·005000	−0 ·000422	0 ·004578	60 ·70	0 ·255
8	238 ·5	0 ·005000	−0 ·000422	0 ·004578	60 ·70	0 ·255
9	238 ·5	0 ·005208	−0 ·000633	0 ·004575	60 ·66	0 ·254
10	273 ·5	0 ·006042	−0 ·000548	0 ·005494	72 ·85	0 ·264
11	273 ·5	0 ·005834	−0 ·000211	0 ·005623	74 ·56	0 ·273
12	273 ·5	0 ·006042	−0 ·000591	0 ·005451	72 ·28	0 ·264
13	273 ·5	0 ·006042	−0 ·000464	0 ·005578	73 ·96	0 ·270
14	273 ·5	0 ·006042	−0 ·000717	0 ·005325	70 ·61	0 ·258
15	273 ·5	0 ·006250	−0 ·000717	0 ·005533	73 ·37	0 ·261
16	273 ·5	0 ·006042	−0 ·000506	0 ·005536	73 ·41	0 ·268
17	269 ·0	0 ·005834	−0 ·000422	0 ·005412	71 ·76	0 ·267
18	273 ·5	0 ·006042	−0 ·000548	0 ·005494	72 ·85	0 ·266
						0 ·260

Table IV.—Copper. Date, January 9, 1904. Temperature 10° C.
Wire No. 22 S.W.G. Length, 75·3 inches.

No.	Pressure, P.	Changes of length.			Compression per million, $10^6 \frac{s}{75{\cdot}3} = \text{S}$.	Linear compressibility, S/P = λ.
		Right arm, *r*.	Left arm, *l*.	Sum, $r + l = \text{S}$.		
	atm.	in.	in.	in.		
1	195 ·5	0 ·005664	−0 ·001687	0 ·003980	52 ·85	0 ·270
2	230 ·0	0 ·006334	−0 ·001687	0 ·004647	61 ·70	0 ·268
3	195 ·5	0 ·005875	−0 ·001814	0 ·004061	53 ·93	0 ·276
4	195 ·5	0 ·006125	−0 ·001772	0 ·004353	57 ·81	0 ·296
5	247 ·0	0 ·007417	−0 ·001856	0 ·005561	73 ·85	0 ·299
6	230 ·0	0 ·006750	−0 ·001434	0 ·005316	70 ·60	0 ·307
7	282 ·5	0 ·007751	−0 ·001519	0 ·006232	82 ·76	0 ·293
						0 ·288

Table V.—Aluminium. Date, January 11, 1904. Temperature, 9° C.
Wire No. 20 S.W.G. Length, 75·35 inches.

No.	Pressure, P.	Changes of length.			Compression per million. $10^6 \frac{s}{75 \cdot 35} = \mathrm{S}$.	Linear compressibility, S/P = λ.
		Right arm, *r*.	Left arm, *l*.	Sum, $r + l = \mathrm{S}$.		
	atm.	in.	in.	in.		
1	195·5	0·005542	0·002616	0·008158	114·22	0·584
2	161·5	0·004900	0·002152	0·007052	93·58	0·579
3	230·0	0·006667	0·002742	0·009409	124·86	0·543
4	178·5	0·005334	0·002109	0·007443	98·77	0·553
5	256·0	0·007251	0·002995	0·010246	135·96	0·531
						0·558

Table VI.—Magnesium. Date, January 17, 1904. Temperature, 9° C.
Wire No. 22 S.W.G. Length, 75·2 inches.

No.	Pressure, P.	Changes of length.			Compression per million, $10^6 \frac{s}{75 \cdot 2} = \mathrm{S}$.	Linear compressibility, S/P = λ.
		Right arm, *r*.	Left arm, *l*.	Sum, $r + l = \mathrm{S}$.		
	atm.	in.	in.	in.		
1	204	0·009167	0·007120	0·016288	216·61	1·062
2	204	0·010001	0·006202	0·016203	215·48	1·056
3	204	0·009917	0·006329	0·016246	216·07	1·059
4	204	0·010418	0·005991	0·016409	215·57	1·057
5	204	0·010543	0·005442	0·015985	212·60	1·042
6	204	0·011251	0·004852	0·016103	214·16	1·050
						1·054

Table VII.—Summary.

Substance.	Year.	Atomic weight.	Density.	Compressibility.	
				Linear.	Cubic.
Platinum	1904	194	21·5	0·1835	0·5505
Gold	„	197	19·3	0·260	0·780
Copper	„	63	8·9	0·288	0·864
Aluminium	„	27	2·6	0·558	0·1674
Magnesium	„	24	1·75	1·054	3·162
Mercury	1875	200	13·6	1·33	3·99
Glass, flint	1880	..	..	0·973	2·92
„ „	1904	..	2·968	1·02	3·06
„ German	„	..	2·494	0·846	2·54

In the summary, Table VII, the compressibilities of English flint glass and of the glass of which ordinary German tubing is made as well as that of mercury have been included for purposes of comparison. The compressibility of mercury rests upon a large number of observations made in the "Challenger,"* by which its apparent cubic compressibility was found to be 1·5 per million per atmosphere. The piezometers which were used for this purpose were made by myself on board. The divided stems were of lead glass, because I had no other, and the bulbs or reservoirs, which had a capacity of about 20 c.c., were made of German glass, for the same reason. I have, therefore, applied to the values then found for the apparent compressibility of mercury, the value of the absolute compressibility of German glass found in January of this year, and the result is that the absolute cubic compressibility of mercury at temperatures between 1° and 3° C. is 3·99.

With regard to the metals quoted in the tables, the figures speak for themselves. The number of different metals is very small and, until the investigation has been extended so as to include at least the greater number of the metals which can be easily procured in the form of rod or wire, it is not likely that any very general features or laws will be apparent. It will, however, be observed that in the case of the five metals used as wire, their compressibility increases as their density and atomic weight diminish, yet there is no reason to suppose that the compressibility is a continuous function of the atomic weight, like the specific heat. Mercury, although in the fused state, shows this clearly. But besides this, it happens that two pairs out of the five metals, namely, platinum-gold and aluminium-magnesium, are contiguous in the atomic weight series, yet the compressibility of magnesium is, roughly, double that of aluminium, and the compressibility of gold is half as much again as that of platinum. If, however, we compare gold and copper, which occupy parallel positions in Mendeléieff's scheme, we see that they are very much alike, and the same holds with regard to magnesium and mercury which occupy a homologous position. If these facts indicate anything more general, we should expect the metals of the palladium and the iron groups to have a low compressibility like platinum, zinc and cadmium to have a very high compressibility like magnesium, and thallium an intermediate but still considerable compressibility like aluminium.

It will be observed that the two kinds of glass mentioned in Table VII are more compressible the greater their density. This may, however, be due to a specific feature of the oxide of lead which enters largely into the composition of the flint glass.

It is obvious that there is here a great field for interesting research, and fortunately the method is capable of great refinement; only, the successful application of it requires considerable manipulative skill,

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* 'Chem. Soc. Jour.' (1878), vol. 33, p. 453.

as well as great patience. The necessity to have, as part of the apparatus, two glass tubes which are exposed to the high pressure on the inside only, introduces an element of chance into the work which is sometimes annoying and sometimes exciting. It is impossible to say beforehand whether a particular glass terminal will stand or not. It is necessary to be provided with a large reserve of them before beginning work, and when one fails another is put in its place without loss of time. Hitherto I have taken no particular care of my glass terminals, because I can always depend on finding plenty of them which will stand from 200—300 atmospheres, and there is abundance of work to be done at these pressures. When, however, it is desired to use higher pressures, it will be prudent to take some measures for preventing the points of the wires scratching the internal surfaces of the terminals. When some precaution of this kind has been taken, casualties will be less frequent, and the attainment of higher pressures will be merely a question of how many glass ends the observer is prepared to sacrifice in the service.

In the work connected with this paper, which extended over the greater part of 4 weeks, fifteen glass terminals gave way; and oddly enough, the failures were as nearly as possible equally distributed between the two ends; eight of them fell to the left arm and seven of them to the right arm. The bursting of a terminal causes no inconvenience beyond the trouble of replacing it, because the construction of the instrument enables air to be completely excluded from it, and the quantity of water in it to be kept within such limits that its resilience is of no account. When a tube bursts it usually splits longitudinally up the middle into two slabs. One of these almost always remains entire, the other is sometimes broken into fragments, but there is never any projection of material unless the instrument has been carelessly put together and air admitted.

Microseismic Effects.—In a research like the present where the primary object is the numerical determination of a physical constant, the secondary phenomena which reveal themselves are often of equal and sometimes of greater interest, because they generally affect preferentially the natural history side of physics. To this class belong the phenomena observed in connection with the behaviour of ice under the relief of high pressure in my earlier investigation.* In the present case the frequent bursting of the glass terminals afforded the opportunity of observing another and very interesting phenomenon. It is illustrated in fig. 2. It was first noticed when copper wire was being experimented with. The pressure had been raised to 300 atmospheres, and had begun to fall when the tube gave way. On proceeding to replace the broken tube with another I was astonished to find the copper wire twisted into a regular spiral

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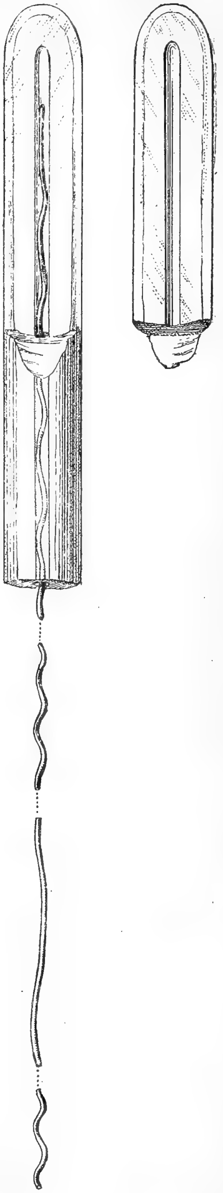
* 'Roy. Soc. Edin. Trans.,' vol. 29, p. 598.

in the tube. It made three complete turns in the length of an inch, and the undulatory form was visible throughout one-half of the length of the wire. Instead of fixing new glass terminals, I cut off the end of the copper wire, which showed this curious seismic effect, and put another wire in its place. An exactly similar effect was produced on the magnesium wire, when a glass terminal burst; only the effect was even more marked. The spiral produced in the glass end was closer, and, indeed, the wire had been shoved over itself and broken, for magnesium wire is very brittle. The undulations of greater amplitude extended through the whole length of the wire, and there were *maxima* at distances of about 35 cm. and 85 cm. from the seat of the explosion. The bursting pressure in this case was no more than 150 atmospheres, yet the effect produced was very much greater than it was in the case of copper.

The experiments with gold and aluminium were carried out without the loss of a terminal.

In the case of platinum, a terminal burst at about 250 atmospheres, but it produced no apparent seismic effect. On the last day of my experiments I proposed to determine the compressibility of a wire of mild steel, but, owing to hurry in putting the apparatus together, it was impossible to get any satisfactory observations, but one of the terminals burst, and at a pressure over 250 atmospheres. Here again there was no seismic effect. The platinum wire had been thoroughly annealed before being used, and the mild steel wire was as soft and ductile as copper, yet, though copper showed the seismic effect beautifully, it was imperceptible in both platinum and steel. Before the experiment with the steel, I supposed that the high density of platinum caused the shock to be opposed by more inertia than it could overcome, but the density of steel is less than that of copper, therefore its immunity to shock must be due to something other than its density.

FIG. 2.



The open ends of the glass terminals which are inside of the watertight collars are cut sharply off and the edges are not rounded in the flame. Special directions were given to the glass blower about this, because the effect of it would be the production of considerable tension in that part of the glass. Notwithstanding my directions, some of the tubes were rounded off in the lamp and the effect was as I had foreseen. The only one of these ends which I used burst. In the case of ends which have been cut off and not heated, the fracture is confined to the part of the tube outside the apparatus. In the case of the end with rounded edges the outside part was fractured in the ordinary way, and in addition the rounded portion, which was exposed to no difference of pressure, exploded out of sympathy, much after the fashion of a Prince Rupert's drop.

I am continuing this investigation, and I hope shortly to be in a position to be able to communicate further results to the Society.

CROONIAN LECTURE.—"The Chemical Regulation of the Secretory Process." By W. M. BAYLISS, F.R.S., and E. H. STARLING, F.R.S.

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In the complex reactions which make up the life of an individual, and the evolution of which has been the determining factor in the individual's existence, we may distinguish two main types; though, as in all attempts at classification of biological processes, the line of division between the two types must be more or less indistinct.

In the first place we have those reactions which depend for their production on some special structural arrangement, and are therefore determinant factors in the evolution of form. In some cases the adaptive production of organs or protective mechanisms may be associated with a direct chemical reaction, as in the formation of protective tissues. In most cases, in higher animals at any rate, such an adaptation will be intimately associated with the development of the central nervous system, of which the peripheral parts of the body must be regarded as the executive mechanisms. In general, however, we may say that this type of adaptation is dependent on the adaptive *growth* of cells.

The second type, the more primitive of the two, involves, in the first place, not so much a change in the growth or arrangement of cells, as a change in the metabolism of pre-formed cells or structures. It may, perhaps, be looked upon as a preliminary to the first type, namely, structural change. It is, however, of special interest, since its mechanism is subject to analysis by physiological methods. Instances of chemical adaptation may again be divided into two groups. In the first place we have the chemical adaptation to the environment, which is found in all living organisms from the lowest to the highest. This adaptation may be conditioned by changes in the food, or may arise as a reaction to the presence of harmful substances in the surrounding medium. As an example may be mentioned the mould *Penicillium glaucum*, which, as shown by Duclaux,* when grown on calcium lactate forms invertase only; on starch, however, it produces amylase in addition, while on milk it produces a proteolytic ferment and rennet. In the higher animals we have all the complex processes by which an animal reacts to the introduction of living or dead poisons, and which result in the production of an acquired immunity. As part of the same process, if we accept Ehrlich's views, we must include the process of assimilation of food, and the adaptation of an animal to profound changes in its diet.

But in all the higher animals the reaction of any part of the body to external changes involves alterations in its relations to other parts, and there is evolved a complex system of internal correlation of the activities of organs, effected partly by the action of the central nervous system, and therefore determinant of changes in form, partly through means of the internal medium—the blood or similar fluid. This latter mechanism of internal correlation has only recently entered the domain of exact investigation. Thus the profound influence exercised by the thyroid gland on the nutrition of the whole body, specially of the central nervous system, and the production of a substance by the suprarenal bodies which maintains the tone of all contractile tissues in the body, have been disclosed to us during the last 15 years. When chemical adaptation occurs in response to changes either in the environment or in definite organs of the body, the adaptive reaction may affect all cells of the body, or may be specific in the case of certain cells. Only in the latter case will the results be apparent to the morphologist as determinant of form.

The researches which we wish to bring briefly before the Royal Society have reference entirely to the last two groups we have mentioned, and deal with the mechanism of adaptation to changes in the food and the chemical correlation of the activities of different organs engaged in the digestion and assimilation of the food.

As we proceed down the alimentary tract, we find that each cavity has its own set of reactive mechanisms arranged so as to pour on the ingested food a juice which shall dissolve one or more of the constituents of the food. In the mouth, as has been shown by the researches of Ludwig, Heidenhain, Langley, and Pawlow, the mechanism for the secretion of saliva is entirely nervous. The mucous membrane is endowed with distinct sensibilities for different classes of food, and the activity of the salivary glands is excited reflexly according to the nature of the substance present in the mouth. In

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* 'Microbiologie,' vol. 2, p. 86.

the stomach, the researches of Heidenhain, and especially of Pawlow,* have shown that the secretion of the gastric juice is, in the first place, controlled by the nervous system, and is excited by appetite, or by reflex impulses arising in the mouth. Only later on, in gastric digestion, does a secretion come on, determined, in some way or other, by the presence and nature of the food in the stomach. This secondary secretion is independent of the central nervous system; but whether it is to be looked upon as a local reflex, or as a chemical excitation, directly or indirectly from the gastric contents, has not yet been determined. As the strongly acid fluid containing the products of gastric digestion leaves the stomach to enter the duodenum, it comes in contact with two other secretions, the bile and the pancreatic juice, which are secreted in such an amount that the duodenal contents become practically neutral. According to Pawlow, the secretion of the pancreatic juice is exactly comparable to the secretion of saliva, and is effected by a nervous reflex. The starting point of this reflex is the stimulation of the duodenal mucous membrane by the chyme, or by substances such as oil, ether, or oil of mustard. Not only is the pancreatic juice turned out into the intestine just at the time when it is required, but, according to Pawlow, the composition of the juice varies according to the food, the proteolytic ferment being increased by a diet of meat, while the amylolytic ferment is increased by a starchy diet. This adaptation of the glandular activity was ascribed by him to a species of "taste" in the mucous membrane. It was imagined that the different constituents of the food excited different nerve endings, which, in their turn, caused reflex activity of different mechanisms in the pancreas itself. The field of these assumed reflexes was considerably narrowed by the researches of Popielski† and Wertheimer,‡ who showed that the introduction of acid into the duodenum was productive of secretion even after destruction of all nerve connections of the pancreas and alimentary canal with the central nervous system, and even after extirpation of the sympathetic ganglia of the solar plexus. It was with a view to determine the mechanisms of this reflex secretion of the pancreas, as well as of the adaptation of the pancreatic secretion to variations in the food of the animal, that we began our researches.

The last-named authors had also shown that the secretion occurred, but in smaller quantities, if the acid was inserted in any part of the small intestine, with the exception of the lower end of the ileum. It was thus easy to examine the effects of the introduction of acid into a loop of ileum in which all nerve connections with the pancreas, or with the rest of the body, had been destroyed. This crucial experi-

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* 'Le Travail des Glandes Digestives,' Paris, 1901.

† 'Gazette Clinique de Botkin,' 1900.

‡ 'Journal de Physiologie,' vol. 3, p. 335, 1901.

ment had, curiously, not been performed by previous workers in the subject. On carrying it out, we found that destruction of all nerve connections made no difference to the result of introducing the acid. The pancreatic secretion occurred as in a normal animal. It was therefore evident that we had to do here with a chemical rather than a nervous mechanism. Previous work had narrowed the question down to such a degree that the further steps were obvious. We knew already that the introduction of acid into the blood-stream had no influence on the pancreas; hence the acid introduced into the intestine must be changed in its passage to the blood-vessels through the epithelial cells, or must produce in these cells some substance which, on access to the blood stream, evoked in the pancreas a secretion. This was found to be the case. On rubbing up the mucous membrane with acid, and injecting the mixture into the blood-stream, a copious secretion of pancreatic juice was produced. It was then found that the active substance, which we call *secretin*, was produced by the action of acid from a precursor in the mucous membrane, probably in the epithelial cells themselves. Once formed by the action of acid, it could be boiled, neutralised, or made alkaline, without undergoing destruction. The precursor of the substance (*pro-secretin*) cannot be extracted by any means that we have tried from the mucous membrane. Even after coagulation of the mucous membrane by heat or alcohol, however, secretin can still be extracted from the coagulated mass by the action of warm dilute acid.

The question then arose whether this chemical mechanism represented the normal mode in which secretion of the pancreatic juice was excited by the presence of food in the gut. It had already been shown by Wertheimer that the secretion evoked by the presence of acid diminished as the acid was placed further down in the small intestine, and was absent when the acid was placed in the lowermost section of the ileum or in the large intestine. We found a corresponding distribution of pro-secretin. The most active extracts of secretin were to be obtained from the duodenum. The extracts from the jejunum were less powerful, while those from the lower 6 inches of ileum or from the large intestine were practically inert. The proof that secretin is really carried by the blood to the gland has been furnished by Wertheimer,* who has shown that the blood coming from a loop of intestine into which acid has been introduced, when injected into another dog, evokes in the latter a secretion of pancreatic juice. All authors who have investigated the matter since our first publication on the subject have confirmed our results; but many of them are still loth to give up the idea of a nervous connection between the gut and the pancreas. Pawlow had obtained evidence of the existence of secretory nerves to the pancreas in the vagus as well

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* 'C. R. Soc. de Biologie,' 1902, p. 475.

as in the splanchnics. In all his experiments, however, it was difficult to exclude the possibility of the secretion having been excited by the contraction of the stomach or relaxation of the pylorus, causing the passage of some acid contents of the stomach into the duodenum, since both these results may occur on stimulation of the vagus. We have been unable to obtain secretion from stimulation of any nerves in any case where this possibility was excluded, and we are inclined to believe that the chemical mechanism we have described is the *only* method by which the pancreas is awaken to secrete. The inhibition of secretion obtained by some authors in an unanæsthetised animal on stimulation of the vagus is, we believe, a secondary phenomenon due to interference with the blood supply or more probably with the flow of acid chyme from the stomach, or perhaps to the rapid emptying of the upper part of the gut of its acid contents.

Secretin can be split off from its precursor in the mucous membrane by the action of acids or boiling water. Many acids are able to effect this conversion, their power being roughly proportional to their ionic concentration. We have, therefore, concluded that the process is one of hydrolysis. According to Fleig,* a secretin can also be prepared from mucous membrane by the action of soaps, and secretin has been detected in the blood flowing from the loop of intestine into which oil of mustard had been introduced. Fleig regards the secretin produced by the action of soaps as different from that produced by the action of acids; but it is difficult to see on what grounds he makes this distinction, since the action of the secretin prepared in the two ways is identical. The production of secretin by the action of oil of mustard as well as the well-known secretion of pancreatic juice evoked by the introduction of ether into the duodenum, suggests that the hydrolytic dissociation which gives rise to secretin may occur in the living cells as a result of stimulation or severe lesion, since neither of these two substances will produce secretin from an excised and dead mucous membrane.

We have not yet succeeded in determining the chemical nature of secretin, though we have obtained chemical evidence which will serve to exclude certain classes of substances. Thus the fact that it will stand boiling shows that it is neither a coagulable proteid nor a ferment. It is soluble in 90 per cent. alcohol in the presence of ether, but it is insoluble in absolute alcohol and ether. It is slightly diffusible through animal membranes. It can be filtered through a gelatinised Chamberland filter. It is not precipitated by tannic acid, thus excluding bodies of alkaloid nature as well as di-amido compounds. This evidence, slight though it is, points to secretin being a body of relatively small molecular weight and not a colloid. It may be compared to the active principle of the suprarenal glands, adrenalin,

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* 'C. R. Soc. de Biologie,' 1903.

which has been obtained in a crystalline form and the chemical constitution of which has been approximately determined. This is, indeed, what one would expect of a substance which has to be turned out into the blood at repeated intervals in order to produce in some distant organ or organs a physiological response proportional to the dose. The bodies of higher molecular weight, such as the toxins, which owe their activity, according to Ehrlich, to the fact that they can be directly assimilated by the cells of the body, and built up into the protoplasmic molecule, always give rise to the production of anti-bodies, a process which, while not preventing necessarily their utilisation in the body, would prevent their acting as a physiological stimulus to certain definite cells. Adrenalin and secretin on the other hand belong to the class of drugs which act by their physico-chemical properties, and whose physiological effect is determined by the total configuration of their molecule. It was suggested to us early in our experiments that the secretion of pancreatic juice, evoked by secretin, was essentially a sudden production of an anti-body; such a sudden production is unknown in the animal body, and the anti-character of the secretion is at once negatived by the fact that secretin can be mixed with a freshly secreted juice without in any way destroying its efficiency.

Like adrenalin, secretin is extremely easily oxidised, and it is probable that it is got rid of in this way from the body, since, even after repeated injections of secretin, it is impossible to find this substance or any precursor of it either in the pancreas, the urine, or other tissues of the body. Just as in the case of adrenalin, so we find that secretin is not specific for the individual or species. An extract of the mucous membrane of the dog will evoke secretion in the pancreas of the frog, the bird, rabbit, cat, or monkey. In the same way the pancreatic secretion of the dog can be excited by injection of secretin prepared from the intestine of man, cat, monkey, rabbit, fowl, salmon, skate, frog, or tortoise. The evolution of this mechanism is, therefore, to be sought at some time anterior to the development of vertebrates.

The action of secretin is not confined to the pancreas. It has long been known that the pancreatic juice, in order to exert its full activity on the food stuffs, needs the simultaneous presence of bile, and the fact that in many cases the two fluids are poured into the duodenum by a common orifice shows the close connection which must exist between them. Digestion of fats is impossible unless both fluids have access to the gut, and even in the digestion of carbohydrates, as was shown by S. Martin and Dawson Williams many years ago, the presence of bile greatly hastens the digestive powers of the pancreatic juice. Whenever, therefore, a secretion of pancreatic juice is required, a simultaneous secretion of bile is also necessary. It is interesting to note that this simultaneous secretion is provided for by the same mechanism by which the secretion of pancreatic juice is evoked. If the flow of bile be determined by measuring the outflow from a cannula placed in the bile duct, it will be found that introduction of acid into the duodenum causes a quickened secretion of this fluid. The same increase in the secretion of bile can be produced by injecting solutions of secretin into the blood stream. This influence of secretin on the liver has been fully confirmed by Falloise. This observer has shown that acid extracts of the intestinal mucous membrane cause an increase in the bile secretion most marked when the extract is made from the duodenum and diminishing as the extract is taken from the lower parts of the gut, that from the lower section of the ileum being quite ineffective.

In some cases the injection of secretin is followed by a secretion of glairy saliva, but this is at once abolished on section of the nerves going to the salivary glands, and is simply a result of the lowering of blood pressure which occurs when any extract of the intestinal mucous membrane is injected into the blood stream. On no other glands of the body has secretin the slightest influence. We must, therefore, regard secretin as a drug-like body having a specific excitatory effect on the secreting cells of the liver and pancreas.

The discovery of secretin has placed in the hands of physiologists the power of controlling the activity of a gland by purely physiological means, and we have taken opportunity of the control thus acquired to investigate the exact character of the changes induced in the pancreas under this physiological stimulus. So far as we can tell secretin has no specific influence on any one constituent of the pancreatic juice. When injected it causes secretion of a juice which is normal in that it resembles the juice secreted on entry of food into the duodenum, and contains a precursor of trypsin, amylopsin, and steapsin. Secretin, in fact, appears to cause the pancreatic cells to turn out the whole of the mesostates which they have accumulated during rest in preparation for the act of secretion. If secretin be injected at repeated intervals until the gland will no longer respond to the injection, it is found on microscopic examination that the cells have discharged the whole of their granules. In sections stained with toluidine blue and eosin the whole of the cells stain blue in marked contrast to the normal resting gland, where one-half or two-thirds of the inner margin of the cells is taken up with brilliantly stained red granules. This effect is not produced in all cases. In some animals we have injected secretin at frequent intervals over a period of 8 hours, and obtained at the end of the experiment a secretion as vigorous as after the first injection. The pancreas in this case was evidently not fatigued, and on killing the animal and examining this organ microscopically, it was found to give the typical picture of a resting pancreas. One may say, therefore, that under healthy conditions the activity of the pancreas is two-fold in character, and that the normal stimulus of secretin excites not only a breaking down of the protoplasm and a discharge of granules, but also a building up of the protoplasm and a new formation of granules. So marked, in fact, is this power of self-restitution that it is often advisable to diminish the resistance of the animal by bleeding or other means if it is desired to obtain a specimen of exhausted gland.

A study by Mr. Dale of the stages of exhaustion carried out in this way has brought to light a remarkable behaviour in the cells of the pancreas, to which we have no analogies in other secreting glands of the body. After the discharge of the granules the cells seem to undergo a still further involution, losing the whole of their chromophile substance, diminishing in size or undergoing vacuolation, and finally being transformed into cells undistinguishable from those which have long been known as forming the so-called "islets of Langerhans." Mr. Dale has, in fact, shown that in all probability these "islets," which are generally regarded as pre-formed structures, really represent stages in the functional activity of the secreting cells of the gland, and he is of opinion that the activity of the gland is always associated with a cycle of changes in which the islets are formed, to be afterwards regenerated into secreting tissue. Other observers have noted in the embryo a development of secreting tubules from tissue undistinguishable from the "islets of Langerhans," and it is interesting to note that the depletion of the gland caused by long starvation has a similar effect to that caused by over-excitation, namely, the conversion of a large proportion of the gland tissue into "islet" tissue.

Although secretin acts in this apparently coarse manner in turning out all the pre-formed secretory products present at the time in the pancreatic cells, the conditions of its formation determine a close adaptation of the pancreatic activity to the needs of the animal. Formation of secretin depends on the presence of acid chyme in the duodenum. This acid chyme is squirted in small quantities into the stomach at varying intervals after the taking of food. As soon as it enters the gut, secretin is formed in the mucous membrane, absorbed by the blood-vessels and carried to the pancreas, and it will continue to be formed until the secreted pancreatic juice exactly neutralises the acid of the intestinal contents. The presence of an excessive amount of acid in the duodenum is prevented by the reflex pyloric mechanism revealed by the researches of Von Mering and of Serdjunow.[*] These observers have shown that so long as the contents of the duodenum are acid the pylorus remains firmly closed. As soon, however, as they become neutral or alkaline the pylorus opens and allows a further

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[*] Pawlow, 'Das Experiment,' 1900, p. 17.

quantity of acid gastric contents to enter the duodenum. By this double mechanism, which is partly nervous, partly chemical, it is provided that the acid contents of the stomach shall pass on into the gut only in such quantities as can be dealt with by the secretory mechanisms of the intestine.

One more chain in the link of adaptive reactions may be briefly mentioned. The pancreatic juice, as secreted, contains only a weak proteolytic ferment. But it contains also trypsinogen. As soon as this juice enters the gut it causes a profuse secretion of intestinal juice. This latter contains another ferment, enterokinase, which acts on the trypsinogen, converting it into a body trypsin, one of the most active proteolytic ferments with which we are acquainted.

So far we have dealt only with the correlation of the activities of the cells lining the intestinal tube with those forming the masses of the pancreas and liver, and have seen that a very large part in this correlation is played by a chemical substance which acts, so to speak, as a chemical messenger between these various organs. A striking feature, however, of the pancreas is its alleged power of adapting its secretion to the nature of the food taken in by the animal. It has been stated by Pawlow that according as the food consists chiefly of proteids, carbohydrates, or fats, so do we find a relative preponderance of the ferments acting respectively on each of these three classes of foods. The evidence on which this statement is based, although lending to it considerable support, is not absolutely convincing. Vasilieff* examined the pancreatic juice of dogs which were fed on meat, or bread and milk alternately for periods extending over several weeks for each kind of diet. This observer found that the transition from bread and milk diet to a meat diet caused a rapid rise in the proteolytic power of the juice, which reached its maximum after several days of meat feeding. A return to a diet of bread and milk caused a slower fall in the proteolytic power of the juice, but a rise in the amylolytic power. Similar results were obtained by another pupil of Pawlow—Jablonsky†—who also extended his observations to the fat-splitting ferment. At the time that these observations were made the function of enterokinase was unknown, and it is therefore impossible to say what proportion of the trypsinogen of the juice secreted in these experiments had been converted into trypsin by the small amount of intestinal mucous membrane at the mouth of the duct. While, therefore, we are unable to ascribe much importance to the results as regards the proteolytic power of the juice, there seems no reason to doubt the results obtained by these workers as regards the starch-digesting power of the juice. In 1899 Walther‡ made a series of observations

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* 'Archives des Sciences Biologiques,' St. Petersburg, 1893.

† *Ibid.*, 1896.

‡ *Ibid.*, 1899, vol. 7, p. 1.

on a dog with pancreatic fistula in order to determine whether the amounts of ferments secreted were determined by the nature of the food at any given meal. He was satisfied that his results showed that, even without prolonged adherence to one diet, the composition of pancreatic juice was adapted to the nature of the meal taken. His results do not entirely bear out his contentions, as is seen by the following table, in which it will be noticed that although milk contains no starch it evokes the secretion of a large amount of amylopsin, and that meat causes a secretion of more steapsin than does milk, although this latter contains much more fat than the meat diet.

Table I.—Results of Walther's Experiments.

Diet.	Total amount of enzyme secreted.		
	Proteolytic.	Amylolytic.	Fat-splitting.
600 c.c. milk	1044	2310	4125
250 grammes bread	2360	6343	1218
100 grammes meat	1720	2498	4410

Of course Walther, as well as the other observers mentioned, regard the adaptation as determined by the stimulation of special nerve endings in the mucous membrane by each constituent of the food, a conclusion hardly borne out by the results just quoted. Another disturbing factor in these experiments is the large variation in total quantity of juice secreted with different food stuffs.

Table II.—Amount of Pancreatic Juice Secreted for different Food-stuffs (Walther).

Food.	Hours of secretion.									Total amount.
	1.	2.	3.	4.	5.	6.	7.	8.	9.	
600 c.c. milk	8·2	6·0	23·0	6·2	1·75	..	..	..	..	45 c.c.
250 grammes bread	35·5	47·0	20·5	16·5	10·0	12·0	6·5	3·0	..	151 „
100 grammes meat	45·0	52·0	35·0	9·75	..	..	..	..	..	142 „

The quantity of juice secreted will depend on the amount of secretin turned into the circulation, and this, in its turn, on the amount of acid entering the duodenum from the stomach. The amount of juice will, therefore, be measured by the stay and resistance to digestion of the substance in the stomach rather than to any direct nervous or other influence of the duodenal contents on the pancreas. A repetition of Walther's experiments by Popielski,* working independently, has in fact led the latter to deny altogether the adaptation of the pancreatic juice to the nature of the food. Popielski concludes from his experiments that variations in the juice depend only on the intensity and duration of the stimulus, the intensity of the stimulus determining the amount of enzymes, whilst its duration determines the total quantity of juice.

In the meantime the question had been attacked from another side. It had been shown by Fischer and Niebel† as well as by Portier‡ that watery extracts of the pancreas of the cow, horse, and dog had no influence on lactose. Weinland in 1899 confirmed these results so far as concerns the pancreas of dogs on an ordinary diet free from milk. On the other hand, he found that extracts of the pancreas of dogs, which had been fed for several days on milk, sometimes with the addition of lactose, invariably contained lactase in considerable amount, and these results have been confirmed lately by Bainbridge working in our laboratory. Here then we have a definite instance of adaptation of the pancreas, the pancreatic juice or pancreatic extracts of dogs on normal diet containing no lactase, while the administration of lactose to these animals causes the appearance of lactase in both cases. Since in this case we have to determine, not simply an increase or diminution in the amounts of ferments always present in the juice, but the presence or absence of a definite substance, this was evidently the best starting point for an investigation of the mechanism by which the pancreas can adapt itself to the nature of the food, an investigation which has been carried out and completed by Dr. Bainbridge.

What are the limiting conditions ? In the first place the reaction is absolutely specific. Unless the animal is taking lactose in its diet no lactase is ever found in the pancreas or in its secretion. The pancreas of new-born animals, for instance, is quite free from lactase, which, however, makes its appearance 2 or 3 days after birth as the result of the milk diet. The production of lactase is not a direct reaction of the pancreas to the presence of lactose in the blood, since subcutaneous or intravenous injection of lactose does not cause the appearance of lactase in the pancreas. The intestinal mucous membrane of all animals, whether on a milk diet or not, contains lactase and has an inverting action on lactose. It might be thought therefore that the production of lactase by the pancreas was a reaction to the

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* 'Centralblatt f. Physiologie,' vol. 17, 1903.

† 'Sitzungsberichte der K. Preuss. Akad. d. Wiss.,' 1895, p. 73.

‡ 'C. R. Soc. de Biologie,' 1898, p. 387.

presence of the products of inversion of lactose in the blood. This was found not to be the case. Subcutaneous injection of galactose for several days was not followed by any appearance of lactase in the pancreas or its juice. Nor was the appearance of lactase due to the increased production of this ferment in the mucous membrane, and its escape into the blood. Injection of an extract of mucous membrane rich in lactase, repeated several days in succession, was not followed by any appearance of lactase in the pancreas. Injection of lactose into the duodenum, and the subsequent injection of secretin after an interval of 1 hour, was inefficacious in causing the appearance of lactase in the pancreatic juice. For the production of lactase in the pancreas, or its juice, it is therefore necessary that lactose should act on the intestinal mucous membrane for some time. The reaction is a slow one, like the adaptation in Vasilieff's experiments, and is certainly not due to the stimulation of certain nerve endings in the mucous membrane by the lactose.

The problem was somewhat similar to that presented by the action of acid in the duodenum, since this introduced into the duodenum produces secretion of juice, whereas, when introduced into the blood stream, it has no effect whatever on the pancreas. The question suggested itself whether, under the influence of lactose, a special secretin was formed in the intestinal mucous membrane which, on access to general circulation, evoked the formation and secretion of lactase by the pancreas. Secretin was therefore made in the usual way (*i.e.*, acidification, boiling, neutralisation, and filtering) from the mucous membrane of milk-fed dogs. The secretion evoked by the injection of this liquid resembled that obtained from the injection of ordinary secretin, and contained no lactase.

Yet it was evident from the results already obtained that lactose must act on the pancreas through the mucous membrane of the intestine. An extract was therefore made from the mucous membrane of the whole small intestine of a milk-fed dog. This was filtered through muslin, and about 10 c.c. injected subcutaneously into a biscuit-fed dog once a day for three days. The dog was then anæsthetised, a cannula placed in its pancreatic duct, and ordinary secretin injected. A flow of pancreatic juice was obtained, and this juice was found to contain lactase. This experiment was performed eight times, and in each case the juice obtained from a biscuit-fed dog which had been injected with an extract of the mucous membrane of a milk-fed dog contained lactase.

Here then at last we have some glimpse into the mechanism of the adaptation of the pancreas to the nature of the food. As the result of injection of lactose some substance which we may call x is produced in the mucous membrane of the small intestine. This substance is carried by the blood to the pancreas, and there slowly gives rise to the formation of lactase, which is turned out in the juice when secretion is excited by the entry of acid chyme into the duodenum. We have no knowledge as yet as to the nature of this substance x. All we can say is that it is destroyed at a boiling temperature, since boiled extracts of the mucous membrane of milk-fed dogs do not, when subcutaneously injected, cause the appearance of any lactase in the pancreatic juice of biscuit-fed dogs.

Table III.—Effect on Milk Sugar of Pancreatic Juice from *Biscuit-fed* dogs, which had received Subcutaneous Injections during 3 days of Extracts of the Mucous Membrane of *Milk-fed* dogs.

The figures represent c.c. of lactose solution which reduced 50 c.c. Pavy's solution.

Exp.	Controls.		Lactose + pancreatic juice.	Percentage of inversion.
	Solution of lactose.	Lactose + pancreatic juice (boiled).		
1....	7·4	..	6·8	18·1
2....	8·2	8·2	7·6	16·5
3....	8·2	8·15	7·85	9·7
4....	7·95	7·9	7·65	8·5
5....	7·8	..	7·5	8·8
6....	7·0	7·05	6·75	8·1
7....	4·1	..	3·75	20·8
8....	9·25	..	8·2	25·9

Whether the qualitative adaptation of the juice in respect of its trypsin, amylopsin, and steapsin is carried out in a similar fashion we cannot as yet say. We hope that an investigation of the mechanism of this adaptation, which is now proceeding, may throw light, not only on the factors involved, but also on the nature of the substance which is formed in the mucous membrane, and has this marked effect on the activity of the pancreatic cells. Involving, as it does, two distinct sets of cells, this chemical adaptation is more complex than any yet investigated, and shows the intimate relation which must exist between the chemical activities of very different organs of the body.

"Experiments on a Method of Preventing Death from Snake Bite, capable of Common and Easy Practical Application."

By Sir LAUDER BRUNTON, M.D., F.R.S., Sir JOSEPH FAYRER, Bart., K.C.S.I., F.R.S., and LEONARD ROGERS, M.D., B.S., etc., Indian Medical Service. Received February 22,—Read May 5, 1904.

Although this paper is a joint one, the authors wish to mention that each has had a different part in its production. The whole research may be fairly regarded as the natural outcome of the work begun in India nearly forty years ago by one of us (Fayrer), and this is the only ground on which his name can be associated with this paper. The instrument employed was designed by another of us (Brunton), and the actual experimental work was entirely carried out by a third (Rogers).

The first experiments on the use of permanganate of potash as an antidote to snake poison was made by one of us (Fayrer), in 1869, both by the local application of a solution and by injection into the veins,* on the ground of its being a chemical antidote. The animals experimented upon were dogs, but the permanganate of potash did not seem to have any power to avert the lethal action of the poison. It was shown also by Wynter Blyth† that Cobra venom when mixed *in vitro* with permanganate of potash becomes innocuous. His results were confirmed by two of us, who showed that some other substances had a similar power.‡ They tried by the injection of strong solution of permanganate of potash, and also by its local application to an incision made over the bite, to destroy the lethal action of Cobra poison previously injected, but their experiments were unsuccessful, the permanganate appearing to be unable to overtake the poison which had got the start of it.

In 1881 Messrs. Couty and Lacerda § made a number of experiments upon the effect of permanganate of potash on serpents' venom, and Lacerda found that permanganate of potash not only destroyed the lethal action of the venom when mixed with it *in vitro*, but also preserved life when a 1-per-cent. solution of permanganate was injected into the tissues close to the place where the venom had been

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* 'The Thanatophidia of India,' 1872, p. 95, by J. Fayrer, M.D., London, J. and A. Churchill.

† "The Poison of the Cobra," by A. Wynter Blyth, M.R.C.S., 'The Analyst,' February 28, 1877, p. 204.

‡ "Note on the Effect of Various Substances in Destroying the Activity of Cobra Poison," Brunton and Fayrer, 'Roy. Soc. Proc.,' June 20, 1878, vol. 27, p. 465.

§ Couty and Lacerda, 'Comptes Rendus,' vol. 92, p. 465.

previously injected, and also when both venom and antidote were injected directly into the vein. At the time of presenting his note to the Academy of Science in Paris, M. Lacerda was apparently unaware of the previous experiments by Blyth, Brunton and Fayrer. In a later publication* he discusses their experiments, but claims for himself to have scientifically demonstrated permanganate of potash to be a precious antidote to serpent venom, and to have brought it into common use, and thinks, therefore, that the priority belongs to him; but he was apparently unaware that instructions for its use with the ligature had many years before been promulgated by Fayrer in India.

In the winter of 1881 a number of experiments were made by Dr. Vincent Richards, who found, like the previous experimenters, that Cobra poison was completely destroyed by permanganate of potash when mixed with it *in vitro*, so that death did not follow the injection of the mixture either hypodermically or into a vein. He found also that when Cobra poison was injected into a dog, and the injection followed either immediately or after an interval of 4 minutes by a hypodermic injection into the same part of a solution of permanganate of potash no symptoms of Cobra poisoning resulted, but after the development of symptoms of Cobra poisoning permanganate of potash failed to have any effect whether injected locally or into a vein or both.

These results obtained both by Lacerda† and Richards seemed to give good hope that permanganate of potash might be used to lessen the appalling fatalities from snake bite in India, but it is evident that the hypodermic injection of a solution can never be widely employed because the hypodermic syringe is expensive, it is liable to get out of order just at the times that it is wanted and the solution may become dried or spilt or may not be available. It is evident that the first requisite for any antidote to snake poisoning is that it shall be always at hand; second, that it shall be easily applied; and thirdly, that it shall be cheap.

About two years ago one of us (Brunton) was asked on behalf of a young officer going out to India, to design an instrument which might be used in case of snake bite. He did so accordingly, and he has since had a similar one made for him by Messrs. Arnold and Sons which seems to combine the three requisites just noted. It consists of a lancet-shaped blade about half an inch long, long enough in fact to reach the deepest point of a bite by the largest snake. He has had some instruments made with a double edge like an ordinary lancet, and others with one edge sharp and the other edge blunt, so as to

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* Lacerda, 'Comptes Rendus,' vol. 93, p. 466.

† "O Veneno ophidico e seus antidotos," Dr. J. B. de Lacerda, Rio de Janeiro, Lombaerts, &c., 1881, p. 64.

press in the permanganate. The lancet is set in a wooden handle about an inch and a half long, which is hollowed at the other end so as to form a receptacle to hold the permanganate. Two wooden caps are fitted over the ends of the instrument, one to keep in the permanganate, and the other to protect the lancet. Such an instrument, if turned out in large numbers, could be sold at such a small price as to be within reach of even the Indian labourer, and might be sold everywhere in the same way as packets of quinine are at present.

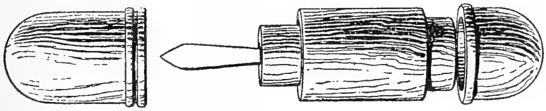

FIG. 1.—Lancet for use in snake bite, showing the steel blade, the cap which covers it, the hollow wooden handle for holding crystals of permanganate of potash, and the cover which retains them.

The plan now proposed is to make a free opening into the site of the bite, and to rub in crystals of permanganate. For this purpose the limb should be surrounded by a tight bandage above the bite, the puncture of the tooth or teeth should be freely cut into by the lance-shaped blade and the crystals of permanganate introduced and rubbed round. A few drops of saliva may be added.

To test the efficacy of the proposed plan several lethal doses of venom dissolved in a few drops of water, so as to resemble, as far as possible, the natural poison, are to be injected into the limb of an animal, a ligature placed round the limb above the seat of injection, an incision made, and crystals of permanganate placed in the wound, moistened and rubbed in.

Experimental Investigation, by Leonard Rogers.

In order to test in as practical a manner as possible the value of the suggestion of the two first-named authors of this communication, the following experiments were carried out at the Physiological Laboratory of the London University by the third-named author. In the first place it was necessary to ascertain if crystals of permanganate destroy the activity of other venoms besides that of the Cobra, for we are not aware that its action in this direction has been tested against any extensive series of snake venoms. As the value of the suggested treatment would evidently be greatly enhanced if the permanganate could be shown to act efficiently against every class of snake venom, a series of experiments were carried out to test this point. The venoms in solution were mixed with small quantities of a 10-per-cent. solution of pure crystalline permanganate of potash in 0·9 per cent. NaCl, and after given times the mixtures were injected into pigeons, several times a lethal dose of each venom being used, so that if recovery took place it would be evident that the permanganate had destroyed the activity of the poisons. The following table (p. 327) summarises the results of these experiments.

It will be seen that the table includes venoms of each main subdivision of snakes, namely, the two true vipers, the *Daboia Russellii* of India and the Puff Adder of Africa, the Pit Viper, the *Crotalus horridus*, the Colubrine snake the *Bungarus fasciatus*, and one of the Hydrophidæ or Sea-snakes, namely, the *Enhydrina bengalensis*. In the case of each ten or more lethal doses were neutralised by very small quantities of permanganate in solution, and in most of them twenty lethal doses were readily thus rendered harmless. The only failure was in Experiment 7, in which 32·2 milligrammes of *Bungarus fasciatus* venom was added to 25 milligrammes of permanganate of potash in solution, and in this case by far the greater part of the poison must have been neutralised, for in previous experiments one-eighteenth part of the venom per kilogramme, used in Experiment 7, killed a pigeon in 1 hour. Further experiments showed that 25 milligrammes of the permanganate of potash did entirely neutralise 16·1 milligrammes of *Bungarus fasciatus* venom. It is evident then that the salt will neutralise about its own weight of this venom, but that its power in this direction has a definite limit as might have been expected. It is clear, then, that this agent does act on every class of snake venom and renders them inert.

Owing to the limited time available and the small number of animals for which a license had been obtained, the actual experiments on the treatment after injection of the venoms have been so far limited to those of the Cobra as a typical representative of the Colubrine class, and of the *Daboia Russellii* as a common and deadly viper. Rabbits and cats were used in the investigation, the latter on account of their mixed diet and firmer tissues resembling more closely the human subject. The venoms were dissolved in as small a quantity of sterile normal saline solution (0·9 per cent. NaCl) as possible, so as to resemble in concentration the natural venom. The portion of the limb to be operated on was cleaned of hair by scissors beforehand (as the human subject is free from this obstacle to treatment). The strong solution of venom was then injected into the subcutaneous tissues of the cleaned part of a hind limb a little above the paw, as most snake bites in the human subject occur on the distal parts of the extremities. After a given measured time a ligature consisting of a piece of bandage was tied loosely round the thigh and twisted up tightly by means of a piece of stick or a pencil so as to temporarily stop the circulation through the distal part of the limb in order to

Table I.

Number of experiment.	Weight of pigeon.	Dose in milligrammes.	Number of lethal doses.	Dose per kilogramme weight.	Amount of 10 per cent. K_2MnO_4.	Time mixed before injection.	Symptoms.	Result.
				I. Daboia Venom.				
	grammes.			milligrammes.	c.c.	mins.		
1..........	350	3·5	3	10	0·5	30	Nil	Recovered.
2..........	320	6·2	7	20	0·5	10	Do.	Do.
3..........	280	14	17	50	0·5	10	Do.	Do.
				II. *Cretalus horridus* Venom.				
4..........	300	15	10	50	0·5	10	Do.	Do.
				III. African Puff Adder.				
5..........	270	21·6	20	80	0·25	5	Do.	Do.
6..........	300	12	10	40	0·25	5	Do.	Do.
				IV. *Bungarus fasciatus.*				
7..........	230	32·2	20	140	0·25	5	Colubrine	Died in 2 hours.
8..........	230	16·1	10	70	..	10	Nil	Recovered.
				V. *Enhydrina bengalensis.*				
9..........	250	25	20		0·25	10	Do.	Do.

check further absorption of the poison. An incision was then made in the long axis of the limb over the seat of injection of the poison, and the edges dissected up slightly on either side so as to fully expose the affected tissues and to form a small pocket, into which the crystals of permanganate were next placed, and after moistening with a few drops of sterile normal salt solution (water, or even saliva, would serve in an emergency) they were well rubbed in until the exposed tissues presented a uniformly blackened appearance. About 3 minutes were usually occupied by the little operation, on the completion of which the ligature was released and a dressing and bandage applied to the wound. The animals were under chloroform throughout the operation, including the injection of the venom. The amount of permanganate held by the instrument made for these experiments was $\frac{1}{4}$ gramme, this quantity being used in each of the experiments.

The results of the experiments so far performed may most conveniently be summarised in the following table, by means of which they may readily be studied. The actual doses of venoms injected are given in Column 4, and the dose per kilogramme weight in Column 5. The time which was allowed to elapse after the injection of the poison before the application of the ligature (Column 6) was usually $\frac{1}{2}$ minute, which it was calculated would be sufficient to allow a handkerchief, or in the case of a native a strip of a pugari or of the cotton garments commonly worn by the poorer classes in the tropics, being tied round the limb and twisted up to form an efficient ligature. In a few of the later experiments this application of the ligature was delayed for 5 and 10 minutes. In Column 8 the time is shown which was taken over the operation from the application to the release of the ligature, while the ultimate result is shown in Column 9. In most of the control experiments a ligature was applied round the thigh for about the same time as in the operations, as it appeared possible that the ligature might delay somewhat the absorption of the poison, although it could scarcely affect the ultimate result of its action, owing to the poison being an essentially cumulative one.

The first six experiments of Table II were performed on rabbits, with the result that only prolongation of life was obtained. Thus, after a dose of 10 milligrammes per kilogramme (Experiment 1), death took place only a little quicker than after one-tenth of this dose in a control animal (Experiment 5). Again 5 milligrammes per kilogramme in a treated animal caused death in $3\frac{1}{2}$ hours (Experiment 2), but 0·5 milligramme per kilogramme in a control killed in the same time (Experiment 6). The rapidity of death in this last animal shows that 0·5 milligramme per kilogramme is still much above the minimal lethal dose of Cobra venom for rabbits, so that the doses used in the treated cases were many times a lethal dose (about five to fifty times),

Table II.—Experiments with Cobra Venom.

No.	Animal.	Weight.	Actual dose.	Dose per kilogramme.	Time of ligature.	Amount of permanganate.	Ligature released after.	Result.
		kilogrammes.	milligrammes.	milligrammes.	seconds.	grammes.	mins.	
1	Rabbit	1	10	10	30	0·25	2	Died, 1 hr.
2	,, ...	1½	7·5	5	..	..	2½	,, 3½ ,,
3	,, ...	1½	3·75	2½	..	..	3½	,, 3½ ,,
4	,, ...	2¾	2·75	1	..	..	3	,, 3½ ,,
5	,, ...	1·8	1·8	1	..	Nil (control)	..	,, 1½ ,,
6	,, ...	2½	1·25	0·5	30	,,	3½	,, 3⅓ ,,
7	Cat	1¾	17·5	10	..	0·25	3	Recovered.
8	,, ...	3	30	10	..	Nil (control)	3½	Died, 3 hrs.
9	,, ...	3	15	5	..	0·25	3	Died, 30–38 hrs.
10	,, ...	2¾	13·75	5	..	..	3½	Recovered.
11	,, ...	4	20	5	5 minutes	..	3½	,,
12	,, ...	3	15	5	..	Nil (control)	3	Died. 28 hrs.
13	,, ...	3½	10·5	3	10 minutes	0·25	3¼	Recovered.
14	,, ...	2	4	2	5	..	3	,,
15	,, ...	3½	3½	1	..	Nil (control)	..	Died, 50 hrs.

and were thus mostly proportionally larger doses than a Cobra could eject in the case of a man. The tissues of a rabbit are also more delicate than those of a cat or of a man, so that absorption of the poison may be unusually rapid in rabbits, which are extremely susceptible to snake venoms.

Turning next to the results of the experiments on cats, much more satisfactory results were obtained. Thus, the control experiments showed that 1 milligramme per kilogramme produced death in 50 hours, this being the minimal lethal dose of the Cobra venom used in these experiments for cats (Experiment 15). A dose of 5 milligrammes per kilogramme caused death in 28 hours, the time having probably been prolonged by the application of a ligature after the injection (Experiment 12). A dose of 10 milligrammes per kilogramme proved fatal in 3 hours, although a ligature had been applied as in the treated cases (Experiment 8). On comparing the result of treated cases with the above control we find only one death occurred in six experiments. The one fatal result took place after a dose of 5 milligrammes per kilogramme (Experiment 9), this having been the first case treated, in which the permanganate was not as thoroughly rubbed in, and the site of injection was not as completely exposed as in later experiments, and in this case death did not take place until over 30 hours. On the other hand, in Experiment 7 recovery took place after 10 milligrammes per kilogramme (ten lethal doses), while in two other cases recovery took place after five lethal doses had been injected, in one of which (Experiment 11) 5 minutes were allowed to elapse before the treatment was carried out, while in Experiment 13 recovery ensued from lethal doses treated 10 minutes after injection.

The above results are very encouraging, for it appears from D. D. Cunningham's observations that the average amount of venom ejected by a full-sized Cobra is not more than ten lethal doses for a man, while other writers give much smaller amounts. Further, in many cases, the full dose will not actually be injected into the human tissue for various reasons.

In Table III a similar series of experiments with Daboia venom are summarised. Here again, in the case of rabbits, only very marked prolongation of life was obtained, although the dose used in Experiment 17 was less than four lethal doses, so that it is clear that in the case of rabbits the method was not very successful.

On the other hand, the experiments with cats were as successful as in those of the Cobra series given above; for only one of the six cases treated with permanganate died, and in this instance (Experiment 21) the very large dose of 50 milligrammes per kilogramme was injected, and the treatment was delayed for 5 minutes. This dose is probably relatively larger than could be injected by any known viper in the case of a full-grown man. Further, in this case death did not take place until upwards of 24 hours after the injection, while in a control experiment with the same dose (Experiment 22) a fatal result occurred in 4 hours. Further, with the same large dose recovery took place when treatment was carried out $\frac{1}{2}$ minute after injection. Again, 30 milligrammes per kilogramme (three lethal doses) killed a control cat in $4\frac{1}{2}$ hours, but in three cases treated $\frac{1}{2}$, 5 and 10 minutes respectively after injection all recovered, as did one after 10 milligrammes per kilogramme, although a control with this last dose died in 30—40 hours. In all the experiments of both series the recovered animals were alive and well 5 days and upwards after the injection of the venoms, which is 2 days longer than death has ever taken place in any of the control animals.

The above results are very encouraging, as the Viperine poisons are much less powerful, weight for weight, than are most of the Colubrines and Hydrophidæ, so that the amount of venom ejected by them can seldom, if ever, be more than two or three times a lethal dose for man.

In the course of the experiments it was observed that, even when the incision was made only 30 seconds after the injection of the poison into the subcutaneous tissues, a distinct blood-stained effusion is found, which serves as a very useful guide to the location and limits of the injected poison; after 5 or 10 minutes the effusion is more extensive, and in these cases the incisions were prolonged up the limb for about 2 inches in order to try and destroy as much of the venom as possible. The fact that as favourable results have been obtained after 5 minutes as after $\frac{1}{2}$ minute, may very possibly depend on the effusion noted materially checking the absorption of the poisons, so that at the end of that time the rate of absorption may become very much less rapid than during the first few seconds after its injection. That a very rapid absorption occurs during the first few seconds after the injection (probably on account of the action of the poison in preventing clotting of the blood locally) is certain, for it was shown by Fayrer many years ago that a dog bitten in the tail by a full-sized Cobra died in spite of the tail being cut off between the bitten part and the body a few seconds after the bite. In such cases, however, the dose received is relatively much larger than could be injected by a Cobra in the case of such a large animal as man, so that in practise (except in the very rare cases where the poison is injected directly into a vein) a fatal dose may not enter the system for some considerable time after the bite. This probability is supported by the fact that, in the case of Colubrine poisons at any rate, the minimal lethal dose is the same whether the venom is given subcutaneously or intravenously, yet it takes 1 or 2 days to produce death when injected under the skin, but only 5—20 minutes when inserted into a vein, so that under the former conditions the whole of the poison does not enter the circulation for a long period. These facts

Table III.—Experiments with Daboia Venom.

No.	Animal.	Weight.	Actual dose.	Dose per kilogramme.	Time of ligature.	Amount of permanganate.	Ligature released after.	Result.
		kilogrammes.	milligrammes.	milligrammes.		gramme.	minutes.	
16	Rabbit	2	100	50	30 secs.	0·25	3½	Died, 9—17 hrs.
17	,, ...	2	20	10	30 ,,	0·25	4	,, 26 ,,
18	,, ...	2	20	10	..	Nil (control)	..	,, 3 ,,
19	,, ...	2¾	6·875	2·5	30 ,,	,,	2½	Recovered.
20	Cat..........	1¾	87·5	50	30 ,,	0·25	3	,,
21	,, ...	2	100	50	5 mins.	0·25	3½	Died, over 24 hrs.
22	,, ...	2½	125	50	30 secs.	Nil (control)	3	,, 4 ,,
23	,, ...	2	60	30	30 ,,	0·25	3¼	Recovered.
24	,, ...	1½	45	30	5 mins.	0·25	3½	,,
25	,, ...	3	90	30	10 ,,	0·25	1½	,,
26	,, ...	2	60	30	30 secs.	Nil (control)	3	Died, 4½ hrs.
27	,, ...	2	20	10	30 ,,	0·25	4	Recovered.
28	,, ...	3	30	10	30 ,,	Nil (control)	3	Died, 30–40 hrs.

suggest the hope that the method of treatment here advocated may produce good results even when it is not put into operation until considerably longer periods than in any of the above experiments, especially when only slightly *supra*-minimal lethal doses have been received into the tissues.

Conclusions.

Further experiments will be necessary to ascertain the exact limits of the value of this form of treatment, and they will be undertaken immediately by one of us (Rogers) in India, fresh venoms being tried, as it is possible that they may be more rapidly absorbed than those which have been dried and redissolved. We think, however, that the results reported in this communication are sufficiently promising to make it advisable to place them on record, with a view to a trial being given to the method in suitable cases, especially as the crystals of permanganate of potash are actively antiseptic without acting as more than a superficial escharotic, so that the treatment has no markedly injurious effect which can be weighed for an instant against the terrible results of bites by venomous snakes. The process here recommended has already yielded experimental results far in advance of anything hitherto attained.

It is worthy of note that the earlier experiments of the first two authors were stopped nearly 30 years ago by the passing of the Act for regulating experiments on animals in England, but for which this logical sequence of their earlier work might very probably have been made many years ago.

"The Effects of Changes of Temperature on the Modulus of Torsional Rigidity of Metal Wires." By Frank Horton, D.Sc., B.A., St. John's College, Cambridge; 1851 Exhibition Research Scholar of the University of Birmingham. Communicated by Professor J. J. Thomson, F.R.S. Received March 17,—Read April 28, 1904.

(Abstract.)

This paper contains an account of some experiments performed with the object of ascertaining, as accurately as possible, the manner in which the modulus of torsional rigidity varies with the temperature. The law governing this variation has been expressed in several different forms by those who have investigated it, but all the more recent experimenters have been content with finding the modulus at two temperatures only and assuming a linear law to hold between them. The first part of the paper contains a short account of the results obtained by previous investigators, and possible sources of error in the methods employed are pointed out. The rest of the paper is divided into the following sections:—

(1) Description of apparatus, etc.
(2) Account of the experiments.
(3) Summary of results and comparison with those of other observers.
(4) Determination of the coefficients of expansion of the wires.

The metals experimented on were copper, iron, platinum, gold, silver, aluminium, tin, lead, cadmium, all chemically pure, and also specimens of commercial copper and of steel pianoforte wire. The wire sused were of approximately the same length and diameter, and were carefully annealed before the rigidity determinations were begun.

The method of experimenting employed was a dynamical one, the torsional oscillations of the wire under test being timed by a method of coincidences capable of great exactness. Some of the observations made in the course of this work yielded what seemed to be interesting information as to the internal viscosities of the wires used. This is recorded in the paper and compared with similar observations by other experimenters.

The vibrator generally used was a circular disc of gunmetal, and this was completely enclosed with the wire in a heating jacket, the temperature of which could be varied as required. Observations of the period of torsional vibration were made, in general, at five temperatures, viz., at the temperature of the room, about 16° C., at 35° C., 55° C., 75° C., and 100° C., and also in some cases at 126° C., the higher temperatures being obtained by using the vapours of various liquids boiling under atmospheric pressure.

The method of coincidences used in timing the torsional vibrations consists in observing the reflections of a vertical flash occurring once a second, in two mirrors, one of which is fixed in position, and the other is attached to the vibrator and vibrates with it, swinging just above the fixed mirror and being parallel to it when at rest. The reflections are observed by means of a telescope, in the field of view of which, in general, two flashes are seen, one always occurring in the same position, and the other appearing in different parts of the field according to the position of the moving mirror at the instant the flash occurs. If a second signal happens exactly when the two mirrors are parallel, the two flashes coincide, and it is from these "coincidences" that the time of vibration is obtained. The method of coincidences is usually only applied to the comparison of two nearly equal times, but it is shown in the paper to be equally applicable to any two periods even if they are quite different.

In order that the observed periods at different temperatures may be comparable, it is necessary that they should be corrected for the increased length and radius of the wire, and also for the expansion of the vibrator at higher temperatures. For this purpose the coefficients of expansion of the wires, and of a gunmetal bar cast at the same time as the vibrator, were determined by means of the measuring bench in the Physical Laboratory of the University of Birmingham, which Professor Poynting kindly placed at my disposal. A description of the instrument (which has not otherwise been described), and the results of the experiments, are given in Part 4 of the paper.

In addition to the determination of the periods of vibration, observations of the logarithmic decrement of the amplitudes of the oscillations were taken at each temperature, and thus the effect of temperature on the internal viscosity of the wires was observed. At the end of the rigidity determinations for each wire, a series of observations was usually taken to ascertain the manner in which the logarithmic decrement and the torsional period varied with the amplitude of vibration, amplitudes up to about 10° being used. The main observations for the rigidity determinations were all taken at the constant average amplitude of 14′.

The following is a summary of the principal results :—

1. In all the materials examined, with the exception of pure copper and of steel, the modulus of rigidity at one temperature is not constant, but increases as time goes on. The rate of increase of rigidity with time is greater the higher the temperature, and repeated heatings to the same temperature gradually lessen the rate of alteration with time at that temperature, but even in the course of months of experimenting the increase of rigidity with time cannot be entirely eliminated.

2. The diminution of the modulus of rigidity per degree rise of

temperature between 10° C. and 100° C. is constant for pure copper and for steel, but not for any of the other materials examined.

3. In the case of the metals iron, gold, tin, lead and commercial copper, the rigidity temperature curve is of such shape as to suggest that if it were possible to obtain the values of the rigidity modulus at the different temperatures, all within a very short space of time (so as to avoid the "time effect"), the resulting curve would be a straight line.

4. In the case of the metals platinum, silver, aluminium, the shape of the rigidity-temperature curve shows that the effect of the gradual increase of rigidity with time is such as to make the alteration with temperature approximate more closely to a linear law than would be the case if the observations were all taken within a very small interval of time. For these metals, therefore, the decrease of torsional elasticity per 1° C. rise of temperature increases with the temperature.

5. In general, the effect of heating to a high temperature is to increase the value of the rigidity modulus at lower temperatures. (This applies even to pure copper, of which the modulus of rigidity at the ordinary laboratory temperature is slightly greater after the wire has been heated to higher temperatures. The rigidity of steel was quite constant, and with silver the value of the modulus at the temperature of the laboratory in the last few experiments was unaltered by a temporary increase of temperature. Tin was the only case in which the rigidity modulus at the ordinary laboratory temperature was lessened by heating.)

6. The internal viscosity of all the metals examined, with the exceptions of soft iron and steel, increases with the temperature. This increase varies very much with different metals, being greatest with aluminium and least with platinum. The internal viscosity of soft iron decreases rapidly with rise of temperature and reaches a minimum value at about 100° C. There is a slight decrease also in the case of steel.

7. Repeated heating and continued oscillation through small amplitudes decrease the internal friction.

8. Both the internal friction and the period of torsional vibration increase with the amplitude of oscillation. The increase is generally greater the higher the temperature of the wire. It is least in the case of steel and is small in the case of soft iron.

9. Vibration through a large amplitude considerably alters both the logarithmic decrement and period of oscillation at smaller amplitudes. The nature of the alteration varies with different metals, being in some cases an increase and in some a decrease.

10. The internal viscosity of a well-annealed wire suspended and left to itself gradually decreases.

11. The internal viscosity of an unannealed wire is enormously reduced by annealing.

"The Sparking Distance between Electrically Charged Surfaces.—Preliminary Note." By P. E. Shaw, B.A., D.Sc. Communicated by Professor J. H. Poynting, F.R.S. Received March 22,—Read April 28, 1904.

Lord Kelvin* first made systematic measurement of the relation between potential difference and sparking distance. He noticed that these factors do not vary in proportion and he surmised that this might be due to "the air near dense bodies being condensed and so becoming a better insulator."

Later G. A. Liebig† published results bringing voltages used to as low as 800, the discharge distance being 67 micra. Both the above observers used frictional machines to produce E.M.F. and worked in electrostatic units.

R. F. Earhart‡ made a new departure, using a continuous current from accumulators, and obtained consecutive results with voltages from 1000 to 38; the corresponding sparking distances, ranging from 100 μ to $\frac{1}{4}$ μ, were measured by Michelson's interferometer.

One very interesting result of his research was that the potential gradient, dv/dx, suddenly changes when the sparking distance is about 2 μ (see fig. 2); for greater distances dv/dx is 7 (volts per micron) whereas for less distances it is 200. Thus a distinct "knee" is formed in the curve connecting V and x. I have suggested an explanation of this.§

It has been shown that the thickness of the condensed water film on solid surfaces at ordinary pressure and temperature is about 0·8 μ. If two surfaces approach one another to a distance of 2 μ then, deducting 0·8 μ for the film on each surface, we have an air distance left of only 0·4 μ, which would be easily bridged by small vibrations of the surfaces, and the bridge would be stable on account of capillarity. Thus any discharge would take place through the water film and not through air. The film on recently cleaned metal would be very pure and would have a dielectric strength greater than air (from the numbers quoted it appears to be thirty times as great), hence the sudden rise in dv/dx.

Mr. Earhart tried the effect of increasing and decreasing the atmospheric pressure. Increase in pressure causes small increase in dv/dx below the "knee" and large increase above the "knee"; but what is more important to notice is that increase in pressure causes the "knee" point to move up the curve corresponding to greater sparking distance. These last results support the theory

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* Thomson, 'Roy. Soc. Proc.,' 1860.

† 'Liebig, 'Phil. Mag.,' vol. 24, 1887.

‡ Earhart, 'Phil. Mag.,' vol. 1, 1901.

§ Parks, 'Phys. Soc. Proc.,' Discussion, p. 418, 1903.

put forward, for the film would increase and decrease in thickness directly with pressure.

W. R. Carr* made similar investigations, but as he worked with reduced pressures and his discharge distance was never less than 1 mm., whereas mine is never greater than 1 μ, our work does not overlap.

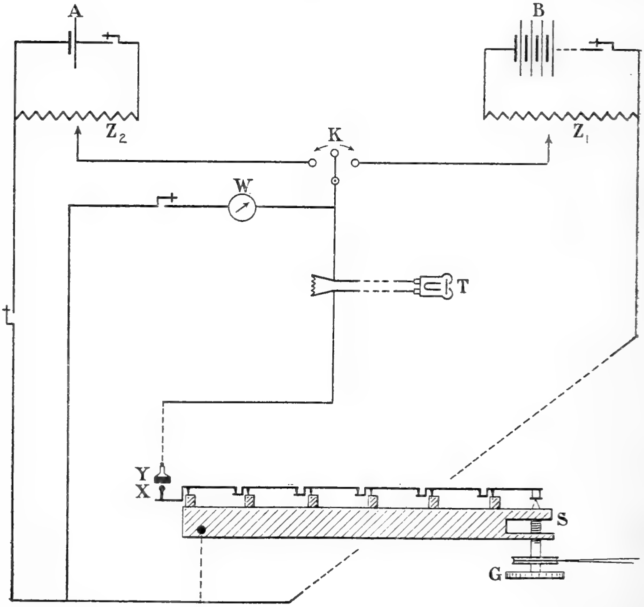
Suppose two surfaces having a constant potential difference (V) approach one another, the tension between the charged surfaces at last breaks down the medium when the distance apart is x. The space between the contact surfaces may be occupied by one or more media, each medium having dielectric strength, either constant or varying from point to point. Two cases concern us—

1. One medium, uniform in dielectric strength; we can express the critical potential in terms of potential gradient. The curve between V and x is a straight line to the origin.

2. Two or more media uniform in dielectric strength. The curve is a succession of straight lines.

Referring to fig. 2 it will be seen that Earhart's curve is composed of two *straight* portions. Hence on the above hypothesis the water film has a constant dielectric strength and so has the air outside, the strength of the former being about thirty times that of the latter.

Fig. 1.



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* 'Phil. Trans.' vol. 201, 1903.

I have investigated this subject by means of the electric micrometer,* which is specially well adapted for the purpose, as it measures by the principle of electric touch. Briefly expressed, it has a fine micrometer screw S (fig. 1) with a graduated wheel G; and a system of six levers is arranged to minify the movement of the screw point, thus 1 μ movement of the screw-head becomes 1 $\mu\mu$ or less of the point X. A fixed surface Y is arranged near the adjustable one X, and when X and Y touch a circuit is completed as shown through the telephone T. To find the position of contact the observer works the screw S, watches the wheel G, and listens at T. The spark gap is across XY. A battery B capable of giving 200 volts has a potential divider Z_1, which enables us to apply to XY any required voltage, which is read on the voltmeter W. A key K enables us to use either the large voltage from B on a small voltage ($\frac{1}{20}$ volt) from A, also provided with a potential divider Z_2. To make an observation apply the small voltage from A, ascertain the position of contact by G and separate the contacts XY. Now apply the large voltage from B and slowly bring the contacts together until discharge occurs, as observed in the shunted telephone. G is again read. The results follow (p. 341).

The table shows Earhart's results with mine. There are some points of interest:—

1. The last two columns show potential gradients. There does not appear to be any change in gradient nearer the origin than 2 μ, hence we infer that there is no inner film or change in dielectric strength.

2. In working the electric micrometer I commonly use a potential difference across the contacts of $\frac{1}{20}$th volt.; it might be supposed that this voltage would produce irregular discharge and make the readings of the instrument uncertain. The table shows that the sparking distance would be about $\frac{1}{2}$ $\mu\mu$. So that even if there were a large percentage error in this distance the errors in measurement would be insignificant.

The curves are shown in fig. 2. The curves on the left side indicate good agreement between the two observers, where they overlap.

We should expect more regular results when there is a pure liquid between the surfaces than when there is an air gap; for dust particles which would interpose in the air could not break into the liquid. Earhart's results show great difference in regularity on the two sides of the "knee." The curves on the right are on an open scale to show my results better.

The potential gradient for Earhart's results is 200—for mine 150, the curves crossing one another for voltage 60.

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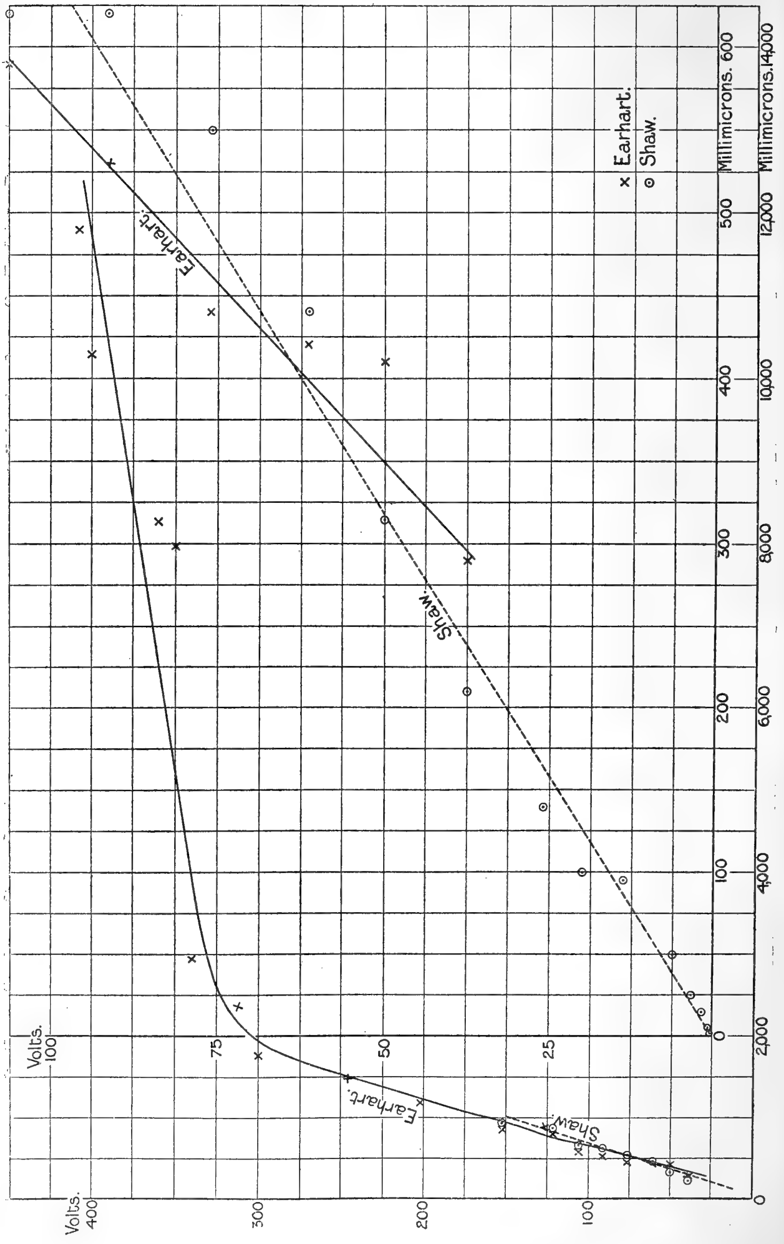
* For an account of this instrument, see Shaw, 'Phil. Mag.,' December, 1900; March, 1901; 'Electrician,' February, 1901.

FIG. 2.


× Earhart.
⊙ Shaw.
Earhart.
Shaw.
Earhart.
Shaw.
Volts.
400
300
200
100
0
Volts.
100
75
50
25
Millimicrons.
0
100
200
300
400
500
600
Millimicrons.
2,000
4,000
6,000
8,000
10,000
12,000
14,000

V (volts).	Millimicrons.		Volts per micron.	
	Earhart.	Shaw.	Earhart.	Shaw.
408	11800	..	7	
400	10300	..	”	
360	8260	..	”	
350	7960			
344 336 >	2950			
312	2360			
300	1770		200	
240 250 >	1480	..	”	
202	1180	..	”	
152	880	950	”	150
121	800	860	”	”
106	590	700 650 > 670	”	”
91	530	600 650 > 620	”	”
76	440	550	”	”
61	420	440	”	”
50	410	300 330 > 315	”	”
38	290	210	”	”
26	..	140	..	”
20	..	100	..	”
13·5	..	95	..	”
6	..	50	..	”
3	..	25	..	”
1·5	..	15	..	”
0·6	..	5	..	”
0·5	..	5 4 > 4·5	..	”
0·2	..	2 3 > 2·5	..	”

Details.

Previous observers have been careful to have plain or spherical surfaces opposed so that the conditions of discharge should be definite. But when the discharge distance is, as in this case, always less than a micron and sometimes only a few millimicrons, the form given to the surface is of little importance, for in a polished surface there will exist irregularities on each surface of the order of the distance between the surfaces. In that case discharge will occur from a point on one surface to the nearest point on the other surface. I generally used a bead of iridio-platinum (1 mm. diameter), and a plane of iridio-platinum, both highly polished.

In using high voltage the surface must be repolished after every discharge, but for small voltage (less than 5) the surface will not be so much shattered and can be used two or three times before repolishing.

I made special tests with a copper bead and copper plate, but there was no change in results. Also using a copper point and copper plane the gradient came out about half as much as indicated in the table above. Again, when using a bead and disc, it is found to be immaterial which is positive and which negative, so that for small distances there does not appear to be a different positive and negative tension as was found by Faraday for large distances.

I hope to continue the research, varying the conditions. My best thanks are due to the Royal Society for grants in aid of research extending over the last three years.

I wish also to express my great obligation to Professor W. H. Heaton for granting me every facility in my investigation.

"Further Note on some Additional Points in Connection with Chloroformed Calf Vaccine." By Alan B. Green, M.A., M.D. (Cantab.). Communicated by Dr. W. H. Power, C.B., F.R.S. Received April 19,—Read May 5, 1904.

(From the Government Lymph Laboratories.)

Since April, 1903, the date of my preliminary note* on this subject, the preparation of calf vaccine by the chloroform process has been carried on in at these laboratories and a large number of vaccines have now been treated by this method. These lymphs have been freed from their non-sporebearing extraneous bacteria within a period ranging between 1 and 8 hours after their collection from the calf, and have, subject to the usual tests, been issued for general vaccination purposes about 2 weeks after collection. Their use has resulted in high "case" and "insertion" success.

The following points in connection with these vaccines have been investigated.

The Effect of Temperature in the Elimination of Extraneous Micro-organisms from Crude Calf Vaccine by the Chloroform Process.

It has been ascertained that the temperature at which vaccine emulsion is subjected to the chloroform process determines largely the rate at which the extraneous bacteria of that emulsion are eliminated. This has been shown in two ways.

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* 'Roy. Soc. Proc.,' 1903.

In the first set of experiments vaccine emulsion, consisting of one part by weight of pulp and two parts by weight of water, was divided into several equal portions. Each portion was then submitted to a pre-determined and separate temperature along with the set of apparatus to be used for its treatment with chloroform. These temperatures ranged from 10° C.—37° C. In each case after both emulsion and apparatus had reached the requisite temperature of the experiment, passage of chloroform vapour and air through the emulsion was effected.

It was found by means of plate cultivations, that elimination of extraneous micro-organisms was most rapidly effected in the case of the emulsions chloroformed at 37° C., and that elimination was most slowly effected in the case of the emulsions chloroformed at 10° C. Between these temperatures the gradation of germicidal action was very constant.

The potency of these vaccines was subsequently tested by inoculations on calves, with the result that vaccines which had been subjected to the chloroform process at temperatures below 30° C. caused rather better vesiculation than the vaccines treated at temperatures above 30° C.

In the second set of experiments, vaccine emulsion, prepared as before, was divided into a number of equal portions, each of which was placed in a separate test-tube. In this case, the emulsions were all subjected to the chloroform process at 15° C. After the passage of chloroform vapour and air through the emulsions for half an hour, the entrance and exit of each vaccine tube were clamped in order that none of the contained chloroform might escape, and the tubes were placed in temperatures ranging from 10° C—37° C. for 24 hours. In the case of these emulsions, where the amount of chloroform present in each must have been approximately the same, a similar gradation of germicidal action was evidenced as in the former set of experiments; elimination occurring most rapidly in the case of emulsions submitted to a temperature of 37° C., and most slowly in the case of emulsions submitted to a temperature of 10° C.

The potency of each of these vaccine emulsions was tested as before by inoculations on calves, with the result that vaccines which had been submitted to temperatures below 30° C. gave slightly better vesicles than vaccines which had been submitted to temperatures above 30° C.

Both series of experiments indicate that the temperature at which extraneous bacteria are killed most quickly in vaccine emulsion by the chloroform process, the specific germ being left meanwhile in state of full activity, lies probably between 18° C. and 23° C.

The Species of Micro-organisms Killed by the Chloroform Process.

In addition to the extraneous micro-organisms mentioned* as commonly occurring in crude calf vaccine and as being eliminated therefrom by means of chloroform, experiments have been made with further species of bacteria. These, with the exception of *B. proteus vulgaris* and *B. coli communis*, have never been found in vaccine lymph at these laboratories, but they have been subjected to the action of chloroform in order that knowledge of the germicidal value of that process might be extended. The bacteria thus experimented with are :—*B. proteus vulgaris*, *B. prodigiosus*, *B. pyocyaneus*, *B. fluorescens liquefaciens*, *B. coli communis*, *B. typhosus*, *B. diphtheriæ*, *B. mallei*, *B. pestis*, *B. tuberculosis*, *and Spirillum choleræ Asiaticæ.*

Broth emulsions of these bacteria were first experimented with. As soon as an emulsion was established a suitable culture medium was inoculated with a small portion of it and duly incubated, in order to ascertain whether the species of micro-organism in question could be easily recovered from the emulsion. The passage of chloroform vapour and air through the emulsion was then begun, and during the process at regular intervals further cultivations were made from it. In every instance the bacteria of experiment were found to have been killed, at intervals ranging from 1—8 hours from the commencement of the process.

In a second set of experiments vaccine emulsions were prepared by mixing one part by weight of pulp with two parts by weight of water. To these emulsions the bacteria to be experimented with were added, and cultivations at once made therefrom in each instance, to ascertain whether recovery of the bacteria was possible. Passage of chloroform vapour and air through the emulsions was then carried out and further cultivations made from them from time to time. As in the case of the broth emulsions, after the passage of the chloroform vapour and air for a few hours, the bacteria in the vaccine emulsions were killed.

In the case of each experiment of the two foregoing series, a "control" preparation was established in which the bacteria were found to be still alive after the bacteria of the corresponding experimental preparation had been killed.

The vaccine used in these experiments was collected for experimental purposes only.

The Keeping Properties of Chloroformed Vaccines.

The keeping properties of chloroformed vaccines have been tested in the following ways:

First, vaccine lymph freshly collected from the calf was divided into

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* 'Roy. Soc. Proc.,' 1903.

two portions. One portion after emulsification with water was subjected to the chloroform process and, after elimination of its extraneous micro-organisms, the chloroform was partly removed by the passage through the emulsion of a stream of sterile air. The other portion was emulsified with 50 per cent. of glycerine and water solution. These chloroformed and glycerinated emulsions were, within 24 hours of their collection from the calf, each sub-divided into five parts, one part of each being stored at 10° C., 20° C., 25° C., 30° C., and 37° C. The potency of these several parts was subsequently tested on calves at regular intervals of time. The result showed: (*a*) that the highest potency was retained for the longest time, alike for the chloroformed and glycerinated portions, by the emulsions stored at 10° C., and for the shortest time, alike again by both portions, by emulsions stored at 37° C.; and (*b*) that the potency as between the chloroformed and glycerinated vaccine kept at any one of the foregoing temperatures was practically the same, the only marked difference occurring in case of the vaccines kept at 37° C., a temperature at which the chloroformed emulsions retained potency for a longer period than did the glycerinated emulsions.

Upon the completion of the foregoing experiments, a further number of vaccines collected from vesicles of good average quality were emulsified with water in the proportion of one part by weight of pulp to two parts by weight of water, and subjected as before to the chloroform process. When elimination of their extraneous bacteria had been effected, part of the chloroform was removed from the emulsions and these were placed in the ice chest at 10° C. Immediately prior to the issue of these vaccines, the remainder of the chloroform was removed and glycerine added in the proportion of two parts by weight of glycerine to the original weight of vaccine pulp. These vaccines were later, after passing the requisite tests, issued for general vaccinating purposes, a similar interval being allowed to elapse between their collection from the calf and subsequent issue as is usual at these laboratories in the case of glycerinated vaccine, namely, about 6 weeks. In the case of two of these chloroformed vaccines an interval of 2 months elapsed between collection and issue.

The use of these chloroformed vaccines has been attended with results showing high "case" and "insertion" success.

Summary.

Experience of the further use of the chloroform process in the preparation of a large number of vaccines during the past year confirms the conclusions arrived at in a former paper.* And mean-

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* *Loc. cit.*, 1903.

while important additional knowledge has been gained, namely, that chloroformed calf vaccine, if originally of sufficiently high potency, will, when prepared and stored under suitable conditions, retain for a considerable time a high degree of potency, and this notwithstanding that the extraneous organisms had been rapidly eliminated from it in an early stage of its preparation.

"Further Experiments on the Production of Helium from Radium." By Sir WILLIAM RAMSAY, K.C.B., F.R.S., and FREDERICK SODDY, M.A. Received April 14,—Read April 28, 1904.

The research, of which a preliminary account has already been given in the 'Proceedings,' vol. 72, pp. 206 and 208, has been continued with the view of ascertaining the volume of emanation produced in a given time from a known weight of radium in the form of bromide, and also the quantity of helium resulting from the spontaneous change of the emanation.

Owing to the minute quantity of material at our disposal, the research has been a somewhat tedious one; but we have succeeded in obtaining fairly concordant measures of the volume of both emanation and helium. The present paper gives a description of the apparatus employed, the methods of experiment, and the quantitative relation between radium and its products.

The inactive nature of the emanation from thorium was the subject of an investigation by Rutherford and Soddy.* They concluded "that it is a chemically inert gas analogous in nature to the members of the argon family." And they continue: "The speculation naturally arises whether the presence of helium in minerals and its invariable association with uranium and thorium may not be connected with their radio-activity." The discovery was thus the subject of prediction. It followed an attempt to obtain the spectrum of the emanation. Thinking that the spectrum, if brilliant, might be observed by mixing the emanation with a gas of simple spectrum, the first experiments were made by mixing it with helium; but it soon became evident that the helium spectrum overpowered that of the emanation to such an extent as to mask it entirely. And experiments on the removal of gases not belonging to the argon group from the emanation convinced us that its quantity was so small as to require special contrivances in order to deal with it. All apparatus, consequently, was constructed on a minute scale of capillary tubing, less than half a millimetre in

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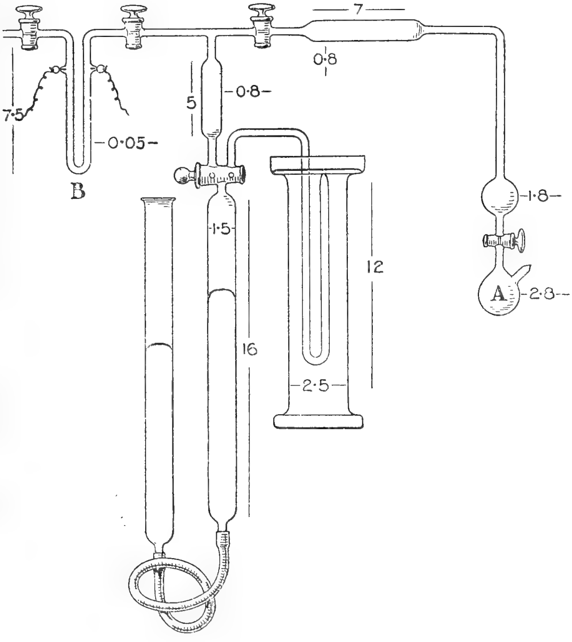
* 'Phil. Mag.,' 1902, 6, vol. 4, p. 581.

diameter. Approximate measurements of the scale of the apparatus in centimetres are given in the sketches.

Fig. 1 gives a sketch of the first apparatus, which was soon abandoned; suffice it to say that an attempt was made to accumulate the emanation in A, which contained a solution of several grammes

FIG. 1.



of impure chloride, obtained from very impure carbonate, and to examine its spectrum in the U-tube B, made of capillary tubing, with electrodes of platinum as shown. The spectrum was that of carbon monoxide and dioxide.

Experiment 1.—A brief description of this experiment has already been given.* Twenty milligrammes of radium bromide was dissolved by admitting water boiled *in vacuo* to the crystals in the bulb A (fig. 1), which had previously been freed from air with the pump. The bromide, as a letter from the seller informed us, had been prepared in the solid state about 2½ months. The evolved electrolytic gas containing the emanation was collected through the pump and introduced into the apparatus, of which a sketch is given in fig. 2. Before this was done, the whole apparatus had been exhausted, and washed out with oxygen several times, admitted through the gas-

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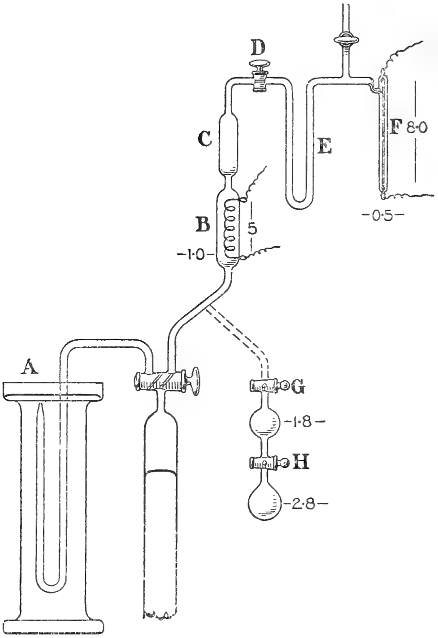
* *Loc. cit.*, p. 206.

burette. The emanation, too, was collected in a tube which had been used for oxygen, the object being to keep nitrogen out of the tubes, and so to avoid its spectrum, which is difficult to remove.

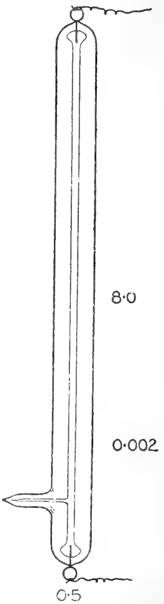
The gas, of which there was about half a cubic centimetre, was admitted into the gas-burette through the inverted siphon A; the stop-cock being reversed, it was passed slowly into the tube B, which contained a spiral of thin, partially oxidised copper wire, and which had previously been exhausted; during the introduction of the gas the copper spiral was kept red-hot by a current. The water produced was absorbed in the tube C, which contained phosphorus pentoxide. Mercury was then admitted into B and C, so as to displace the gas through the stop-cock D, which was then shut. The vacuum tube F had been previously glowed out until phosphorescent. This vacuum tube is represented in natural size in fig. 3; its capacity was about one-third of that of the U-tube and accessory tubing. The spectrum of carbon dioxide was alone seen. With a jar and spark-gap interposed, on comparing the spectrum with the jar discharge in a similar tube containing carbon dioxide, a yellow line was visible in the gas from radium, and also a bright blue line, absent in the spectrum of the pure dioxide. The spectrum of helium was then thrown in

Fig. 2.



through a comparison prism, when no doubt remained that the yellow line was actually D^3. By cooling the U-tube, the emanation and dioxide were condensed, and the helium spectrum increased greatly in brilliancy. After half an hour the tube was sealed off. The position of the D^3 line was confirmed to within one-tenth of the distance between the two sodium lines, D^1 and D^2.

Fig. 3.

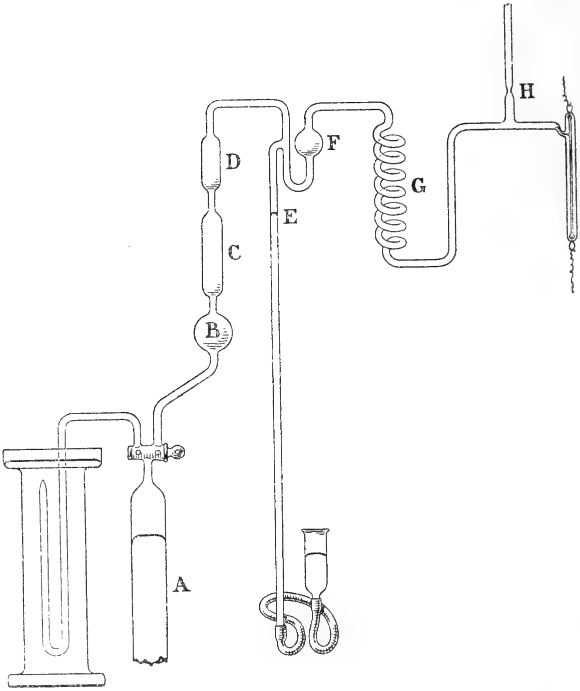


Experiment 2.—A second apparatus similar to the last was made of entirely fresh glass, so as to exclude any possibility of contamination with helium; and the observation was repeated with 31·8 milligrammes of radium bromide, kindly lent by Professer Rutherford, which had been kept in the solid state at least 3 months. The apparatus was slightly modified, as shown in the dotted lines in fig. 2, so as to avoid taking the gas through the pump. As before, the whole apparatus was washed out with oxygen, and the copper spiral was glowed in oxygen, so as to oxidise it superficially, and so render it able to deal with the excess of hydrogen, as well as with the constituents of the water which had been decomposed. After the gas had been admitted by opening the stop-cocks G and H, the copper spiral was kept glowing for three-quarters of an hour. The U-tube was then cooled with liquid air, and tap D was opened. D^3 was seen. Mercury was admitted to the tubes D and C, and the vacuum-tube was sealed off. It now showed all the visible helium spectrum except the faint least refrangible red, as well as the yellows, green, and violet of mercury. Two unidentified lines were also measured, of approximate wave-length 6145 and 5675, the former faint but distinct, the latter moderately bright. The vacuum tube did not glow visibly in the dark, showing that the emanation had been almost completely removed. The U-tube was next placed in communication with the pump, still surrounded with liquid air, but no gas could be extracted; now the U-tube had probably two or three times the capacity of the vacuum tube; and at the low temperature of liquid air, almost twenty times the quantity of helium must have remained in it. It shone brilliantly in the dark. The passage to the pump was then closed, and the liquid air removed. On again establishing communication, a brilliant phenomenon was observed in the dark; the glowing emanation passed through the capillary somewhat slowly, rushed along the wider connecting tubing was delayed in passing through the tightly packed phosphorus pentoxide, and finally filled the barrel of the Töpler pump. On raising the reservoir, the gas grew more luminous as its volume was decreased, and on lowering it, the glowing gas appeared to lie on the surface of the mercury for a fraction of a second, falling with the falling mercury; but it soon spread through the whole barrel by diffusion.

The bubble of gas pumped out was treated with a drop of caustic potash, when a considerable fraction was absorbed. By next day the volume of the bubble had increased.

Inasmuch as all samples of emanation showed the spectrum of carbon-dioxide, the presence of which was believed to be due to the oxidation of the tap-grease, an apparatus was constructed in which the use of taps was, as much as possible, avoided. All the emanation from about 60 milligrammes of radium bromide was introduced into the burette A, the only gas present being oxygen. From the burette it passed through the bulb B, which contained concentrated potash solution; it then passed through C, which was charged with solid potash and was deprived of moisture by contact with phosphorus pentoxide in D. The level of the mercury in the trap was at E, so that the emanation reached the spiral G, cooled with liquid air; the

Fig. 4.



whole of the emanation was washed into the spiral by admission of a little pure oxygen from A; on exhaustion with the pump the gas was not luminous, showing that the emanation had been almost completely retained in the spiral. Mercury was then allowed to rise in the trap until the bulb F was filled; the connection to the pump was then sealed at H and the spiral allowed to warm up. The emanation in the vacuum tube showed a bright green spectrum, but on filling the spiral with mercury and sealing off the vacuum tube, the spectrum of carbon dioxide became visible; D^3 was not seen.

Next day this line was seen, but very feeble; its strength increased from day to day, and in 5 days the yellow, green, and two blues were visible as well as the violet; their identity was proved by means of a comparison spectrum.

Subsequent experiments were made in which the heated spiral of copper was replaced by a tube containing a fragment of phosphorus; the emanation was washed out of the condensing tube by a few bubbles of oxygen. The bulb of potash solution was retained, but the solid potash was replaced by solid barium hydroxide. This plan was not so effective in removing carbon dioxide, yet on keeping the tube for 3 days, and condensing the carbon dioxide with liquid air, D^3 was easily visible, although weakened by the spectrum of carbon monoxide.

On two subsequent occasions the gases evolved from both solutions of radium bromide were mixed after 4 days' accumulation, which amounted to about 2·5 c.c. in each case, and were examined in a similar way. In this case the non-condensable gases alone were examined, the emanation being retained. Whereas with the emanation almost the whole can be introduced into the vacuum tube, with permanent gases only about one-twentieth part is available for the purpose of the spectrum. The D^3 line of helium could not be detected.

The vacation now intervened, and the bulbs containing the dissolved radium bromide were connected with a mercury reservoir and with a gauge, so that the pressure should not rise and burst the bulbs. The gas accumulated during 60 days; its composition was: Hydrogen, 19·48 c.c.; oxygen, 10·37 c.c.; nitrogen 1·02 c.c. = 30·87 c.c.

The nitrogen was manifestly derived from leakage; after deducting one-fourth of its volume of oxygen, the remaining gas has practically the composition of electrolytic gas. The rate of accumulation is about $\frac{1}{2}$ c.c. a day.

The object of the experiment, of which an account will now be given, was to form an estimate of the amount of helium produced by comparing the intensity of its spectrum with that of a known quantity of helium at a known pressure.

Experiment 3.—This gas was exploded and left a residue of nitrogen it was then mixed with a large excess of oxygen, and sparked in presence of caustic soda for some hours to remove nitrogen. The oxygen was next withdrawn by means of phosphorus, and the minute bubble left was mixed with a bubble of oxygen, in order to wash it into the apparatus to which the vacuum tube was sealed. As already described, this apparatus did not differ from that shown in fig. 2, except for the fact that the tube containing the copper spiral was replaced by one containing a fragment of phosphorus, in order to withdraw the oxygen. The phosphorus was warmed and withdrew the oxygen. The gas was then forced by means of mercury through a cooled U-tube, and a portion reached the vacuum tube. On passing a current for an instant, the D^3 line was plainly seen, but nitrogen was also present in small amount. The tube was then sealed off.

The volume of the spectrum tube, including the U-tube, had been previously estimated by filling it twenty times with air and pumping out each time; from this measurement the total volume was found to be 0·310 c.c., and after the U-tube had been sealed off, the same process was repeated with the U-tube and connections, minus the spectrum tube. The volume of the spectrum tube was thus found to be 0·165 c.c.

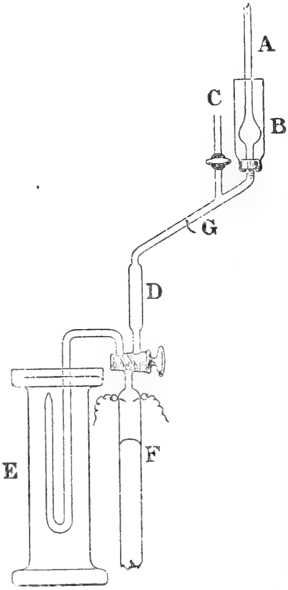
An exactly similar spectrum tube made of the same glass and having the same length was attached to a bulb tube, from which it could be cut off by turning a stop-cock; the bulb tube in its turn could be cut off from the pump by a stop-cock. The capacity of the spectrum tube as well as of the bulb tube was known. A known amount of helium was introduced into the bulb tube and the spectrum tube by means of an inverted syphon furnished with two stop-cocks; the volume between the stop-cocks was 0·0268 c.c. As the volume of the spectrum tube and connections was 1·68 c.c., and that of the bulb was 1·25 c.c., when the gas contained in the spectrum tube was allowed to expand into the evacuated bulb tube, its volume was reduced in the ratio $1{\cdot}68/1{\cdot}25+1{\cdot}68$, or 0·57. The spectrum tube containing the fraction of the helium from 60 days' accumulation was placed in series with that containing helium, so that the same current traversed both, and their spectra were compared as regards luminosity of the D^3 line. It was necessary to divide the contents of the helium tube seven times before the D^3 line could be regarded as of comparable intensity in both spectrum tubes. Multiplying this ratio by the volume of the helium admitted at atmospheric pressure into the apparatus, the volume remaining in the apparatus after rarefaction is given:

$$(0{\cdot}57)^7 \times 0{\cdot}0268 = 0{\cdot}000517 \text{ c.c.}$$

Now, the volume of the helium tube and connections was by chance practically ten times that of the spectrum tube alone (1·65 and 0·165), hence the spectrum tube contained 0·000052 c.c., or 0·052 cub. mm. The total quantity obtained was twice this amount, or 0·1 cub. mm.

As 1 litre of helium weighs 0·18 gramme, for its density is twice that of hydrogen, 0·1 cub. mm. weighs 0·000018 milligramme. This amount is the product of 50 milligrammes of radium bromide in 60 days; hence, 1 gramme of the bromide should give in a year 0·0022 milligramme. It should be mentioned that the spectrum of argon was present, and it may have seriously interfered with this estimation. The helium, too, may have penetrated and been retained in the glass.

Fig. 5.



Experiment 4.—It appeared feasible to attempt to measure the actual volume of the emanation in a fine capillary tube. Thinking that any bought capillary tube would be too wide, we drew a very narrow one, which had an electrode sealed into its end. It turned out, however, to be very irregular, and the results as regards volume are not very trustworthy. A is the capillary tube, with a platinum electrode of very fine wire sealed into its upper end; the mixed hydrogen and oxygen containing the emanation were introduced into the explosion burette F through the inverted syphon E; some moist caustic potash had been melted in the top of the burette, so as to remove from the gases any possible carbon dioxide which might have been produced by the flame causing an organic dust in the burette to burn. After the gases had been exploded, the excess of hydrogen, together with the emanation, was allowed to stand for some time in contact with the caustic potash. The upper part of the apparatus having been completely evacuated, the connection with the pump was closed, and the tube leading to the reservoir of the burette was clipped; on making communication by turning the tap of the burette, the hydrogen and the emanation entered the apparatus. Liquid air was then poured into the tube C, so as to cool the bulb B, where the emanation condensed. After raising and lowering the reservoir of the burette several times, in order to convey the emanation into the bulb B, the tap of the burette was closed, and that leading to the pump opened. Again opening cautiously the tap of the burette, the mercury was allowed to rise, passing through the tube D containing phosphorus pentoxide as far as G; the evacuation was then completed until not a trace of a bubble passed through the pump. The tap leading to the pump was closed, and that of the burette opened, until the mercury had risen to near the bulb B. On darkening the room the bulb B was brilliantly luminous; indeed, it was possible to read a watch by its light. The liquid air was allowed to evaporate away, and the reservoir of the burette lowered and its tap opened; by gently raising the reservoir the emanation was all collected in the capillary tube A. The volume of the emanation was read from day to day by help of a reading telescope. It contracted regularly; the tube was coloured deep purple after some days, and this made reading difficult, but by a brilliant illumination behind the rise of the mercury could be followed. No attempt was made to pass a discharge for 28 days; after that lapse of time the emanation had contracted to a volume occupying only 0·1 of a millimetre of the capillary tube at a pressure of about 50 mm., yet it maintained its brilliancy till the very last; only the length of tube illuminated grew shorter and shorter. On freezing out the mercury vapour by cooling the bulb B with liquid air, the helium spectrum was visible, and at the same time the effect of passing the discharge was to reproduce gas in the capillary tube.

After the conclusion of the experiment, the tip of the tube was cut off immediately below the platinum wire, and the capillary depression was measured at different levels. The capillary tube was then cut off, and the volume determined by weighing with mercury; it was then calibrated by a shifting thread of mercury, under a reading microscope. The final results were:—

Time.	Volume.
Start	0·124 cub. mm.
1 day	0·027 „
3 days	0·011 „
4 „	0·0095 „
6 „	0·0063 „
7 days	0·0050 cub. mm.
9 „	0·0041 „
11 „	0·0020 „
12 „	0·0011 „
4 weeks ...	0·0004 „

The comparatively large volume at the start is very remarkable; we can only record it, it may possibly have been due to the mercury sticking in the capillary tube, which was narrower below.

Experiment 5.—The former experiment was repeated, this time with a regular capillary tube, of which the volume per centimetre was 0·24 cub. mm. It was regular in bore, and the depression due to capillarity was 56·2 mm. of mercury. It was heated, as well as the bulb in which the emanation was to be condensed, to incipient redness during evacuation. The emanation was introduced, the accompanying hydrogen pumped off, and the liquid air jacket removed. The volume of the emanation was read at once, at different pressures. The following table gives the lengths of the capillary tube, the corresponding volumes, the pressures corrected for capillarity, and the products of volume into pressure:—

Length of tube.	Volume in cub. mm.	Pressure in mm.	Volume × pressure.
6·80	0·163	132·4	21·6
2·30	0·0552	333·4	18·4
1·55	0·0372	518·1	19·3
1·20	0·0288	644·8	18·6
0·95	0·0228	765·8	17·5
2·55	0·0612	309·2	18·9
11·90	0·372	55·3	20·6

The mean value of the product is 19·3, and the volume at normal pressure 0·0254 c.mm. The same afternoon, numerous readings were taken, and it was found that the sticking of the mercury in the capillary tube made it difficult to ascertain the true volume. As the pressure, however, was first raised, and then lowered, the mean cannot be far from the truth. Now, a most remarkable circumstance must be chronicled. Whereas the emanation in the previous experiment contracted during its whole life, in this experiment a regular expansion was observed, rapid at first, and slowly falling off from day to day. Between 5 minutes past 1 o'clock and 7 o'clock, the pressure being kept constant at 55·3 mm., the value of P.V. increased from 20·6 to 48·4. This was on January 20th. On the 21st, the P.V. had increased to 71·2, and remained fairly constant all day, and three small bubbles appeared in the thread of mercury, below the level of the emanation. On the 22nd, the value of P.V. had diminished to 56·5, and the volume of the bubbles had increased to 2·7 mm. length in the tube. On the 23rd, the emanation occupied practically the same volume, but the length of the bubbles had increased to 4·1 mm. The presence of these bubbles made it impossible to obtain correct readings, for the "sticktion" of the mercury was much increased. On the 25th, P.V. had further diminished to 51·2 and the length occupied by bubbles had increased to 5·5 mm. On February 3rd, the bubbles were united with the emanation; the value of P.V. was 132·5 and the volume of the gas under normal pressure, 0·174 cub. mm. On the 9th, the P.V. had increased to 166, and the volume at normal pressure to 0·224 cub. mm. Lastly, the volume of the gas was measured on the 12th at atmospheric pressure; it amounted to 0·262 cub. mm. The level of the mercury was then lowered, and the gas pumped out, it showed a brilliant spectrum of helium. The tube was then heated, and the volume of the absorbed gas was 0·103 cub. mm. at atmospheric pressure; it, too, showed the helium spectrum, but the tube punctured before this could be confirmed.

These results are somewhat inexplicable in the light of the former experiment. More stringent precautions had been taken to free the capillary tube from gas in the second experiment than in the first, and yet bubbles appeared below the surface of the mercury. It may be that owing to the quality of the glass of which the first tube was made, the helium found means to enter into its substance more easily than into that of the second. But at any rate, the volume produced is of the same order, as the following considerations will show.

On the view that the emanation results from a definite fraction of the radium disintegrating per second, this fraction can be calculated from the volume of the emanation, and the time of accumulation. The emanation accumulates until the rate of production is balanced by the rate of disappearance, and then the quantity remains constant. Let Q_∞ be the equilibrium quantity, and Q_t the quantity present after time t,

$$Q_t/Q_\infty = 1 - e^{-\lambda t},$$

where t is expressed in seconds, and λ is a constant representing the proportion of the emanation changing per second, and equals 1/463,000.* The radium bromide employed weighed about 60 milligrammes. Assuming that the compound contained about half its weight of the element (radium, 225; bromine + $2H_2O$, 196), the quantity of radium may be taken as about 0·03 gramme. In the first experiment, the time of accumulation t was 8 days = 691,200 seconds; Q_t therefore equals 0·775 Q_∞. The volume taken (0·027 cub. mm.) in the first experiment was that at the end of the first day, and a correction must be applied to allow for the amount that had changed in this interval. The quantity remaining after the lapse of 1 day is 0·83 of the initial quantity. The volume, 0·027 cub. mm., is therefore

$$0{\cdot}83 \times 0{\cdot}775\ Q_\infty = 0{\cdot}643\ Q_\infty.$$

The average life of the particle in a system in which a constant fraction λ of the number of particles changes per second can be shown to be $1/\lambda$. The equilibrium quantity, Q_∞, is the quantity produced in the period of average life of the atom of the emanation, or $Q_\infty = Q_0/\lambda = 463{,}000\ Q_0$, where Q_0 is the quantity produced per second. And $0{\cdot}643\ Q_\infty = 297{,}830\ Q_0$. The volume of Q_0 is thus $0{\cdot}027/297{,}830 = 0{\cdot}9 \times 10^{-7}$ cub. mm. This is in the case of 0·03 gramme of radium; 1 gramme of radium, therefore, produces 3×10^{-6} cub. mm. of emanation per second.

Since the emanation resembles the gases of the argon family in chemical inertness, its molecule is probably monatomic, and its atomic weight must be twice its density in terms of hydrogen as unity. The density is not accurately known; but diffusion experiments indicate a value of about 80. The atomic weight being therefore in the neigh-

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* Rutherford and Soddy, 'Phil. Mag.,' 1903, 6, vol. 5, pp. 445 and 576.

bourhood of 160, not more than one atom of emanation can be produced from one atom of radium. To determine the ratio between the quantity of emanation and the quantity of radium producing it, it is necessary to know the volume that would be occupied by the radium in the form of monatomic gas. This is for 1 gramme of radium $(2 \times 11{\cdot}2)/225 = 0{\cdot}1$ litre $= 10^5$ cub. mm. One gramme of radium produces 3×10^{-6} cub. mm. of emanation per second, and if one atom of radium produces one atom of emanation, then λ, the proportion of the radium changing per second, is 3×10^{-11}. The proportion changing per year is $9{\cdot}5 \times 10^{-4}$; thus slightly less than one-thousandth part changes per year. The average life of the radium atom is $1/\lambda = 3{\cdot}3 \times 10^{10}$ seconds $=$ 1050 years.

In the second experiment, the emanation was accumulated 6 days, and measured 0·0254 cub. mm. In this case,

$$Q_t = 0{\cdot}674\ Q_\infty = 312{,}060\ Q_0,$$

and $Q_0 = 0{\cdot}81 \times 10^{-7}$ cub. mm.; $\lambda = 2{\cdot}4 \times 10^{-11}$, and $1/\lambda = 1250$ years. The mean of the two experiments, therefore, gives for 1 gramme of radium (element) $Q_0 = 2{\cdot}85 \times 10^{-6}$ cub. mm.; $Q_\infty = 1{\cdot}3$ cub. mm., $\lambda = 2{\cdot}85 \times 10^{-11}$, and $1/\lambda = 1150$ years.

Rutherford and Barnes* have shown that 75 per cent. of the total heat-evolution of radium which has reached its equilibrium state is derived from the emanation and its subsequent products of change. Since 1 gramme of radium evolves 100 calories per hour (Curie), 1·3 cub. mm. of emanation emit 75 calories per hour. The total quantity of heat H emitted during the complete change is given by multiplying h the emission per second, by the average life of the emanation in seconds, thus giving $H = h/\lambda = 9{,}646$ calories. One c.c. of emanation would therefore emit $7{\cdot}4 \times 10^6$ calories during its complete change. A cubic centimetre of hydrogen and oxygen in the proportion required to form water evolve 2·04 calories on explosion, or a quantity 3,600,000 times less than is emitted by an equal volume of the radium emanation. If the density of the emanation is assumed to be 100, the ratio of the energies emitted by equal weights of emanation and of water is 216,000 to 1.

The total quantity of energy evolved during the change of 1 gramme of radium is given by multiplying the energy emission per second by the average life of the radium atom in seconds, and is 10^9 calories. The energy evolved in the formation of 1 gramme of water is $3{\cdot}8 \times 10^3$ calories; hence the ratio is again about 250,000 to 1.

The volume of Q_∞, the equilibrium quantity of emanation produced by 1 gramme of radium, was theoretically calculated by Rutherford† from the energy emitted by radium per second, and the energy of

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* 'Phil. Mag.,' 1904, 6, vol. 7, p. 202.

† 'Nature,' August 20, 1903.

the α-particle, as calculated from its mass and its velocity. By making certain assumptions about to be considered, he deduced that Q_∞ must lie between 0·6 and 0·06 cub. mm. The maximum estimate, which is almost exactly one-half the experimental value found by us, was based on the assumption that the whole, and the minimum estimate on the assumption that only one-tenth, of the energy of disintegration is manifested in the kinetic energy of the α-particle expelled. It was further assumed that only one α-particle was expelled from each atom, at each disintegration accompanied by α-radiation that is known to occur. If more than one α-particle is expelled at each disintegration, the theoretical estimate must be correspondingly reduced. Since the experimental value exceeds the maximum theoretical estimate, it follows that there are now direct experimental reasons for believing that—

(1) Only one α-particle is expelled from the atom at each disintegration.

(2) The greater part of the energy of disintegration appears in the form of kinetic energy of α-radiation.

(3) The emanation is a monatomic gas.

It must be remembered that the experimental value is necessarily a maximum value; for if any impurity were present with the emanation, it would increase the volume measured.

"Corrigenda in Mr. W. Shanks's Tables 'On the Number of Figures in the Reciprocal of a Prime.'" By Lieut.-Colonel ALLAN CUNNINGHAM, R.E., Fellow of King's College, London. Communicated by the Secretaries.

This paper contains the result of an examination of the late Mr. W. Shanks's MS. Tables, with above title, now deposited* with the Royal Society. These tables have been *collated*† with the following printed tables,—

1. 'Periodische Dezimalbrüche,' by H. Bork, Berlin, 1895. The Appendix (pp. 36—41) contains a Table (computed by Dr. F. Kessler) giving the *Residue-Index* (q), *i.e.*, the *maximum divisor* yielding $10^{(p-1)\div q} \equiv +1$ (mod. p) for every prime (p) $\not> 100{,}000$, for which $q > 2$.

2. "Periode des Dezimalbruches für $1/p$ wo p eine Primzahl," by H. Hertzer, printed in Grunert's 'Archiv der Math. und Phys.,' vol. 2, 1902, p. 249. The Table (pp. 249—251) is a *continuation* of the preceding Table for primes up to $p \not> 112{,}400$, and is arranged in the same manner.

Shanks's MS. Tables give the *period-length* (say ξ) of $1/p$ for *all* primes from 30,000 to 120,000. The collation was effected by simply multiplying Shanks's value of ξ by Kessler's or Hertzer's value of q; the product of ξq should $= (p-1)$ in every case. The collation was, of course, only possible for such primes as have $q > 2$ (being the only ones shown by Kessler and Hertzer), thus excluding about two-thirds of the total number of primes; the ξ, q of these excluded primes are, however, easily computed when required.

By this collation a number of discrepancies (102 in all) were discovered between Shanks's MS. and the printed German tables. The values of ξ have in all these cases been *re-computed*,‡ with the result of detecting *errata*, as follows:—

Shanks	p missing 5;	ξ wrong 66;	total 71	Total 102.
Kessler	p missing 8;	q wrong 20;	total 28	
Hertzer	p missing 1;	q wrong 2;	total 3	

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* Part of the MS., viz., for primes from 30,000 to 60,000, is bound up with a small volume marked *Constants and Primes* (with the Press-mark 103 d 15), and the rest, viz., for primes from 60,000 to 120,000, is bound up with the 'Royal Society Archives,' vols. 60, 61. Mr. Shanks's MSS. bear dates as follows:—

For primes 30,000 to 60,000, dated 1875.
For primes 60,000 to 75,000, dated 1876.
For primes 75,000 to 110,000, dated 1877.
For primes 110,000 to 120,000, dated 1880.

† By the writer of this paper, with the help of an assistant (Miss E. Cooper), by permission of the Council of the Royal Society.

‡ By the writer himself, and verified in part by Miss E. Cooper.

The table following gives the *Corrigenda* on Shanks's MS. This list is probably far from complete; there is reason to suspect a good many errors in Shanks's MS. among those primes for which $q \ngtr 2$, but, as the table has not been published, it did not seem worth while examining these.

Corrigenda on Shanks's MS. Table III [*primes from* 30,000 *to* 112,400].

Insert five primes (p) *missing* in MS.: 33797, 59369, 94111, 95089, 104383.
Correct the period-lengths (ξ) opposite the primes (p) as below.

p.	ξ.	p.	ξ.	p.	ξ.	p.	ξ.
33,797	8,449	65,011	21,670	86,143	1,758	103,813	17,302
34,871	1,585	70,001	35,000	86,323	14,387	104,381	104,380
42,773	289	70,867	3,937	87,121	4,840	104,383	3,866
43,753	14,584	70,921	3,546	87,151	2,075	104,707	17,451
44,893	22,446	71,821	4,788	87,517	21,879	105,367	35,122
46,153	15,384	72,559	87	87,697	29,232	105,613	26,403
46,649	686	72,661	24,220	88,003	14,667	105,929	26,482
47,093	23,546	72,871	1,735	89,689	22,422	106,031	2,305
47,711	1,835	72,901	8,100	92,107	6,579	106,921	2,430
53,857	17,952	73,351	7,335	93,319	15,553	107,647	15,378
55,021	7,860	74,413	37,206	94,111	9,411	107,837	26,959
55,681	9,280	74,687	214	95,089	23,772	109,441	9,120
55,933	27,966	78,079	3,003	95,791	15,965	110,051	22,010
57,457	2,128	79,111	7,911	96,601	4,830	110,917	18,486
57,493	14,373	80,347	13,391	96,911	4,405	110,969	27,742
58,031	4,145	82,021	16,404	98,893	24,723	111,149	148
59,369	14,842	85,411	17,082	101,051	2,150	112,249	14,031
61,007	1,034	85,447	1,818	102,793	34,264		

"A Research into the Heat Regulation of the Body by an Investigation of Death Temperatures." By EDRED M. CORNER, M.A., M.B., B.C. (Cantab.), F.R.C.S., B.Sc. (Lond.), Surgeon to Out-patients, St. Thomas's Hospital, and Assistant-Surgeon to the Hospital for Sick Children, Great Ormond Street, Erasmus Wilson Lecturer, Royal College of Surgeons, and JAMES E. H. SAWYER, M.A., M.D. (Oxon.), M.R.C.P., Honorary Anæsthetist to the Ear and Throat Hospital, Birmingham, lately House Physician to St. Thomas's Hospital. Communicated by Professor J. N. LANGLEY, F.R.S. Received April 4,—Read May 5, 1904.

The fact has long been known that, in many forms of disease, variations of the bodily temperature occur as death approaches. Our knowledge of such variations is limited to certain apparently sporadic cases. Hitherto, no attempt has been made to ascertain the special class of disease in which such deviations occur most frequently, nor has the subject been examined in a scientific light, so as to bring these death temperatures in line with the present knowledge of the mode of production of pyrexia. It is towards this latter object that this inquiry has been directed. Further encouragement to bring forward this new source of knowledge is given by Sir John Burdon Sanderson, who says "the subject (of pyrexia) is one in respect of which results as valuable can be obtained by clinical investigation as by experiments on animals."*

This communication has been divided into two parts. In the first part, a general account is given of the bodily temperature immediately preceding death, and of the variations which are seen in these thermometric records. A series of problems are shown, in order to ascertain, where possible, the different factors which may influence the variations in the death temperatures. The figures quoted are, in all cases, the absolute minimum, as numbers of these cases have to be rejected for many reasons; for example, those in which tepid sponging has been employed, and others where the records are incomplete. In the second part, an attempt is made to ascertain the possible influences which may cause such variations in the bodily temperature, and to explain, from a study of these changes, the mechanism by which the temperature of the body is regulated in health, and the reasons for which deviations from the normal occur in disease.

PART I.

As the fatal result approaches, the curve on the temperature chart may exhibit several changes. In 49 per cent. of surgical, and in

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* Clifford Allbutt, 'System of Medicine,' vol. 1, p. 152.

19 per cent. of medical cases, a definite change of over 1°·5 F. occurs during the 12 hours immediately preceding death. These figures are the absolute minimum for about 2500 consecutive cases which have been collected from the records of St. Thomas's Hospital. In instances where no such sudden change occurs, the temperature may remain about the same level, but the more usual procedure is for it to slowly and steadily fall. On the other hand, it is not common for the bodily temperature to rise slowly, for, when an elevation occurs at all, the change is generally a sudden one. The character of the temperature charts as death approaches is naturally affected by the types of fever from which the patients may be suffering, such as remittent, intermittent, etc. The influence of these fevers cannot be eliminated, and it is an interesting point that such cases frequently show a very decided variation in the character of the temperature near the fatal termination. The performance of tepid sponging, or the exhibition of antipyretic drugs, just before the death of the patient, renders the chart valueless for this research. For these, and similar reasons, many variations of temperature have been necessarily neglected.

There are other changes in the temperature charts which have to be considered in these investigations. Sometimes, about 24 hours before death, the temperature falls suddenly, anything from 1—5° F., and then rises rapidly, and at this point death may occur. On the other hand, the fatal result may be postponed, and, after the temperature has reached its maximum, defervescence may begin again. Such fluctuations as these are more commonly found among surgical than among medical cases. The following is a good example of these changes :—a baby girl, aged 7 weeks, was severely burnt, and died on the fifth day; the temperature during the last 36 hours of life represented these variations; it fell 4°·3, rose 6°·4, falling again 4°·2. Again, in a woman, aged 28 years, who suffered from general peritonitis and appendicitis, there was a fall of 4° in the temperature, followed by a rise of 8°, when death occurred. These early deviations of the temperature begin about 24 hours before the death of the patient, and may exaggerate either the death rise or the death fall, or may mask them altogether.

Of the 2500 cases which were examined, 1305 were medical cases, the remainder, 1195, being surgical. Changes in the bodily temperature of over 1°·5, occurring during the last 12 hours of life, were found in 34 per cent. of all the records examined; in 49 per cent. of surgical, and in only 19 per cent. of medical cases. On account of this great difference of 30 per cent., the two lists of cases have been kept separate, although in many instances they overlap. The explanation of the difference which is found between the medical and surgical cases has to be sought for in the other particulars.

Of the variations found among the fatal medical cases, 75 per cent. were rises of temperature and 25 per cent. falls. For the surgical cases, the corresponding percentages were 73 and 27; the two classes agreeing very closely. Elevations of temperature occurred in 26 per cent., and falls in 8 per cent., of all the cases examined.

The relative proportions of male to female deaths were also investigated, and the following figures were obtained:—

Medical Cases.

Proportion of male to female deaths during the same period	1·75	male to	1	female.
Ditto, showing death rise of temperature	1·57	,,	1	,,
Ditto, showing death fall of temperature	1·45	,,	1	,,

Surgical Cases.

Proportion of male to female deaths during the same period	1·63	male to	1	female.
Ditto, showing death rise of temperature	1·75	,,	1	,,
Ditto, showing death fall of temperature	1·39	,,	1	,,

The following deductions may be made from these figures, indicating lines along which an explanation of the phenomenon may be sought. In surgical cases males are more apt to show a death rise; females, on the other hand, show a marked tendency to death falls. In medical cases the difference is not so marked, but female patients are relatively more liable to death variations of temperature than are male. A fall of temperature in both medical and surgical cases is more common in females than in males.

There is one point on which medical and surgical cases differ from each other, and that is, that in the latter deaths due to injury are included. As to the actual cause of death in disease and in injury the difference is not so great, but, as will be seen in the following table, the death temperatures vary considerably in one important point in the two classes. Only the surgical cases are here considered:—

Proportion of deaths due to disease and injury	1·88	disease to	1	injury.
Ditto, showing death rise	1·84	,,	1	,,
Ditto, showing death fall	3·29	,,	1	,,

These figures show that rises in the bodily temperature, just before death occurs, are found to be present in fairly equal proportion in patients dying from disease and from injury. Falls of temperature, however, are very much more common in patients suffering from disease. The following table shows the same proportions in percentages:—

	Disease.	Injury.
Proportion of death changes to total ...	51·7 per cent.	46·0 per cent.
" " rises " ...	30·5 "	37·3 "
" " falls " ...	15·2 "	8·7 "

From this table it would appear that changes of temperature, as death approaches, occur less frequently in injuries than in disease, and that this difference is due to the comparative rarity of death falls in the former. The rises of temperature are present in equal proportion in the two classes of cases.

In the following charts an attempt is made to show the amount and frequency of the various deviations of temperature :—

CHART 1.

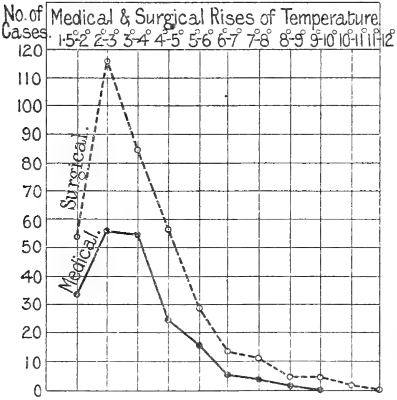
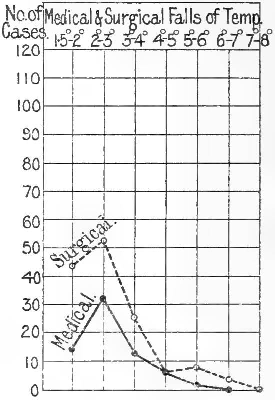


CHART 2.



The following conclusions may be drawn from Charts 1 and 2 :—

1. That death rises of temperature are naturally larger than death falls.

2. That small variations of temperature are more common than large.

3. That death rises are comparatively rare over 5° ; more so over 6°.

4. That death falls are comparatively rare over 4° ; more so over 5°.

5. That the variations of temperature in the medical and surgical cases agree fairly well, although death rises in medical cases are relatively more common from 3—4°.

6. That death rises are more frequently of greater magnitude in surgical than in medical cases.

Charts 3 and 4 show the actual temperature at the time of death (or rather the last recorded while life was still present) in cases in which there had been a previous thermometric variation of over 1°·5.

CHART 3.

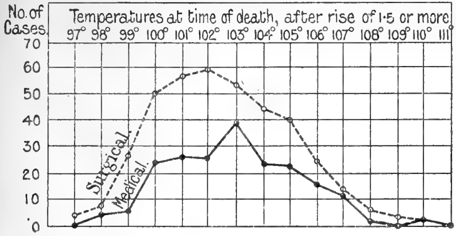
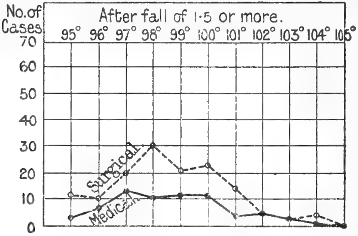


CHART 4.



From these charts the following deductions are made :—

1. That there is very little difference between medical and surgical cases as regards the final temperature, whether it be preceded by a rise or by a fall.

2. That, when death rises occur, the final temperature is most frequently between 101° and 103°, but quite commonly ranges from 100—106°.

3. That, after death falls, the most common final temperature is between 97° and 100°, but falls as low as 95° are fairly frequent. The high temperatures which are recorded under the death falls are due to the previous rises of temperature, just as the low temperatures under the death rises are due to previous falls.

The variations of the temperature of the preagonistic stage are considered in the following table with respect to the duration of the illness as estimated roughly by the length of the period between admission to the hospital and the death of the patient. This method by no means tells correctly the duration of the illness; but by no other way can it be estimated on account of the histories being so unreliable. It was decided in consequence to accept the above method only in surgical cases, as indicating fairly well in this class the state of affairs :—

Duration of stay in hospital.	Rises.		Falls.	
1 day	72	1st week, 236	21	1st week, 68
2 days	57		12	
3 ,,	18		8	
4 ,,	21		6	
5 ,,	41		12	
6 ,,	14		7	
7 ,,	12		2	
7 ,, to 10 days	29	2nd week, 63	12	2nd week, 22
10 ,, ,, 14 ,,	34		10	
14 ,, ,, 21 ,,	25		18	
21 ,, ,, 28 ,,	19		9	
1 ,, ,, 2 months	35		16	
2 ,, ,, 3 ,,	5		0	
3 ,, ,, 4 ,,	4		4	
Over 4 ,,	4		2	

Owing to the obvious errors that enter into the composition of such a table, it was decided not to continue it into details. The nature of the disease would be expected to affect the death changes of temperature far more than the mere duration of the illness would. But one point stands out in the above table, namely, that the shorter the illness the greater the number of cases in which variations of temperature are found. How far this is relative or absolute has not been calculated, for the reasons which have just been given. The greatest number of rises are found in short illnesses; but the corresponding statement, as founded on the above table, which is composed only of surgical cases, does not hold good for the falls of temperature, but, as will be seen later, there is distinct evidence that sudden depressions of temperature as death approaches occur frequently in diseases of long duration.

The disease which brings about the fatal termination may be regarded as the most important factor in causing the changes of temperature which occur as death approaches. The influence of the disease upon the character of the pyrexia, when death is associated with a rise of the bodily temperature, has already been slightly indicated. The following diseases are those in which the greatest variations were found:—

Medical Cases.

Rise of 8°.—Acute yellow atrophy of the liver.
7°.—Septic meningitis, tubercular meningitis, typhoid fever (hæmorrhage, preceded by a fall of temperature).
6°.—General tuberculosis, cerebral hæmorrhage, septic meningitis, peritonitis, marasmus and diarrhœa.
5°.—Peritonitis; intussusception, preceded by a fall (two cases); general tuberculosis (three cases), pneumonia (two cases), diphtheria, chronic renal disease, carcinoma of the liver, intestinal obstruction, carcinoma of the small gut, hemiplegia, ulcerative endocarditis, acute yellow atrophy.

The list of diseases for the smaller elevations of temperature is too long for reproduction. Two points are clearly seen in this table—that in severe toxic diseases a large rise of temperature is common, and that in heart diseases it is a rare affection.

Surgical Cases.

Rise of 10°.—Fractured base of skull.
9°.—Fractured spine, fractured skull, burn.
8°.—Strangulated hernia and peritonitis, tetanus, pyæmia.
7°.—Septicæmia, intestinal obstruction and peritonitis, fractured vault of skull, fractured base, burn (two cases), pyæmia, suppurative nephritis, septic broncho-pneumonia.

Rise of 6°.—Peritonitis, various causes (five cases); meningitis (two cases), burn, scald, spina bifida, imperforate anus, gluteal abscess.

From this list the following conclusions may be drawn:—

1. That injuries to the head and spine generally give rise to high death changes of temperature. This may be emphasised by the low temperature which precede the final rise and which are the result of the shock caused by the injuries. Such a condition is frequently found in those patients who succumb within 24 hours after admission to the hospital.*

2. That the following diseases are the most frequent in taking the first places among the death rises of temperature:—

Head injuries: 10°, 1; 9°, 1; 8°, 1; 7°, 2; 6°, 1; 5°, 5—11 cases.
Spinal injuries: 9°, 1; 6°, 1; 5°, 1—3 cases.
Burns: 9°, 1; 7°, 2; 6°, 1; 5°, 6—10 cases.
Scalds: 6°, 1; 5°, 1—2 cases.
Meningitis: 6°, 2—2 cases.

3. Besides the above, death in those cases associated with a rise of temperatures is almost always due to some form of poisoning by septic organisms. Thus the remainder of the list of cases with a death rise of 5° or more, can be summed up as follows:—Peritonitis, septicæmia, pyæmia, septic meningitis, septic broncho-pneumonia, suppurative nephritis, cellulitis. It appears from this that, besides the injuries already mentioned, a septic process causes the death rise in almost all cases. The predominance of this process in causing death in surgical cases probably accounts for the difference in the numbers of the death variations in medical and surgical cases.

Medical Cases.

Fall of 5°.—Phthisis.
4°.—Typhoid fever, hæmorrhage (two cases); intestinal obstruction, broncho-pneumonia, cardiac failure, phthisis (two cases).
3°.—Diphtheria, mitral disease, cirrhosis of the liver, hæmorrhage, cardiac failure, bronchitis, myelitis, pneumonia, chronic renal disease (two cases), tubercular meningitis (two cases), phthisis (two cases).

This list shows that large falls of temperature occur in diseases of long duration, such as phthisis, the absence of diseases of the nervous system and the comparative frequency with which cardiac affections is found are also striking facts.

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* Sawyer, "The Temperature of Coma," 'Brit. Med. Journ.,' Dec. 26, 1903.

Surgical Cases.

Fall of 6°.—Intestinal obstruction, cyst of ovary, erysipelas.

5°.—Sarcoma of pelvis, carcinoma of rectum and obstruction, compound depressed fracture of vault, scald, septicæmia, pyæmia, abscess.

4°.—General peritonitis (two cases), pyæmia (two cases), tubercular laryngitis and phthisis, epithelioma of tongue, sarcoma of tibia.

3°.—Injury to chest and bronchitis, fractured ribs, sarcoma of face and scalp, suppurative nephritis, erysipelas, pyæmia, sarcoma of neck, cellulitis, imperforate anus, burn, cut throat, ruptured gut.

The general absence of injury and the great preponderance of sepsis are striking features in the above list. Some of the falls of temperature are dependent upon the shock following a surgical operation.

The distribution as regards the age of the patients with variations of temperature is shown in the following two charts.

CHART 5.

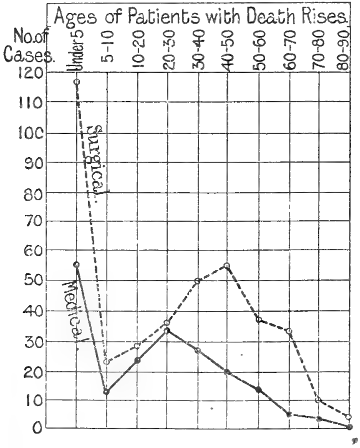
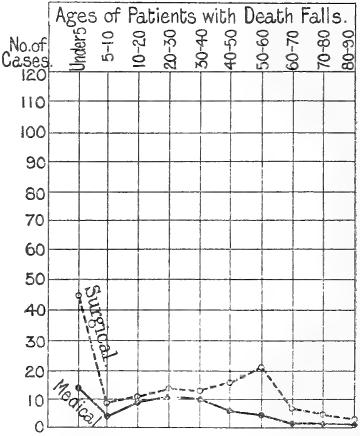


CHART 6.



The deductions, which may be made from Charts 5 and 6, are:—

1. That the variations in the temperature as death approaches are more frequent in children under the age of 5 years than at any other period of life.

2. That between the ages of 5 and 10 years there is a marked decrease in the frequency of the variations of temperature.

3. That between the ages of 20 and 50, that is, during the most active period of life, variations of temperature more commonly occur than at any other period, except in children under 5 years of age.

4. That rises and falls of temperature are relatively of the same frequency to each other at all periods of life.

PART II.

As in so many physiological controversies the various theories fall under two headings, firstly the inanimate, in which the question is examined from the point of view of chemistry, physics, etc., and secondly the animate, which deals with the phenomena of living tissues, so also in the subject of pyrexia there is found a similar condition of affairs. The inanimate theories deal with the ratio of the production and the loss of heat to each other, the disturbances of which must lead to change of temperature. The first question to be discussed is how the approach of death affects the interchange.

(*a*) It seems hardly rational to expect that the capacity for the production of heat will increase as the body approaches death. The physiological processes of the body become less and less active until they cease. In diseases such as tetanus, in which tremendous convulsions take place, there must be an enormous production of heat, and yet in this condition there is usually no pyrexia. Towards death the frequency of the convulsive seizures may somewhat subside, and after this change there is often an elevation of temperature. Under such conditions the increase in temperature may be brought about by an excess of heat production over heat loss. The majority of the other cases of death rises of temperature cannot be accounted for in this manner. Again, curari causes muscular paralysis by suspending the activity of the neuro-muscular system and, as a result, a fall in the temperature of the body occurs; but during the convulsions which are first caused by the drug the temperature rises.*

With regard to the falls of temperature at the approach of death, an explanation which is very tempting to urge, is that with the loss of the activity of the bodily processes a diminution also occurs in heat production. In support of this view may be put forward the fact that the greater the duration of the illness the more frequent is there a sudden depression of temperature. For, in diseases which are of long duration, the capacity of the organism for heat production is much more likely to become diminished than in illnesses lasting only a short time.

(*b*) The heat loss as death approaches must be diminished in almost all cases by the slowness of the circulation, shallowness of respiration, suppression of urine, etc. These changes would tend to cause a rise of temperature. Such an event would naturally be expected to be more frequent when the illness is short and the organs of heat production

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* Pembrey, 'Text-Book of Phsiology,' 1898, edit. by Schäfer, vol. 1, p. 841; Bernard, 'Leçons sur la Chaleur Animale,' 1876, p. 157.

are not worn out by work under a prolonged strain. And, as has been pointed out above, if the disease should be of long duration, a fall of temperature rather than a rise would be expected to occur. The reason for this supposition is that as death approaches in long illnesses, there must presumedly be a considerable diminution of heat production, which is not counter-balanced by the small reduction of the heat lost at the surface. The investigations in the first part of this paper support these ideas, since they seem to show that rises in the bodily temperature occur more frequently in short diseases, and falls of temperature in those of longer duration.

The animate factor in pyrexia is chiefly that of the action of the central nervous system. "The nervous system exercises a control upon the loss of heat by means of the vaso-motor system, which regulates the amount of blood in the deep and the superficial parts of the body, and by the respiratory centre which controls the frequency and depth of respiration; upon the production of heat through the nerves which control the activity of the tissues, chiefly the muscles."* The heat production in a tissue is probably not under the control of that tissue itself, but its thermogenetic function is governed by its proper segment of the spinal cord. The nervous centres cannot of themselves produce heat; they can only govern the manufactories.

As the brain centres exercise a tonic control over the spinal centres which are in connection with the reflexes, sphincters, etc., so, in a similar manner, the higher centre or centres of the brain may hold in check the lower centres in the spinal cord, which give the tissues their power of heat production. The brain centres are the last to be evolved in the history of animal life, and it may be urged that they are the most complex, and therefore the most easily thrown out of gear. For this reason a generally acting death agent, such as a toxic condition, will affect and inhibit the action of the higher centres before or to a greater degree than it will damage the lower. Whenever the higher centre is cut off from the lower, the latter becomes exaggerated in its action. This is well seen in the spastic condition when the reflexes etc., are exaggerated. In a similar way, with the commencement of dissolution of the higher centres, a death elevation of temperature may be expected to result.

The fallacy in reasoning from the above analogy is that, in the one case, the facts deal with a reflex centre, and in the other, as far as is understood, with an automatic. How far these differ in regard to their relation to the higher centres, it is impossible to say, but one can instance the acceleration of the automatic and rhythmic centre of the heart when the inhibitory control of the vagus is cut off.

An important fact which this investigation shows is that, as death approaches, there is a tendency for a sudden rise in the bodily

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* Pembrey, Schäfer's 'Text-Book of Physiology,' 1898, vol. 1, p. 854.

temperature. In about 2,500 medical and surgical cases, an elevation of temperature of over 1°·5 was found in 26 per cent., or just over a quarter of all the cases, while a fall occurred in 8 per cent. only. The percentage of rises of temperature in surgical cases is 37, and in medical 15. It is a most remarkable fact that this sudden elevation of temperature is observed to take place so frequently in surgical cases (in over a third of all the cases examined), and that this change should be found to occur more than twice as often in surgical as in medical cases. As the surgical diseases in these records are of shorter duration, as a rule, than the medical, the tissues in consequence have not been so long exposed to abnormal conditions, and so their heat producing functions are less likely to be impaired. Again, falls of temperature are proportionally much more common in patients dying from disease than from injury. It may be presumed, therefore, that the loss of control of the nerve centres in an exhausted organism only occasionally results in an elevation of the bodily temperature, whereas with less exhausted tissues an increase in the animal heat occurs. From these considerations it would appear that heat production is constantly in excess, and that in consequence of this the organism must exercise some tonic control over the process, in order to keep its temperature at a constant level.

The idea of the thermogenetic control of the higher centre over the lower gives point to the modified and accepted view of Liebermeister that, "in consequence of the injurious action of the fever-producing cause, the organism loses its power of keeping itself at the normal temperature."* The poison will, unless it has special affinities, affect the higher and the more complex centres before the lower. Hence, the "spastic" over-production of heat which may result in fever. The great frequency of death rises of temperature in cases of head injuries, some spinal injuries, meningitis, brain diseases, etc., emphasises the possible cutting off of the controlling function of the higher centres. On the other hand, it should be remembered that many of the patients had pyrexial temperature charts, it may be for some days before the preagonistic variations of temperature occurred. The regulation of the bodily heat in these cases was already partially out of the control of the nervous system, and the further elevation of temperature during the last 12 hours of life may be thought to be due to an increasing loss of this control on the part of the organism.

In small animals, after section of the spinal cord in the cervical region, the temperature falls rapidly; in larger animals, such as dogs, if kept in an envelope of non-conducting material in an ordinary room, the temperature of the body rises to above that of fever, but without clothing the temperature rapidly falls until the animal dies

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* Burdon Sanderson, Allbutt's 'System of Medicine,' vol. 1.

in collapse.* In man, according to Sir Benjamin Brodie, section of the spinal cord in the cervical region causes pyrexia, but very discordant results have been obtained by other observers on cases with similar injury to the nervous system. Dr. Pembrey's explanation of these contradicting results seems to be the correct one. He says: "The section of the spinal cord high up in the cervical region abolishes the power of regulating temperature. When the patient is exposed even to moderate cold, his temperature falls owing to the increased loss of heat and to the diminished production of heat. On the other hand, if the weather be hot and the patient be too well covered with bed clothes, his temperature rises and may reach a dangerous height, owing to the diminished loss and the increased production of heat in the body. In the paralysed man the production of heat rises and falls *with* the external temperature."† And Sir John Burdon Sanderson writes: "Section only shows the abnormal facility with which the body yields to the influence of outside conditions."‡

It might be urged that the variations of temperature, just before death, are due to a similar condition, but obviously this cannot be the case, for after section of the spinal cord many other factors arise which are not present in those patients who suffer from no such lesion of the nervous system. After section, the respiratory movements are altered in character, and respiration is entirely performed by the action of the diaphragm, and in consequence there is less loss of heat through this channel. Again, the muscles are paralysed, and, therefore, cannot produce the normal amount of heat, while the sweat glands are no longer active, and thus less heat is lost by the evaporation of moisture from the external surface. For these reasons alone the death variations of temperature found in the patients who are considered in the first part of this paper are in no way analogous to the changes which occur after section of the spinal cord in the cervical region.

Arguing from the supposition that the higher centres have a tonic control over the lower, it is to be expected that stimulation of the higher centres should lead to a still further control and diminution of the action of the lower. In this way falls of temperature may be caused. How far a toxic agent will stimulate it is difficult to say. It is possible that some antipyretic drugs may act in this way, such as quinine and salicylic acid. Other substances seem too powerful to stimulate and would appear to paralyse the higher centres. Smaller doses of the poison, however, may act as a stimulant, first to the higher and then to the lower centres, the former being affected before

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* Burdon Sanderson, "On the Process of Fever," 'The Practitioner,' vol. 16, 1876, p. 426.

† Pembrey, Schäfer's 'Text-Book of Physiology,' 1898, vol. 1, p. 862.

‡ Burdon Sanderson, "On the Process of Fever," 'The Practitioner,' vol. 16, 1876, p. 428.

the latter. It may be that for this reason, there is a slow rise of temperature in some diseases; while in others of more severe onset, there is a sudden elevation of temperature, which may be due to paresis of the higher centres. Septic conditions show frequently changes of death temperature, which may be the result of :—

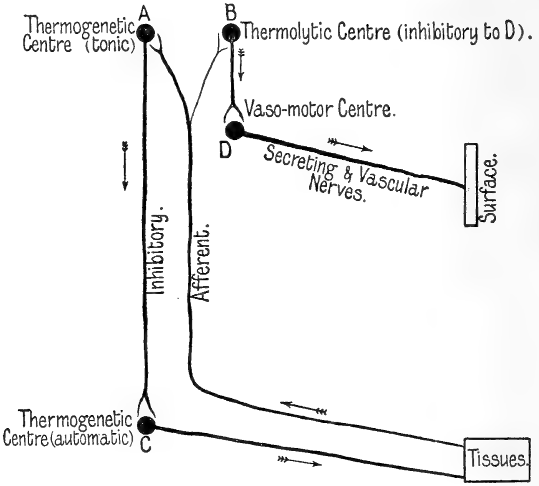
1. Stimulation of the higher centres over the lower, producing fall of temperatures.
2. Paralysis of the higher centres over the lower, producing rise of temperatures.
3. Simultaneous paralysis of higher and lower centres producing no change.
4. Special poisons may affect higher and lower centres differently.

There seems to be some evidence, therefore, in favour of the view that a higher centre in the brain controls the thermogenetic centres in the spinal cord, but it does not follow, when there is an increased heat production through the removal of this control, that there must necessarily be pyrexia. An increase in the loss of heat may keep the temperature at the normal level. In patients, however, who are dying, there is a tendency for the amount of heat lost to be diminished, the evidence for which has been pointed out above. Besides this natural tendency, means are constantly employed to prevent falls of temperature in the failing, by increasing the amount of bed-clothes, by using hot bottles, by bandaging the limbs before operation, etc. This slight diminution in the amount of heat lost may be able to prevent the temperature of the body falling, but it would hardly be sufficient to raise it many degrees without some increase in heat production. From these considerations it would appear that the variations in bodily temperature as death approaches must be dependent in many instances upon the increased amount of heat production and not upon the diminution of heat lost. As these variations are so constant, there is an indication that, although the centre which controls thermolysis, heat-loss, may be the chief factor in keeping the body at a normal temperature, yet the centre which controls thermogenesis plays a more important part than has lately been attributed to it.

The thermolytic centre probably has an inhibitory effect upon the vasomotor centre, *i.e.*, over vaso-constriction, in a similar manner, we think, to the action of the upper thermogenetic centre upon the lower in the spinal cord. As the lower thermogenetic centres tend to exaggerate the amount of heat produced in the tissues, so the vasomotor centre by constricting the vessels tends to diminish the amount of heat being lost; the latter effect being counterbalanced by the inhibitory action of the thermolytic centre. The action both of the lower thermogenetic centre and also of the vasomotor centre, when not controlled by the higher centres, seems to be that of raising the bodily temperature.

Whatever effect the fever-producing agent has upon the controlling power of the higher thermogenetic centre, it will probably act in a similar manner upon the thermolytic centre. If this should be the case, paralysis or weakening of the functions of these two centres would result in a much higher elevation of temperature than if the thermogenetic controlling centre in the brain alone were affected.

The following diagram shows the hypothetical relationship of the different nerve centres which take part in the regulation of the bodily temperature. The afferent path from the surface to the vaso-motor centre has been left out for the sake of clearness.



If A or B be paralysed or weakened, there is a tendency to pyrexia; if A and B be paralysed or weakened, there is a tendency to hyperpyrexia; if C or D be paralysed or weakened, there is a tendency to a fall of temperature.

If A or B be stimulated, there is a tendency to a fall of temperature; if A and B be stimulated, there is likely to be an extreme fall of temperature; if C or D be stimulated, there is a tendency to pyrexia.

From these considerations, and from the facts which have been elicited by a careful study of the death variations of temperature, the following theory for the causation of pyrexia seems to present itself. Pyrexia is due to two factors, to an augmented production of heat owing to the activity of the thermogenetic centres in the spinal cord being no longer perfectly controlled by the higher centre in the brain, and to a diminished loss of heat owing to the weakening of the functions of the thermolytic centre; the power of the two higher centres being weakened or paralysed by the morbid products or toxines of the affection from which the organism is suffering. In other words, normal temperature is preserved by a mutual see-saw action of these centres—the thermogenetic and the thermolytic.

We recognise fully that, for a more perfect understanding of death temperatures, it is necessary for the observers to examine the patients for themselves, and not to trust to records, however many or accurate they may be, so that they can note in each case the changes in the skin, the circulation, the respirations, etc., concurring with the variations of the bodily temperature. Nevertheless, we venture to put forward our investigation and views, not as physiologists, but as clinical observers, with the hope of pointing out new lines of research, by which may be increased the knowledge of the regulation of animal heat.

"A Note on the Action of Radium on Micro-organisms." By Alan B. Green, M.A., M.D. (Cantab.). Communicated by Sir Michael Foster, K.C.B., F.R.S. Received April 11,—Read May 5, 1904.

[Plate 11.]

The radium salt used in these experiments was 1 centigramme of radium bromide, bought of Messrs. Buchler and Co., of Brunswick, in June, 1903. It was contained in a vulcanite and brass capsule, fronted with thin talc. The radium was enclosed immediately behind the talc, and the circular area over which it was spread was about 3 mm. in diameter. The radium emanations which were applied to micro-organisms were such as passed through the talc, *i.e.*, the β and γ rays.

Dr. E. F. Bashford, to whose kindness I am greatly indebted for the use of the radium, has informed me that Sir William Ramsay tested the preparation for the intensity of its combined β and γ rays, the latter being a practically negligible quantity. The results showed that, on comparison with samples of radium bromide giving a virtually pure spectrum of radium, these rays were practically 100 per cent. This radium salt was, in fact, a pure preparation of radium bromide.

Dr. Bashford also informs me that this radium bromide caused pigment to disappear after 18 days from a mole with 15 minutes' exposure, the talc being in contact with the surface of the mole. Five minutes such exposure produced a marked skin reaction, while 20 minutes' exposure caused a reaction proceeding almost to ulceration.

I found that the radium was itself luminous, and that it could illuminate a screen of zinc sulphide through a sheet of lead over 1 cm. thick. It discharged a gold leaf electroscope, highly charged with + or − electricity, at a distance of over 6 feet. It caused a brown colouration of glass or talc when applied at a distance of 1 mm. for 12—24 hours.

The experiments which have been made are of two kinds. In the first set investigation was made of the germicidal action of radium emanations, and in the second, endeavours were made to ascertain whether micro-organisms exposed to the emanations became thereby themselves radio-active.

I.—*The Germicidal Action of Radium Emanations.*

The following have been subjected to the emanations of radium:—

(*a*) Calf vaccine, together with its contained extraneous bacteria, which in these experiments consisted of *S. pyogenes aureus*, *S. pyogenes albus*, *S. cereus flavus*, *S. cereus albus*. Both freshly collected and stored calf-vaccine pulp were exposed to radium emanations in the following way:—A layer of pulp not exceeding 0·5 mm. in thickness was spread in the centre of the depression of a hollow-ground glass slide, around the circumference of which depression a metal ring had been cemented. The capsule containing the radium bromide was placed upon the metal ring in such a way that the salt was brought within 1—2 mm. of the lymph pulp, nothing separating them but the talc of the capsule and the intervening air. The pulp was thus used in the thinnest practicable layer, in order that the emanations might act as uniformly as possible on all the component parts.

The vitality of the vaccine and of its extraneous bacteria was tested before, and at varying intervals of time after the exposure to radium; the former by inoculations on calves, the latter by cultivations on nutrient media. For each such test a small portion of vaccine was removed from the preparation and was mixed with enough sterile water to form a semi-fluid emulsion. A loopful of emulsion was used to inoculate a liquefied tube of nutrient agar-agar, and a plate was established in the usual way. The remainder of the emulsion was used for inoculating a calf.

(*b*) The following species of micro-organisms have also been separately subjected to the action of radium:—*S. pyogenes aureus*, *S. pyogenes albus*, *S. cereus flavus*, *S. cereus albus*, *Streptococcus pyogenes*, *B. prodigiosus*, *B. proteus vulgaris*, *B. pyocyaneus*, *B. typhosus*, *B. coli communis*, *B. mallei*, *B. pestis*, *B. tuberculosis*, the bacillus of Malta fever, *Spirillum choleræ Asiaticæ*, and sporing cultures of *B. mesentericus vulgatus*, *B. mesentericus ruber*, *B. subtilis*, *B. anthracis*, *B. tetani*, Gärtner's bacillus, the bacillus of malignant œdema, and the bacillus of Rauschbrand.

In the case of these micro-organisms, growth was removed from solid media and was exposed to the radium emanations as a film in the depression of a hollow-ground slide, the radium being applied as in the case of vaccine. The vitality of these micro-organisms was tested both before and after exposure to radium by cultivations on media by a similar method to that used in the case of vaccine.

In some instances metal rings were placed round colonies of bacteria *in situ* on the surface of nutrient medium, and exposure of bacteria under these conditions made by placing the capsule containing the radium on the rings.

In the case of each experiment with vaccine and with micro-organisms in pure culture, a control was carried out by making a similar preparation and subjecting it to similar conditions as the experimental preparation with the exception of exposure to radium; it was also subjected to tests for vitality similar to those used for the experimental preparation, at corresponding intervals of time. All experiments and their controls were made at room temperature.

The exposure of micro-organisms in liquid media to radium was found unsatisfactory owing to the presence of material of a complex nature between the radium and the micro-organisms, and owing also to the constant variation in the distance between the radium and the micro-organisms suspended in the liquid.

It was found from the foregoing experiments and their controls that a marked germicidal action was exerted on the specific and extraneous micro-organisms of vaccine and on the other above-mentioned micro-organisms as a result of their exposure to the radium at a distance of 1—2 mm. for varying lengths of time.

The following is a summary of the results of these experiments and their controls:—

Results of Experiments with Vaccine.

The specific germ in no case survived a longer exposure to radium than 22 hours, at the end of which time it had completely lost its ability to cause vesiculation or any visible irritation at the site of inoculation on a calf. In seventeen out of a total of twenty-five experiments its potency was destroyed after 10 hours' exposure to radium and in four cases after 2 hours. The controls remained fully potent after the experimental vaccines had been rendered inert.

The extraneous micro-organisms of these vaccines, as has been previously mentioned, consisted of *S. pyogenes aureus*, *S. pyogenes albus*, *S. cereus flavus*, *S. cereus albus*. In each experiment these bacteria were destroyed after exposure to radium in rather less time than was the potency of the specific germ. In no case did they survive a longer exposure to radium than 15 hours.

The extraneous micro-organisms of the control vaccines were alive after those of the experimental vaccines had been killed. The following experiment may be related to explain in greater detail this action of radium.

Experiment.

On November 12, 1903, vaccine pulp was exposed to radium at 10 A.M. Portions of this pulp were removed from the influence of the radium at the end of 2, 6, and 10 hours' exposure. Liquefied nutrient agar-agar tubes were inoculated with one platinum loopful of each portion immediately after its removal from the radium, and plates were established in the usual way. The remainder of each portion was inoculated on a calf on the following day, November 13.

Control.

Plates were similarly established from the control at 10 A.M. and 8 P.M. on November 12, and the remainder of the vaccine was used for inoculating the calf on the following day, November 13.

On November 18, the experimental portion of vaccine exposed to radium for 2 hours had caused good vesiculation on the calf; the portion exposed for 6 hours caused very poor vesiculation, and the remaining portion exposed for 10 hours caused no trace of vesiculation.

The number of extraneous bacteria originally present in the vaccine were 1200 per platinum loopful of emulsion (the mixture of this emulsion has been previously described). In the portion of vaccine exposed to radium for 2 hours the number left alive was 1050 per platinum loopful; in the portion exposed for 6 hours there were fifty bacteria, and in the portion exposed for 10 hours there was no evidence of living bacteria at all.

On November 18 the control portion of the vaccine had caused good vesiculation; and the agar-agar plate poured at 8 P.M. on November 12 contained practically the same number of colonies of extraneous bacteria as were present in the plate poured from the same vaccine at 10 A.M. on the same date.

Results of Experiment with Non-sporebearing Bacteria.

All the non-sporebearing bacteria previously mentioned were killed after exposure to radium for 2—14 hours. A description in detail of the results of some of these experiments may be of use in illustrating this germicidal action.

One of a Series of Experiments with S. pyogenes aureus.

Before exposure to radium, plate cultivations showed 84,000 bacteria present per platinum loopful of emulsion. After exposure to radium for 6 hours, this number had decreased to 31,000; at the end of 10 hours' exposure to 260, and at the end of 14 hours' exposure no bacteria were left alive.

At the end of 14 hours the control preparation showed bacteria alive in practically undiminished numbers.

One of a Series of Experiments with B. coli communis.

Before exposure to radium, 75,000 bacteria were present per platinum loopful of emulsion. After 3 hours' exposure this number was reduced to 3000, and after 6 hours' exposure all the bacteria were killed.

The control at this time showed practically the same number of bacteria as were present originally.

One of a Series of Experiments with Spirillum choleræ Asiaticæ.

Before exposure to radium, 47,000 bacteria were present in a platinum loopful of emulsion. After 3 hours' exposure to radium, 2100 were left alive per platinum loopful, while no bacteria survived an exposure of 6 hours.

The control preparation showed at this time practically no decrease in the number of bacteria originally present.

Results of Experiments with Bacteria containing Spores.

Bacteria containing spores were by far the most resistant to the germicidal action of radium of any micro-organisms used in these experiments, for they were not killed by less than 72 hours' exposure. This corresponds with the time given by R. Pfeiffer and E. Friedberger* as necessary for the killing of spores by the emanations of the radium used by them.

The following are examples of experiments with these micro-organisms :—

One of a Series of Experiments with B. mesentericus vulgatus (*Sporing*).

Immediately before exposure to radium the preparation showed 170,000 micro-organisms per platinum loopful of emulsion. After 48 hours' exposure this number had decreased to 260, and at the end of 72 hours all micro-organisms per platinum loopful had been killed.

The control preparation showed practically no decrease in the number of micro-organisms at the end of 72 hours.

One of a Series of Experiments with B. anthracis (*Sporing*).

There were originally present 11,000 micro-organisms per platinum loopful. After 48 hours' exposure to radium, 120 only were left alive, and at the end of 72 hours all micro-organisms per platinum loopful were killed.

The control showed micro-organisms present in undiminished numbers at the end of 72 hours.

One of a Series of Experiments with B. tetani (*Sporing*).

In the experiments with tetanus spores the actual numbers of micro-organisms were not investigated, but only the presence or absence of living germs by means of cultivations in the depth of sugar agar.

After 48 hours' exposure to radium, the presence of living micro-organisms was still evidenced, but after 72 hours' exposure no growth followed the inoculation of a sugar agar tube.

Thus in these experiments the non-sporebearing bacteria exhibited the least resistance to the germicidal action of radium emanations, withstanding exposure for 2—15 hours only.

The resistance of the specific germ of vaccine was slightly in excess

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* 'Berl. Klin. Woch.,' July 13, 1903.

of this; while by far the greatest resistance was shown by spores, these not being killed by less than 72 hours' exposure.

Experiments have also been made from which the following points have been noted.

1. As the distance between the radium and the micro-organisms subjected to its emanations was increased, the germicidal action which was marked at the nearest distance became less evident and finally ceased to be exerted.

In these experiments *Staphylococcus pyogenes aureus* was used, a separate strain being used for each series of experiments. Portions of growth were subjected to the radium emanations for the same time and under the same conditions, except that the distance between the radium and the bacteria was varied. After 30 hours' exposure it was found that—

At 1 mm. bacteria were killed.

At 1 cm. bacteria were usually lessened in numbers, but all were not killed.

At 10 cm. no definite germicidal action was apparent.

The following is an example:—

Experiment. *Staphylococcus pyogenes aureus* exposed to radium emanations for 30 hours at different distances.

Series.	Number of bacteria originally present per platinum loopful of emulsion.	Number of bacteria present per platinum loopful of emulsion after 30 hours' exposure to radium.		
		At 1 mm.	At 1 cm.	At 10 cm.
1 (*a*)	1060	0	..	..
1 (*b*)	1700	..	160	..
1 (*c*)	1200	..	..	987

2. As extra thicknesses of mica or glass were interposed between the radium and the micro-organisms exposed to their influence, the time of germicidal action was delayed. Finely woven copper gauze also caused slight delay of germicidal action. A sheet of lead 0·1 mm. thick, placed between the radium and the micro-organisms, caused weakening of germicidal action, and as extra thicknesses of lead were interposed and the β-rays were cut off, germicidal action became less and less evident.

II.—*Induced Radio-Activity of Bacteria.*

It has been found that after exposure at a distance of 1 mm. to the radium emanations for 24—120 hours, micro-organisms themselves may

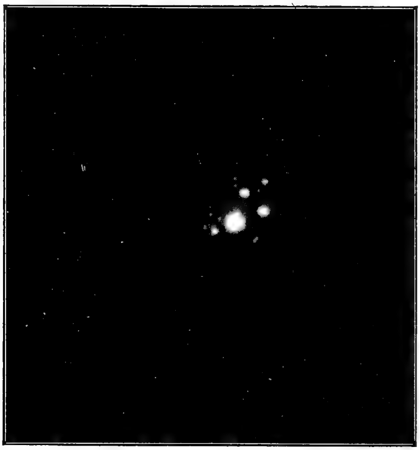

Fig. 1.—Photograph of a mass of *B. mesentericus vulgatus* (sporing), made radio-active by exposure to radium bromide for 72 hours and killed by the exposure.

Fig. 2.—Photograph, taken through a double layer of lead-foil, of a mass of *B. mesentericus vulgatus* (sporing), made radio-active by exposure to radium bromide for 72 hours and killed by the exposure.

show signs of radio-activity. It has not yet been ascertained whether living micro-organisms can exhibit induced radio-activity, but micro-organisms which have been killed by exposure to the radium emanations can do so.

In these experiments no radio-activity has been found in bacteria not exposed to the action of radium.

Induced radio-activity of micro-organisms has been shown in the following manner:—A small mass of bacteria, removed from the surface of nutrient medium, after subjection to the radium emanations at a distance of 1 mm., for, as a rule, 72 hours, was removed from the depression of the hollow-ground glass slide in which it had been exposed to the emanations, and was placed between two thin sheets of glass, generally coverslips, which were not themselves radio-active. These sheets of glass, with the small mass of bacteria pressed between them, were next, in a dark room, brought into contact with the film of an Ilford "special rapid" photographic plate. A cotton-wool pad was placed on the glass sheets to keep them in position, and the whole was wrapped up in a light-proof package. Twenty-four hours later the photographic plate was developed and a photograph was obtained of the bacterial mass. An image has been developed after only 1 hour's exposure of a sensitised plate to the radio-active bacteria, and in some instances after a fortnight's exposure.

Faint images have been thus produced on sensitised plates by *S. p. aureus* and *albus* which had been subjected to radium emanations, but, so far, the best photographs have been obtained from bacterial masses containing a number of spores, after their subjection to the emanations for 24—120 hours (Plate 11, fig. 1).

Radio-active micro-organisms have continued to give off photo-actinic emanations after 3 months have elapsed since their exposure to radium.

Photographs of masses of micro-organisms, possessing induced radio-activity, have been obtained, through a double layer of lead foil (Plate 11, fig. 2). A sheet of lead 3 mm. in thickness interposed between a radio-active bacterial mass and the sensitised plate has prevented the passage of photo-actinic emanations from the glass to the plate. These photographs would seem, therefore, to be caused by β-rays emitted by the micro-organisms.

My best thanks are due to Mr. Power, Medical Officer of the Local Government Board, for the facilities he has afforded me in the research, and for the kind and valuable advice he has given me concerning it.

"On certain Physical and Chemical Properties of Solutions of Chloroform in Water, Saline, Serum, and Hæmoglobin. A Contribution to the Chemistry of Anæsthesia.—(Preliminary Communication.)"

By Benjamin Moore, M.A., D.Sc., Johnston Professor of Bio-chemistry, University of Liverpool, and Herbert E. Roaf, M.B., Toronto, Johnston Colonial Fellow, University of Liverpool. Communicated by Professor C. S. Sherrington, F.R.S. Received April 12,—Read May 5, 1904.

The number of substances which have been shown to possess more or less well-marked anæsthetising properties reaches some hundreds, and hence it is obvious that the action cannot have a different explanation in each case, but rather depends upon some general type of interaction between the anæsthetic and the active part of the cell, which is the cell-protoplasm.

Further, the action occurs not only with nerve-cells, but with ciliated and other epithelial cells, with muscle-cells of all types, with bacteria, amœbæ, and other unicellular organisms, and with all types of vegetable cells in which activity is suited to experimental demonstration. In all these varied types of living cell, activity decreases alike with increasing dose of the anæsthetic, and, with sufficient concentration, all sign of life becomes obliterated.

Hence the action of the anæsthetic must be due to some change brought about in the only material which is uniformly present in all these types of cell, that is, the cell-protoplasm.

Accordingly, in briefly reviewing, as an introduction to our experiments, the previous theories of anæsthesia which have been advanced by various observers, we believe we may justly cast aside those which attribute it fundamentally to anything peculiar in the structure or chemical composition of the nerve-cell, or to any alteration in the nutrition of the nervous system, brought about by variations in its blood supply or otherwise.

It is true that cells differ in the degree of their reaction to anæsthetics, but not in kind, and ultimately the metabolic processes of bacteria are stilled as effectually as are those of the mammalian nerve-cell. Any such effects as anæmia or hyperæmia of the brain, which have been alternately described by various observers, must accordingly be only set down as secondary effects, and not as primary causes of anæsthesia.

Similarly, theories which are based on the peculiarly high content in cholestearin, lecithin, and fatty derivatives soluble in ether, of the nerve-cell and its processes, cannot furnish an explanation of anæsthesia, for these substances are not present in demonstrable quantity in by far the greater number of animal and vegetable cells.

Turning to the views of anæsthesia which rest upon an interaction between the anæsthetic and the cell-protoplasm, we find the speculation first thrown out by Claude Bernard* in 1875, that anæsthesia consists in a semi-coagulation of the substance of the (nerve) cell, a coagulation which may not be definite, that is to say, in which the substance can return structurally to its primitive state after elimination of the toxic agent. Bernard supports his view chiefly by analogy, and instances the stiffening and opacity of skeletal muscle when exposed to chloroform vapour.

A similar view was expressed by Binz,† who stated that sections of cerebral cortex placed in 1-per-cent. solution of hydrochlorate of morphia soon showed a cloudy appearance, and fine granules appeared in the nuclei; the protoplasm also became granular. The stage at which the cell-protoplasm was merely cloudy, and not discretely granular, could be recovered from by washing away the morphia, but, when once the granules appeared, they could not be made to disappear again. Similar results were obtained by exposing cortical nerve-cells to vapour of chloroform, or to solution of chloral hydrate.

Neither Binz nor Bernard showed, however, that there was any precipitation or semi-coagulation at or near the concentrations which correspond to anæsthesia, nor were the optical methods used capable of demonstrating effects upon the protoplasm short of precipitation.

Similar speculations of a *general* nature regarding the action of toxic agents, as being due to the formation of a loose, easily-dissociated compound between the toxic agent and the cell-protoplasm, have been thrown out by various writers, as, for example, Buchheim (1856) and Schmiedeberg (1883).

Demoor‡ has shown that, subsequent to prolonged and deep anæsthesia, the dendrites of nerve-cells acquire moniliform swellings, and has founded on this a mechanical theory, which rests on the view that the swellings observed are due to a retraction of the protoplasm of the dendrites, so that the communication of cell with cell is interrupted.

The swellings described by Demoor have also been observed by Hamilton Wright,§ who also found that they became larger, more numerous, and encroach more and more on the dendritic stems the longer the anæsthesia is kept up.

These effects are of importance as evidence of an interaction between protoplasm and anæsthetic, but the retraction theory of Demoor based

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* 'Leçons sur les Anesthésiques,' etc., 1875, p. 153.

† 'Vorlesungen über Pharmakologie,' p. 175.

‡ 'Arch. de Biol.,' 1896, vol. 14.

§ 'Journ. of Physiology,' vol. 26, 1900, p. 30; vol. 26, 1901, p. 362.

on them will not hold in view of more recent work on the fibrillar mode of communication between cell and cell. Further, as pointed out above, any adequate view as to how anæsthetics produce their effect must be applicable also to the unicellular organism, and not merely to the nerve-cell, or any colony of cells.

Wright obtained further effects as a result of prolonged anæsthesia, which led him to adopt the view that the action is bio-chemical in character. He found, for example, that the Nissl's bodies lost their affinity for basic dyes, such as methylene blue, but that this effect was only temporary, and disappeared as soon as the anæsthesia had passed off.

These changes, however, do not begin to appear immediately the anæsthetic commences to produce its effect, and are rather a signal of the changes produced in the protoplasm in marked degree by the prolonged action of the anæsthetic than an indication of the first reaction between cell-protoplasm and anæsthetic.

Our own attention was first attracted in this direction by witnessing the experiments of Sherrington and Sowton* upon the effects of chloroform on the excised mammalian heart, fed by a current of Ringer's or Locke's solution, and through which later a similar current, but containing in addition small amounts of chloroform, could be perfused.

These authors observed that a concentration of chloroform in the Locke solution, amounting to only 1 in 100,000, produced a marked and unfailing action in diminishing the extent of the cardiac contractions, and further that this effect appeared rapidly after the dilute chloroform solution reached the heart, lasted just as long as the chloroform in this excessively low but yet adequate concentration was passed through, and ceased almost immediately as soon as the normal Locke's solution was recurred to, the heart attaining again its normal force.

This effect could be repeated as often as was desired, and there was no cumulative action whatever, that is, no matter how prolonged the passage of the chloroform solution on passing back to the normal Locke's solution, the chloroform effect rapidly disappeared, and again recovered when the chloroform was once more turned on.

It was this latter effect which suggested the experiments on the chemistry of anæsthesia recorded in this paper. It was quite obvious that the effect of the chloroform upon the cardiac muscle fibres depended solely upon the concentration (solution tension, or osmotic pressure) of the chloroform in the cell for the time being, and not at all upon the total amount of chloroform which had been fed to the heart up to the moment of observation.

This experimental fact suggested the view that the effect upon the

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* Thompson Yates, 'Johnston Laboratories Reports,' vol. 5, Part 1, p. 69.

cell was due to some combination being formed between the protoplasm and the chloroform, and further that this combination was not a stable fixed one, leading to permanent removal of the protoplasmic activity, but an unstable one, which existed only so long as the pressure of the anæsthetic was kept up to a definite level, and gradually dissociated as the level of chloroform pressure was allowed to fall, and as a result left the protoplasm free for a renewal of its metabolic processes—to choose a familiar analogy, that the protoplasm of a cell undergoing anæsthetisation entered into a combination with the anæsthetic, similar to that between hæmoglobin and oxygen, unstable in character, and only lasting so long as the pressure of the anæsthetic was kept up.

It occurred to us, as protoplasm is built up chemically of proteid, that a certain amount of evidence as to the formation of such an unstable compound might be obtained, in the first instance, by experimenting with proteids.

We accordingly experimented with the proteids of the blood, and have obtained a number of results which together point to the formation of such compounds as are indicated above.

It is our intention to proceed further and study in a similar fashion the effects of chloroform upon various types of living cell, but we here present the work done upon proteids, which appears to us to prove that an easily dissociable compound is formed between proteid and chloroform.

Our experiments may be described under the following headings :—

1. On the obvious physical and chemical changes produced in serum and in hæmoglobin solution by the addition of chloroform.

2. On the relative solubility of chloroform in water, normal saline solution, serum, and hæmoglobin solution.

3. On the relative vapour pressures of chloroform when dissolved in water, saline, serum, and hæmoglobin solutions respectively, and on the variations in the coefficient of distribution in these solutions.

4. On the solubilities of gases in serum and hæmoglobin solution in presence of chloroform.

I.—*Effects of Chloroform on Serum and on Hæmoglobin Solution.*

On adding chloroform* to either serum or hæmoglobin solution, and allowing the mixture to stand, changes occur which are obvious to the eye, and were to us previously unknown, but on consulting the literature we found that they had been observed by E. Salkowski† in

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* The chloroform used for all the experiments described in this communication was presented to us by Messrs. Duncan and Flockhart, of Edinburgh.

† 'Deutsche Med. Wochensch.,' 1888, No. 16; 'Zeitsch. f. Physiol. Chem.,' vol. 31, 1900, p. 329. The fact that the red blood corpuscles combine with chloroform is also mentioned by Schmiedeberg, 'Arch. f. Heilkunde,' 1867, p. 273.

using chloroform as a preservative for these fluids, and were also described by Formánek,* the bearing of such phenomena upon the question of anæsthesia was not, however, appreciated by these previous observers, who had approached the matter from a different standpoint, and as we have in some respects amplified their observations, and in others have obtained results not quite in accord with theirs, we feel justified in here recording our experiments.

It was observed by E. Salkowski in 1888 that blood could not be preserved by adding chloroform, because it gradually became converted into a thick mass.

In 1891 it was observed by Horbaczewski† that hæmoglobin was precipitated from a solution containing it, and kept at a temperature of 40—50° C., to which chloroform was added as a preservative.

The subject was investigated more minutely by Formánek‡ in 1900, and this observer found that a solution of hæmoglobin kept at 50—55° C. for some time with chloroform was completely precipitated, the filtrate being entirely free from hæmoglobin.

Formánek dried and analysed the precipitate, and from the absence of chlorine after fusion with sodium carbonate and potassium nitrate came to the conclusion that the precipitate is not a chloroform compound of hæmoglobin. In our opinion this is not a valid proof, as the chloroform need not be so stably combined with the chloroform as to stand drying at 130° C., and subsequent fusion as employed by Formánek. The precipitate was dissolved by Formánek after thorough washing to remove the chloroform by the addition of a few drops of sodium carbonate solution, and the solution gave the bands of oxy-hæmoglobin, and on treatment with ammonium sulphide reduced hæmoglobin. Formánek also found that blood serum and egg-albumin were precipitated (when the reaction of the fluid was acid or neutral) on keeping at a temperature of 50—55° C. in presence of chloroform. From these experiments this observer came to the conclusion that the precipitate with which he was dealing was a mixture of hæmoglobin and other proteids thrown out of solution by the chloroform. It is also stated in this paper that oxy-hæmoglobin is only slowly and incompletely precipitated by the action of chloroform at room temperatures.

E. Salkowski, in his later paper,§ states that blood kept at a temperature of 40° C. for 24—48 hours in presence of chloroform changes to a thick mass, but found that the precipitation of the hæmoglobin was not complete under such circumstances.

Regarding the action of chloroform on serum, he states that serum

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* 'Zeitsch. f. physiol. Chem.,' vol. 29, 1900, p. 416.

† Quoted by Formánek, *loc. cit.*

‡ 'Zeitsch. f. physiol. Chem.,' vol. 29, 1900, p. 416.

§ 'Zeitsch. f. physiol. Chem.,' vol. 31, 1900, p. 329.

can be preserved in contact with chloroform for years without precipitation at room temperatures, and finds this in agreement with Formánek's results, who found, in presence of an alkaline reaction, no precipitation of serum by chloroform even at a temperature of 50—55° C. Formánek does not state whether his alkaline reaction is the natural alkaline reaction of the serum. Salkowski also found a precipitating action of chloroform upon solutions of albumose and casein.

Our experiments were conducted with hæmoglobin and serum obtained from pig's blood. The serum used was obtained from clotted blood and was thoroughly centrifugalised before use. The hæmoglobin was in all cases obtained by centrifugalising the blood corpuscles three times with normal saline, and then laking with distilled water and making up to the same volume as that of the blood taken.

In the case of serum we found that the fluid acquired, with less than 1 per cent. of chloroform (and greater quantities up to saturation), a peculiar opalescent and fluorescent appearance, but remained quite transparent to transmitted light. On the addition of over 2 per cent. of chloroform, there is a tendency to precipitation even in the cold, and at the end of 24—48 hours there is a slight precipitate present, but the effect is much hastened on placing the mixture in an incubator at 40° C., so that it becomes impossible to determine the maximum solubility of chloroform in serum at body temperature. Both in obtaining precipitation in the cold and more rapidly at 40° C. in presence of the natural alkaline reaction of the fluid, our results are at variance with those of Formánek and Salkowski. The results were obtained several times in succession.

The marked opalescence in the serum was obtained in preparation of solutions of known concentration in chloroform for purposes of measurement of their vapour pressures, and led us to doubt at first whether we were not dealing with a fine emulsion of chloroform in the serum. Since this point was of vital importance to our experiments on vapour-pressure, we investigated it as completely as possible.

In the first place, examination with the microscope of the opalescent fluid showed no visible globules of chloroform, even with the highest powers.

To make certain of the matter, a current of air from an aspirator was bubbled first through chloroform contained in a Woulff's bottle, afterwards through a similar bottle containing water, and then, *at the same temperature*, was sent through a third Woulff's bottle containing serum. By this procedure the serum never came in contact with fluid chloroform, nor with air more highly charged with chloroform vapour than corresponded to the saturation of the air in contact with it or passed through it.

There could, hence, be no condensation of chloroform and no means by which an emulsion of chloroform, finer even than could be seen with a microscope, could be formed.

Very soon, however, after the chloroform vapour began to pass through, a distinct difference in appearance was observable between the serum and a control placed alongside, and after a time the serum charged with chloroform in this manner was as opalescent as the specimens made in the usual way by shaking with weighed quantities of chloroform, and gave similar results with regard to vapour pressure.

These results show that the marked opalescence is not due to an emulsion of chloroform, and further that it is not due to precipitation of proteid in the ordinary sense of the word, for no precipitate can be seen with the microscope in the opalescent fluid.

Further, experiments on the vapour pressure show that the value of this is a long way from the maximum value, which it must obviously possess if an emulsion was present.

A similar opalescence is obtained on adding to serum other organic liquids which possess anæsthetic properties, thus we have obtained it on saturation of serum with ether,* benzol and xylol, but have not followed the matter up as we have done with chloroform.

In the case of hæmoglobin solutions, we have not been able to observe an opalescence, similar to that seen in the case of serum, up to the strength at which precipitation begins. In order to study where precipitation commences it is necessary to keep the hæmoglobin and chloroform constantly stirred, or otherwise before the chloroform has dissolved the lower layer of hæmoglobin solution in contact with the liquid chloroform becomes precipitated. When precautions are taken to prevent this occurring, precipitation is found to take place when about 2 per cent. of chloroform has been added, that, is long before saturation is reached (*vide infra*). This precipitation prevents both the determination of the solubility of chloroform in hæmoglobin solutions, and the observation of the vapour pressure with high concentrations. With concentrations of 1 per cent. or under no precipitation whatever was found to occur. Contrary to Salkowski and Formánek, we found that no raising of the temperature was required to cause precipitation of the hæmoglobin; in fact, with strong solutions, the precipitation occurs within a few hours, even in the ice chamber. With time the precipitation is complete, and the precipitate is insoluble in water or saline, but in dilute sodium carbonate it is easily soluble, and we have found that the solution then shows the spectrum of alkaline hæmatin and not that of oxy-hæmoglobin as was found by Formánek.

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* The opalescence with ether is not nearly so well marked, but increases on standing, and the solution in time becomes extremely viscid.

II.—*On the Relative Solubility of Chloroform in Water, Saline, Serum, and Hæmoglobin Solution.*

In so far as we have been able to discover, no attention has been paid by previous experimenters to the maximum amount of chloroform capable of solution in the blood or serum as compared with that taken up by water or saline solution isotonic with blood.

When any reference is made to the matter it has been usually assumed on general principles that the serum or plasma will behave like a saline solution of equal concentration and dissolve somewhat less chloroform than water.* In other words, that there is no specific action of the proteids or other substances in the plasma.

This supposition we have found experimentally to be entirely erroneous, for both serum and solutions of hæmoglobin dissolve much more chloroform than water or normal saline solution. This fact is of importance in regard to the mode of action, as it definitely points to an interaction between the chloroform and the proteid present.

The presence of fats would of course increase the apparent solubility of chloroform in serum, and hence it is necessary in all cases to use perfectly clear serum, free from suspended fat; this precaution we have always been careful to observe, and in addition the serum has always been centrifugalised.

In this connection it may be added that the hæmoglobin solutions which we have employed could not contain fatty matter, and hence the high solubilities which we have observed could only arise from chemical interaction between the hæmoglobin and the chloroform.

Methods for Determining Maximum Solubility.

Three methods have been used in the determination of the maximum solubility of chloroform in the solvents mentioned above, which have given concordant results and shown that the solubility in proteid solution is much higher than in water or saline.

In the first method we have determined the amount of chloroform dissolved by obtaining the product of volume and vapour pressure at low pressure and with a small volume of fluid, so that practically all the chloroform was simply pumped off into the vacuum. In this method the volume of fluid experimented with is necessarily small, and this gives rise to experimental error of measurement, which is added to by the volume measured being large and pressure small, so that the results are only approximative, yet it is observable that they confirm those obtained by the more accurate methods described below.

The details of the method are described in the succeeding section on the relationship between vapour pressure and concentration of

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* Overton, 'Studien über die Narkose,' p. 93.

chloroform, and it need only be mentioned here that we have obtained solubilities of 0·95 per cent., in normal saline (0·75 per cent.), 3·33 per cent. in serum and 4·42 per cent. in whipped blood, by this method.

The second method employed consists in weighing out known amounts of chloroform into water and serum and hæmoglobin solution respectively, and then determining by direct observation that concentration in each case at which the chloroform ceased to be dissolved.

This method of observation is made easy in the case of chloroform by the high specific gravity of that fluid, as a result of which on inverting the flask in which the determination is being made even minute globules of undissolved chloroform can be seen falling through the fluid.

The determinations of solubility by this method are made on the following plan. Pure chloroform is dropped from a fine capillary pipette into a tared graduated flask of 25, 50 or 100 c.c., and carefully weighed to definite amount, corresponding, when the flask has been filled by the solvent under experiment, to a definite percentage of chloroform. A series of such flasks is prepared, and immediately after each flask is filled with the desired solvent and either shaken thoroughly by hand until solution is complete, or placed on a rotary shaking machine. After the lapse of several days, during which time the flasks are never opened and are kept shaken up, it is noticed at what level of concentration the chloroform ceases to be completely dissolved, and so the solubility is determined.

The lower strengths of known value short of saturation, and also the saturated solutions so prepared, were kept and used also for the experiments on vapour pressure at varying concentration described in the next section of this paper.

The following results were obtained by the application of this method at room temperatures, approximately (13° C.), for the percentage by weight dissolved:—

Water, 0·8 per cent. dissolved, 0·9 per cent. dissolved, 1 per cent. not dissolved completely. Estimated solubility 0·95 per cent.

Saline Solution (0·75 per cent. sodium chloride in water), 0·7 per cent. dissolved, 0·8 per cent. dissolved, 0·9 per cent. not dissolved. Estimated solubility about 0·83 per cent.

Serum, 3 per cent. dissolved, 3·5 per cent. dissolved, 4 per cent. all dissolved save a few small globules.

Hæmoglobin Solution or Blood.—Over 6 per cent. by weight is taken up when chloroform is shaken with blood or hæmoglobin solution of equal strength to the blood, prepared from blood by centrifugalising several times with saline, and subsequent laking with distilled water, and no globules of chloroform can be seen on careful examination with the microscope. But the solution rapidly changes in colour, and a precipitate is thrown out on standing, as above described, which is quite insoluble in water or saline, but easily soluble in dilute sodium carbonate solution, and then gives the spectrum of alkaline hæmatin.

The blood begins to give this precipitate when about 1·5 per cent. of chloroform has been added at room temperature, but with a lower concentration, and more rapidly when heated to body temperature in the incubator. Two per cent. of chloroform gives a precipitate in the cold, and on heating to 40° C. a red flocculent precipitate leaving a clear colourless fluid above.

The third method for determining the solubility of chloroform in the fluids experimented with consists in shaking up thoroughly for several hours with an excess of chloroform, and then pipetting off and determining the amount of chloroform in the solution.

The difficulty here is a rapid and accurate method of determining the amount of chloroform contained in a measured volume of the given saturated solution.

The procedure finally employed for this purpose, which is also being experimented with as a method for quantitative estimation of chloroform in blood and serum at lower values reaching to the anæsthetising value, was as follows, and led to very accurate results.

A measured volume (usually 10 c.c.) of the fluid saturated with chloroform is placed in a flask fitted airtight with a double bored cork, and a stream of hydrogen is aspirated through the solution, the oxygen present in the flask and connections is absorbed by passing through alkaline pyrogallate, and the mixture of hydrogen and chloroform is then burnt by passing over heated palladium asbestos placed in a very short combustion tube. All the chlorine in the chloroform is thus burnt to hydrochloric acid, and the amount of this absorbed in standard alkali is then estimated, first by back titration against standard acid, and then further checked, either by volumetric titration with standard silver nitrate solution, or by gravimetric determination as silver chloride.

The serum used in these determinations was examined for chloroform emulsion by the microscope, but no undissolved chloroform in suspension was observed. The precipitate in serum at atmospheric temperature obtained by this method of shaking up with excess of chloroform was very dense, so that the serum became quite opaque.

The results obtained by employing this method were as follows :—

Distilled water, dissolved 0·95 per cent., and serum, dissolved 5·08 per cent.

III.—*On the Vapour Pressure of Chloroform Dissolved in Varying Concentration in Water, Saline, Serum, and Hæmoglobin Solutions respectively.*

A determination of the vapour pressure of an anæsthetic in solution at varying concentrations in serum, in hæmoglobin, or in blood, is of high practical importance, since it is upon the relationship of this vapour pressure to the concentration of the solution that the amount of anæsthetic taken up by the blood circulating through the lungs depends.

It has hitherto been taken for granted that the Dalton-Henry law can be applied, and that the amount of anæsthetic taken up is strictly proportional to, and varies directly with, the percentage of the vapour of the anæsthetic in the inspired air.

This has never, however, to our knowledge, been experimentally tested, and it seemed to us desirable to attempt such a determination. We have investigated from this point of view solutions of chloroform in serum, hæmoglobin solution (of equal strength in hæmoglobin to the blood from which the hæmoglobin was prepared) and whipped blood, and have contrasted the pressures obtained with those of solutions in chloroform, in water, and normal saline at equal concentrations.

The vapour pressures have been measured corresponding to concentrations ranging from considerably below the anæsthetising values for chloroform vapour pressure in air (viz., 8—10 mm.) observed by Paul Bert, up to the saturation points in most cases.

Apparatus.

The instrument employed for this purpose was a form of "differential densimeter," which, after passing through many modifications, took the form represented in the accompanying sketch (fig. 1), which is drawn approximately to a scale of $\frac{1}{4}$.

The two tubes shown are exactly similar, and are graduated in cubic centimetres and tenths in the upper portion, and in centimetres in the lower and wider portion.

The tubes are connected as shown by means of thick-walled rubber tubing and a glass Y-piece to a stout glass mercury receiver capable of holding more than enough mercury to fill both tubes and their connections.

The tubes are held in a vertical position by clamps attached to the strong vertical iron bar of a massive retort stand, and each tube is capable of being moved in its clamp vertically up and down for purposes of adjusting the mercury levels.

In order to keep a constant temperature (in the case of the experiments carried out at body temperature) the upper portion of each tube, from about the middle of the wide part to the level of the stopper at the top, was encased in a hot-water jacket of the form shown in detail in fig. 2, and omitted for clearness from fig. 1.

It was found convenient to use for the outer glass tubing of this jacket the largest size of a common variety of paraffin-lamp chimney, which measured 8 cm. in diameter above, bulged out as shown to 9 cm., and then narrowed below to 6 cm.

The wider top and bulge were found very useful in facilitating the introduction and vigorous movements of the bar electro-magnet used as a stirrer in connection with the iron stud (seen in figs. 1 and 2, at the top of the mercury), which was dropped into each tube, and, during an experiment, agitated up and down so as to thoroughly mix the fluid under experiment, and so bring it into equilibrium more rapidly with the vapour in the space above it.

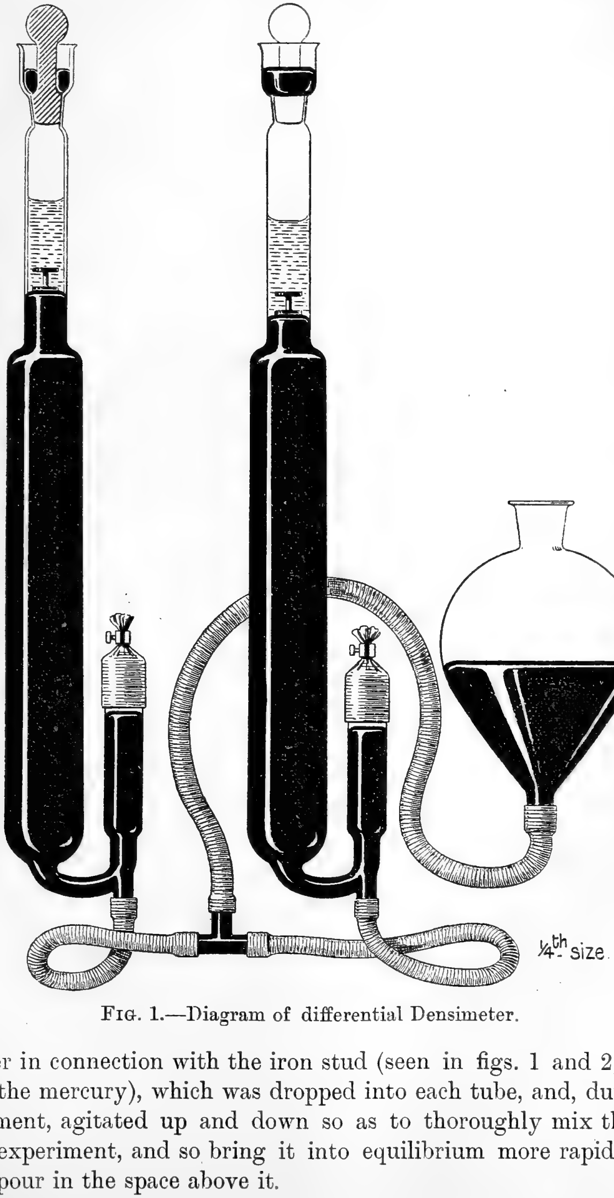

Fig. 1.—Diagram of differential Densimeter.

The hot-water jacket was made watertight below by means of an india-rubber cork, as shown (fig. 2); the rubber cork was also bored in each case for two narrow glass tubes, for the purpose of carrying

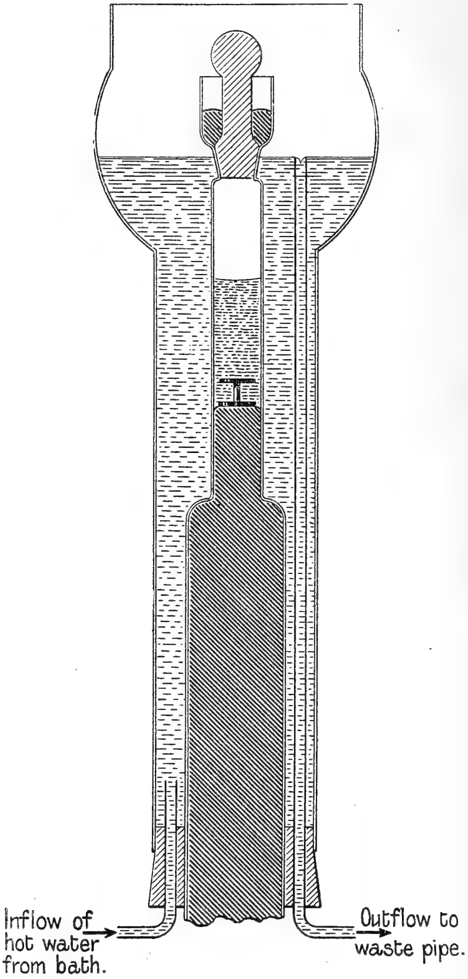

Fig. 2.—Section taken through the upper portion of one tube of the differential Densimeter and the hot-water jacket, showing the inflow and outflow tubes. Scale $\frac{1}{3}$.

water to and from the jacket. The two tubes carrying the water in, stopped about 2 cm. above the upper surface of the rubber cork in each tube, so as to prevent blocking by mercury accidentally run over from the top when the inner tubes were being cleared out at the termination of a measurement. The outer ends of these two tubes were attached by means of narrow rubber tubings and a glass Y-tube to a bath of hot water, placed at a higher level, and a screw-down clip on each rubber tube regulated the flow until a thermometer placed in the corresponding hot-water jacket showed the desired temperature. A constant level of water was kept up automatically in the warm supply bath, and its temperature was regulated so as to lie 2—3° above that of the jackets. The two outflow tubes passed up, as shown, inside the jacket to the level of the ground in glass stopper, and their outside ends were connected by means of rubber tubing to the waste pipe.

In fig. 1 the upper portion of the left-hand tube is shown in section, and that of the right-hand one in outline. The ground in stoppers shown were found, when sealed with mercury, to be much more effectual against minute leakages, which entirely vitiate the results, than any form of tap, and they are also much more convenient for introducing the solutions to be experimented upon, and for cleaning out the apparatus. Further, since they do not require to be operated between the commencement and termination of each determination of vapour pressure, they are better adapted to their particular purpose than a tap. In the course of our experiments, we also found it necessary to be able to dilute a solution with more of its solvent without allowing it to come in contact with any appreciable volume of air, and for this purpose found the stopper arrangement most convenient.

The side tube shown at the lower end of each main tube was designed to trap air which was found to slowly leak in through the rubber pressure tubing, when a vacuum was established by lowering the mercury receiver, and for a long time was a source of annoyance. We subsequently learned that the device had been first introduced by Lord Rayleigh. At the end of an experiment, any air which has collected is discharged by raising the mercury-holder, opening the screw-clip shown, and allowing enough mercury also to pass through to form a seal in the rubber tubing above the clip.

The mercury-holder was suspended by means of a ring and loop of wire attached around the bulb from a vertical rod, swivel, and hook, possessing a slow screw movement in a block attached to a horizontal rod fixed in a clamp, which could be moved up and down on a heavy retort stand. For large movements, the clamp was slid up or down the retort standard, and for fine movements the screw was raised or lowered in its block.

In using the apparatus, the two vertical tubes are first placed at the same level, the mercury-holder is filled with mercury, and, with the two glass stoppers out, the whole apparatus is filled with mercury, the two stoppers are next inserted, enough mercury being left above them to form a seal, and the mercury-holder is then lowered until the two vertical tubes become evacuated. The receiver is then raised again to the level of the stoppers, and any bubble of air found is discharged.

The apparatus is now ready for an experiment, and, with stoppers out, the levels of mercury are adjusted until there is an equal volume left above the mercury on each side. A given volume of the solvent (say 5 c.c.) is now introduced on the one side (say left), and an equal volume of the solution of chloroform in the same solvent on the other side. In each case, immediately after the fluid has been introduced, the stopper is inserted, care being taken to prevent any air being included, either as a bubble at the mercury surface, or between the surface of the introduced fluid and the stopper. To achieve the latter end, we have almost always introduced above the mercury 2 or 3 c.c. more than the required quantity, so that it stood in the neck and slightly above, and then, by easing the stopper and gently adjusting the level of the mercury-holder, have brought the level of the mercury in the tube to the desired volume mark. After the solvent on the one side, and the solution of chloroform of the desired strength on the other side, have been successfully introduced in equal volume, and without any bubble of air, the mercury-holder is lowered until a space containing vapour has appeared on each side. The level of the mercury will be found to be lower on the chloroform side, and it is obvious, the instrument being independent of variations in atmospheric pressure, and the only different factor being the added chloroform on the one side,* that the difference in pressure will give the vapour pressure directly for that strength of chloroform solution at that particular temperature.

There is hence no need to determine pressure due to dissolved gases on the two sides,† or pressure of aqueous vapour, since these balance, and the quickness with which readings can be directly obtained makes it possible to carry out a long series of determinations at varying strengths, without the proteid solutions having time to undergo bacterial change.

Certain precautions have to be taken, however, and corrections made which may here be mentioned:—

1. Before taking a reading it is essential to move the tubes vertically and adjust the levels until the volumes of the vapour spaces above the upper aqueous solution meniscus in each case are exactly equal, otherwise inequality in pressure of gases pumped off on the two sides gives rise to an error, which is greater the smaller the vapour space.

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* There will be a small difference in the pressure of water vapour on the two sides, due to there being a stronger solution on the chloroform side, but this is in all cases too minute compared to the pressure of the chloroform vapour to make any appreciable error.

† Slight differences in dissolved gases gave a disturbance with very dilute chloroform solutions, and this was later obviated by pumping the gases off (*vide infra*).

2. Before taking a reading, there must be certainty that each fluid is in equilibrium with its vapour space. This is shown by absence of variation when the apparatus is left at rest.

For rapid and accurate working the mechanical stirring by means of the studs and magnet is indispensible, for even after the lapse of an hour when at rest the solution has not completely discharged its proper amount of chloroform into the vapour space. When once the control, containing, of course, no chloroform, has been thoroughly stirred it remains constant, and need not be changed at the end of each determination, but can be used throughout an entire experiment.

By vigorous stirring, equilibrium can be attained in 5—10 minutes, and the level does not afterwards change no matter how long stirring and observation be kept up. This important experimental observation we have taken occasion to verify several times during our experiments.

3. For very accurate working, especially with the dilute solutions and low pressures, it is necessary in the case of serum and hæmoglobin to pump off the dissolved gases by means of a Töpler pump, otherwise these come off unequally from solvent and solution and disturb the results at the low pressures. The chloroform solutions are then made up from the pumped-out solvent, which also must be used for control and for making the dilutions.

4. The temperature must be the same in the jackets surrounding each tube at the time when each reading is taken, and in a series of determinations at varying strength and a constant temperature, that temperature must be closely maintained throughout. The temperature error is a maximum when the solutions are near saturation, for then the variation in vapour pressure per degree is very large; fortunately here the differences in level under observation are also very large, which diminishes the percentage error arising from small deviations in temperature.

At concentrations away from saturation, the variations arising from small differences in temperature approximately obey the gas law, and under the conditions of our experiments become quite negligible.

5. A correction must be made in all cases, upon the concentration of the solution introduced into the tube, for the amount of chloroform pumped off from the solution into the vapour space. This correction is, of course, larger in the case of the more concentrated solutions with high vapour pressures.

This has been done in the experiments of which records are given below, and accounts for the concentrations not being exact percentages or small fractions of exact percentages.

The amount of chloroform in the vapour space is readily calculated from the product of the observed vapour pressure and the volume of the vapour space, and this amount deducted from the quantity contained in the chloroform solution when it was introduced, gives the necessary datum for calculating the concentration in chloroform of the solution corresponding to the observed vapour pressure in the vapour space.

The ratio of the vapour concentration in the fluid to the concentration in the vapour space gives the coefficient of distribution (*coefficient de partage*, *Theilungscoefficient*): this should remain constant if the absorption of the chloroform vapour by the liquid were normal and strictly proportional to the vapour pressure, and if it varies it points to a physical or chemical aggregation or compound between the chloroform and the fluid or its constituents (*vide infra*).

Method of Reading.—The readings were taken with a cathetometer,* placed about 4 feet from the tubes, both for greater accuracy in reading than direct measurement would give and to avoid changes in temperature.

Two Methods of Experimentation.—We have employed two different methods of experimentation in investigating the variation in vapour pressure with varying concentration. It is obvious that the concentration of a measured volume of a strong solution introduced into the densimeter may be diminished by pumping off more and more chloroform, by increasing the volume of the vapour space above the solution.

A series of readings of differences in pressure may thus be obtained in which the vapour space on the two sides is kept equal and of known and increasing value throughout the series. This method we have called the method of "*variable vapour space.*"

On the other hand, a series of solutions of known and steadily diminishing or increasing concentration may be introduced into the densimeter and measured one after another as to their vapour pressure, in each case with a known fixed volume of vapour space. As stated above, the concentration at which each vapour pressure in the series is measured is then accurately known. This method we have called the method of "*constant vapour space.*"

Method of "Variable Vapour Space."—In this method, unless the volume of the tubes of the densimeter is very large, the volume of solution and solvent respectively introduced must be very small. We have usually taken $\frac{1}{2}$ c.c. on each side, either of a saturated solution or of a very strong solution of known strength, and by altering the levels of the tubes and mercury receiver, have taken a long series of readings, in each case with equal volume of vapour space on each side, at every increasing volume of vapour space, until the difference in pressure became of small value, and the product of volume and vapour pressure became approximately constant, showing that practically all

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* The instrument used was made by Pye and Co., of Cambridge, and, by means of a vernier and divided screw-head, read to $\frac{1}{100}$ of a millimetre. We have frequently observed that we were able to take readings within five divisions, that is $\frac{1}{20}$ mm., which is far within the accuracy of other portions of our determinations.

the chloroform had been pumped off from the solution. This constant product then gave the necessary datum for calculating the concentration of the original solution introduced into the densimeter, the product of vapour pressure and volume at each stage gave the datum for calculating the quantity of chloroform pumped off from the solution, and therefore for deducing the corresponding concentration of solution. Or, also, by plotting vapour pressures as abscissæ, and the product of vapour pressure and volume of vapour space as ordinates, the ratio of vapour pressure and amount of chloroform absorbed at each stage could be shown.

The method of "*variable vapour space*" has, however, two working disadvantages, which caused us in the end to abandon it and replace it by the method of "*constant vapour space*." The first objection is that the amount of solution taken is small, hence it is difficult to measure it with accuracy, and to make it equal on the two sides.

The second objection is that at the low concentrations the increase of volume for a small fall in pressure is very large, and hence the determinations become inaccurate, a small error in pressure reading making a large deviation. The values for high pressures are also inaccurate, but for a different reason; these readings are taken with small volumes of vapour space, and unless the vapour spaces are accurately equal on the two sides, there is a large disturbance due to inequality in pressure of the previously dissolved gases pumped off on the two sides.

The results, however in the intermediate pressures are accurate and are given below, as they confirm those given by the other method.

Method of "Constant Vapour Space."—In using this method we have always introduced a volume of 5 c.c. of the solvent on one side, and 5 c.c. of a solution of known strength on the other, and have invariably adjusted the levels so that at the temperatures of observation there was a vapour space of exactly 5 c.c. on each side.

In some cases we have started with a saturated solution of chloroform and have then made dilutions of different percentages of that solution, in the manner described below. In the later experiments we found it, however, more expedient, on account of knowing the exact concentration directly, to prepare a solution of known strength, say 1 per cent., and for the more dilute solutions to use various percentage dilutions of this stock solution. The more concentrated solutions were obtained by making up solutions varying by 1 per cent. in strength, and ½ per cent. differences were got by mixing these with each other in equal proportions. For percentages less than 0·1 per cent., a 0·1-per-cent. solution was first prepared by making a ten-fold dilution of the 1-per-cent. solution, and this 0·1-per-cent. solution was then diluted similarly to the 1-per-cent. solution.

Precautions in Preparing, Preserving, in Unaltered Strength, and Diluting by known Amounts, of the Solutions Used.

In working with the solutions, it is indispensable that due precautions be taken against loss by escape of chloroform into the air during the various manipulations.

If a portion, for example, of a stock solution of known strength be pipetted off for use in the densimeter, and then the stock bottle be merely stoppered, the air space over the portion of solution left in the bottle rapidly becomes charged with chloroform vapour at the expense of the stock solution, and a second sample taken later from the same bottle will be found to be weak and give a wrong result. The way to guard against this is to fill the bottle with mercury up to the neck immediately after drawing off, and at once stopper up. Similar devices were employed in all dilutions to complete the process out of contact with air.

The stock solutions were made by direct weighing, by dropping from a fine pipette, into graduated glass-stoppered flasks of 25, 50, or 100 c.c. capacity, according to the amount required, immediately filling up to the mark with the particular solvent, and setting at once upon a slowly rotating disc, driven at such a rate that the chloroform globules have just time to fall through the solution each time the flask is inverted. In this way, a rapid solution is effected, so saving much time. Further, in certain cases such shaking is indispensable, for a solution of hæmoglobin left in contact with even a small amount of chloroform without continuous shaking soon forms a precipitated layer at the bottom.

The stock solutions were kept carefully stoppered, unless at the moment of introducing the pipette to remove a portion for experiment, and then the space within the bottle was always filled to the stopper with mercury in the manner above described.

We found much difficulty owing to leakage of chloroform during dilution in the case of the strengths intermediate between the stock strengths, until it occurred to us to make these dilutions in the densimeter itself. For this purpose we always placed the mercury level at 10 c.c. below the stopper, on the side of the densimeter in which the mixing was to be carried out, then the proper amounts of the two solutions necessary to give the desired strength was drawn up in two graduated pipettes and run into the densimeter tube, the stopper was then inserted, including only a minute bubble of air, and now by thorough agitation of the iron stud through the fluid by means of the electro-magnet a thorough mixing of the two fluids was attained, then by raising the mercury-holder and slightly easing the stopper 5 c.c. were allowed to escape, the mercury level being placed accurately at 5 c.c. A seal of mercury was finally dropped in above the stopper, and so the dilution was effected.

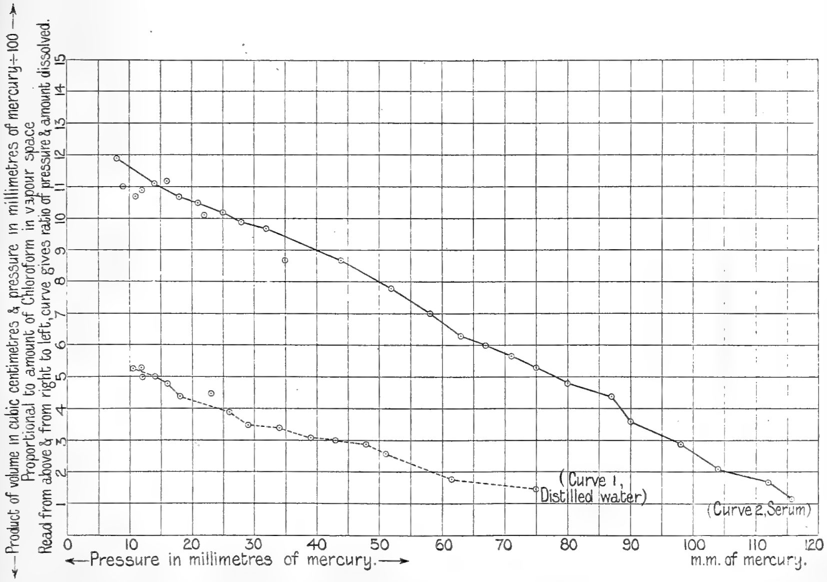
For example, to obtain a dilution of 1·5 per cent., 5 c.c. of 1-per-cent. solution and 5 c.c. of 2-per-cent. solution were drawn off into pipettes, placed in the densimeter and mixed as above described; to obtain a 0·4-per-cent. solution, 6 c.c. of the solvent were taken and 4 c.c. of a 1-per-cent solution, and similarly treated; to obtain a 0·03-per-cent. solution, 7 c.c. of solvent and 3 c.c. of 0·1-per-cent. solution were taken, and so on.

Certain of our experiments were carried out at room temperature and others approximately at body temperature (40° C.); the following protocols and accompanying curves show some of the typical results obtained, which have been confirmed in most cases by duplicates :—

Variable Vapour Space.

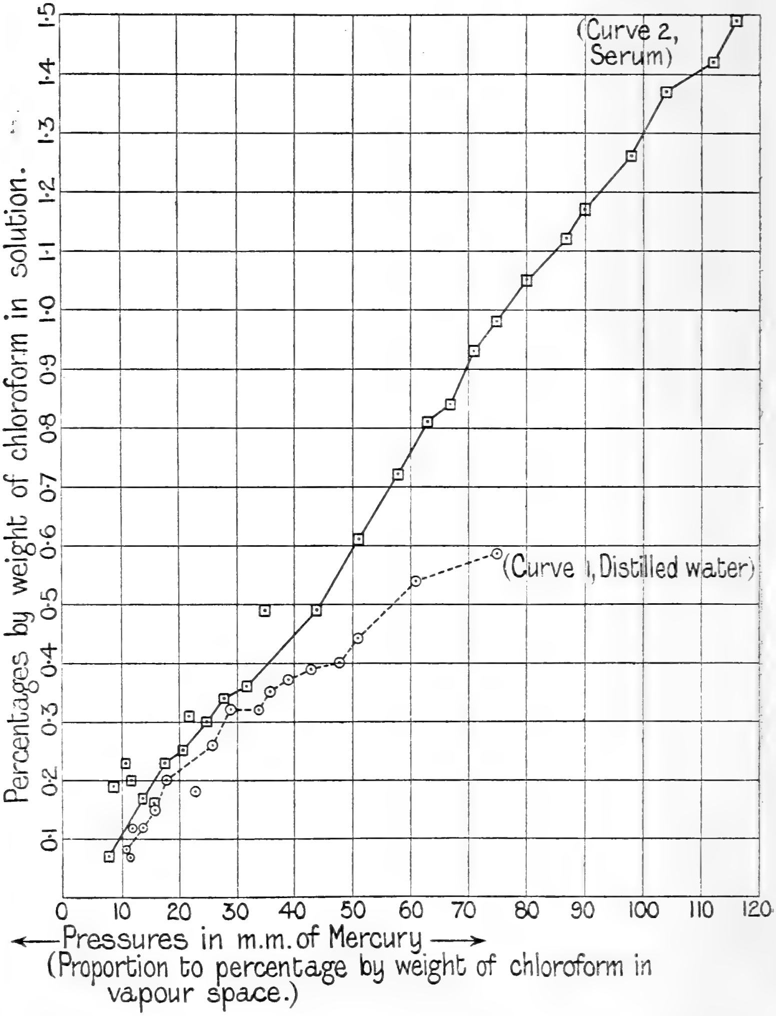
Experiment 1.—Distilled water containing approximately 0·78 per cent. of chloroform. Half a cubic centimetre was introduced into each tube of the densimeter, of the chloroform water on one side and of the same distilled water without chloroform on the other. The temperature at which the experiment was carried out was 17° C., and the volume at which readings of pressure were taken varied from 2—50 c.c. The percentage of chloroform in the water was not known directly, but was calculated by extrapolation of the curve showing V.P., see Curve 1, fig. 3.

Fig. 3.



The following table gives the results of the experiment, which are also shown graphically in Curve 1, fig. 3, in which the abscissæ show pressures of chloroform vapour and the ordinates the product of the volume and pressure of vapour. In Curve 1, fig. 4, the same experiment is shown with pressures of chloroform vapour as abscissæ, and percentage of chloroform dissolved at each pressure as ordinates :—

FIG. 4.



Experiment 1.—Distilled Water.

Temperature 17° C.

Volume of vapour space.	Pressure of chloroform in vapour space in mm. of mercury.	Percentage by weight of chloroform in vapour space.	Weight in grammes of chloroform in vapour space.	Weight in grammes of chloroform in solvent.	Percentage by weight of chloroform in solvent.	Coefficient of distribution between vapour space and solvent.
2	74 ·61	0 ·04957	0 ·00099	0 ·00293	0 ·586	1 : 11 ·8
3	60 ·62	0 ·04027	0 ·00121	0 ·00271	0 · 542	1 : 13 ·4
5	51 ·29	0 ·03408	0 ·00170	0 ·00222	0. 444	1 : 13 ·0
6	47 ·69	0 ·03168	0 ·00190	0 ·00202	0 ·404	1 : 12 ·7
7	43 ·08	0 ·02862	0 ·00197	0 ·00195	0 ·390	1 : 13 ·6
8	38 ·91	0 ·02585	0 ·00207	0 ·00185	0 ·370	1 : 14 ·3
9	35 ·98	0 ·02390	0 ·00215	0 ·00177	0 ·354	1 : 14 ·8
10	34 ·36	0 ·02283	0 ·00228	0 ·00164	0 ·328	1 : 14 ·4
12	28 ·83	0 ·01915	0 ·00230	0 ·00162	0 ·324	1 : 16 ·9
15	26 ·16	0 ·01738	0 ·00263	0 ·00129	0 · 258	1 : 14 ·8
20	22 ·62	0 ·01503	0 ·00301	0 ·00091	0 ·182	1 : 12 ·1
25	17 ·65	0 ·01173	0 ·00293	0 ·00099	0 ·198	1 : 16 ·9
30	15 ·84	0 ·01052	0 ·00316	0 ·00076	0 ·152	1 : 14 ·5
35	14 ·37	0 ·00954	0 ·00334	0 ·00058	0 ·116	1 : 12 ·2
40	12 ·43	0 ·00835	0 ·00334	0 ·00058	0 ·116	1 : 13 ·9
45	11 ·87	0 ·00787	0 ·00354	0 ·00038	0 ·076	1 : 9 ·7
50	10 ·54	0 ·00700	0 ·00350	0 ·00042	0 ·084	1 : 12 ·0

Experiment 2.—Serum containing approximately 1·65 per cent. of chloroform, the amount being determined by extrapolation of Curve 2, fig. 3. Experiment conducted in all respects similarly to Experiment 1. Graphic records of results shown by Curve 2, in figs. 3 and 4.

Experiment 2.—Serum.

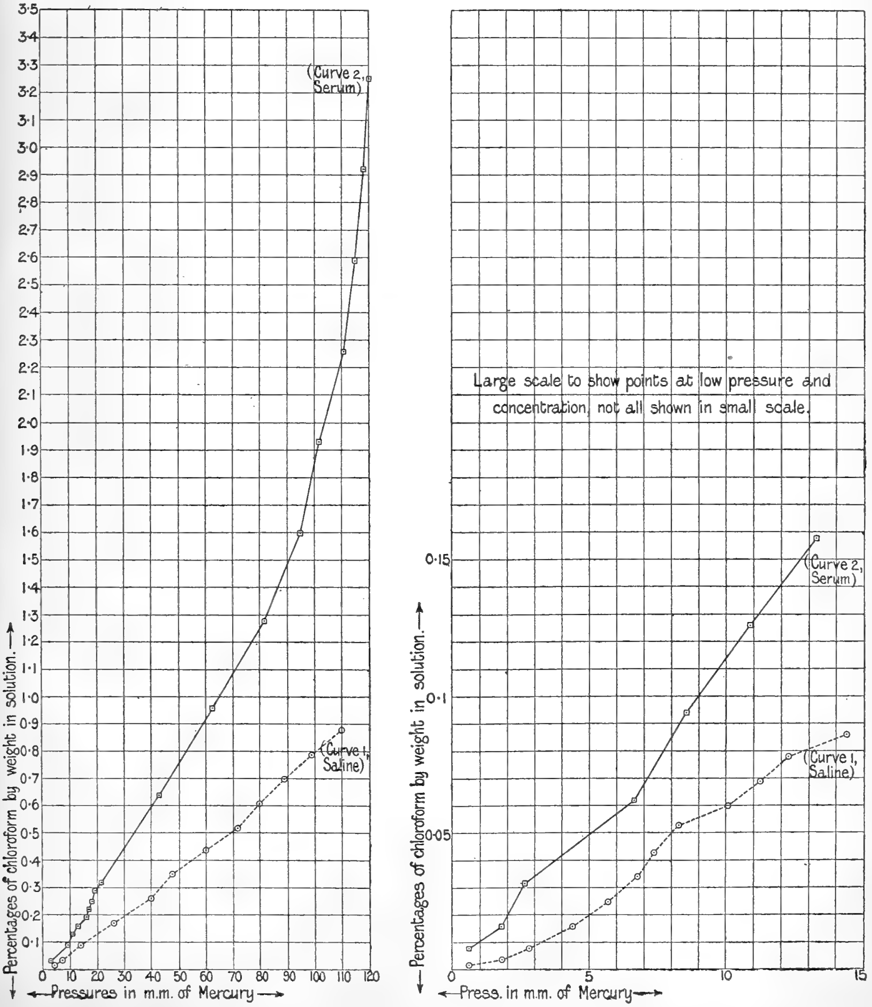
Temperature 18° C.

Volume of vapour space.	Pressure of chloroform in vapour space in mm. of mercury.	Percentage by weight of chloroform in vapour space.	Weight in grammes of chloroform in vapour space.	Weight in grammes of chloroform in solvent.	Percentage by weight of chloroform in solvent.	Coefficient of distribution between vapour space and solvent.
1	115·63	0·07655	0·00077	0·00746	1·492	1 : 19·5
1·5	111·85	0·07405	0·00111	0·00712	1·424	1 : 19·2
2	104·27	0·06904	0·00138	0·00685	1·370	1 : 19·9
3	98·15	0·06498	0·00195	0·00628	1·256	1 : 19·3
4	89·89	0·05763	0·00237	0·00586	1·172	1 : 20·4
5	87·05	0·05277	0·00264	0·00559	1·118	1 : 21·2
6	79·70	0·04975	0·00299	0·00524	1·048	1 : 21·7
7	75·14	0·04730	0·00331	0·00492	0·984	1 : 20·6
8	71·46	0·04487	0·00359	0·00464	0·928	1 : 20·7
9	67·16	0·04447	0·00402	0·00421	0·842	1 : 18·9
10	63·24	0·04187	0·00419	0·00404	0·808	1 : 19·3
12	58·05	0·03843	0·00461	0·00362	0·724	1 : 18·9
15	52·31	0·03463	0·00519	0·00304	0·608	1 : 17·6
20	43·64	0·02889	0·00577	0·00246	0·492	1 : 17·0
25	34·97	0·02315	0·00579	0·00244	0·488	1 : 21·1
30	32·38	0·02144	0·00643	0·00180	0·360	1 : 12·1
35	28·20	0·01867	0·00653	0·00170	0·340	1 : 18·2
40	25·44	0·01684	0·00674	0·00149	0·298	1 : 17·7
45	22·40	0·01483	0·00668	0·00155	0·310	1 : 20·9
50	21·03	0·01392	0·00696	0·00127	0·254	1 : 18·3
60	17·91	0·01186	0·00712	0·00111	0·222	1 : 19·0
70	16·03	0·01061	0·00743	0·00080	0·160	1 : 15·1
80	13·93	0·0092	0·00737	0·00086	0·172	1 : 18·7
90	12·12	0·0080	0·00722	0·00101	0·202	1 : 25·3
100	10·70	0·0071	0·00708	0·00115	0·230	1 : 32·4
125	8·77	0·0058	0·00726	0·00097	0·194	1 : 33·4
150	7·91	0·0052	0·00786	0·00037	0·074	1 : 14·3

Experiments with Constant Vapour Space.

Experiment 3.—Saline solution containing 0·75 per cent. of sodium chloride, and approximately 0·95 per cent. of chloroform, determined from product of pressure and volume after pumping off. Five c.c. of the saline introduced on one side as a control and 5 c.c. of the chloroform solution on the other, and a vapour space of 5 c.c. being formed on each side. Temperature of experiment 15° C. The results are shown graphically in Curve 1, fig. 5.

FIG. 5.



Experiment 3.—Saline.

Temperature 15° C.

Percentage of chloroform by weight in solution originally introduced.	Pressure of chloroform in vapour space in mm. of mercury.	Percentage by weight of chloroform pumped off into vapour space.	Percentage of chloroform by weight remaining in solution.	Coefficient of distribution between vapour space and solvent.
0·0024	0·56	0·0004	0·0020	1 : 5·0
0·0048	1·78	0·0012	0·0036	1 : 3·0
0·0095	2·76	0·0018	0·0077	1 : 4·3
0·0190	4·44	0·0030	0·0160	1 : 5·3
0·0286	5·65	0·0038	0·0248	1 : 6·5
0·0381	6·79	0·0045	0·0336	1 : 7·2
0·0476	7·38	0·0049	0·0427	1 : 8·7
0·0571	8·34	0·0056	0·0515	1 : 9·2
0·0666	10·14	0·0068	0·0598	1 : 8·8
0·0762	11·28	0·0075	0·0687	1 : 9·2
0·0857	12·25	0·0082	0·0775	1 : 9·5
0·0952	14·43	0·0096	0·0856	1 : 8·9
0·1904	25·68	0·0172	0·1732	1 : 10·1
0·2856	39·76	0·0256	0·2600	1 : 10·2
0·3808	47·96	0·0321	0·3487	1 : 10·9
0·4760	60·16	0·0402	0·4358	1 : 10·8
0·5712	72·21	0·0483	0·5299	1 : 10·8
0·6664	79·75	0·0534	0·6130	1 : 11·5
0·7616	88·67	0·0593	0·7023	1 : 11·8
0·8568	99·16	0·0663	0·7905	1 : 11·9
0·9520	110·39	0·0738	0·8782	1 : 11·9

Experiment 4.—Serum containing approximately 3·33 per cent. of chloroform determined as in Experiment 3. Experiment performed similarly to Experiment 3 and temperature also 15° C. Results plotted in Curve 2, fig. 5.

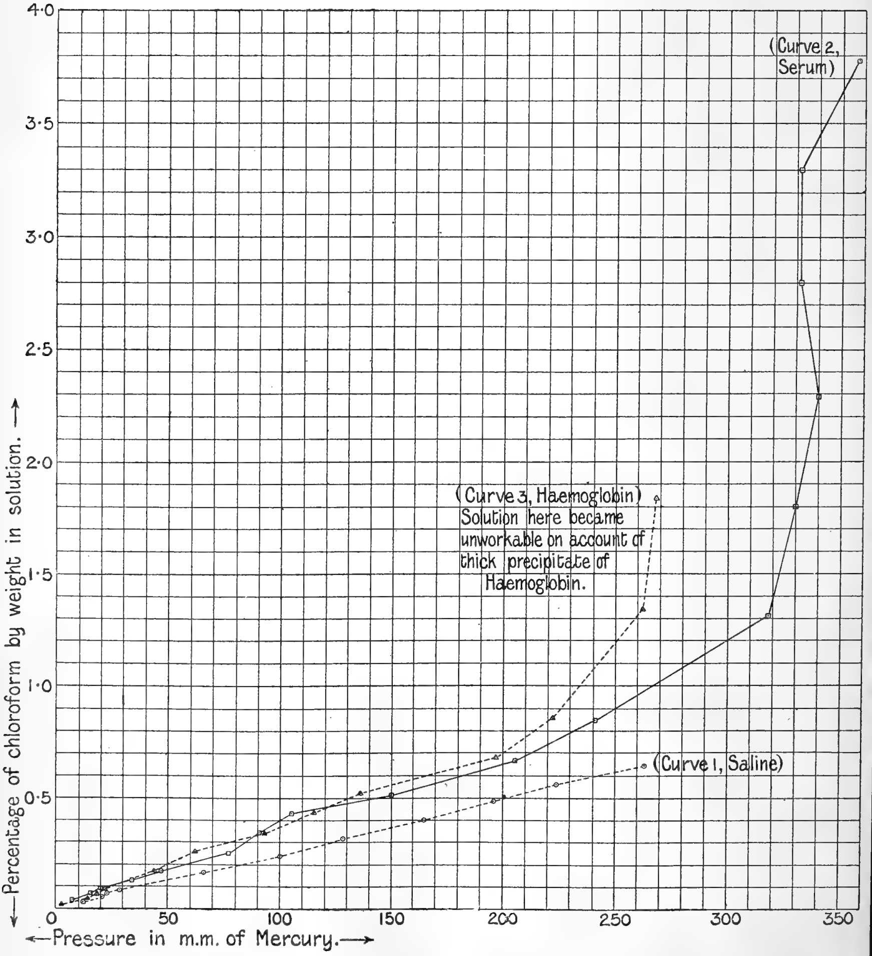
Experiment 4.—Serum.

Temperature 15° C.

Percentage by weight of chloroform originally introduced.	Pressure of chloroform in vapour space in mm. of mercury.	Percentage by weight of chloroform pumped off into vapour space.	Percentage by weight of chloroform remaining in solution.	Coefficient of distribution between vapour space and solvent.
0·0083	0·66	0·0004	0·0079	1 : 19·7
0·0167	1·82	0·0012	0·0155	1 : 12·8
0·0333	2·74	0·0018	0·0315	1 : 17·5
0·0666	6·66	0·0045	0·0621	1 : 13·8
0·0999	8·59	0·0057	0·0942	1 : 16·5
0·1332	10·85	0·0073	0·1259	1 : 17·3
0·1666	13·28	0·0089	0·1577	1 : 17·7
0·1999	15·54	0·0104	0·1895	1 : 18·2
0·2332	16·96	0·0113	0·2219	1 : 19·5
0·2665	18·37	0·0123	0·2543	1 : 20·7
0·2998	19·19	0·0128	0·2870	1 : 22·4
0·3331	22·49	0·0150	0·3181	1 : 21·2
0·6662	43·02	0·0281	0·6381	1 : 22·7
0·9993	63·15	0·0422	0·9571	1 : 22·7
1·3324	82·16	0·0550	1·2774	1 : 23·5
1·6655	95·30	0·0638	1·6017	1 : 25·1
1·9986	101·64	0·0680	1·9306	1 : 28·4
2·3317	110·53	0·0740	2·2577	1 : 30·5
2·6648	115·30	0·0771	2·5877	1 : 33·6
2·9979	117·53	0·0786	2·9123	1 : 37·1
3·3310	120·41	0·0810	3·2500	1 : 40·1

Experiment 5.—Saline containing 0·75 per cent. of sodium chloride, and the amounts of chloroform shown in the table, the maximum being 0·8 per cent. originally in solution. In this and succeeding experiments the amount of chloroform was known directly by weighing out as described above. The temperature in this and succeeding experiments was 40° C. The results of this experiment are shown in Curve 1, fig. 6.

FIG. 6.



Experiment 5.—Saline (0·75 per cent.).
Temperature 40° C.

Percentage by weight of chloroform originally introduced.	Pressure of chloroform in vapour space in mm. of mercury.	Percentage by weight of chloroform pumped off into vapour space.	Percentage by weight of chloroform remaining in solution.	Coefficient of distribution between vapour space and solvent.
0·04	11·75	0·0722	0·0328	1 : 4·6
0·06	20·59	0·0127	0·0473	1 : 3·8
0·08	23·29	0·0143	0·0657	1 : 4·6
0·1	29·35	0·0181	0·0819	1 : 4·5
0·2	65·62	0·0404	0·1596	1 : 4·0
0·3	99·83	0·0615	0·2385	1 : 3·9
0·4	129·11	0·0795	0·3205	1 : 4·0
0·5	166·04	0·1022	0·3978	1 : 3·9
0·6	196·05	0·1207	0·4793	1 : 4·0
0·7	224·04	0·1379	0·5621	1 : 4·1
0·8	262·52	0·1616	0·6384	1 : 4·0

Experiment 6.—Serum containing amounts of chloroform shown in table by direct weighing. Results shown in Curve 2, fig. 6.

Experiment 6.—Serum.
Temperature 40° C.

Percentage by weight of chloroform originally introduced.	Pressure of chloroform in vapour space in mm. of mercury.	Percentage by weight of chloroform pumped off into vapour space.	Percentage by weight of chloroform remaining in solution.	Coefficient of distribution between vapour space and solvent.
0·04	6·80	0·0042	0·0358	1 : 8·5
0·05	8·51	0·0052	0·0448	1 : 8·6
0·06	11·64	0·0072	0·0528	1 : 7·3
0·08	15·09	0·0093	0·0707	1 : 7·6
0·1	19·74	0·0122	0·0878	1 : 7·2
0·15	34·12	0·0210	0·1290	1 : 6·1
0·2	47·27	0·0291	0·1709	1 : 5·8
0·3	77·43	0·0477	0·2523	1 : 5·3
0·4	91·46	0·0563	0·3437	1 : 6·1
0·5	105·31	0·0663	0·4337	1 : 6·5
0·6	149·54	0·0921	0·5079	1 : 5·5
0·8	205·22	0·126	0·674	1 : 5·3
1·0	240·49	0·148	0·852	1 : 5·8
1·5	317·74	0·195	1·305	1 : 6·7
2·0	329·75	0·203	1·797	1 : 8·9
2·5	339·81	0·209	2·291	1 : 10·9
3·0	332·36	0·205	2·795	1 : 13·6
3·5	332·00	0·204	3·296	1 : 16·2
4·0	357·08	0·220	3·780	1 : 17·2

Experiment 7.—Solution of hæmoglobin, of equal strength in hæmoglobin to blood from which obtained, and containing the amounts of chloroform shown in the table. The results are shown graphically by Curve 3 of fig. 6.

Experiment 7.—Hæmoglobin Solution.

Temperature 40° C.

Percentage by weight of chloroform originally introduced.	Pressure of chloroform in vapour space in mm. of mercury.	Percentage by weight of chloroform pumped off into vapour space.	Percentage by weight of chloroform remaining in solution.	Coefficient of distribution between vapour space and solvent.
0·02	2·85	0·0018	0·0182	1 : 10·1
0·04	7·31	0·0045	0·0355	1 : 7·9
0·06	14·52	0·0089	0·0511	1 : 6·6
0·08	17·79	0·0110	0·0690	1 : 6·3
0·1	22·34	0·0138	0·0862	1 : 6·3
0·2	43·85	0·0270	0·1730	1 : 6·4
0·3	62·01	0·0382	0·2618	1 : 6·8
0·4	93·15	0·0573	0·3427	1 : 5·9
0·5	114·91	0·0707	0·4393	1 : 6·2
0·6	136·47	0·0840	0·5160	1 : 6·2
0·8	197·10	0·1213	0·6787	1 : 5·6
1·0	222·64	0·1371	0·8629	1 : 6·3
1·5	262·26	0·1614	1·3386	1 : 8·3
2·0	268·00	0·1650	1·8350	1 : 11·1

IV.—*Solubilities of Gases in Serum and Hæmoglobin in Presence of Chloroform.*

It was thought that such compounds as are shown by the above experiments to be formed between chloroform and serum or hæmoglobin solution, might interfere with the carriage of oxygen and carbon-dioxide by the blood. Accordingly, experiments were carried out upon serum and hæmoglobin solutions to test this point.

A volume of about 500 c.c. of serum or of hæmoglobin solution obtained as for the experiments in Section 3, was completely deprived of gases by exhaustion with a Töpler pump at 40° C., afterwards saturation with air or air and carbon-dioxide, was carried out upon two equal volumes contained in similar bottles to one of which a sufficient quantity of chloroform was added to make a 1-per-cent. solution, while the other served as a control. The gases dissolved in each case were then collected by means of the Töpler pump as before, and analysed.

The results throughout were negative, and thus proved *a fortiori* that at the anæsthetising values chloroform does not depress the solubility of the respiratory gases in the blood.

As an example, an experiment with serum shaken up with a mixture of air and carbon-dioxide may be quoted :—

A volume of 500 c.c. of serum was exhausted as above described.

(*a*) A volume of 150 c.c. of this serum was poured into a 500 c.c. stoppered bottle, and shaken up with a mixture of air and carbon-dioxide.

Exhaustion and analysis of the gases in 70 c.c. gave the following results at 14° C. and 759 mm. :—

$$CO_2\ 41{\cdot}2 \text{ c.c.},\ O = 1{\cdot}4 \text{ c.c.}\quad N = 5{\cdot}4 \text{ c.c.}$$

(*b*) A second volume of 150 c.c. of the exhausted serum treated exactly similar, but with 1·5 grammes of chloroform added, gave the following results from 70 c.c., at the same temperature and pressure :—

$$CO_2\ 41{\cdot}4 \text{ c.c.},\ O = 1{\cdot}8 \text{ c.c.}\quad N = 6{\cdot}4 \text{ c.c.}$$

There was obviously a slight leakage of air, but the figures are sufficient to show that there is no appreciable change in the solubilities due to the presence of the chloroform.

Summary and Conclusions.

1. We believe that the experiments recorded above justify the conclusion that chloroform forms an unstable chemical compound or physical aggregation with the proteids experimented with, and that it is carried in the blood in such a state of combination. Since proteids build up the protoplasm of living cells, it appears to us probable that chloroform, and other anæsthetics, must form similar combinations with protoplasm, and that anæsthesia is due to the formation of such compounds which limit the chemical activities of the protoplasm. The compounds are unstable, and remain formed only so long as the pressure of the anæsthetic in the solution is maintained. Such compounds are formed not only by hæmoglobin but by serum proteid, and hence the position taken by the anæsthetic in hæmoglobin is not that of the respiratory oxygen. This is further shown by the fact that the oxygen-carrying power of hæmoglobin is not interfered with in presence of chloroform.

The effect of chloroform upon various forms of protoplasm will form the subject of future experiments.

The facts upon which we rely as proofs of the formation of a compound or aggregation between chloroform and serum proteid or hæmoglobin may be summarised as follows :—

(*a*) Chloroform has a much higher solubility in serum or hæmoglobin solutions than in saline or water.

(*b*) *Even in dilute solutions* at the same pressure the amount of chloroform dissolved in serum or hæmoglobin solution is considerably higher than in saline or water.

(*c*) The curve of pressures and concentrations in the case of water and saline is a straight line, while in the case of serum and hæmoglobin solution it is a curve, showing association at the higher pressures.

(*d*) In the case of serum, chloroform causes a marked opalescence, and also a slow precipitation at room temperature (15° C.), and at body temperature (40° C.) a rapid, though incomplete precipitation. In the case of hæmoglobin, 1·5—2 per cent. of chloroform causes a change of colour and commencing precipitation at room temperature, which becomes almost complete in the thermostat at 40° C., while 5 per cent. and over causes complete precipitation even at 0° C.

2. The relations between chloroform pressure and concentration in solution have been worked out throughout a long range, from below the anæsthetising values (8—10 mm.) to nearly saturation in the case of water, saline, and serum.

Attention may be drawn here to the important practical fact that with the same percentage of chloroform in the air breathed, serum or hæmoglobin, and therefore the blood, will take up much more chloroform than would water or saline under equal conditions. Thus at the anæsthetising pressure, and at 40° C., the coefficient of distribution in the case of water and saline is approximately 4·6, while that of serum is 7·3; at room temperature (15° C.) these coefficients become 8·8 and 17·3 respectively.

"On the Changes of Thermoelectric Power produced by Magnetisation, and their Relation to Magnetic Strains." By SHELFORD BIDWELL, M.A., Sc.D., F.R.S. Received April 11,—Read April 28, 1904.

Summary.

It is well known that magnetisation generally produces a change both in the thermoelectric quality of a magnetisable metal, and also in its linear dimensions. In an article published in October, 1902,* I directed attention to the remarkable qualitative correspondence which appeared in several cases to exist between the two classes of phenomena, and the experiments which form the subject of the present paper were undertaken with the view of investigating their apparent relation. Some of the results were of an unexpected character. The accepted statements regarding certain thermoelectric effects to which in this connection special importance was attached, turn out to be at least not generally true, and, as far as my own observations go, altogether erroneous. Several experimenters appear to have been misled by regarding as unmagnetised a piece of metal which either retained some permanent magnetism or was continuous with the piece subjected to magnetisation. What they in fact observed was not the thermoelectric power of magnetised with respect to unmagnetised metal, but that of more strongly magnetised with respect to less strongly magnetised; and the effects may be, as will appear, directly opposite in the two cases. Probably also mistakes have arisen from the assumption that for a lengthened period the electromotive forces in the circuit underwent no changes other than those due to magnetisation. It is hardly possible to keep the temperatures throughout so nearly constant as to avoid gradual changes of electromotive force considerably greater than those which it is desired to measure. In my own work careful precautions were taken against both these sources of error; before every observation the metal was demagnetised by reversals, and the galvanometer reading for no magnetisation was noted.

Although, as mentioned, some of the chief grounds which formed the basis of my conjecture as to a possible relation between the thermoelectric and the strain effects have disappeared, strong evidence is nevertheless forthcoming, that for iron and nickel there is such a relation, and that not merely qualitative but quantitative. As regards one important detail there is an unexplained inconsistency in the behaviour of the two metals, but the coincidences observed in both cases are too numerous and too varied to be the result of accident.

Iron.—For iron, when allowance is made for the purely mechanical

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* 'Ency. Brit.,' vol. 30, p. 449, article "Magnetism."

compression due to magnetisation,* the change of thermoelectric power appears to be proportional to the change of length. Change of thermoelectric power expressed in microvolts is nearly (and perhaps under perfect experimental conditions would be exactly) numerically equal to the "corrected" change of length in ten-millionths multiplied by a factor which is constant for the same specimen in the same physical condition, but differs for different specimens, and for different physical conditions of the same specimen. For my sample of pure iron when in a free state, the factor giving the best agreement is 183×10^{-5}; for a sample of good commercial iron it is $63{\cdot}6 \times 10^{-5}$; and for the pure iron stretched by a load of 1620 kilogrammes per sq. cm. its value is 112×10^{-5}. The curves expressing the relations of thermoelectric power and of change of length to magnetising force are not indeed exactly coincident, as may be seen by reference to figs. 4 and 5; but since identical specimens of the metal were not used in the two sets of experiments, while the conditions were necessarily somewhat different, the divergencies cannot but be regarded as very small; sometimes, indeed, they hardly exceed the limits of experimental error. The thermoelectric and the elongation curves for iron appear to be similarly influenced by the physical condition of the metal; I have shown† that the elongation curve is lower for annealed than for unannealed iron, and that tensile stress also lowers the curve; the same is the case with the curves of thermoelectric power. The strength of the magnetic field at which, as indicated by the corrected curve, there would be no change of length under tensile stress, appears to be just the same as that at which the thermoelectric force becomes zero; when this strength is exceeded, retraction occurs instead of elongation, and a simultaneous reversal occurs in the direction of the thermoelectric force. Unlike previous experimenters I find that when the iron is free from tensile stress the direction of the thermoelectric force is never reversed by magnetisation, even in fields up to 1600 C.G.S. units; neither does the curve of change of length when "corrected" for mechanical stress ever cross the horizontal axis.

Nickel.—Partly on account of the smaller magnetic susceptibility of nickel, and partly in consequence of the relatively great changes of length which that metal undergoes when longitudinally magnetised, the correction for mechanical stress is almost negligible, averaging less than 3 per cent. for fields up to 1200. Here, too, the forms of the curves for change of length and change of thermoelectric power in relation to H are strikingly alike (see fig. 7), the correspondence being even closer than in the case of iron. For a specimen of pure nickel the increase of thermoelectric power in microvolts, due to magnetisa-

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* It is a disputed point whether there actually is any such compression. The subject is discussed later.

† 'Roy. Soc. Proc.,' vol. 55, p. 228, 1894; vol. 47, p. 469, 1890.

tion in any field up to 1600 (the strongest reached), was found to be about equal numerically to the retraction in ten-millionths multiplied by 145×10^{-5}. The curves for two pieces of an impure nickel also exhibit general similarity of form, but since the dimensional ratios (ratio of length to cross section) of the two pieces are very different, the curves are not strictly comparable. The changes of thermoelectric force are, like the changes of length, much greater for nickel than for iron. Tensile stress produces, as in iron, corresponding variations in the two classes of curves. The effect of tension upon the magnetic retraction of nickel is, as I have shown in a former paper,* not so simple as in the case of iron. In weak fields the contraction is diminished by tension; in fields of more than about 150 units the contraction is increased by tensile stress up to a certain critical value of the stress depending upon the strength of the field, and diminished by greater tension. Thus it happens that the retraction curves for a wire loaded with two different weights may cross each other. In one of my published experiments the retraction curves for a nickel wire carrying loads of 420 and of 980 kilogrammes per sq. cm. crossed when H reached 220. With nearly the same loads, the two curves of thermoelectric force also crossed, though in a weaker field, H being only 150. Wire of the same quality was used in both experiments, but in the first it was hard, while in the second it was annealed, which may or may not be the reason of the difference. In any case, it is interesting to find this complex and unexpected phenomenon qualitatively reproduced by the thermoelectric curves.

The anomaly above referred to consists in the fact that the direction of the thermoelectric force due to magnetisation is the same for nickel as for iron, whereas length is affected oppositely in the two metals, iron being extended, nickel contracted.

Cobalt.—For cobalt I have not succeeded in finding any relation between the thermoelectric and dimensional changes attending magnetisation. The thermoelectric curve somewhat resembles that for nickel, but it is much lower. The strain curve is opposite in character to that of iron; in weak fields the metal contracts, in strong fields it is elongated.

Nomenclature.

When the direction of the thermoelectric current between two metals, A and B, was from A to B through the hot junction, it was formerly the custom to say that A was positive to B, and B negative to A; bismuth, for example, was said to be thermoelectrically positive to antimony. Of late years this custom has to a large extent been reversed, and the terminology is at present in an unsettled condition. Following what I believe to be the best modern authorities, I shall in

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* 'Roy. Soc. Proc.,' vol. 47, p. 474, 1890.

this paper speak of a metal A as being positive to B when the thermo-current passes from A to B through the *cold* junction, or from B to A through the hot. The thermoelectric power of bismuth with respect to lead is thus negative, while that of antimony is positive.

Previous Researches.

The results of my own experiments compel me to dissent from some of the conclusions which have been reached by other workers. The discrepancies noticed are, I think, in most cases due to the fact already referred to, that the thermoelectric behaviour of weakly magnetised with respect to strongly magnetised iron and impure nickel may be very different from that of the unmagnetised with respect to the magnetised metal.

The earliest observations of the effects of magnetisation upon thermoelectric power are those of Professor W. Thomson (Lord Kelvin), who in 1856* announced that magnetisation rendered iron and steel positive to the unmagnetised metals (the thermo-current passing from unmagnetised to magnetised through hot), while in nickel the opposite effect was produced. The latter statement, though it has never, I believe, been disputed, is beyond doubt erroneous. Iron, steel, and nickel, when free from mechanical stress, all become more positive when magnetised.

The effects of tension and magnetisation upon the thermoelectric quality of iron have been investigated by Ewing,† who found that "the presence of load diminishes the general thermoelectric effect of magnetisation, and finally reverses it when the load is great." Although this happens to be a correct statement of the facts, it is, I venture to think, exceedingly doubtful whether a genuine reversal was actually attained in a field of only 17 units, the strongest applied by Ewing, even with a load on the wire of 4000 kilogrammes per sq. cm. From the description of the apparatus (p. 368) it appears that the piece of wire regarded as unmagnetised was continuous with the magnetised portion, the hot junction separating the magnetised from the unmagnetised iron being just outside the magnetising coil.

Chassagny‡ was the first to announce that the increase of thermoelectric power due to magnetisation reaches a maximum in a moderate field and diminishes in stronger fields. In his experiment the electromotive force was greatest in a field of 55 units, falling to about half its maximum value in a field of 200.

In a similar experiment by Houllevigue§ a maximum was indicated at $H = 42$, and at $H = 352$ the increase of thermoelectric force due

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* Bakerian Lecture, 'Phil. Trans.,' 1856, p. 722.

† 'Phil. Trans.,' 1886, p. 361.

‡ 'C. R.,' vol. 116, p. 977, 1893.

§ 'Journ. de Physique,' vol. 5, p. 53, 1896.

to magnetisation had apparently become zero. His paper contains no record of an actual reversal of the force in stronger fields, though the probability of such reversal is suggested. My experiments, which were made with several different specimens of iron, show no approach to zero in fields nearly five times as strong as that mentioned. As regards the behaviour of steel, Houllevigue's results are at variance both with those of W. Thomson and with my own.

Mr. E. Rhoads* has given an account of two thermoelectric experiments, one of which was made with iron, the other with nickel, his work having been undertaken "in looking for some property of iron that would vary with the magnetisation in the same way as the length." After referring to the experiments of Chassagny, and to those of Houllevigue "who found that the diminution [of thermoelectric force] continues in higher fields, and that a reversal actually takes place," he remarks that "this suggests change of length, and made me wish to work out the cyclic curve for comparison with it." The cyclic curve which he obtained exhibits hysteresis, and bears a general resemblance to one given for change of length, though the latter is not carried beyond $H = 90$. The thermoelectric curve for iron is represented as crossing the axis of H at $H = 400$, continuing to fall in an almost straight line up to $H = 500$, the strongest field applied. Rhoads considers that it agrees with the curve for change of length when the latter is corrected for "Maxwell's stress" (which is given as $B^2/4\pi$) being apparently unaware of the fact demonstrated by More, and afterwards by Klingenberg, that the corrected curve never crosses the horizontal axis. The thermoelectric curve for nickel is shown in Rhoads's diagram as lying, like the curve for change of length, below the horizontal axis (in agreement with Thomson's experiment), while for iron, both the thermoelectric and the corrected length curves are above the axis. "The two properties," he says, "turn out to be related in the opposite sense in the two metals." As regards form, the nickel curves for change of thermoelectric power and change of length are of the same character, even though, as is remarked, they are not strictly comparable, being made with very different specimens. No mention is made of Maxwell's stress in relation to nickel.

Mechanical Stress due to Magnetisation.

Before describing the experiments it is desirable to discuss briefly the controverted question of compressive stress which has such an important bearing upon the results. In a paper† on the changes of

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* 'Physical Review,' vol. 15, p. 321. This paper was published about 2 months later than my article above referred to. My experiments were begun before I had heard of Mr. Rhoads's work.

† 'Phil. Trans.,' vol. 179, p. 216, 1888.

dimensions due to magnetisation, published in 1888, I wrote: "One of the influences tending to produce retraction must certainly be of a purely mechanical nature. Suppose a uniformly magnetised rod to be transversely divided through the middle. The two halves, if placed end to end, will be held together by their mutual attraction, pressing against each other with a certain force per unit of area, which can be measured by the weight necessary to tear one half from the other. The same pressure will exist between any two portions of the rod separated by any possible cross section, and a certain longitudinal contraction of the rod will be the consequence. If, now, the rod, having been first demagnetised, be placed in a vertical position upon a fixed base, and loaded at the upper end with a weight equal to the greatest it could support when magnetised, it will undergo the same contraction as before, the [compressive] stresses being equal in the two cases." It is pointed out that in the latter case the contraction is expressed as a fraction of the original length by P/M, M being Young's modulus, and P the load, both in grammes weight per square centimetre. Assuming the magnetisation to be such that the divided rod can just support a weight of P grammes per square centimetre, it is inferred that the contraction due to the mechanical effect of magnetisation would again be P/M, and it is shown that this accounts for a part only of the observed change of length. In an earlier paper* it was calculated that P, the weight per square centimetre supported in the field H, was equal to $(2\pi I^2 + HI)/g$, I being the magnetisation, and g the intensity of gravity. This expression (which is equivalent to $(B^2 - H^2)/8\pi g$), is applicable when the magnetic metal is magnetised longitudinally and uniformly by an external field, as was approximately the case in the experiments with which the present paper is concerned, where the wires were placed in the axis of an independent magnetising coil. For the special case in which each half of the divided rod is surrounded by a separate magnetising coil wrapped tightly around it, another term, $H^2/8\pi$, must be added for the mutual action of the two coils, and we shall have

$$Pg = 2\pi I^2 + HI + \frac{H^2}{8\pi} = \frac{(4\pi I + H)^2}{8\pi} = \frac{B^2}{8\pi}.$$

Also for a permanent ring-magnet, in which there is no magnetic force H,

$$Pg = 2\pi I^2 = 2\pi\left(\frac{B}{4\pi}\right)^2 = \frac{B^2}{8\pi}.$$

It should be noticed that since, except in very strong fields, H^2 is negligible in comparison with B^2, the force in the first case considered

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* 'Roy. Soc. Proc.,' vol. 47, p. 486, 1886.

may generally without sensible error be taken as $B^2/8\pi$, instead of $(B^2 - H^2)/8\pi$ or $2\pi I^2 + HI$.*

In the year 1895 it was pointed out by More,† who was apparently ignorant of what I had written on the subject, though he was certainly familiar with most of my work, that the change of length attending magnetisation must be due to several causes, among which are the mechanical stresses created in the rod. "The first of these mechanical stresses is the tractive force of the magnet, and is measured by $B^2/8\pi$." This force, he remarks, tends always to contract the rod, and for high intensities becomes one of the most important factors in the observed changes of length. In plotting his curves (in which abscissæ are I instead of H) he makes "correction for the contraction due to the $B^2/8\pi$ force. This correction is obtained from the formula

$$\frac{\delta l}{l} \times 10^7 = \frac{B^2/8\pi}{M},$$

where M is the modulus of elasticity. The effect of this correction is to make the elongation much greater for a given intensity. The maximum value of the elongation is more than twice as great as the observed maximum, and the greatest intensity employed, 1300 C.G.S., produces an elongation and not a contraction as observed."

The publication of More's paper led to a discussion‡ in which several well known physicists took part; but the views expressed were by no means concordant, and I believe that at the present time it is not agreed whether there is in fact any such mechanical stress; whether, supposing one to exist, it is compressive or tensile, and whether it is "Maxwell's stress" or some other. The compressive stress which I contemplated was, of course, quite unconnected with Maxwell's theory of stress in the electro-magnetic field, and the expression employed for its value was based upon principles which were well known long before the date of Maxwell's work.§

Several papers have been more recently published‖ in which the

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* 'Phil. Mag.,' vol. 29, p. 440, 1895. The difference due to the term $H^2/8\pi$ is, when $H = 500$, 0·05 per cent.; when $H = 900$, 0·16 per cent.; and when $H = 1400$, 0·35 per cent.

† 'Phil. Mag.,' vol. 40, p. 345, 1895.

‡ 'Nature,' vol. 53, pp. 269, 316, 365, 462, 533.

§ The magnetic force inside a narrow transverse gap in a longitudinally magnetised bar is $B = H + 4\pi I$. Supposing one portion of the bar to be fixed, the force acting upon the face of the other portion is less than B by $2\pi I$, the part due to the face itself; thus the attractive force per unit area $= (B - 2\pi I) \times I = 2\pi I^2 + HI$. The stress between any two portions of a magnetised bar, divided by an imaginary transverse plane, is sustained by the intermolecular springs, whatever their physical nature may be, to which the elasticity of the metal is due.

‖ Klingenberg, 'Rostock Univ. Thesis,' Berlin, 1897; Brackett, 'Phys. Rev.,' vol. 5, p. 257, 1897; Rhoads, *loc. cit.*

writers insist upon the necessity of correcting curves of magnetic strain for mechanical stress. Brackett attributes the recognition of this necessity to Rowland, who, he says, defended his position as follows:—The compressive force which will tend to close up a very thin air gap in a divided magnet must exist in any magnet, for according to all our ideas of matter there is no real difference in the case where the air gap exists and where it does not, because we must still consider the gaps between the molecules.

I venture to think that the results of my thermoelectric experiments with iron and nickel afford strong evidence of the reality of the mechanical stress in question. Before any comparison is possible between the two phenomena of change of length and change of thermoelectric power, considered as due to the molecular effects of magnetisation, it is clear that the effect of any extraneous mechanical action tending to alter the length of the metal must be eliminated. The stress under discussion, if it exists, is for our present purpose no less an extraneous one than if it were produced by loading the metal with a weight. Conjecturing that there might be a relation between the thermoelectric and the strain effects of magnetisation, I plotted curves for the two phenomena side by side (see curves (D) and (*b*), fig. 4); but although these curves bear a superficial resemblance to each other, it is clear that the phenomena to which they relate are by no means in correspondence. In particular, one curve cuts the axis at an early stage, indicating a reversal of sign, while the other appears to become asymptotic. Curve (*d*) which is drawn to the same scale as curve (*b*) indicates, in ten-millionths of length, the correction to be made in respect of the hypothetical compression due to mechanical stress; its ordinates are proportional to P/M, P being the greatest weight which (as found experimentally) could be lifted when the iron was magnetised by the corresponding field, M being Young's modulus. Adding together curves (*b*) and (*d*), we get the "corrected" curve (E), which, as will be seen, coincides with the thermoelectric curve (D) as nearly as could be expected under the conditions of the experiments.

Still more striking is the case of the wire under tension, curves (F) and (*f*) fig. 5. Here there is not even a superficial resemblance; the curve for change of length is an almost straight line inclined to the horizontal axis and lying entirely below it, while the thermoelectric curve begins above the axis and crosses it at H = 450. By making the same addition for stress as in the former case, curve (*d*) being again employed for the purpose, we obtain a corrected curve of length, indicated by the crosses on curve (F), which, when the ordinates are plotted to a suitable scale, closely follows the thermoelectric curve (F), and would, if it were continued a little farther,* evidently meet the axis at just the same point.

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* All the curves for change of length referred to in this paper have appeared in

So far the presumption in favour of the reality of the compressive stress appears to be fairly strong, but it is, I think, greatly strengthened by the results obtained for nickel. In fig. 7 (L) and (*k*) are curves of change of thermoelectric power and of change of length for this metal. There is obviously a strong likeness between the two, though they lie on opposite sides of the axis. Curve (*n*) is derived from (*k*) simply by inverting the latter and plotting the ordinates to a slightly different scale. The inverted curve is seen to be throughout its whole length almost a counterpart of the thermoelectric curve; and this although it has not been "corrected" for compressive stress. Why then should the correction which is indispensable for iron be unnecessary for nickel? The answer is that while for iron the calculated correction for mechanical stress is relatively very considerable, for nickel it turns out to be very small, so small as to be negligible. The changes of length indicated by the compression curve for iron, (*d*), fig. 4, are generally much greater than those indicated by the curve (*b*), to which the correction is to be applied; and the two curves have very different forms. On the other hand the compression curve for nickel (*m*), fig. 7, is an almost straight line making a very small angle with the axis of H, the changes of length which it indicates amounting to not more than 2 or 3 per cent. of the changes exhibited by the uncorrected curve for corresponding fields. Both curves, moreover, rise gradually from the origin to their highest points. Thus it happens that the uncorrected and the corrected curves for nickel, if referred respectively to scales of ordinates so chosen that the two curves may be of the same height, are sensibly identical. Curve (*n*) may be regarded either as the uncorrected or as the corrected curve, according as the ordinates are referred to the scale given in the right-hand margin or to a different scale in which each unit is very slightly increased.

Thus the absence of any need for the correction in the case of nickel where *a priori* it ought not to be required, tends to show that the success of its application in the case of iron is not a mere accident, and that the compressive stress is consequently a *vera causa.*

Apparatus and Methods of Experiment.

Two methods of experiment were employed. In the one the thermoelectric force of the magnetisable metal in conjunction with copper was opposed by a similar couple in which the first metal was unmagnetised; in the other it was opposed by an electromotive force derived from a battery.

The arrangement adopted for *Method I* is shown diagrammatically in

former communications to the Royal Society. It is unfortunate in the present connection that some of the experiments were not carried further.

fig. 1, which, for the sake of clearness, is not drawn to scale. AB is a compound rod made by soldering together end to end five straight wires in the order (1) copper, (2) iron (or other magnetic metal), (3) copper, (4) iron, (5) copper. The length of each iron wire M, N, is 10 cm., and that of the copper wire between them 12 cm. The middle and end portions of the rod are surrounded by three separate brass tubes, in the sides of which are inlet pipes, C, G, H, D, and outlet pipes, E, K, F, for the admission and egress of water and steam. The rod is held axially in the tubes by well-tallowed corks, through which it passes, each of the four copper-iron junctions being exactly opposite an inlet. Cold water enters at C and D and emerges at E and F; steam from a small boiler is admitted at G and H, escaping at K. It was ascertained experimentally that with the low resistances and small electromotive forces employed water was a good insulator. The iron wire M is magnetised by the coil PP. The field at N due to the coil is less than a hundredth part of that in the interior of the coil, and is regarded as negligible; but if the apparatus were to be reconstructed, I should, with my present experience, consider it desirable to increase the distance between M and N. The two ends of the compound rod AB were joined to a galvanometer T of low resistance.

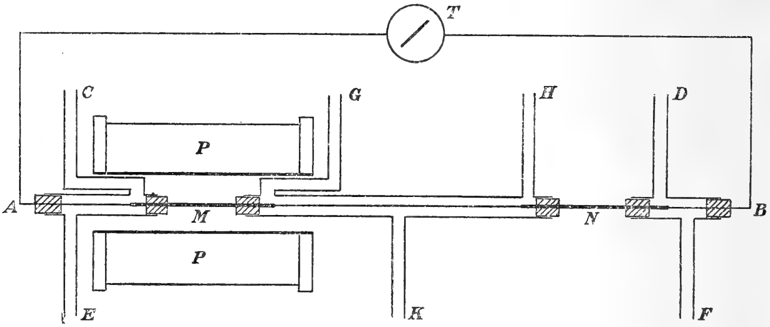

Fig. 1.

Method II is diagrammatically illustrated in fig. 2. AB consists of a wire or strip of the magnetic metal M soldered between two copper wires; it passes through two short and wide brass tubes provided with corks at their ends. Inlet and outlet tubes are fitted to the corks, as shown in the diagram, C admitting cold water and G steam. Cold water also flows through D, F, to equalise the temperatures at A and B. The electromotive force of the thermo-couple is opposed by the potentiometer arrangement shown. S is a dry cell having an electromotive force of 1·46 volt. The low resistance r was either 10 or 2 ohms.; the high resistance R ranged from some hundreds to some thousands of ohms in the different experiments. Before an experiment was begun, R was adjusted until the galvanometer T showed no deflection when the circuit was closed, though a small permanent current was really of no consequence. The deflection (or increase of deflection), which occurred when M was magnetised was thus entirely due to the change of thermoelectric force by magnetisation.

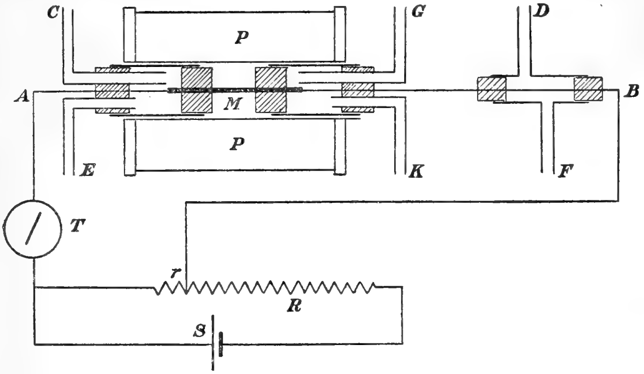

FIG. 2.

The aperiodic galvanometer T, which was placed at a distance of 3·5 metres from the magnetising coil, is of the form designed by Ayrton and Mather; the resistance of its moving coil together with the suspension is 4·70 ohm. In most of the experiments the distance between the mirror and the scale was 12 feet (3·66 metres), when the deflection for 1 microampère was found to be 54 scale divisions of $\frac{1}{40}$th inch (0·0645 cm.).

Two magnetising coils were used; one is 11·5 cm. in length and contains 876 turns of No. 18 wire, the field near the middle of its interior due to 1 ampère being 92 units. The length of the other is 20·4 cm., and the number of turns of wire 2,770; 1 ampère produces a field of 174 units in its interior. The magnetising current was derived from a battery of 27 storage cells and was measured by a moving-coil ammeter, reading from 0—15 ampères by tenths. The ammeter was calibrated by comparison with a tangent galvanometer, and the readings were found to be sufficiently accurate for the purpose in view. Currents of more than 15 ampères were measured by the engine-room ammeter; but on account of their excessive heating effect such strong currents were used in only a few of the experiments.

The Experiments.

Iron.—The results of experiments with two different samples of iron and one of steel are shown by curves (A), (B) and (C) in fig. 3, where the ordinates are proportional to the temporary increase of thermoelectric force produced by the application of the corresponding external field H. Curve (A) relates to a specimen of iron wire 0·28 mm. in diameter, supplied by Messrs. J. J. Griffin and Sons as pure. Details of the experiment are given as an example of the manner of working:—

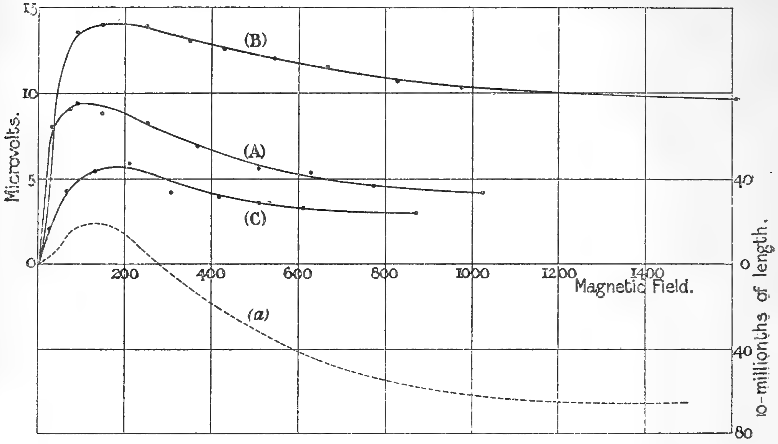

FIG. 3.—Curves (A), (B), and (C) show changes of thermoelectric force for pure iron, commercial iron, and steel respectively. Curve (*a*) shows corresponding changes of length for iron.

Method II.—R = 14,000; *r* = 10 ohms.
Coil No. 1.—H = ampères × 92.
Temperature of cold water, 14° C.

Resistance of galvanometer	4·70	ohms.
,, leading wires	0·39	,,
,, potential coil	10·0	,,
,, thermo-rod	0·20	,,
Total resistance in circuit ...	15·29	,,

Electromotive force of 1 microvolt gives 1/15·29 microampères = 54/15·29 scale divisions, or deflection in scale divisions × (15·29/54 =) 0·283 = microvolts.

Before every observation the iron was demagnetised by Ewing's method of reversals, alternating currents gradually decreasing to zero being switched into the magnetising coil.

The difference of the temperatures of the water at 14° and of the steam at 100° was 86°; assuming that the actual temperatures of the junctions were respectively $\frac{1}{2}$° above and below those of their surroundings, the change of thermoelectric power for a given field may be found by dividing the change of microvolts by 85. In all the experiments the temperature of the cold water differed little from 14°, the mean temperature being therefore 57°.

Table I.

Ampères.	Galvanometer deflections.	Mean.	H = ampères × 92.	Microvolts deflections × 0·283.
0·35	28, 28, 28, 29	28·3	32	8·0
0·8	31, 33, 34, 32, 32	32·2	74	9·1
1·0	34, 33, 33, 33, 34	33·3	92	9·4
1·6	31, 32, 31, 31, 30	31·0	147	8·8
2·6	29, 29, 29	29·0	249	8·2
4·0	24, 24, 25	24·3	368	6·9
5·5	20, 19, 20	19·7	506	5·6
6·8	19, 19, 19	19·0	626	5·4
8·4	16, 17, 16	16·3	773	4·6
11·1	15, 15	15·0	1020	4·2

Curve (B) was given by a piece of commercial iron wire, which was hard, springy, and easily broken. Changes of thermoelectric force, like changes of length, are greater for such a specimen than for one which is purer and softer.

Curve (C) shows the results for a piece of steel knitting needle in the state in which it was bought. The experiment was repeated with the wire in a glass hard state, but the curve was almost unchanged.

The dotted curve (*a*), copied from a former paper,[*] shows for comparison the changes of length exhibited by an iron wire in fields up to 1500. The main point of difference is that whereas the curve for change of length crosses the axis of H, indicating a reversal of the sign of the phenomenon in moderate fields, the other does not. Two other thermoelectric experiments, made with different pieces of iron, gave results of a like character. Even with the high field of 1600, which greatly exceeds any before used in similar work, there is no indication that a reversal of sign would ever be reached.

The curves in fig. 4 are intended to show the effect of "correction" for mechanical stress. The pieces of wire used in the three experiments concerned were all cut for the same hank, and imperfectly annealed by heating in a Bunsen flame. Curve (D) is the thermoelectric curve for the sample, and (*b*) the curve of elongation and retraction.[†] Curve (*c*) shows the relation of lifting power to field,[‡] the ordinates being referred to the scale of grammes weight on the

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[*] 'Phil. Trans.,' vol. 179, p. 228, fig. 6, 1888.

[†] *Loc. cit.*, p. 224, fig. 5.

[‡] 'Roy. Soc. Proc.,' vol. 40, p. 491, fig. 1, 1886.

right-hand margin of the diagram. Curve (*d*) indicates the contraction in ten-millionths of length due to the mechanical compression, and is derived from (*c*) as follows :—If P = the weight supported per

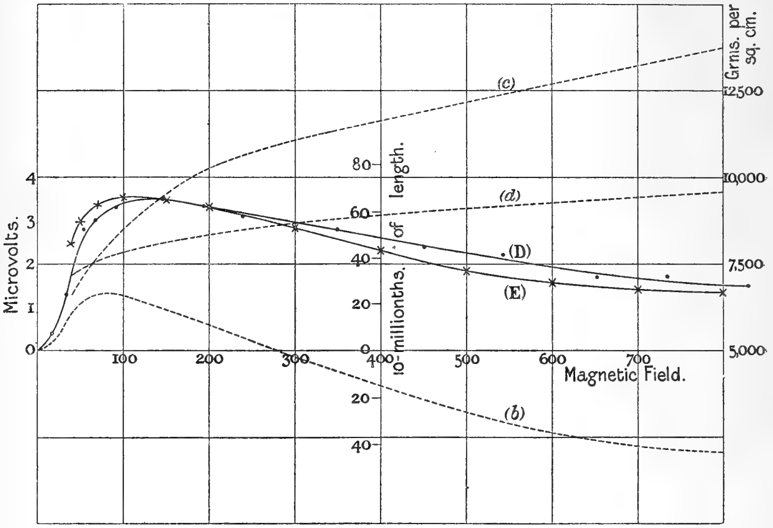

FIG. 4.—(D) shows change of thermoelectric power; (*b*) change of length; (*c*) lifting power; (*d*) mechanical compression, deduced from (*c*). (E) = (*b*) + (*d*), and is the "corrected" curve of change of length.

square centimetre for a given value of H, as shown in (*c*), and M = Young's modulus in grammes weight per square centimetre, the mechanical contraction in ten-millionths = $P \times 10^7/M$. The value of M is taken as 2×10^9. (It might without sensibly affecting the form of the curve have been $1{\cdot}9 \times 10^9$ or $2{\cdot}1 \times 10^9$, and its actual value no doubt comes within these limits.) Hence, $P \times 10^7/M = P/200$, or the mechanical contraction in ten-millionths is numerically equal to the grammes weight supported divided by 200; ordinates of curve (*d*) are therefore simply P/200. In the following table values of P are obtained by measuring ordinates of curve (*c*); elongations and retractions (E) are similarly derived from the published original of curve (*b*).*

Ordinates of the three curves (*b*), (*d*), and (E) are referred to the scale of ten-millionths given in the middle of the diagram. For the sake of easy comparison this scale was so chosen that the heights of the thermoelectric and the corrected elongation curves (D) and (E) are about equal. As before remarked, the close agreement between the

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* *Loc. cit.*

Table II.

P = tractive force. E = elongation.

H.	P.	P/200.	E.	P/200 + E.
40	6,550	33 ·0	12 ·0	45 ·0
50	7,000	35 ·0	20 ·0	55 ·0
70	7,700	38 ·5	24 ·0	62 ·5
100	8,500	42 ·5	23 ·0	65 ·5
150	9,500	47 ·5	17 ·0	64 ·5
200	10,250	51 ·0	10 ·0	61 ·0
300	11,100	55 ·5	− 2 ·5	53 ·0
400	11,600	58 ·0	−15 ·0	43 ·0
500	12,200	61 ·0	−27 ·0	34 ·0
600	12,700	63 ·5	−35 ·0	28 ·5
700	13,200	66 ·0	−40 ·0	26 ·0
800	13,700	68 ·5	−44 ·0	24 ·5

curves can hardly be the result of accident, the less so as the same similarity is manifested under different conditions.

Tension has the effect of lowering the elongation curve of iron, the maximum becoming smaller and contraction beginning at an earlier stage of the magnetisation. If the tension is great there is no preliminary elongation at all, and the wire begins to contract with the smallest magnetising forces. Curves (*e*) and (*f*), fig. 5,* show the

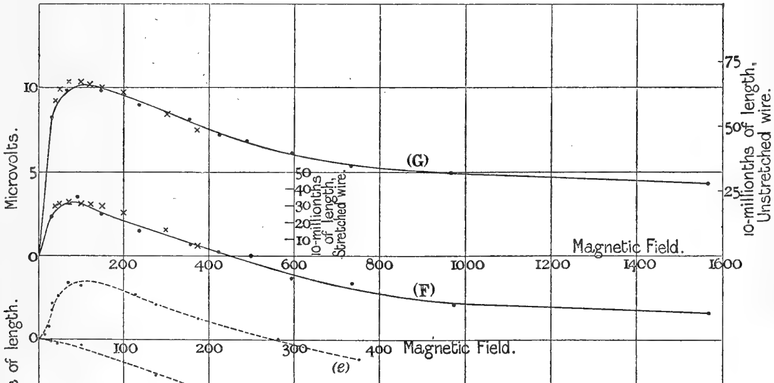

Fig. 5.—Curve (G) shows changes of thermoelectric force for an unstressed iron wire; (F) same when the wire was loaded with 1620 kilogrammes per sq. cm. Curves (*e*) and (*f*) show changes of length for iron loaded with 351 and 1600 kilogrammes. Crosses on (F) and (G) indicate (*e*) and (*f*) corrected for mechanical stress.

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* 'Roy. Soc. Proc.,' vol. 47, p. 474, fig. 2, 1890.

changes of length undergone by an iron wire when loaded with weights of 351 and 1600 kilogrammes per sq. cm., the curve for the first-named load being practically the same as for an unloaded wire. Curves (F) and (G) indicate the changes of thermoelectric force with field for an iron wire, first when loaded with 1620 kilogrammes per sq. cm., and afterwards without load, the points of observation being marked by dots. The crosses on (G) and (F) indicate the course of the curves (*e*) and (*f*) after correction for mechanical stress; the correspondence is so close that it was impracticable to draw the corrected curves separately. The vertical scale of the corrected curves is not the same in the two cases; that for the upper one is given in the right-hand margin, and that for the lower in the middle of the diagram. Data for the construction of these curves were obtained in the same manner as before, and are given in the annexed table:—

Table III.

P = tractive force in grammes per sq. cm. E = elongation.

H.	P.	P/200.	E, Curve (*e*).	E, Curve (*f*).	P/200 + E, Curve (*e*).	P/200 + E, Curve (*f*).
40	6.550	33·0	26	− 3	59·0	30·0
50	7,000	35·0	28	− 4	63·0	31·0
70	7,700	38·5	27	− 6	65·5	32·5
100	8,500	42·5	23	−11	65·5	31·5
120	9,000	45·0	20	−14	65·0	31·0
150	9.500	47·5	16	−18	63·5	29·5
200	10,250	51·0	10	−25	61·0	26·0
300	11,100	55·5	− 2	−40	53·0	15·5
375	11,500	57·5	−10	−52	47·5	5·5

It is to be noted that the three sets of experiments—for change of thermoelectric force, change of length, and lifting power—were made with three different samples of iron wire.

The effect of annealing upon the changes of length and of thermoelectric force is illustrated in fig. 6. Curve (*g*) shows the elongation of a wire in the state in which it was bought,* while (*h*) indicates its behaviour after it had been carefully annealed. This operation was performed by enclosing the wire in an iron tube which was placed in a hot fire and allowed to cool gradually as the fire died out. Curve (H) shows the changes of thermoelectric force in a piece of the same kind of wire when in its original state, and (K) the modification which resulted from heating the wire to redness in a Bunsen flame and cooling it in air. In both cases the effect of annealing is to depress the curve.

* 'Roy. Soc. Proc.,' vol. 55, p. 230, fig. 1, 1894.

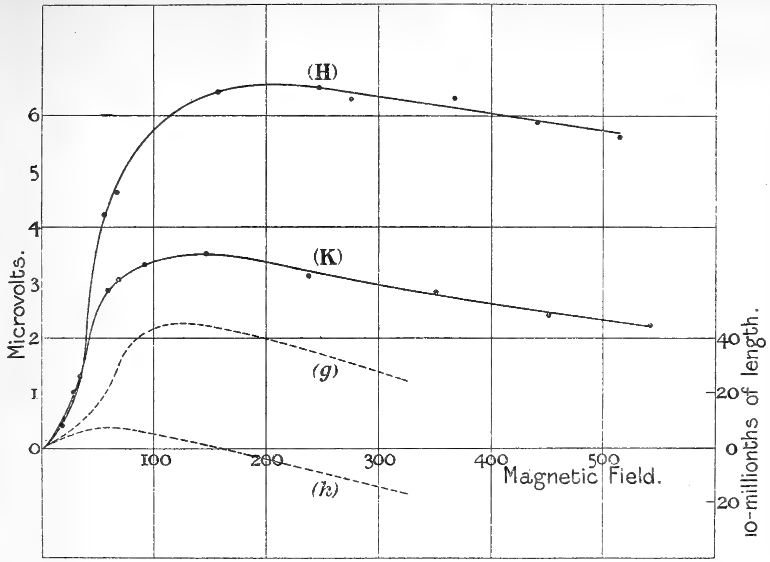

FIG. 6.—Curve (H) shows changes of thermoelectric force in an unannealed iron wire; (K) changes in the same wire when annealed. Curves (*g*) and (*h*) show changes of length in another wire when unannealed and after annealing.

A glance at any of the curves of thermoelectric force for iron will show how easily errors might arise if it were assumed that a wire which had been subjected to a magnetic field became perfectly demagnetised when the field was withdrawn. In all my experiments the curves would have been much lower but for the demagnetisation by reversals before every observation. The dimensional ratio of the short wires which I used was generally so small, and the self-demagnetising consequently so great, that an apparent reversal of thermoelectric force in strong fields did not often occur. But in the case of a wire 17·5 cm. in length and 0·026 cm. in diameter, this spurious reversal appeared in fields above 500. When the *demagnetised* wire was subjected to a field exceeding this strength, there occurred a galvanometer deflection to the right, indicating a genuine increase of thermoelectric force; but when the magnetising current was interrupted, the spot of light, instead of going back again, went still further to the right, the thermoelectric force due to the residual magnetism being greater than that due to the strong field. When the magnetising circuit was again closed before the wire was demagnetised, the spot of light, of course, moved to the left, and if the residual magnetism were disregarded, it would naturally be supposed that a reversal of thermoelectric force was indicated. The spurious reversal was also very conspicuous in the case of the steel wires.

Nickel.—Since the effect of magnetisation upon a nickel rod or wire is to shorten it, while, if the "correction" be admissible, iron is always lengthened by magnetisation, it was to be expected from analogy that the effects upon thermoelectric power would be opposite in iron and in nickel. In iron the thermoelectric power of the magnetised with respect to the unmagnetised metal is positive; in nickel, therefore, it should be negative. This view, though in accordance with what is generally accepted as the fact, is, however, at variance with the results of all my experiments.

Five different samples of nickel were used, curves for four of these (L), (M), (N), (O), being given in fig. 7.

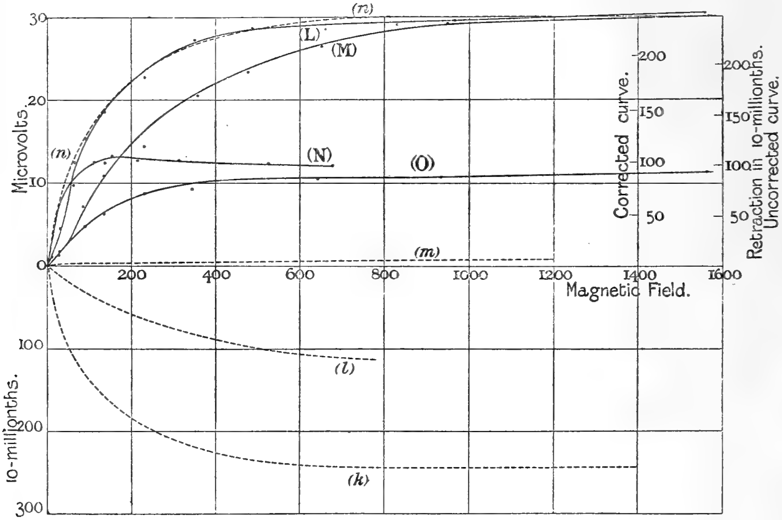

Fig. 7.—Curves (L) and (M) show changes of thermoelectric force for pure nickel; (N) and (O) the same for impure nickel. Curves (*k*) and (*l*) give changes of length, and correspond to (L) and (O); (*m*) shows mechanical compression; the dotted curve (*n*) is (*k*) inverted and plotted to the scale of ordinates in the right-hand margin.

Curve (L) relates to a piece of thick wire 2·95 mm. in diameter, bought of Messrs. Johnson and Matthey as pure. The retraction curve* for another piece of the same wire is that marked (*k*).

Curve (M) was given by a wire 0·65 mm. in diameter; this was also supplied by Messrs. Johnson and Matthey, and described as being "as pure as conveniently possible."

Curve (N) shows the result of an experiment with a sample of wire supplied by Messrs. J. J. Griffin and Sons, which did not purport to be pure and probably contains a considerable proportion of iron.

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* 'Phil. Trans.,' vol. 179, p. 228, fig. 6, 1888.

Curve (O) was obtained from a strip about 1·4 mm. wide and 0·75 mm. thick, which was cut from a rolled sheet purchased at a metal warehouse. Curve (*l*) shows the retraction of a strip of the same metal 9 mm. in width.*

The fifth specimen was a wire taken from a piece of nickel gauze. With this, too, the magnetised was found to be always thermoelectrically positive to the unmagnetised metal.

[A sixth specimen, consisting of a wire 3·5 mm. in diameter, has been recently tested, with the same result.—*May* 23, 1904.]

The form of curve (N) which rises to a maximum at about H = 150 suggested a possible source of error by which Thomson may have been misled, and I therefore repeated his experiment. The arrangement which is shown in fig. 8, is essentially the same as that employed by Thomson. A piece of Griffin's impure wire was bent into the shape of a horse-shoe, as shown, one of the limbs passing through the small magnetising coil, PP; the ends of the horse-shoe were connected by brass binding-screws to wires leading to the galvanometer T. Heat was applied at or near Q by touching the wire with a hot copper rod, and when the magnetising circuit was closed, the galvanometer T indicated a thermo-current which usually had the same direction as in the other experiments—from unmagnetised to magnetised through hot. It was, however, found possible to adjust the strength of the field and the position of Q so that a thermo-current flowed in the opposite direction. This was the case, for example, when the field-strength was 400 and the distance of Q from the coil 8 cm. If a slight change were made in the position of Q, the application of the same field again produced a current in the normal direction. This deceptive effect, which, of course, really occurred between more strongly and less strongly magnetised portions of the metal, could not be obtained with the pure nickel wire, nor with the wire taken from the gauze, which were the only other specimens tested. With an iron wire, however, it was quite easily produced.

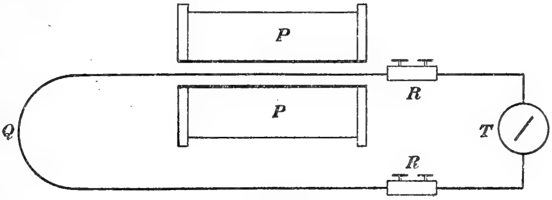

FIG. 8.

Curve (*m*), fig. 7, shows in ten-millionths of length the mechanical compression due to magnetisation; in comparison with that for iron,

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* *Loc. cit.*, p. 214, fig. 4.

curve (*d*), fig. 4, it is seen to be quite insignificant. Since no experiments have been made to determine the lifting-power of nickel, this was calculated from the expression $(2\pi I^2 + HI)/g$. Values of I were obtained from a table given by Ewing, based upon results of an experiment by Du Bois;* the specimen used was an ovoid instead of a cylindrical wire, but the values might be increased or diminished by 10 per cent. without influencing the corrected curve to any appreciable extent. Young's modulus M was taken as $2{\cdot}2 \times 10^9$, approximating 2175×10^6, the value found† for nickel by H. Tomlinson; this also need not be very accurately known for the present purpose. Table IV shows how the ordinates of the compression curve (P/220), and those of the corrected curve (R − P/220), were determined.

Table IV.

$P = (2\pi I^2 + HI)/g$. $C = P \times 10^7/M = P/220$. R = retraction in ten-millionths.

H.	I.	P.	C.	R.	R − C.
100	313	659	3·0	136	133
200	375	977	4·5	181	177
300	406	1180	5·3	210	205
400	428	1348	6·1	224	218
500	441	1470	6·7	232	225
600	450	1572	7·1	239	232
700	456	1657	7·5	242	234
800	459	1724	7·8	244	236
1200	471	1997	9·0	245	236

Curve (*n*) which nearly coincides with the thermoelectric curve (L) is the uncorrected retraction curve (*k*) inverted and plotted to the scale of ordinates in the right-hand margin. If the corrected curve were plotted to the slightly different scale just within the margin, it would be substantially identical with (*n*), which may therefore be regarded as representing either the corrected or the uncorrected curve of change of length.

The small difference noticeable in the forms of the two curves (O) and (*l*) for the two nickel strips is just such as would be caused by the difference between their dimensional ratios.

As to the curves in fig. 9, which show a remarkable qualitative correspondence with regard to the complex effects of tensile stress, nothing need be added to what has already been said. The dotted curves‡ are of course inverted.

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* 'Magnetic Induction,' 3rd edit., p. 164.

† 'Roy. Soc. Proc.,' vol. 37, p. 390, 1884.

‡ 'Roy. Soc. Proc.,' vol. 47, p. 475, fig. 3, 1890.

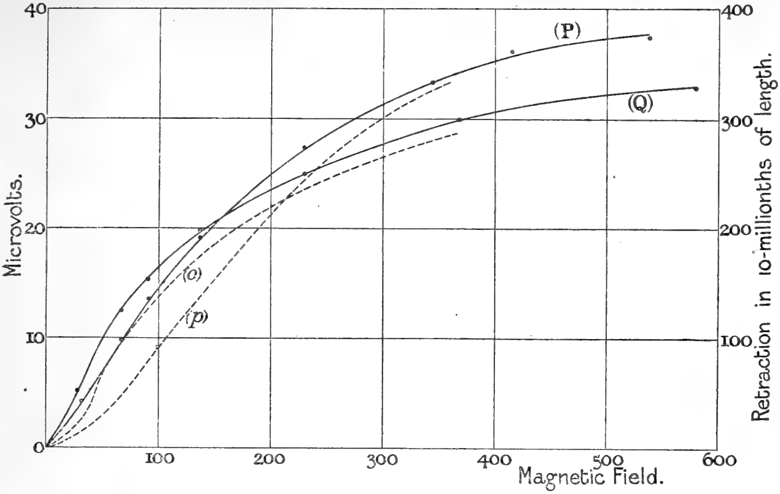

FIG. 9.—Curves (P) and (Q) show changes of thermoelectric force in an annealed nickel wire when loaded respectively with 970 and 447 kilogrammes per sq. cm. Curves (*o*) and (*p*) show changes of length for an unannealed wire when loaded with 980 and 420 kilogrammes per sq. cm.

Cobalt.—Experiments are made with two different specimens of cobalt, the results being given in fig. 10.

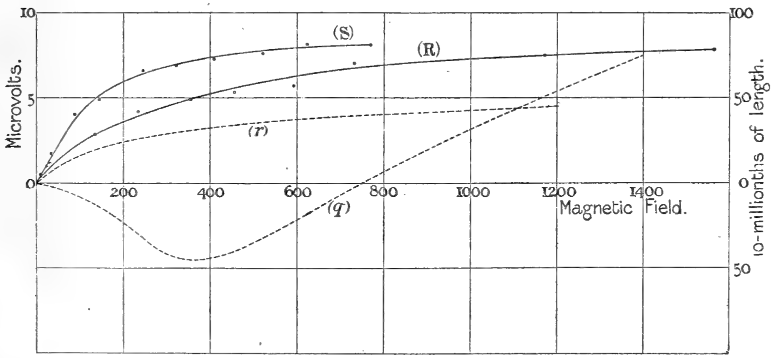

FIG. 10.—(R) and (S) show thermoelectric changes for cast and rolled cobalt respectively; (*q*) shows changes of length for the cast cobalt; (*r*) is the curve of mechanical compression.

Curve (R), showing thermoelectric changes, relates to a cast rod supplied by Messrs. Johnson and Matthey; the change of length curve (*q*) was obtained with another piece of the same rod.*

This latter curious curve is quite unlike the thermoelectric curve (R),

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* 'Phil. Trans.,' vol. 179, p. 228, fig. 6, 1888.

and evidently cannot be made to resemble it by applying the correction for mechanical compression. The compression curve (*r*) was constructed, like that for nickel, from values of I furnished by Ewing; Young's modulus was taken as 2×10^9, the value found by Tomlinson* for unannealed cobalt being 2005×10^6.

The thermoelectric curve (S) was given by a strip of rolled cobalt, for which I am indebted to the kindness of Messrs. Henry Wiggin and Co., of Birmingham. It is of interest as indicating the similar behaviour of a very different sample of the metal.

If there is any relation between the thermoelectric and the strain phenomena in cobalt, it is obviously disguised by some cause which has yet to be discovered.

[*Note added May* 23, 1904.—Some further experiments have been made with specimens of cobalt which had been very thoroughly annealed. For cast cobalt the change-of-length curve is entirely altered by annealing, becoming, at least in fields up to 1360 units, a straight line represented by $R = 0{\cdot}056H$, where R is the retraction in ten-millionths. The thermoelectric curve is considerably lowered, but in other respects is not much affected. No relation between the thermoelectric and the strain phenomena could be traced.

From an examination of the curves in fig. 10 it appears that the thermoelectric power of the unmagnetised with respect to the magnetised cast cobalt is proportional to the compressive stress, and consequently to the square of the magnetic induction.]

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* 'Roy. Soc. Proc.,' vol. 39, p. 530, 1885.

"Experimental Determinations for Saturated Solutions." By the EARL OF BERKELEY. Communicated by F. H. NEVILLE, F.R.S.

Received March 28,—Read May 19, 1904.

(Abstract.)

The object of this research is the experimental determination of those physical constants of concentrated solutions, which are necessary for the tentative application of the gas-law equations. Saturated solutions were chosen because, presumably, dissociation is relatively at a minimum.

This part of the work deals with the densities and solubilities of the chlorides, sulphates, and nitrates of sodium, potassium, rubidium, cæsium, and thallium, and also with their respective alums, except that of sodium.

The densities were determined by means of a small pipette-shaped pyknometer, of about 5 c.c. capacity, the lower end of which was turned upwards and the upper, 120 mm. long, was graduated. This was filled with the saturated solution and weighed, and from the known capacity of the pyknometer, together with the weight of solution it contained, the density was calculated. The solubility was obtained by washing out the contents of the pyknometer and evaporating to dryness, the weight of salt left giving the solubility.

The densities and solubilities were determined in two ways. In one the saturated solution, which was in contact with an excess of salt and continuously stirred, was cooled to the temperature of observation and the density and solubility determined. In the other an unsaturated solution was raised to the temperature of observation, being continuously stirred in contact with an excess of salt (in both cases the solution is kept at the temperature of observation by means of a thermostat), and the density determined at intervals of about 12 hours until constant.

This constant density and resulting solubility was averaged with the density and solubility obtained in the first method, and the mean was assumed to be the true density and solubility of the saturated solution. The observations were made at intervals of 15°, in this manner, between 0° C. and 90° C.

The constants were also determined at the boiling point of the saturated solutions in an apparatus in which steam was caused to bubble vigorously through the solution with excess of salt, until the temperature became constant, this constant temperature being assumed to be the boiling point.

The boiling point itself was not accurately determined, as it was

found that no emergent column correction could be satisfactorily applied; the pressures, however, under which the saturated solutions boiled were recorded.

The results are given in a tabular form at the end of the paper.

"A Method of Measuring directly High Osmotic Pressures." By the Earl of BERKELEY and E. G. J. HARTLEY. Communicated by W. C. D. WHETHAM, F.R.S. Received April 21,—Read June 2, 1904.

This paper gives an account of some preliminary experiments made in furtherance of a scheme of work outlined by one of us in a communication to the Royal Society.*

The ordinary method of determining osmotic pressures, *i.e.*, that adopted by Pfeffer,† Adie,‡ and others, is evidently not suitable for high pressures; the difficulty of attaching the manometer to the porous pot in a manner such that it will not move at the junction is practically unsurmountable.

It seemed likely that if a porous plate were tightly squeezed between two hollow hemispheres, the necessary conditions of stability might be attained, and the first apparatus carried out this idea. It was made by Messrs. Müller in 1901, and, being of glass, was designed only to stand moderate pressures, though it was hoped that, in a way described below, it might be used to measure indirectly the osmotic pressures of concentrated solutions. It consisted of two glass globes A and B (see fig. 1), holding the porous plate C between them. The plate was glazed round the edge, and carried the semi-permeable membrane of copper ferrocyanide on the face adjacent to the solution. A rubber ring on either side of C and between it and the glass flanges of A and B, served to form a watertight joint when A, B and C were strongly pressed together by means of a suitable brass fitting, which, however, is not shown in the figure.

It had been intended to put solutions of different concentrations in the two vessels, and measure the difference between their osmotic pressures; it was found that, although it would be possible to obtain the desired result, yet the time taken for the pressure to develop was too long.

An experiment with a solution of 114·7 grammes of sugar in the

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* Earl of Berkeley, "Experimental Determinations for Saturated Solutions," read May 19, 1904, see p. 435.

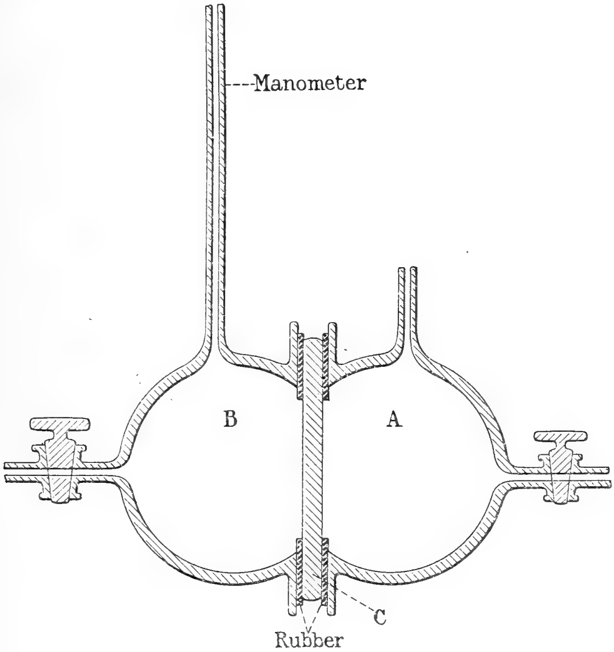
† W. Pfeffer, 'Osmotische Untersuchungen,' Leipsic, 1877.

‡ R. H. Adie, 'Jl. Chem. Soc.,' 1891, p. 344.

litre, gave an osmotic pressure of 8·13 atmospheres, the theoretical value, derived from the gas laws, is 7·89.

This apparatus was discarded, and attempts were made to determine osmotic pressures in a quicker way by a method somewhat similar to that which Tammann* tried, *i.e.*, by directly applying to the solution a gradually increasing pressure until the osmotic pressure has been reached, and meanwhile noting the change in the volume of the solvent. We replaced the two glass globes by iron cylinders, and connected one of them, B, which was filled with the solution, to a powerful pressure apparatus. Water filled the other, A, and also a graduated glass capillary, connected to A by a perforated rubber stopper. Pressure was applied to the solution and the rate at which the water rose in the capillary was noted. The rate was found to be, apparently, slightly different before the osmotic pressure had been reached to that which it was after that pressure had been passed. The phenomenon was so complicated, however, by the volume effect due to the compression of the washers placed on either side of the plate, that no very clear results could be obtained.

Fig. 1.



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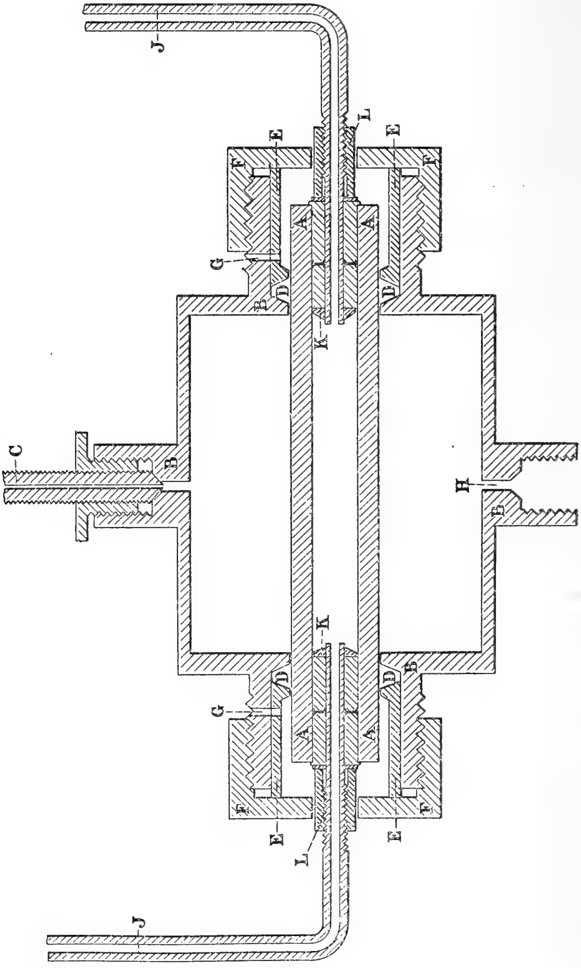
* G. Tammann, 'Zeit. f. Phys. Chem.,' vol. 9, p. 97.

Final Form of Apparatus.—We had foreseen this defect, and a means of overcoming it, should it mask the desired result, was suggested by Dr. Burton, of the Cambridge Scientific Instrument Company. It consisted in replacing the plate by a porcelain tube; surrounding the

FIG. 2.



middle portion only by the solution and leaving the ends free. Thus, when the pressure was applied to the solution, the compression of the "dermatine" rings, which confine it, would have no effect on the volume of water inside the tube.

This is embodied in the final form of apparatus which is shown, in vertical section, in fig. 2.

The porcelain tube, AA, was made of Dr. Puckal's paste by the Royal Berlin Factory, and of a porosity slightly less than that of the porous pots they make for Dr. Thorpe's arsenic apparatus. It is about 15 c.m. long, and has an outside diameter of 2·5 c.m., and an inside diameter of 1 c.m. The ends are glazed and the membrane was formed on the whole length of the outer surface. The outside brass casing BB, contained the solution, to which the pressure was brought by the steel pressure tube C. The porcelain tube is secured to B by the "dermatine" rings DD, and the latter are strongly compressed, between B and the metal sleeves EE, by screwing down the nuts FF, the lateral expansion causes them to grip the tube tightly. GG are holes perforated through B, and through the sleeves EE, the use of which will be explained later on. H is another perforation through B which allows the apparatus to be emptied without taking it to pieces; in several experiments a Schaffer and Budenberg pressure-gauge was attached here, ordinarily it is closed by a screw-down metal plug.

The ends of the porcelain tube are closed by pieces of thick-walled rubber tubing II, through which the brass tubes JJ pass; the length of rubber is such that they extend further into the tube than the distance between the "dermatine" rings and the ends of the tube. A watertight joint between A and J is obtained by compressing the rubber tubes between the metal washers KK, and the screw sleeves LL. The brass tubes JJ are joined, in one case to a glass tap, and in the other to a glass capillary by rubber tubing. The glass capillary is graduated in millimetres and was calibrated; 1 cm. of the bore contains ·0042 c.c.

The operation of measuring the osmotic pressure of a solution consisted in filling the inside of the porcelain tube with water, and the surrounding vessel B with the solution; then noting the rate at which the level of the water in the graduated tube moves while the pressure is being gradually increased on the solution.

Theoretically, as long as the osmotic pressure has not been reached, the level of the water in the capillary should fall; when the osmotic pressure is exceeded it should rise and the "turning point" should give the osmotic pressure. In either case the rate at which it moves is a function of the difference between the osmotic pressure and the pressure on the solution. In the actual experiments, although the level rose and fell, the "turning point" was at some other pressure higher than the osmotic pressure; the chief cause of this difference was that however tightly the "dermatine" rings were compressed against the porcelain, the sugar solution leaked past them. This was doubtless because the membrane could not be formed quite on the outer surface of the tube, and consequently a very narrow ring of the tube was left open. The leak was so small that no difficulty was experienced in keeping the pressure up, even at 50 atmospheres, but the effect of the leak was gradually to saturate with solution the surface of the exposed portion of the tube, and thus abstract water from the inside through the membrane.

We call this leak the guard-ring leak, and it was hoped that the following method gave us a means of estimating it.

Guard-Ring Leak Correction.—The holes GG (see fig. 2), were bored, and a stream of the solution whose osmotic pressure was to be determined, was directed through them; and to insure that the surface was thoroughly saturated, the whole apparatus was placed in a bath of the same solution. The inside of the tube together with the capillary having been filled with water, and the temperature of the bath and apparatus having become constant, the rate of fall of the level in the capillary was noted. It should be mentioned that the space enclosed by B was also filled with water, and C was replaced by an open glass tube. The level of the water in this open tube was kept at the same height as that in the capillary, so that any small change in the temperature of B did not alter the rate of fall of the level in the capillary.

The guard-ring leak correction was found to be the larger the more concentrated the solution, and it also varied slightly with the height of the water in the capillary.

This correction was of a magnitude such that it *raised* the "turning point," with reference to the pressure, by an amount equal to from 5 to 10 per cent. of the osmotic pressure.

The Solution-Leak Correction.—We have hitherto been unable to make semi-permeable membranes completely impervious to sugar; on testing the water from the inside of the tube a trace was always indicated. It was, therefore, considered advisable to determine the amount of sugar which had come through in each experiment.

At the end of the experiment the inside of the tube was washed out without taking down the apparatus, and its content of sugar analysed by means of Fehling's solution. On the assumption that the resulting quantity of sugar indicated that a corresponding amount of solution had come through the membrane, and on the further assumption that the rate at which this solution came through was proportional both to the time during which it had been subjected to pressure and to the amount of that pressure, the displacement of the "turning point" was calculated.

It was found that, with a good membrane, the solution leak correction was of a magnitude such that it *lowered* the "turning point," with reference to the pressure, by an amount equal to from 2 to 3 per cent. of the osmotic pressure.

In this connection it may be pointed out that a check on the algebraical sum of these two corrections may be obtained from the experiments themselves. For by beginning an experiment at a pressure on the solution less than its osmotic pressure, and gradually increasing the former until the latter is exceeded, then gradually reducing the pressure until again below the osmotic pressure, the application to the corresponding capillary readings of a formula involving the guard-ring and the solution-leak rates will enable one to calculate the sum of the two.

This sum, deduced in this manner from the capillary readings in an experiment with a solution containing 180 grammes in the litre, was 5·36 cm. The separately determined "guard-ring leak" correction was 10·17 cm., and the difference, 4·81, represents the calculated "solution leak." The observed "solution leak," determined by analysis, and calculated on the assumptions enumerated above, gave 5·03 cm.

Some importance is attached to this method of working as it appears to afford a means of determining the osmotic pressures of solutions of substances for which no truly semi-permeable membranes have yet been found.

The Pressure Apparatus.—The pressure is obtained by means of a vertical steel plunger working in a steel cylinder. The plunger is forced into the cylinder by an iron lever, at one end of which weights are hung, and the bottom of the cylinder is connected with C (see fig. 2). The cylinder and pressure tube are filled with the solution whose osmotic pressure is being determined, and the plunger is made to work almost pressure tight by a "dermatine" ring, sleeve and nut, similar to that shown at D, E and F, in fig. 2. A horizontal lever is attached to the plunger and is worked to and fro, at intervals, so as to keep the pressure on.

The pressure applied when different weights were placed at the end of the lever was determined by connecting the press-tube to a Schaffer and Budenberg standard gauge.

The Formation of the Semi-Permeable Membranes.—Numerous different ways of forming the copper ferrocyanide membrane were tried; the most successful was by first depositing the film by diffusion in the manner Pfeffer[*] recommends and then finishing it by the electrolytic methods due to Morse and Horn.[†] The porcelain tubes were immersed in the copper sulphate solution (50 grammes of $Cu\mathrm{SO}_4,5aq$ in 1000 c.c.) in a desiccator, and exhausted free of air; they were then taken out and their inner and outer surfaces dried with filter paper. The ends were then closed by rubber plugs and they were allowed to dry in the air for from $\frac{1}{2}$ to 1 hour, after which they were quickly plunged into a solution of potassium ferrocyanide (42 grammes in 1000 c.c.). When the membrane was seen to be fairly uniform in colour, the tubes were transferred to the electrolytic cell. This consisted of a beaker containing the ferrocyanide solution in which a platinum foil electrode

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[*] *Loc. cit.*

[†] H. N. Morse and D. W. Horn, 'American Chemical Journal,' vol. 26, p. 80.

connected with the negative pole of the battery, was suspended. This electrode was surrounded by a porous pot so as to prevent the solution, when it had been rendered alkaline by the passage of the current, from reaching the membrane on the tube. As mentioned by Morse, we found that the alkaline solution acted injuriously on the membrane. The porcelain tubes were filled with the copper solution, and were furnished with copper electrodes. The voltage used was 100; and when the resistance of the tubes had risen to a constant value, they were taken down and soaked in distilled water for several days. They were then again set up, and the current passed until the resistance was again constant, generally at a higher value than before, upon which they were taken down and washed. This process was repeated till no further change in resistance took place.

The highest resistance obtained was 170,000 ohms; but out of some fifty tubes only eight or nine reached this figure. These latter were those selected for the experiments, and it was found advisable to re-make the membranes electrolytically after they had been subjected to considerable pressures.

[*May* 21.—We have lately found that the membranes are greatly improved if they be re-made electrolytically under high pressure. The pressure should be applied to a very strong solution of sugar in which the potassium ferrocyanide has been dissolved and which surrounds the outside of the tube; the inside of the tube being filled with the usual copper sulphate solution and remaining under atmospheric pressure.]

Results.—The following results with cane sugar were obtained; the experiments were made more for the purpose of testing the method than to get accurate observations of the osmotic pressures; but we think they are within 10 per cent. of the true values:—

Concentration in grammes per litre.	Pressure at which the "turning point" occurred.	Pressure when "turning point" corrected for guard-ring and solution leaks.	Pressure deduced from Boyle's law.
120·7	10·5 atmos.	9·5 atmos.	8·4 atmos.
180·0	15·7 ,,	14·4 ,,	12·5 ,,
240·0	23·6 ,,	21·3 ,,	16·7 ,,
360·0	*	37·0 ,,	25·1 ,,
420·0	45·9 ,,	43·0 ,,	29·2 ,,

Advantages of the Method.—It may be of use briefly to point out what seem to be the advantages of this method:—

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* In this experiment the actual "turning point" was not reached.

1. The membrane, owing to the fact that there is no glazed cap to the tube, goes right up to the ends.*
2. There is no necessity to have pressure-tight joints.
3. Greater speed in working; the actual experiment takes from 2 to 3 hours only.
4. The form of tube used is such that it will withstand very high pressures—it may be of interest to mention that one of the tubes and membrane stood a pressure of 120 atmospheres without apparent harm.

We publish this preliminary notice as it will be some time before the experiments can be continued—a new apparatus has to be cast, and new porcelain tubes are required. We hope, by means of the new apparatus, to reduce greatly the guard-ring leak.

We are glad to avail ourselves of this opportunity to thank Mr. W. C. D. Whetham for the kindly interest he has taken in the research and Mr. H. Darwin for designing the pressure apparatus.

"Colours in Metal Glasses and in Metallic Films." By J. C. Maxwell Garnett, B.A., Trinity College, Cambridge. Communicated by Professor Larmor, Sec. R.S. Received April 19,—Read June 2, 1904.

(Abstract.)

The first part of the paper is devoted to coloured glasses. The phenomena which it seeks to explain were observed by Siedentopf and Zsigmondy.† Expressions are first obtained for the electric vector of the light scattered from a small metal sphere when a train of plane polarised light falls upon it, the investigation following Lord Rayleigh.‡ By means of these expressions it is proved, from the diagrams and statements given by Siedentopf and Zsigmondy, that the metal particles which they observed in gold glass are spherical in shape when the diameters are less than 10^{-5} cm. The fact that such particles are spherical throws light on the manner in which metals crystallise out of solution, the particles taking first a spherical form under the action of surface tension, and later, when they become too large for the forces of surface tension to overcome the crystallic forces, becoming amenable to the latter. Mr. G. T. Beilby has previously arrived at similar conclusions.§

An investigation into the optical properties of a transparent medium

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* *Cf.* Adie, *loc. cit.*

† 'Ann. der Phys.,' January, 1903.

‡ 'Phil. Mag.,' vol. 44, 1897, and 'Collected Papers,' vol. 4, p. 305.

§ 'Brit. Assoc. Report,' Southport, 1903.

containing metal spherules, so that the average distance between two neighbouring spheres is considerably less than a wave-length of light, is next undertaken. It is shown that every such medium has a perfectly definite colour by transmitted light, depending only on the optical constants of the metal of which the spheres are made, on the refractive index of the substance in which they are embedded, and on the quantity of metal, but not on the size or distance apart of the spheres.

The intensity of the absorption of light of each colour is proportioned to μ, the volume of metal per unit volume of medium. It is calculated, by means of the metal constants given by Drude,* that, with glass of refractive index 1·56, gold glass is more red than yellow, silver glass a little more yellow than red, copper glass considerably more red than yellow, and "potassium-sodium" glass much more blue than yellow, provided always that the average distance between two neighbouring particles of metal in the glass be considerably less than one wave-length; in which case, as stated above, the particles must be spherical. Metal glasses for which this provision is satisfied will be called "regular."

It is next proved that the presence of metal spheres accounts for the optical properties of regular gold ruby glass, and that the irregularities in the effects of colour and polarisation, sometimes exhibited by gold glasses, are due either to excessive distance between adjacent gold particles or to excessive size of such particles—the latter, however, involving the former.

Experiments are described, proving that this regular colour can be produced in a colourless metal glass, containing the metal in solution (which is the state in the manufacture of gold or copper ruby glass before the second heating) by the β-radiation from radium. Thus, a piece of clear gold glass and a piece of clear soda glass were exposed to the emanation for two days, when the gold glass had acquired an unmistakable pink tint, while the soda glass had turned an intense blue-violet.

In the second part of the paper, the optical properties of media built up out of metal spheres as before, but now so that the volume of metal may have any value between zero and unity, instead of remaining very small, as in metal glasses, are investigated. The changes of colour, and the final change to almost complete transparency, observed by Mr. G. T. Beilby† in gold and silver films, are accounted for. Explanations are also given of the changes of colour on heating, observed by Professor R. W. Wood‡, in potassium and sodium films deposited on the insides of exhausted glass bulbs. The

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* 'Phys. Zeitschrift,' January, 1900.

† 'Roy. Soc. Proc.,' vol. 72, 1903, p. 226.

‡ 'Phil. Mag.,' 1902, p. 396.

increase in strength of colour, which was generally observed in the light transmitted through these films when the plane of polarisation of obliquely incident light was changed from that of incidence to a perpendicular position is accounted for.

In Part III some evidence is brought to show that the allotropic silvers obtained by Carey Lea* are particular cases of the media which have been considered in the second part.

"The General Theory of Integration." By W. H. YOUNG, Sc.D., St. Peter's College, Cambridge. Communicated by Dr. E. W. HOBSON, F.R.S. Received April 23,—Read May 19, 1904.

(Abstract.)

The paper begins with a recapitulation of the well-known definitions of integration and of upper and lower integration (intégral par excès, par défaut; oberes, unteres Integral). The theorem on which the Darboux definition of upper (lower) integration is founded is stated and proved in the following form:—

> Given any small positive quantity e_1, we can determine a positive quantity e, such that, if the fundamental segment S be divided up in any manner into a finite number of intervals, then, provided only the length of each interval is less than e, the upper summation of any function over these intervals differs by less than e_1 from a definite limiting value (the upper integral).

Next follows a discussion as to whether it is admissible to adopt a more general mode of division of the fundamental segment than that used by Riemann, Darboux and other writers, when forming summations (upper, lower summations), defining as limit the integral (upper, lower integral), of a function over the fundamental segment. It is shown by examples first that the restriction as to the finiteness of the number of intervals into which the fundamental segment is divided cannot be removed without limitations; but that it can be removed, provided the content of the intervals is always equal to that of the fundamental segment. Secondly it is shown that the error introduced by taking the summation over an infinite number of intervals whose content is less than that of the fundamental segment, is not in general corrected by adding to the summation the content of the points external to the intervals multiplied by corresponding value (upper, lower limit) of the function. Similarly it is shown that the more

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* 'Amer. Journ. of Science,' 1886.

general division of the fundamental segment into component sets of points, whose content plays the part of the length of the intervals in the original definitions, leads to summations which do not, in general, have a definite limit even for integrable functions. The lower limit of such generalised upper summations is shown to be not less than the upper limit of such generalised lower summations; but it is shown that in general, only in case of upper continuous functions does the former give us the upper integral, and in the case of lower semi-continuous functions does the latter give us the lower integral. In general, introducing the terms *outer and inner measure of the integral* for these limits, the lower integral is less than the inner measure, which is less than the outer measure, which is less than the upper integral.

The property of semi-continuous functions just mentioned leads to a new form of the definition of the upper (lower), integral in this case, namely, as follows:—

> Divide the fundamental segment S into a finite or countably infinite number of measurable components, multiply the content of each component by the upper (lower) limit of the values of an upper (lower) semi-continuous function at points of that component and sum all such products; then the lower (upper) limit of all such summations for every conceivable mode of division is the upper (lower) integral of the semi-continuous function.

Introducing *upper and lower limiting functions*,[*] we then have the following theorem:—

> The upper (lower) integral of any function is the upper (lower) integral of its associated upper (lower) semi-continuous function.

This leads to a new definition of upper and lower integration, which is as follows:—

> Divide the fundamental segment into any finite or countably infinite number of measurable components, multiply the content of each component by the upper (lower) limit of the maxima (minima) of the function at points of that component and sum all such products; then the lower (upper), limit of all such summations for every conceivable mode of division is the upper (lower) integral of the function over the fundamental segment.

This gives us also a definition of the integral in the case when it exists, that is, when the upper and lower integrals are equal.

This form of the definitions is at once extendable to the case when the fundamental set S is any measurable set whatever, we merely have to replace the word segment by set, or more precisely by measurable

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[*] "On Upper and Lower Integration," 'Lond. Math. Soc. Proc.'

set. A particular form of division of S, analogous to that by means of intervals of the same content as the fundamental segment, is shown to lead infallibly to the upper and lower integrals of any function with respect to S; this mode of division is called *division of* S *by means of segments* (e, e'), it is such that each component lies inside a corresponding interval of length less than e, the content of these intervals being less than $S + e'$, and the points of S which are not internal to the intervals forming a set of zero content.

Based on this division of the fundamental set, we have an alternative definition of upper and lower integration with respect to a fundamental set, which is more nearly allied to the Darboux definitions for the case when the fundamental set is a finite segment. This is as follows:—

> Let the fundamental set, excluding at most a set of points of zero content, be enclosed in or on the borders of a set of non-overlapping segments each less than e, and of content less than $S+e$. Then let the content of that component of S in any segment be multiplied by the upper (lower) limit of the values of the function at points of that component, and let the summation be formed of all such products. Then it may be shown that this summation has a definite limit when e is indefinitely decreased, independent of the mode of construction of the segments and the mode in which e approaches the value zero. This limit is called the upper (lower) integral of the function with respect to the fundamental set S.

In the case when the upper and lower integrals coincide, the function is said to be integrable with respect to S, and the condition of integrability is found in a form agreeing completely with Riemann's condition in the case when S is a segment. To prove this the theorem is required that the *sum of any finite number of upper integrals of upper semi-continuous functions with respect to a fundamental set* S *is the upper integral of their sum*, and the proof of this theorem is given.

It is then shown that, except in the case of upper (lower) semi-continuous functions, the upper (lower) integral over the fundamental set S is not necessarily equal to the sum of the upper (lower) integrals over any set of components of S, but that this is the case when S is divided by means of segments (e, e').

A function which is integrable with respect to S is shown to have the following properties:—

(1) It is integrable over every component set of S.
(2) The integral of the integrable function is equal to the sum of the integrals over every finite or countably infinite number of components into which S may be divided.
(3) The sum of the integrals of any finite number of integrable

functions over S is equal to the integral of the sum of those functions over S.

In § 21 the calculation of upper and lower integrals with respect to any fundamental set S is reduced to a problem of ordinary integration. The formulæ, which are similar in form to those already given by the author for the case when S is a finite segment, in a paper presented to the London Mathematical Society, are as follows:—

The upper integral of any function with respect to a measurable set S is

$$\mathrm{KS}+\int_{\mathrm{K}}^{\mathrm{K}'}\mathrm{I}\,dk,$$

where K is any quantity not greater than the lower limit, and K′ not less than the upper limit of the function for points of S, I being the content of that component of S at every point of which the maximum of the function is greater than or equal to k.

The lower integral is

$$\mathrm{K'S}-\int_{\mathrm{K}}^{\mathrm{K}'}\mathrm{J}\,dk,$$

J being the content of that component of S at every point of which the minimum of the function is less than or equal to k.

These formulæ lead to certain theorems with respect to the distribution of the values of an ordinary continuous function and of an integrable function.

The remainder of the paper is devoted to the discussion of the inner and outer measures of the integral of any function, and in the case when they are equal of the generalised integral of a function, which is, in this generalised sense, integrable. In particular it is shown that such functions are none other than the functions which Lebesgue has named *summable*, and the generalised integral is shown to be identical with the Lebesgue integral in the case when S is a finite segment; a geometrical interpretation of the integral, similar to that used by Lebesgue, is given in the general case.

Contrasting the first definition given of the generalised integral with the geometrical definition, it is seen that they stand to one another in the same relation as the ordinary definition of integration, say of a continuous function, to its definition as a certain area. Just as, however, the mathematical concept of area is more complex than, and, indeed, depends on that of length, so does the theory of the content of a plane set of points depend naturally on that of a linear set. Just as the determination of area requires the application of the processes explained in the first definition of integration of continuous functions, so with the content of a plane set. Thus the comparative simplicity of the geometrical definition is only apparent.

Lebesgue's theorem that the sum of two summable functions is a summable function and its integral is the sum of their integrals is then proved by geometrical considerations, and a more general theorem is given, viz.: —

> If X^0 and X^1 be the outer and inner measures of the content of the ordinate section of a measurable set by the ordinate through the point x, X^0 and X^1 are both summable functions, and the generalised integral of either is the content of the measurable set.

It is here assumed that the content of the set got by closing the measurable set is finite. The content of any measurable set, with this restriction only, is thus obtained in the form of a generalised integral and, therefore, of an ordinary integral; in fact—

> The content of any measurable set (provided the set got by closing it has finite content) is $\int I dx$.

Here I is the content of the component of the fundamental set at which the inner (or the outer) measure of the content of the ordinate section of the given set is greater or equal to k.

It is to be remarked that though in this abstract reference has only been made to linear and plane sets and to the corresponding integrals, the arguments are perfectly general and apply to space of any number of dimensions. For instance the concluding result is as follows:—

> To find the content of a measurable n-dimensional set, take any hyperplane section and project the whole set on to this hyperplane. Any measurable set containing this projection we take as the fundamental set S. Divide S up in any way into a finite or countably infinite set of measurable components, and multiply the content of each component by the upper (lower) limit of the values of the (linear) inner or outer content of the corresponding ordinate sections of the given set, summing all such products, the lower (upper) limit of all such summations is the content of the given set.

"On the Liquefied Hydrides of Phosphorus, Sulphur, and the Halogens, as Conducting Solvents.—Part I." By D. McIntosh and B. D. Steele. Communicated by Sir William Ramsay, K.C.B., F.R.S. Received April 26,—Read May 19, 1904.

Ammonia, water, and hydrofluoric acid, are the only hydrides of the elements which have been systematically investigated with respect to their solvent properties, and in particular with respect to their power of forming electrically conducting solutions. With the object of extending our knowledge of the properties of these hydrogen compounds, when liquefied, and in the hope that more light might thereby be thrown on the question of ionic dissociation, the investigation, of which this is a brief abstract, has been undertaken.

The following hydrides of the fifth, sixth, and seventh groups have been examined, hydrogen phosphide, sulphide, chloride, bromide, and iodide. Since it had been found by preliminary experiments that, with the exception of hydrogen phosphide, all these possessed the power of conducting the current when certain substances were dissolved in them, a number of physical constants were measured before proceeding to the systematic study of the conductivity of solutions.

A brief summary of the results found hitherto is contained in the following tables, in which the temperatures are given to the nearest tenth of a degree.

1.—*The Vapour Pressure Curves.*

These were determined by the method described by Travers, Senter, and Jaquerod,* and used by them for the measurement of the vapour pressures of liquid oxygen and hydrogen.

Hydrochloric Acid.

T.	P.	T.	P.	T.	P.
−109·9	141·0	−97·2	316·0	−85·9	648
−104·5	198·0	−92·9	430·0	−83·2	748
−101·3	245·0	−89·8	522·0	−80·5	868

Hydrobromic Acid.

T.	P.	T.	P.	T.	P.
−104·2	96·0	−89·3	245·0	−76·7	501
−100·7	142·0	−87·1	284·0	−74·0	575
− 96·3	185·0	−83·0	357·0	−70·7	682
− 92·8	214·0	−79·3	431·5	−68·4	775

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* 'Phil. Trans.,' A, 1902, vol. 200, p. 138.

Hydriodic Acid.

T.	P.	T.	P.	T.	P.
− 77·9	74·0	− 54·8	303·5	− 43·5	530
− 73·5	92·0	− 51·4	369·0	− 41·7	578
− 68·4	126·0	− 50·0	376·0	− 39·4	644
− 63·5	185·5	− 47·7	438·0	− 36·9	713
− 59·5	224·0	− 46·1	474·0	− 35·9	769

Sulphuretted Hydrogen.

T.	P.	T.	P.	T.	P.
− 84·0	193·0	− 75·6	314·0	− 69·1	456
− 81·7	220·0	− 73·3	364·0	− 66·1	538
− 78·4	270·0	− 71·6	400·0	− 62·2	676

Phosphoretted Hydrogen.

T.	P.	T.	P.	T.	P.
− 105·9	237·0	− 97·7	393·0	− 88·6	644
− 101·2	319·0	− 93·1	498·0	− 86·6	719

The melting and boiling points as read from the curves are as follows:—

	HCl.	HBr.	HI.	H_2S.	H_3P.
M. p.	—	− 86·0	− 50·8	—	—
B. p.	− 82·9	− 68·7	− 35·7	− 60·1	− 86·2

2.—*The Densities.*

The densities of the pure liquids were determined over a wide range of temperature, and the values at the boiling point are given in the following table:—

	HCl.	HBr.	HI.	H_2S.	H_3P.
Density at b. p.	1·195	2·157	2·799	0·964	0·744

3.—*The Molecular Surface Energy.*

The surface energies were measured over a considerable range of temperature, using a modification of the method of Ramsay and Shields. In the table the value of the molecular surface energy $\lambda(MV)^{\frac{2}{3}}$ at various temperatures is given. The values of $d\lambda(MV)^{\frac{2}{3}}/dT$ and of the association factor x are tabulated separately. From the results it will be seen that of the substances examined, hydrogen, bromide, iodide, and sulphide occur as simple molecules; whilst hydrogen chloride and phosphide are more or less associated.

Hydrochloric Acid.

T.	$\lambda (MV)^{\frac{2}{3}}$.	T.	$\lambda (MV)^{\frac{2}{3}}$.	T.	$\lambda (MV)^{\frac{2}{3}}$.
163·1	263·7	175·8	244·8	187·2	229·3
168·5	255·9	180·1	239·0	189·9	223·6
171·8	250·8	183·2	233·6	192·6	221·0

Hydrobromic Acid.

181·9	330·1	188·9	314·6	198·2	294·8
184·8	325·6	193·4	307·3	200·5	292·2
186·1	320·1	195·3	299·6	203·9	283·8

Hydriodic Acid.

225·3	367·0	230·9	355·3	235·0	348·0
227·1	362·8	232·9	351·0	236·5	344·6
229·3	358·5	—	—	—	—

Sulphuretted Hydrogen.

189·1	349·5	197·4	334·1	203·9	324·7
191·3	345·3	199·7	328·3	206·9	316·7
194·6	338·0	201·5	326·6	210·8	308·6

Phosphoretted Hydrogen.

167·1	287·2	175·4	273·4	—	—
171·8	279·6	179·9	265·4	—	—

Variation of molecular surface energy with temperature and the association factor—

	HCl.	HBr.	HI.	H_2S.	H_3P.
$d\lambda (MV)^{\frac{2}{3}}/dT$...	1·47	2·03	1·99	1·91	1·70
x................	1·5	1·0	1·0	1·1	1·4

4.—*The Viscosities, and Viscosity Temperature Coefficient.*

The viscosities of the pure liquids were determined by comparison with that of distilled water at 22°. The object of the measurements was to procure data for the comparison of the temperature coefficients of viscosity and of electrical conductivity.

Viscosity Temperature Coefficient, $d\eta/dT$.

Hydrochloric Acid.

T	160·8	166·7	171·7	177·0	183·2	188·2
η.........	0·590	0·569	0·530	0·514	0·493	0·477

$$d\eta/dT = 0·88.$$

Hydrobromic Acid.

T	186·8	188·8	190·8	193·7	197·3	199·4
η.........	0·911	0·902	0·890	0·877	0·857	0·851

$d\eta/d\mathrm{T} = 0·57.$

Hydriodic Acid.

T	223·3	225·6	227·2	230·6	231·5	233·9	236·4
η......	1·479	1·454	1·437	1·426	1·402	1·377	1·353

$d\eta/d\mathrm{T} = 0·70.$

Sulphuretted Hydrogen.

T	191·0	193·3	198·2	201·2	206·1	209·8
η.........	0·547	0·528	0·510	0·488	0·470	0·454

$d\eta/d\mathrm{T} = 1·10.$

5.—*Solubilities and Conductivities.*

Of the substances examined at this stage, the organic ammonium salts were found to be readily soluble, and to give conducting solutions. Some doubt existed as to whether any metallic salts were dissolved; if so, none were found to conduct the current. The only readily soluble inorganic substances were hydrogen chloride and bromide dissolved in sulphuretted hydrogen. It is somewhat remarkable that these solutions are perfect non-conductors.

The conductivity of a few substances was accurately determined; as, for instance, that of solutions of triethyl ammonium chloride in hydrogen bromide and sulphide. It was found that the molecular conductivity of these two solutions, and of all the others which were examined, increased enormously with increasing concentration, instead of showing a slight decrease, as in the case of aqueous solutions.

The further study of the solubilities and conductivities forms the subject of another paper.*

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* *Infra*, p. 454.

"On the Liquefied Hydrides of Phosphorus, Sulphur, and the Halogens, as Conducting Solvents.—Part II." By E. H. Archibald and D. McIntosh. Communicated by Sir William Ramsay, K.C.B., F.R.S. Received April 26,—Read May 19, 1904.

In continuation of the preceding investigation,* a very large number of substances have been examined with regard to their solubility in the four substances—hydrogen chloride, bromide, iodide, and sulphide.

The following is a brief summary of the results which have been so far obtained :—

No salt of the metals has been found to dissolve in more than traces in either solvent, and in no case was it certain that such substances dissolved at all. We cannot, therefore, confirm the observation of Helbig and Fausti,† who state that stannic chloride dissolves in liquid hydrogen chloride, but does not form a conducting solution.

On the other hand, many organic substances were found to be readily soluble; as for example, the amine salts, acid amides, and certain alkaloids among compounds containing nitrogen; and alcohols, ethers, ketones, phenols, and some organic acids and esters among compounds containing oxygen.

In every case where a conducting solution was formed the dissolved substance was one containing an element the valence of which might be increased, thus dyad oxygen or sulphur becoming tetrad, or triad nitrogen becoming pentad.

The conductivity of a large number of solutions was measured, and in all cases the molecular conductivity increased enormously with concentration. No case has been met with in which the molecular conductivity varies in the same way as in aqueous solutions. The substances rarely conduct better than $n/25$ KCl.

A few of the measurements are given in the following table, in which the concentration C is given in gramme molecules per litre, and the conductivity in reciprocal ohms × 10^{-6}.

Substance.	Solvent.	C.	Conductivity.
Acetamide	HBr	0·011	65·2
,, ...	,,	0·713	3155·0
Acetonitrile........................	HCl	0·0463	1512·0
,, ...	,,	1·232	9636·0
Ethyl oxide........................	HI	0·10	19·5
,, ...	,,	1·14	2208·0
Triethyl ammonium chloride...	H_2S	0·014	117·0
,, ,, ...	,,	0·401	1580·0

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* *Ante, p.* 450.

† 'Zeit. für Angewandte Chemie,' vol. 17, 1904.

A large number of temperature coefficients have been measured. These were found to be in the majority of cases positive, *i.e.*, the conductivities increase with rise of temperature.

All the experiments which have been hitherto carried out lead to the conclusion that it is the dissolved substances (*i.e.*, the acetamide, etc.) which carries the current and not the halogen hydride. In other words, we are dealing with solutions in which the organic and not the inorganic substance undergoes electrolytic dissociation.

Further experiments are at present in progress, having for their object the measurement of the molecular weight of the dissolved substances (McIntosh and Archibald) and the determination of the transport numbers (Steele).

Discussion of the results so far obtained is deferred until these experiments are completed.

"Note on the Lymphatic Glands in Sleeping Sickness." By Captain E. D. W. GREIG, I.M.S., and Lieutenant A. C. H. GRAY, R.A.M.C. Communicated by Colonel BRUCE, F.R.S., at the desire of the Sleeping Sickness Commission. Received and Read May 5, 1904.

Captain Greig, in a letter dated March 17, 1904, writes that following a suggestion of Dr. Mott, they have examined the contents of lymphatic glands during life from fifteen sleeping-sickness patients. In all of them actively motile trypanosomes were very readily found in cover-glass preparations taken from the cervical glands. They were also present in other glands such as the femoral, but were not nearly so numerous.

They found the trypanosomes to be far more numerous in the glands than in the blood or cerebro-spinal fluid, and believe that the examination of fluid removed from lymphatic glands will prove to be a much more rapid and satisfactory method of diagnosing early cases of sleeping sickness than the examination of the blood.

At first the glands were excised, but this was soon found to be unnecessary, as it is easy to puncture a superficial gland with a hypodermic syringe and suck up some of the juice into the needle and blow this out on a slide. The actively moving trypanosomes were readily found after a short search in these slides, when a prolonged search in similar preparations of the blood from the finger failed to discover them. In stained specimens, in addition to well-formed trypanosomes, there exist many broken-down remains, which suggests that a destruction of the trypanosomes takes place in the glands.

The authors also examined the cervical lymphatic glands of the five natives suffering from trypanosomiasis who have been under observation for the past year, and found actively motile trypanosomes in the liquid withdrawn from the glands in all of them. Tabula, one of these patients, is employed in the hospital, and the dispenser reports he is getting very stupid.

The lymphatic glands were also examined for streptococci by staining and culture, but in every case were found to be sterile. Some of the cases, the glands from which were examined for streptococci, were very far advanced. The streptococcus invasion must, in the opinion of the authors, be a very late one and only occur shortly before death.

Observations made upon the blood show a constant increase in the percentage of lymphocytes, but the total leucocytes are not increased.

The authors consider that these observations throw a new light upon the glandular enlargements which have been so constantly noticed in sleeping sickness, and that the disease is essentially a polyadenitis brought about by the arrest of the trypanosomes in the glands where many of them are destroyed, but whence some escape from time to time into the blood stream and thus occasion the increase which has been observed in the peripheral circulation.

They regard their observations upon the presence of trypanosomes in number in the lymphatic glands of both early cases of trypanosomiasis and advanced cases of sleeping sickness, as affording important evidence of the unity of these diseases, and further proof that the trypanosomes are the essential cause of sleeping sickness.

"The Behaviour of the Short-Period Atmospheric Pressure Variation over the Earth's Surface."

By Sir NORMAN LOCKYER, K.C.B., LL.D., F.R.S., and WILLIAM J. S. LOCKYER, Chief Assistant, Solar Physics Observatory, M.A. (Camb.), Ph.D. (Gött.), F.R.A.S. Received April 13,—Read April 28, 1904.

[PLATES 12 AND 13.]

In a paper which we communicated to the Society in June, 1902,* we drew attention to the fact that in our investigation dealing with the percentage frequency of prominences and changes in atmospheric pressures, we found that the pressures in India and Cordoba behaved in an opposite manner, the short period variations of one being the inverse of those of the other; both, however, were closely associated with the prominence frequency.

In a subsequent paper† we showed that these two regions, in which these inverse pressure-variation conditions were clearly distinguishable, were, as far as had then been investigated, of considerable extent, the Indian region extending to Ceylon, Java, Mauritius and Australia, and that of Cordoba to the southern part of the United States.

The facts there collected were stated to be so suggestive that the inquiry was being continued by collecting and discussing observations made in other areas on the earth's surface, so as to note the extent of these similar pressure areas.

The present communication contains the results that have so far been obtained.

The greater portion of the facts here collected has been discussed some time, but as it was considered desirable, before communicating the present paper, to include as many regions on the earth's surface as could be obtained, a longer delay than was anticipated has taken place; even now there are many regions which we have been unable to include. The regions for which further observations are desired include the west coast of Africa, the northern part of South America, and the north-western portion of North America, and Polynesia in the South Pacific Ocean.

In our previous papers we have pointed out the advisability of dividing the year into groups of months according as the pressure is above or below the mean value for the year. In this way the high or the low pressure months can be dealt with separately, if necessary, and any excess or deficiency from a mean value exhibited in either or both of these from year to year can be closely followed.

Such a division of the year can be accurately determined for places

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* 'Roy. Soc. Proc.,' vol. 70, p. 500.

† 'Roy. Soc. Proc.,' vol. 71, p. 134.

which have a regular and pronounced annual pressure variation, such as India, and where the yearly barometric range is of far greater magnitude than any other aperiodic fluctuation.

In those regions where the mean yearly curve is more misleading than otherwise, the division, according to the two seasons included in the two groups of months, April to September and October to March, is best adapted.*

The system adopted in the present investigation was to take the pressure variations over India and Cordoba as the chief types of each region, denoting those of the former by the symbol (+), and those of the latter by (−). The pressure curve of any other place was then taken and compared with each. If, for example, it was found that the curve extending over several years exhibited an excess pressure at those epochs when the Indian pressure curve was in excess, then it was classified as being similar to the Indian type and represented by a (+). If it was seen that although it was more like the Indian curve than that of Cordoba, but yet not quite the exact counterpart of India, then it was denoted by (+ ?). In a similar way pressure curves like Cordoba were classified as (−), and those more like Cordoba than India as (− ?).

In some regions the pressure variation curves were distinctly a mixture of both the Indian and Cordoba types, and it was difficult to classify them satisfactorily by the above method. The symbol adopted for these cases was (± ?). Again, there were further some curves in which even this mixed type of symbol was not sufficient to exhibit the relationship of their variations to the other curves, so a special symbol (?) denoting ambiguity was used.

In the present investigation of this similarity or dissimilarity of atmospheric pressure changes over large areas, it was found that the special types were apparent sometimes in the yearly curves, sometimes in those for one or other of the high or low pressure groups of months, or sometimes in both of these. It did not, however, appear to follow that, because the type was distinguishable in the yearly curves, it was necessarily apparent in both the curves of the high and low pressure months.

The accompanying table, although yet somewhat incomplete, gives a tabulated statement of the data employed in the present survey.

The table explains itself, but it may be remarked that in Columns 6

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* To show the misleading nature of the mean annual pressure-variation curve over, for example, the British Isles, it is only necessary to plot the actual monthly values of pressure for any one year on this mean curve and draw a curved line through them, when it will be seen that there is practically no relationship whatever between the two curves. If, on the other hand, the actual monthly pressure values during any one year be plotted on the mean annual pressure-variation curve for India, the former follow very closely the swing and amplitude of the latter.

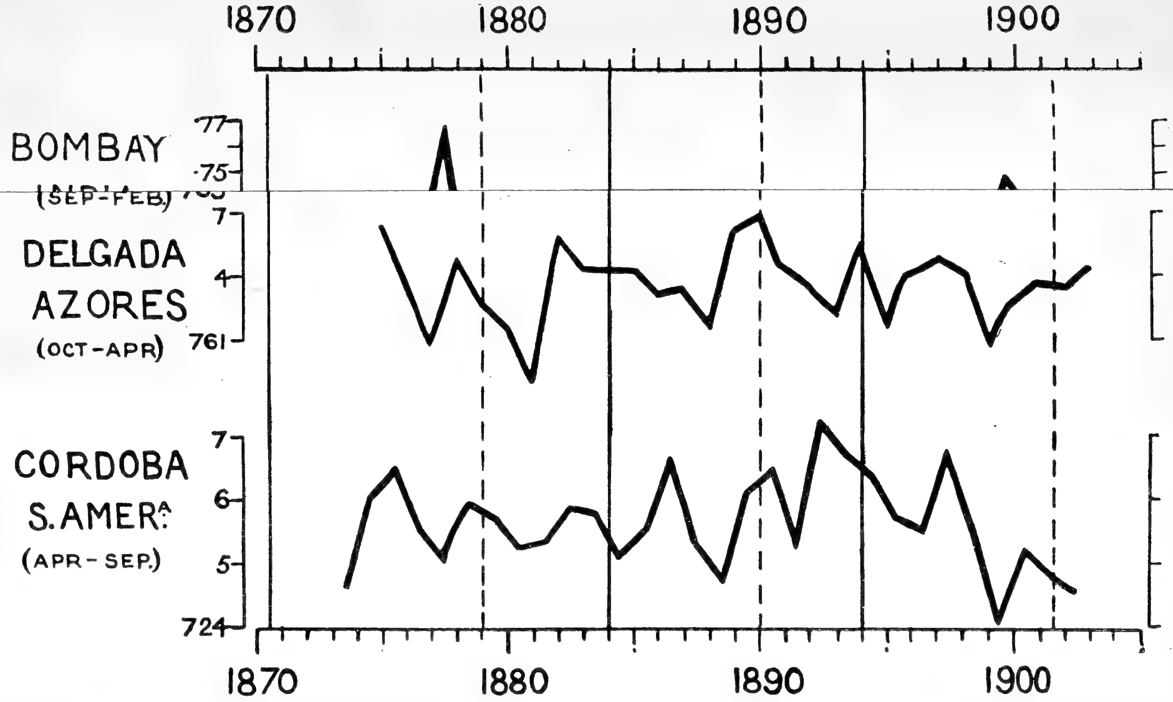
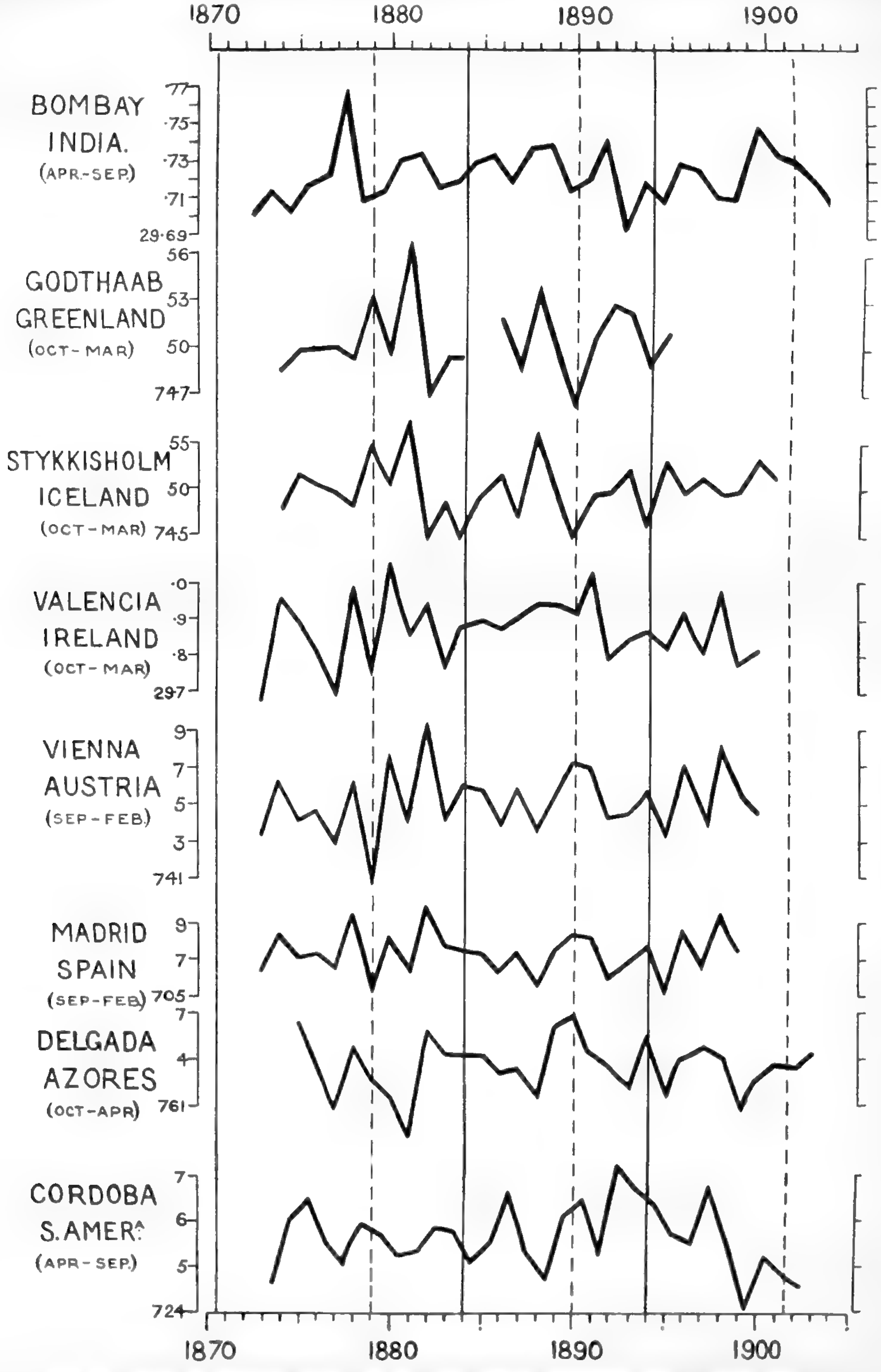


Plate 12.—Shewing the Relationship between the Short Period Pressure Variations for places in Western Europe, and the two Main Types of Pressure Variations, namely: India (+) and Cordoba (−). This Plate should be studied in relation to the map in fig. 1. *Note.*—The continuous and broken vertical lines denote the epochs of sunspot maxima and minima as determined from the mean daily areas of both hemispheres of the sun.



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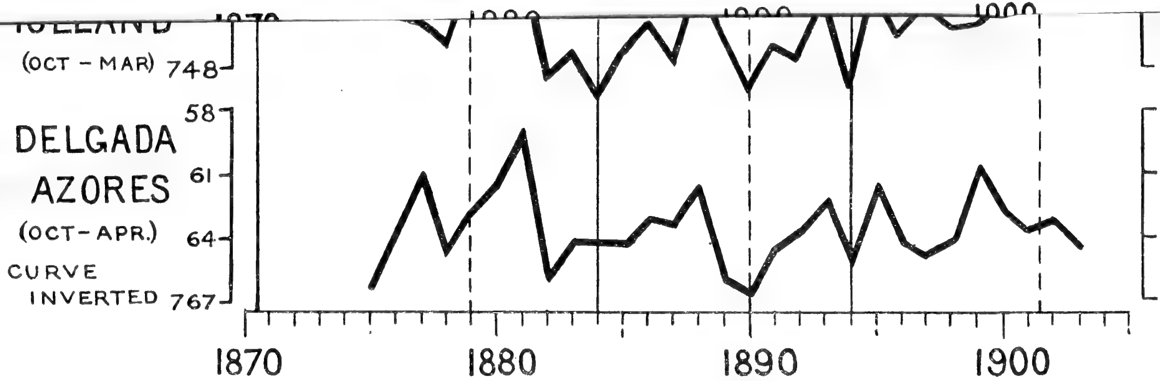
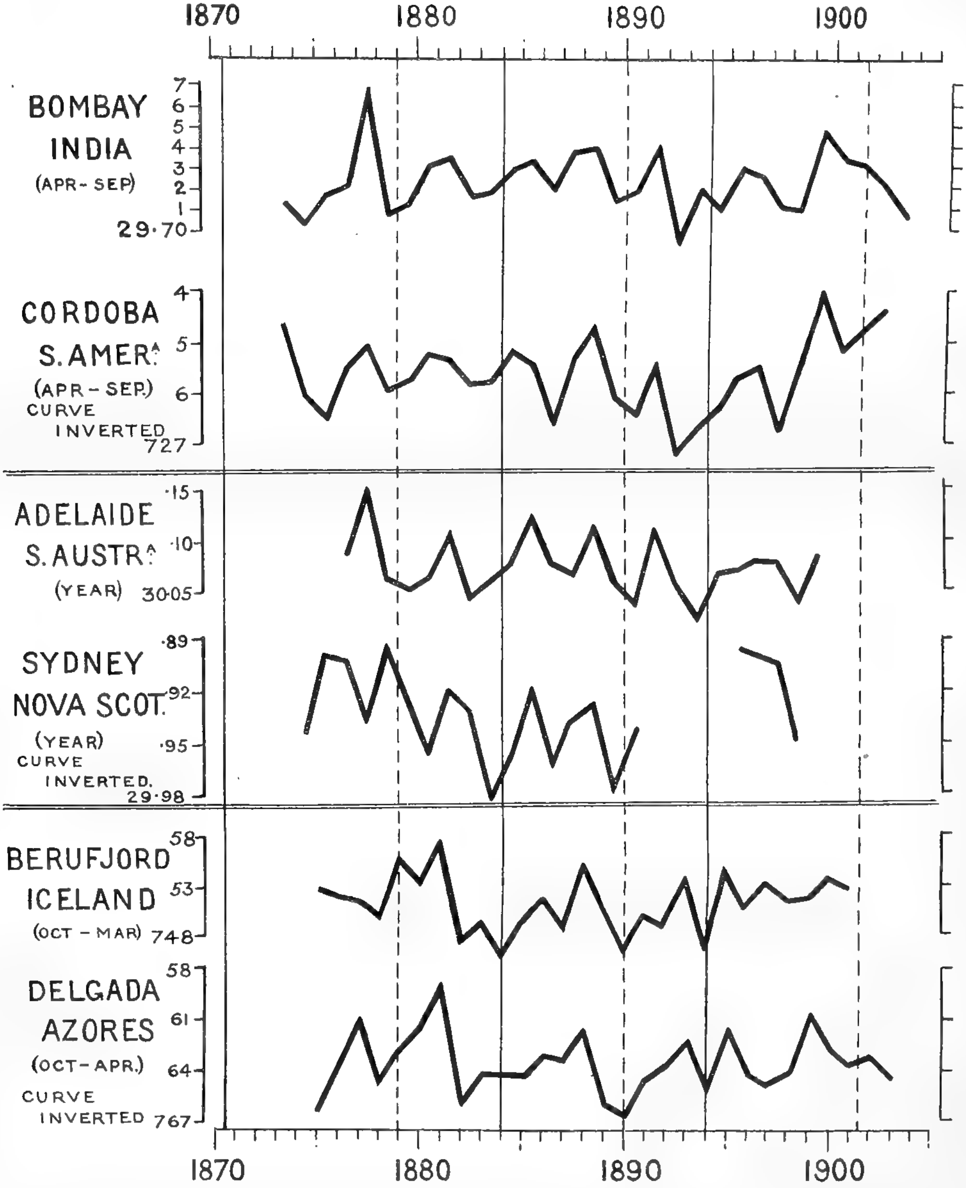


PLATE 13.—Three pairs of Curves to illustrate Reverse Pressure Conditions at stations widely separated geographically, such as India and Cordoba (S. America), Adelaide (S. Australia) and Sydney (Nova Scotia), and at two stations near each other, as Berufjord (Iceland) and Delgada (Azores). In each case the second curve has been reversed. *Note.*—Vertical lines same as in Plate 12.



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and 7 are given the groups of months (whether high or low pressure months) in each case for which the pressure curves examined afforded the greatest resemblance to the types which form the basis of the classification. In cases in which the curves of the mean yearly values alone have been utilised, these columns have been left blank and reference made in the column headed "General Remarks."

Although the above classification gives a very fair idea on the whole of the types of pressure variations from one region to another, minor peculiarities have been met with which have tended to add a certain amount of difficulty.

These remarks apply principally to places in the more northern latitudes.

Thus, for instance, Greenland and Iceland have been classified as of the (+?) type, the British Isles, Germany, and Spain of the (±?) type, and the Azores of the (−?) type. From a glance at the accompanying plate (Plate 12), which shows the relation to each other of some of the pressures over these areas, the changes from a (−?) to a (+?) type can be observed.

It will be seen that although practically the same groups of months have been taken in each case, pressure in excess of the mean value in Greenland or Iceland corresponds to a deficiency of pressure over the area covered by Great Britain, Austria and Spain, the curves being in the main the reverse of each other. Again, the pressure curve for the Azores follows more nearly the (−) type, as will be seen by comparing it with the Cordoba curve, but it has a certain similarity to those of Madrid, Vienna, etc., to which it must therefore be closely connected.

While the western portion of Europe is of this (±?) type, the eastern portion gradually assumes the (−?) type, and this region extends not only probably to Norway and Sweden, but right across European and Asiatic Russia. The European Russian type of curve has an undoubted similarity to those of more Western Europe, but there are variations which indicate that the type is more like that of Cordoba than India.

Again, another region in which rather mixed types of pressures are met with is that of Eastern and North-eastern Canada. Curiously enough Prince Edward Island and Sydney (Nova Scotia) correspond very closely to the (−) type, if allowance be made for the differences about the year 1877.

The inverted curve for the latter with the Adelaide (Australia) pressure curve for comparison is shown in an accompanying plate (Plate 13).

In addition to illustrating this reversal between Adelaide (+) and Sydney (Nova Scotia) (−?), this plate shows also, to serve as examples, curves for two other sets of reverse pressure conditions. Thus

Analysis of

Country.	Station.	Type.	Remarks on type.	Period of observations.
India	Bombay	+	Chief (+) type, see Plate 13	1860—1903
	Madras	+	.. ..	1861—1902
	Calcutta	+	.. ..	1860—1902
	Nagpur	+	.. ..	1869—1903
	Darjeeling	+	.. ..	1875—1901
Ceylon.......	Colombo	+	.. ..	1881—1900
Persia	Bushire.............	+	.. ..	1879—1887
Arabia.......	Aden	+	.. ..	1883—1899
Indian Ocean	Seychelles..........	+	.. ..	1885—1898
	Rodriguez	+ ?	.. ..	1885—1898
	Mauritius...........	+	.. ..	1875—1901
East Indies...	Batavia	+	.. ..	1866—1898
Philippines...	Manila	± ?	.. ..	1883—1901
Malay Penin.	Singapore	+	.. ..	1869—1902
Australia	Perth	+	Slight differences	1876—1900
	Adelaide............	+	Remarkably pronounced, see Plate 13	1876—1899
	Sydney	+	.. ..	1860—1899
New Zealand	Dunedin	?	Difficult to classify	1866—1898
	Auckland	?	,, ,, ...	1867—1903
Africa.......	Zanzibar (Island)....	+ ?	.. ..	1880-3—1891-7
	Durban	+	.. ..	1884—1903
	Capetown	+ ?	.. ..	1860—1899
	Cairo..............	+ ?	.. ..	1869—1898
	Alexandria	± ?	.. ..	1870—1896
	Biskra.............	± ?	.. ..	1874—1884
	Sierra Leone.........	− ?	.. ..	1876—1900
	St. Thomas (Island)..	(+ ?)	.. ..	1874—1883
	Kimberley	+ ?	.. ..	1876—1899
	St. Paul de Loando...	+ ?	.. ..	1880—1895
South America	Cordoba	−	Chief (−) type, see Plate 13	1873—1902
	Tugumen	−	Distinctly like Cordoba ..	1885—1897
	Goya	−	Exactly like Cordoba	1877—1897
	Santiago	−	.. ..	1874—1884
	Rio de Janeiro	− ?	.. ..	1860—1899
West Indies..	Jamaica	− ?	.. ..	1866—1900
	Barbadoes	?	.. ..	1865—1900

Pressure Types.

Months in which types are most conspicuous.		Source of data.	General remarks.
Apr.—Sept.	Low press.	Indian Monthly Weather Reviews	Type equally prominent in curve for year.
"	"	" "	" "
"	"	" "	" "
..	..	" "	Yearly curve only examined.
..	..	" "	" "
Apr.—Sept.	Low press.	" "	Type equally prominent in curve for year.
Oct.—Mar.	High press.	" "	Record very short.
Apr.—Sept.	Low press.	" "	Equally prominent in yearly and Oct.—Mar. curves.
..	..	Mauritius Met. Obsns........	Yearly curve alone examined.
..	..	" "	Short and uncertain record. Yearly curve examined.
May—Oct.	High press.	" "	Prominent in yearly curve.
June—Oct.	"	Meteorological Office	" "
..	..	Report of Philippine Commission	
..	..	Met. Obsns. at Straits Settlements	" "
Apr.—Sept.	High press.	Met. Obsns. at Perth and district	
"	"	Met. Obsns. at Adelaide......	Very prominent in yearly curve.
"	"	Met. Obsns. in New South Wales	" "
..	..	New Zealand Statistics (Met.)	1877 pulse absent.
..	..	Met. of New Zealand	
Nov.—Apr.	Low press.	Indian Monthly Weather Reviews	Record very broken and short.
Oct.—Mar.	"	Report of Govt. Astronomer, Natal	
Apr.—Sept.	High press.	Meteorological Office	Broken record, 1871—1877.
"	Low press.	Met. Report of Abassia Obsy., Cairo	
"	"	Met. Zeitschrift, 1897	
Oct.—Feb.	High press.	Kong. Sv. Vet. Ak. Handlingar, vol. 29, No. 3	Record too short.
..	..	Army Medical Dept. Reports	Yearly curve alone examined.
Nov.—May	Low press.	Lisbon Met. Obsns..........	Record too short.
..	..	.. .. ..	Yearly curve alone examined.
..	..	Supplement to Annals of Lisbon Observatory and Loanda Met. Obsns.	" "
Apr.—Sept.	High press.	Anales de l'Oficina Met. Argentina, vol. xiii	
"	"	Anales de l'Oficina Met. Argentina	Broken record.
"	"	" "	Continuous good record.
May—Oct.	"	Kong Sv. Vet. Ak. Handlingar, vol. 29, No. 3	
May—Sept.	"	Meteorological Office	
..	..	Army Medical Dept. Reports	Yearly curve alone examined. Two short breaks in record.
..	..	" "	" "

Analysis of Pressure

Country.	Station.	Type.	Remarks on type.	Period of observations.
North America	Jacksonville	−	Some slight differences	1873—1899
	Mobile	−	„ „	1873—1899
	Pensacola	−	„ „	1880—1899
	Nashville	− ?	Marked differences	1873—1899
	San Diego	−	Slight differences	1873—1899
	St. Louis	− ?	Marked differences	1873—1899
	Bismarck	− ?	„ „	1875—1899
	Kansas City	± ?	.. ..	1890—1899
	Boise	− ?	.. ..	1878—1890
	Salt Lake City	− ?	Slight differences	1874—1899
	Santa Fé	−	Close resemblance	1873—1899
	Denver	− ?	.. ..	1863—1899
	Portland	± ?	Equally like both types	1873—1899
	Galveston	−	Some slight differences	1873—1899
	Alpena	± ?	Difficult to determine	1873—1899
	Buffalo	± ?	„ „	1873—1899
	Pikes Peak	± ?	„ „	1874—1887
	Ft. St. Michaels (Alaska)	+ ?	.. ..	1874—1884
Atlantic Ocean	Bermuda	−	Undoubtedly this type	1866—1899
Canada	Toronto	± ?	.. ..	1874—1890
	Fort Garry (Manitoba)	± ?	.. ..	1874—1891
Nova Scotia	Sydney	− ?	See Plate 13	1874—1898
Sandwich Isles	Honolulu	−	1877 Indian pulse present	1873—1901
Tahiti	Papeete	± ?	Like India, one year in advance	1876—1891
Greenland	Jacobshavn	?	.. ..	1873—1897
	Godthaab	+ ?	Very like Jacobshavn, see Plate 12	1873—1895
Iceland	Stykkisholm	+ ?	Very slight differences, 1877 present, see Plate 12	1874—1901
	Berufjord	+ ?	Very slight differences, 1877 present, see Plate 13	1874—1901
Europe	Tromso	± ?	.. ..	1874—1898
	Vardo	− ?	.. ..	1870—1898
	Aalesund	+ ?	.. ..	1861—1898
	Stornoway	± ?	1877 pulse absent	1863—1903
	Aberdeen	± ?	Very slight differences, 1877 absent	1869—1899
	Armagh	± ?	„ „	1869—1903
	Stonyhurst	± ?	.. ..	1885—1902
	Valencia	± ?	See Plate 12	1868—1899
	Palermo	± ?	.. ..	1874—1884
	Madrid	± ?	Remarkable likeness to (−) at times, see Plate 12	1860—1899
	Gibraltar	− ?	Like Oct.—Apr., Azores	1864—1900
	Lisbon	− ?	.. ..	1860—1896
	Vienna	± ?	See Plate 12	1860—1901
	Constantinople	+ ?	.. ..	1874—1884

Types—*continued.*

Months in which types are most conspicuous.		Source of data.	General remarks.
Nov.—Feb.	High press.	Report of Chief of Weather Bureau, U.S.A.	
Oct.—Mar.	”	” ”	
Nov.—Mar.	”	” ”	
Sept.—Feb.	”	” ”	
Nov.—Apr.	”	” ”	
(Mar.—Aug.)	(Low press.)	” ”	
Apr.—Sept.	Low press.	” ”	
..	..	” ”	Short record.
Oct.—Mar.	High press.	” ”	”
May—Oct.	Low press.	” ”	
Dec.—May	..	” ”	Record broken, 1882—1885.
June—Nov.	High press.	” ”	
..	..	” ”	Yearly mean curve alone examined.
Oct.—Mar.	High press.	” ”	
..	..	” ”	” ”
..	..	” ”	” ”
..	..	” ”	Short record.
..	..	Kong. Sv. Vet. Ak. Handlingar, vol. 29, No. 3	Yearly curve alone examined. Broken record.
Dec.—Mar.	High press.	Army Medical Dept. Reports	Record somewhat broken.
..	..	Report of Met. Service of Dom. of Canada	Yearly curve alone examined.
..	..	” ”	” ”
Aug.—Nov.	High press.	” ”	Record broken after 1891.
Aug.—Feb.	Low press.	Met. Obsns., Honolulu	Record twice broken.
..	..	Met., Zeitschrift, 1892	Yearly mean curve examined.
..	..	Danish Met. Aarbog	Record broken, 1883—1887.
Oct.—Mar.	Low press.	” ”	” 1874—1875.
Apr.—Sept.	High press.	” ”	
”	”	” ”	
Sept.—Mar.	Low press.	Met. Aarbog Norsk	
”	”	” ”	
Apr.—July	High press.	” ”	
..	..	MSS. Met. Office Records	Yearly curve alone examined.
Apr.—Sept.	High press.	” ”	
”	”	” ”	
..	..	Results of met. and mag. observations, Stonyhurst	” ”
Apr.—Sept.	High press.	MSS. Met. Office Records	
..	..	Kong. Sv. Vet. Ak. Handlingar, vol. 29, No. 3	
Mar.—Aug.	Low press.	Madrid Met. Obsns.	
..	..	Army Medical Dept. Reports.	” ”
..	..	Lisbon Met. Obsns.	
..	..	Denkschriften Kaiserlichen Ak. de Wissenschaften	
Apr.—Sept.	Low press.	Kong. Sv. Vet. Ak. Handlingar, vol. 29, No. 3.	Short record of definite type.

Analysis of Pressure

Country.	Station.	Type.	Remarks on type.	Period of observations.
North Atlantic	Azores	− ?	Remarkable, undoubted (−), see Plates 12 and 13	1874—1903
	Las Palmas	− ?	.. ..	1882—1892
Russia	St. Petersburg	− ?	.. ..	1870—1899
	Moscow	− ?	.. ..	1870—1899
	Tarchankut	?	Difficult to classify	1874—1884
	Lugansk	− ?	.. ..	1872—1899
	Orenbourg	− ?	.. ..	1870—1899
	Catherinbourg	− ?	.. ..	1870—1899
	Arkangel	− ?	.. ..	1871—1899
Siberia	Tomsk	− ?	.. ..	1874—1899
	Barnaoul	− ?	.. ..	1870—1899
	Nertchinsk	− ?	.. ..	1870—1899
China	Pekin	?	.. ..	1874—1881
	Zi-ka-wei	+ ?	.. ..	1874—1884
	Hong-Kong (Island)	−	Exactly like Cordoba	1884—1903
Japan	Tokio	± ?	.. ..	1873—1895
	Kioto	± ?	.. ..	1883—1899

Bombay (+) is compared with the Cordoba (−) pressure curve (inverted), and is an example of the adopted types of pressure variation. Iceland is compared with that of the Azores (inverted), and shows the reverse conditions that prevail between a (+ ?) type and (− ?) type.

A fact to which attention was very often drawn in attempting to classify the pressure curves was that some curves after following very closely for many years the Cordoba (−) or Indian (+) type of pressure, as the case may be, would revert back to the opposite type for a period of years. Thus to take the case of one station alone, namely, Sydney (Nova Scotia) as an instance, the pressure curve follows very closely that of India from 1875—1882, after which up to 1890 it has a very close resemblance to the Cordoba type. The behaviour of this Sydney (Nova Scotia) pressure curve can be compared with the Adelaide (Australia) curve in Plate 13, but it must be noticed that the former has here been *inverted.*

There is another important fact which this study has brought to

Types—*continued.*

Months in which types are most conspicuous.		Source of data.	General remarks.
Oct.—Apr.	Low press.	Annales de l'Obsy. do Infante D. Luiz	Prominent in yearly curve.
..	..	Resumé de las Obs. Met. de Provinces (Spanish)	1886 no record.
..	..	Annales de l'Obsy. Cent. Phys. de Russe	Yearly curve alone examined.
Sept.—Feb.	High press.	,, ,,	
..	..	Kong. Sv. Vet. Ak. Handlingar, vol. 29, No. 3	
Oct.—Mar.	High press.	Meteorological Office	
..	..	Annales de l'Obsy. Cent. Phys. de Russe	Break in record 1875—1887. Yearly curve alone examined.
..	..	,, ,,	Yearly curve alone examined.
Jan.—June	High press.	Meteorological Office	
Oct.—Mar.	,,	,, ,,	Prominent in yearly curve.
,,	,,	,, ,,	,, ,,
Apr.—Sept.	Low press.	,, ,,	
..	..	Annales de l'Obs. Cent. Phys. de Russe	
Oct.—Mar.	High press.	Kong. Sv. Vet. Ak. Handlingar, vol. 29, No. 3	Short record. Prominent in yearly curve.
Apr.—Sept.	Low press.	Met. Zeitschrift, 1886, and Observations made at Hong-Kong Observatory	Visible in yearly curve.
,,	,,	Met. Zeitschrift, 1899........	
..	..	Report of Cent. Met. Obsy. of Japan.	All the curves for the half-yearly and yearly values show similar variations.

light and which plays most probably an important *rôle* with regard to the pressure variations at places which exhibit a mixed type of pressure. The earth's surface as has been shown may be divided mainly into two regions, one portion showing excess pressures at certain epochs, while the other shows deficient pressure at the same epochs. If the former region exhibits a greater excess than usual (as an example, the Indian region in 1877), then the region over which this type of pressure occurs may probably be more extensive, and the boundary dividing the two chief types of pressure will necessarily be pushed away from this region. Stations, therefore, that were just on the fringe of this boundary may at these epochs become enveloped in this more extensive high-pressure area, and will exhibit the Indian type of pressure variation.

Should the Cordoba region become more extensive than usual owing to a similar cause, then the border stations will assume the Cordoba type of pressure variation. It is not proposed to enter here into detail on this point, as the subject requires very close examination, but mention may be made of the very great area which was covered by the continuous excessive high pressure that prevailed over the Indian region from the end of 1876 to about the middle of 1878.

On fig. 1 is given a map of the world on which are marked the types of pressure variations in each region which is included in this barometric survey.

An attempt has been made by means of a neutral line to show approximately the mean lines of separation of these two chief pressure types, although it must be remembered that this line is liable to a probable small oscillation about its mean position.

As far as can at present be determined, one line commencing to the west of Alaska, separating this region from Siberia, passes easterly along about the 60° parallel of latitude and runs in a south-easterly direction between South-west Greenland and North-east Canada. It then crosses the North Atlantic, passing to the north of the Azores, and skirts the south-western portion of Portugal. It then strikes down towards the Equator, cutting North-west Africa, as far as can be judged from the scant pressure values available, through the middle of the Sahara. It leaves Africa near the Gold Coast, passes into the South Atlantic, where it cannot be traced further owing to lack of observations in this southern ocean.

The other boundary or neutral line passes to the north-east of Greenland and north of Iceland, crosses the southern portion of Norway and Sweden, and traverses Southern European Russia. It then takes a course somewhat more easterly, skirting the northern part of the Caspian Sea and Turkestan, passes between Tibet and Mongolia, and through China. It then leaves the Continent a little to the south of the Yellow Sea, and passes into the North Pacific Ocean. Here its path cannot be traced, but it evidently passes well to the east of the Philippine Islands, and Solomon Islands, takes a new south-westerly course, skirting the eastern side of Australia and passing between Tasmania and New Zealand. Its track is then again lost in the Southern Pacific Ocean.

Although too much weight must not at present be given to the positions of these neutral lines throughout their whole length, it is interesting to note that they are fairly symmetrical to one another although no attempt has been made to make them so.

Both lines apparently cross the equator at about antipodal points, and both appear to have a similar trend in northern and southern latitudes.

We seem then to be in presence of a general law relating to the pressures which occur simultaneously in two different regions of the globe, separated and defined more or less by a neutral line, this neutral line forming a fulcrum about which see-saws of pressure from one region to another take place. Special cases of such reverse pressure variations have been previously detected.

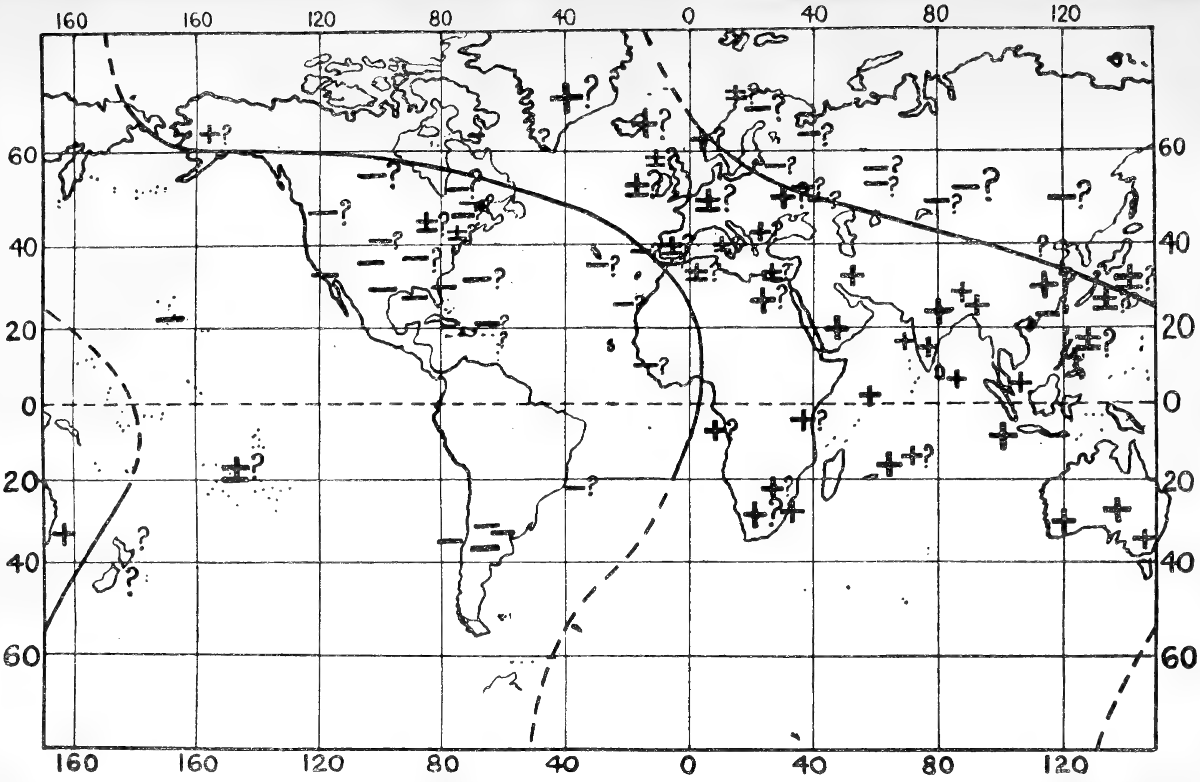

FIG. 1.—Illustrating the Distribution of the two Main Types of Atmospheric Pressure Variations. The symbols signify:—(+) like India, (+ ?) more like India than Cordoba, (−) like Cordoba, (− ?) more like Cordoba than India, (± ?) mixture of India and Cordoba, (?), difficult to classify.

Thus as long ago as 1879 Blanford,[*] from a discussion of the secular variations of barometric pressure over the wide area covering Siberia, Indo-Malaysia and Australia, pointed out that there existed a kind of long-period see-saw of a character that, while the pressure at the tropical stations was low, that in Siberia was high, and *vice versâ*. This fact, it will be seen, is quite in harmony with the pressure-type distribution, as shown in the accompanying map (fig. 1).

Hildebrandsson[†] has discussed the relation between the pressure variations of numerous places mainly situated in the chief centres of action of the atmosphere widely distributed on the earth's surface for the period 1874—1884. In this valuable communication, some of the chief results which he was led to deduce were that there were several regions which exhibited opposite types of pressure variations.

The following places are those to which he calls attention, and for comparison we give the types in brackets which have been allotted according to the method adopted in the present paper; where no type is added the region has not been examined:—

The Azores (– ?) and Iceland (+ ?); Siberia (– ?) and Alaska (+ ?), especially in winter; Tahiti (± ?) and Tierra del Fuego; India (+) and Siberia (– ?); Greenland (+ ?) and Key West (Florida) (–); Buenos-Ayres (– ?) and Sydney (Australia) (+).

It is interesting to note that these results agree well in the main with the present distribution of the regions which have been examined.

Again Hann[‡] has recently drawn attention to the fact that there exists a see-saw between the Azores and Iceland, and he showed that in 80 per cent. of cases the largest positive pressure variations at Stykkisholm (Iceland), corresponded to negative pressure variations at Ponta Delgada (Azores), and that the largest negative pressure variations at Stykkisholm were in 87 per cent. of cases positive variations at Ponta Delgada.

This result obtained from the observations extending from 1846—1900 endorses Hildebrandsson's previous conclusion deduced from observations over the period 1874—1884, and confirms the position of the neutral line shown on fig. 1, dividing the two large types of pressure areas.

Quite recently Professor Bigelow[§] has published a map of the world on which he has indicated the distribution of the pressure types according as they follow the Indian (or direct type, as he calls it) or the Cordoba (indirect) pressure variations.

Professor Bigelow has also found that there are many regions in

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[*] 'Report of the Meteorology of India in 1878,' pp. 2—35.

[†] "Quelques Recherches sur les Centres d'Action de l'Atmosphère," 'Kongl. Svenska Velenskaps-Akad. Handlingar,' vol. 29, No. 3.

[‡] 'Kaiserliche Akademie der Wiss. in Wien,' January 7, 1904.

[§] 'Monthly Weather Review,' p. 509, November, 1903.

which it is very difficult to say exactly which type is followed, and as he says there may be "differences of opinion as to the assignment of some of these curves, but the reader can make any different arrangement that he prefers."

In most of the main features, however, his map suggests a somewhat similar distribution of these pressure types to that given here. Thus, he finds that "the region around the Indian Ocean gives direct synchronism, South America and North America give inverse synchronism, while Europe and Siberia give an indifferent type. Greenland and Iceland seem to have direct type like the Indian Ocean . . . .

"The eastern hemisphere tends to direct synchronism, except in Europe and Russia where the indifferent type prevails, and the western hemisphere to the inverse type."

It may be further pointed out that regions which are the reverse of one another as regards these secular pressure variations should very probably experience opposite kinds of abnormal weather, while those over which the same type of pressure variation exists should have weather of an abnormal but similar nature.

That this is inclined to be so as regards the latter statement has been recently* very forcibly pointed out by Sir John Eliot with respect to the Indian area. He writes:—

"The drought of 1895—1902 was a more or less general meteorological feature of the whole area, including Abyssinia, East and South Africa, Afghanistan, India, probably Tibet, and the greater part or whole of Australia."

The whole of this region, as will be seen from the accompanying map (fig. 1), is embraced by the (+) type of pressure.

In the light, therefore, of the existence of these large regions of opposite pressure types, it is vital in the interest of long-period forecasting that observations from all portions of the globe should be included in any discussion.

Several years ago Eliot† drew attention to these oscillations of pressure of long period, other than the diurnal and annual oscillations in India. In this important memoir he pointed out that "they are directly related to the largest and most important features of the weather in India, viz., the character and distribution of the precipitation of rain and snow in the Indian monsoon area."

There is reason, therefore, to believe that this short period pressure variation will in the future be of considerable assistance in helping

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* 'Broad Views,' p. 193; 'The Meteorology of the Empire during the Unique Period 1892—1902,' by Sir John Eliot, K.C.I.E., F.R.S.

† "A Preliminary Discussion of certain Oscillatory Changes of Pressure of Long Period and of Short Period in India," 'Indian Met. Memoirs,' vol. 6, part 2, 1895.

meteorologists to form a more definite idea of the prospects of approaching seasons.

We wish to express our thanks to Dr. W. N. Shaw, F.R.S., who has kindly assisted the work by permitting us to utilise the valuable collection of pressure data deposited in the archives of the Meteorological Office.

We also owe a debt of gratitude to Messrs. W. Moss and T. F. Connolly, who have shown great zeal in completing the necessary computations and drawing the numerous curves which were required for the different stations that have been investigated.

"The Spectrum of the Radium Emanation." By Sir William Ramsay, K.C.B., F.R.S., and Professor J. Norman Collie, F.R.S. Received May 18,—Read May 19, 1904.

Attempts have been made since July, 1903, to see and map the spectrum of the emanation from radium, for at that date the conversion of the emanation into helium was observed by Ramsay and Soddy, and during the first discharge of the induction current through the emanation, it was believed that a peculiar spectrum was noticed; indeed, three lines were persistent, and were mentioned in the communication on the subject in these 'Proceedings.'

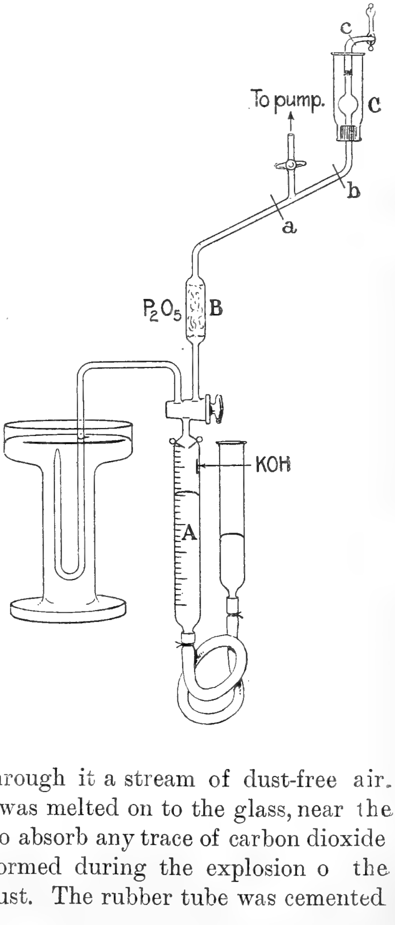
But such attempts have uniformly failed; at the first moment of the discharge, indeed, a brilliant spectrum has twice been observed, which soon became confused and indistinct. It faded before it was possible to map it, and owing to the presence of impurities, generally carbon monoxide, nitrogen, or hydrogen, the special spectrum was obscured. All that could be said was that it appeared to present some brilliantly green lines.

These experiments, however, have not been fruitless; they have led to better knowledge of the precautions which it is necessary to take to eliminate impurities. The arrangement of the apparatus, too, has been simplified, and the manipulation made easier. As it is possible that others may wish to repeat the experiments, and may perhaps have even better success in mapping the spectrum, we think it well to enter into the details of the manipulation somewhat minutely, and to give a woodcut of the apparatus employed.

The stock of radium bromide (about 109 milligrammes) dissolved in about 10 c.c. of water in two small bulbs was attached by sealing to a small Töpler's pump. Between the pump and the bulb there was a stop-cock, greased, of course, to insure freedom from leakage; but in order to prevent the long contact of the emanation with the stop-cock, and its possible contamination with carbon dioxide, the mercury from the pump was caused to flow past the stop-cock by raising the reservoir of the pump and closing the exit tube at its lower end; the mercury slowly leaked past the valve of the pump, passed the tap (which was then shut), and so confined the space above the radium bromide by means of mercury. As radium bromide yields electrolytic gas, containing an excess of hydrogen, the pressure gradually rose; the mercury in contact with this gas remained perfectly bright, and showed no tendency to adhere to the glass; the presence of ozone thus appears to be excluded, but this excess of hydrogen will form the subject of a future communication.

The emanation was allowed to accumulate for 14 days. The pump was exhausted until no trace of a bubble passed down the capillary exit tube. But as even then a trace of air must have remained in the barrel, the tap leading to the bulbs containing the radium bromide was turned rapidly, so as to admit a trace of the electrolytic gas into the pump and "wash it out." This gas was rejected. The remaining electrolytic gas with the emanation was collected in a tube which had previously been heated to redness, and then twice washed out with pure oxygen. The mercury in the collecting tube was then boiled, and the bubble of gas removed. It was hoped thereby to have eliminated every trace of nitrogen. The gas was then introduced into the gas-burette, shown in the figure, through the inverted syphon. All the mercury was freshly filtered and pure. The apparatus, too, was freshly constructed and heated to redness to burn out traces of dust. The gas-burette had been washed out with alkali and with nitric acid, and then with a stream of distilled water; it was dried by drawing through it a stream of dust-free air. Some slightly moist caustic potash was melted on to the glass, near the sparking wires; this was intended to absorb any trace of carbon dioxide which might have chanced to be formed during the explosion of the electrolytic gas by the burning of dust. The rubber tube was cemented

Fig. 1.



on to the burette; the burette was washed out twice with oxygen, and by lowering the reservoir several times the upper end was made a torricellian vacuum; it was left thus for some time, so as to insure the removal of adhering nitrogen from the walls of the tube.

The electrolytic gas was then introduced, and exploded. As the explosion-burette was graduated, the total volume of the gas, as well as that of the residual hydrogen, was read. There were 16·43 c.c. of gas; the residual hydrogen measured 1·01 c.c. at normal temperature and pressure, and thus amounted to 6·18 per cent. of the total. The volume of this gas was increased by lowering the pressure so that it was in contact with the fused potash. It was left for more than an hour; the potash, of course, was wet with the water formed by the explosion.

The capillary tubes above the stop-cock of the gas-burette, which had been twice washed out with oxygen, were pumped as empty as possible, until the vacuum-tube showed only the yellow and green lines of the mercury spectrum, and the faintest trace of a hydrogen spectrum. A strong current was passed between the electrodes so as to heat them and expel occluded oxygen. After this process had been repeated as long as was thought safe, until, as remarked, the hydrogen spectrum was extremely faint, the tap to the pump was closed. The hydrogen containing the emanation was then admitted from the explosion-burette; it was dried by passage through the narrow tube B filled with phosphoric anhydride, and it entered the bulb C, and the vacuum-tube D. This vacuum-tube was made of lead glass, with electrodes of aluminium. It was 2·5 cm. long, with a capillary of about 1 cm. in length. The aluminium electrodes were closely surrounded with glass, fused round them, so as to limit the capacity of the tube as much as possible; it was probably under one-twentieth of a cubic centimetre.

Liquid air was next poured into the jacket surrounding the bulb C, and the reservoir was raised and lowered half a dozen times, so as to convey all the gas into contact with the cooled bulb. The mercury was then raised to the level *a*, and the tap to the pump opened; and while the jacket was kept replenished with liquid air the hydrogen was pumped off, until its spectrum had almost entirely disappeared, the red line being hardly visible. The tap to the pump was then closed, the level of the mercury was raised to *b*, and the liquid air allowed to evaporate. The bulb was so bright that it was easy to read the time on a watch. The mercury was then raised to the level *c*, and the current passed. The spectrum was very brilliant, consisting of very bright lines, the spaces between them being perfectly dark; it had a striking resemblance in general character to the spectra of the gases of the argon group.

A direct-vision spectroscope, made to special design by Heele, with an illuminated scale for reading, had immediately before been standardised by noting the position of the leading lines of helium and hydrogen. They were found to lie exactly on a scale which had previously been constructed. The new lines were read as rapidly as possible, an operation which required about half a minute. During a second reading many of the lines had faded, and the secondary spectrum of hydrogen began to appear, and rapidly grew stronger. It was identified by throwing into a field a hydrogen spectrum through the small prism; and it soon became so powerful as to mask the spectrum of the emanation completely. In order to attempt to recover it, the mercury was again drawn down to *a*, and liquid air again poured into the jacket; the emanation again condensed, and the tap to the pump was opened, and the hydrogen removed by the pump until its spectrum was again hardly visible. On repeating the series of operations already described, the spectrum of the emanation was seen a second time, but it was so transient that only the position of some of the lines could be confirmed.

Next day, only the spectrum of hydrogen was visible; its secondary spectrum was strong. The day after, the same was the case: but interposing a jar and spark-gap brought out two lines which had previously been mapped; they were very feeble.

In the table which follows, all the strong lines which were read are given; the degree of coincidence of those which are of known wave-length shows the approach to accuracy obtained; the error is probably less than five Ångström units.

Wave-length.	Remarks.
6567	Hydrogen C; true wave-length, 6563; observed each time.
6307	Observed only at first; evanescent.
5975	,, ,, ,,
5955	,, ,, ,,
5805	Observed each time; persistent.
5790	Mercury; true wave-length, 5790.
5768	,, ,, ,, 5769.
5725	Observed only at first; evanescent.
5595	Observed each time; persistent and strong.
5465	Mercury; true wave-length, 5461.
5105	Not observed at first; appeared after some seconds; persisted, and was visible during the second examination.
4985	Observed each time; persistent and strong.
4865	Hydrogen F; true wave-length, 4861.
4690	Observed only at first.
4650	Not observed when the emanation was examined again.
4630	Ditto.
4360	Mercury; true wave-length, 4359.

When the spectrum was examined two days later, besides the hydrogen and mercury lines, there were seen:—5595, feeble; 5105, feeble; 4985, very feeble; this was with a jar and spark-gap interposed; the ordinary discharge showed only the primary and secondary spectra of hydrogen, and that of mercury.

Eleven days later, the emanation from the same stock of radium bromide was collected, and treated in exactly the same manner. This time, however, an excess of conscientiousness made us continue to extract gas with the pump from the bulb C containing the frozen emanation, surrounded by liquid air, for too long a time. Every two or three strokes of the pump collected a minute bubble, occupying about the tenth of a millimetre in length of the very narrow fall-tube of the pump, which was really a fine-bore capillary. The yield of gas appeared to be continuous; and when these bubbles were examined in the dark they were brilliantly luminous. This gas was really the emanation, which possesses a feeble vapour pressure even at the temperature of liquid air. Needless to say, on attempting to examine the spectrum, little was seen, for the pressure of gas in the vacuum-tube was too low.

The tube was therefore washed out with the gas which had been pumped off, and the process was repeated. The minute bubbles which passed down the capillary fall-tube of the pump were examined, and pumping was stopped when they showed a very faint luminosity in the dark. On compressing the emanation into the spectrum-tube, the spectrum was again brilliant, and measurements were made. It was found possible to read the lines several times, for although the spectrum faded in less than a minute, it appeared to recover on ceasing to pass the current. But this recovery soon failed and, as before, nothing could be detected after 5 minutes but the primary and secondary spectra of hydrogen. Now the tube was practically vacuous before warming the bulb containing the emanation; no current would pass; but it is, of course, possible that the gas carrying the emanation had not been perfectly dried in passing through the tube B, containing phosphoric anhydride; any water-vapour would have condensed in the cooled bulb C, and would only slowly have vaporised into the vacuum-tube. On arriving there, it would give the hydrogen spectrum. Another possibility is that it may have come out of the electrodes; for it has been frequently noticed in glowing out a vacuum-tube with aluminium electrodes that even after all trace of hydrogen has been removed by passing the discharge so as to heat the electrodes, and by pumping, the hydrogen spectrum has reappeared on admitting a trace of one of the gases of the argon group, and passing the discharge for a longer time; but the intensity of the spectrum which replaced that of the emanation may perhaps warrant the supposition that hydrogen as well as helium is one of the products of the disintegration of the emanation. This, however, is very doubtful, and judgment must be suspended until more satisfactory evidence is forthcoming.

The lines read were:—

Wave-length.	Remarks.
6350	Not observed before; faint.
5975	Observed before; faint.
5955	” ”
5890	Not observed before; faint.
5854	” ” ”
5725	Observed before; fairly strong.
5686	Not observed before; faint.
5595	Observed before; strong and persistent.
5580	Not observed before; faint.
5430	” ” ”
5393	” ” ”
5105	Bright; persistent; observed before.
4985	” ” ” ”
4966	Not observed before; bright, but transitory.
4640	Transitory; possibly 4650 and 4630, which were seen before as distinct lines.

The line 4966 was particularly brilliant at first; but it soon assumed secondary importance. Some lines which had previously been observed were not seen; they are 6307, 5805, 5137, and 4690. An attempt was made to obtain the spectrum with a jar and spark-gap; but only hydrogen and mercury were to be seen. The resistance soon became very high, and there was danger of piercing the vacuum-tube.

Previous attempts in conjunction with Mr. Soddy gave lines with wave-length 5725 (jar), 5595 (no jar), 5105 (no jar), 4985 (no jar); the line 5585 was observed three times, and 5105 twice previously. The lines 6145 and 5675 mentioned in our last paper (April, 1904) were not seen, unless the latter is identical with 5580. It may perhaps be mentioned that the line 5595 was seen by Pickering in the spectrum of lightning, and was not identified with a line in the spectrum of any known gas; it is said to have been a very strong line, of intensity 30.*

There can be no doubt that the lines given are the chief lines in the visible spectrum of the emanation; as for the pressure, the volume of emanation was about 1/30,000th of a cubic centimetre, and the capacity of the vacuum-tube, say, 1/20th; this would make the pressure about 1/10th of a millimetre. It may have been twice as much, for the numbers given are merely estimates.

It may be remembered that, at the Chemical Congress held in Paris

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* 'Astrophysical Journal,' 1901, vol. 14, p. 368.

in 1900, it was suggested that no element should receive a name until its spectrum had been mapped. Of course, the converse does not follow, that, after the spectrum of an element has been mapped, it should receive a name. The "emanation from radium," however, is a cumbrous expression, and sufficient evidence has now been accumulated that it is an element, accepting that word in the usual sense. It is true that it is only a transient element, and ought in justice to be called a compound; but of what? It stands on a wholly different plane to any known compound in the amount of heat with which it parts during its spontaneous change, and in the peculiar electrical phenomena which accompany its transformation. It is a gas; it follows Boyle's law; as Rutherford and Soddy have shown, it resembles the gases of the argon series in its indifference to chemical reagents, for it not merely withstands the prolonged action of magnesium-lime at a red heat, but also, as Ramsay and Soddy have proved, prolonged sparking with oxygen in presence of caustic potash. Its molecular weight has been found to be nearly 200, and, if it is monatomic, that number would also express its approximate atomic weight. Now, it appears advisable to devise a name which should recall its source, and, at the same time, by its termination, express the radical difference which undoubtedly exists between it and other elements. As it is derived from radium, why not name it simply "exradio"? Should it be found that the emanation, which is supposed to be evolved from thorium, is really due to that element, and not to some other element mixed with thorium in exceedingly small amount, a similar name could be given, namely, "exthorio." If the existence of actinium as a definite element is established, its emanation would appropriately be named "exactinio." It is unlikely that others will be discovered, but, if they are, the same principle of nomenclature might be applied.

It should be stated, in conclusion, that Mr. Soddy collaborated in the experiments preliminary to this successful mapping of the spectrum; had he not been obliged to leave England, he would, no doubt, have shared whatever credit may attach to this work.

"Notes on the Statolith Theory of Geotropism. I. Experiments on the Effects of Centrifugal Force. II. The Behaviour of Tertiary Roots." By Francis Darwin, F.R.S., and D. F. M. Pertz. Received May 30,—Read June 9, 1904.

I.

According to the statolith theory,* there are in plants certain cells specialised to act as organs of orientation in space; organs, in fact, functioning like the otocysts of certain animals. In both cases the sense of verticality is believed to be the result of the pressure of certain heavy bodies (usually starch grains in the case of plants), on a sensitive surface, namely, the lining membrane of the otocyst in the case of animals, or in the case of plants the protoplasm lining the statolith-containing cells (statocytes).

In a paper† dealing with the arguments for and against the theory, Jost brings forward as the most serious objection the behaviour of plants to centrifugal force. He found that plants, subjected to centrifugal force equal to from 0·02—0·05 g.,‡ exhibited curvature, but that the starch-grains were uniformly distributed throughout the statocytes, not, as should be the case according to our theory, resting on the cell walls furthest removed from the axis of rotation. Jost sees in these results an absolute proof that, in the cases investigated by him, the starch grains do not function as statoliths.

It seemed to us that this conclusion was a somewhat hasty one and we determined no longer to delay the investigation of Knight's experiment in relation to starch grains, which we had previously recognised as a necessary part of the statolith question. Our experiments were carried out on seedlings of *Setaria* and *Sorghum*, in which the statoliths are in the cells of the cotyledon (coleoptile), and in which the position of the movable starch can easily and rapidly be determined by splitting the cotyledon longitudinally and examining the two halves mounted in iodine solution. The experiments were directed to two points, viz., the centrifugal force needed to produce (*a*) geotropic curvature, (*b*) movement of starch-grains. It will be seen that our results are directly opposed to those of Jost, inasmuch as, according to us, the lowest effective centrifugal force is about the same in the two sets of experiments. The centrifugal apparatus was driven by a hot-air engine regulated by one of Griffiths's gas-regulators

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* Noll, 'Heterogene Induction,' Leipzig, 1892; Haberlandt and Němec, 'Ber. Deutschen Bot. Gesellsch.,' 1900.

† "Die Perception des Schwerereizes in der Pflanze," 'Biolog. Centralblatt,' vol. 22, March 1902, p. 161.

‡ g. being the acceleration of gravity.

made by the Scientific Instrument Co. In the experiments on curvature the seedlings were cut and fixed by melted cocoa-butter to cork supports in small metal boxes, in which the air was kept damp. They were either placed tangentially at right angles to the axis of rotation, or else parallel to the axis. In the experiments on the distribution of the starch the seedlings were cut and placed in grooves in a sheet of cork, being kept in place by damp filter-paper and a second sheet of cork firmly fixed. In the majority of experiments the tip of the seedling was also fixed to a little bar of wax. The seedlings were fixed radially, the apices of some being inward and of others outward. This arrangement gives a striking result in successful experiments, for the starch travels to the apical end of the cells in the specimens whose apices point outwards, while it remains basal in the others.

(*a*) *Experiments on Curvature.*

We give our results in the form of a summary instead of publishing the details of each experiment. It will be seen that there was a good deal of irregular nutation; this is a drawback to the use of *Sorghum* and *Setaria*, but these plants being otherwise convenient we continued to employ them.

Adding together the results obtained with centrifugal forces of 0·02 g., 0·03 g. and 0·04 g. we find that

85 seedlings (68 per cent.) curved to the centre (apogeotropically).
18 ,, (14·4 per cent.) did not curve at all.
22 ,, (17·6 per cent.) curved away from the centre (prosgeotropically).

We conclude from these results that seedlings of *Sorghum* and *Setaria* are to some extent stimulated geotropically by a centrifugal force of from 0·02—0·04 g. The average amount of apogeotropic curvature to the centre is only 20°, and as this is the result of about 22 hours stimulation, we are justified in believing that under our conditions a definite geotropic curvature cannot with any certainty be produced with centrifugal forces of much less value than 0·02 g. The fact that observers working with different plants and by other methods have found curvature with considerably weaker centrifugal force, does not concern us, since our investigation is a comparative one.

(*b*) *The Behaviour of the Statoliths to Centrifugal Force. Horizontal Axis.*

In the first series the seedlings were all placed radially with the apex outwards for 22—24 hours. The behaviour of the starch is not uniform in the cells of the cotyledons, so that it is only possible to give a general impression of the results.

Thus "scattered and apical" means that the starch is to a great extent diffused through the cells, but that in a good many cases the starch has accumulated at the apical ends of the cells. On the other hand "apical and scattered" means that the starch is apical rather than scattered.

Table I.

Plant.	Centrifugal force.	Temperature.	Position of starch.
	g.	° C.	
Setaria.....	0·02	14	Scattered and apical.
,, ...	0·02	16	Scattered, apical, and some basal.
,, ...	0·02	17	Scattered.
,, ...	0·02	17	Scattered, some apical, also some basal.
,, ...	0·02	20	Apical and scattered.

The general conclusion to be drawn is that with 0·02 g. there is a tendency, though a very slight one, for the starch to move radially and accumulate in the apices of the cells.

In Table II, the seedlings were placed both apex outwards and apex inwards; the period of exposure was about 22 hours.

Table II.

Plant.	Centrifugal force.	Temp.	Apex.	Starch.
	g.	° C.		
Setaria.......	0·03	22·0	Out	Two specimens, basal and scattered; one specimen, many cells apical.
			In	Scattered and basal.
,, ...	0·03	18·5	Out	Scattered and apical.
			In	Scattered and basal.
,, ...	0·04	13—18	Out	No distinct difference.
			In	
Sorghum and Setaria	0·04 (about)	16—20	Out	More apical than basal.
			In	Scattered and basal.
Setaria.......	0·04	18—21	Out	Apical and scattered.
			In	Basal and scattered.
,, ...	0·05	21·0	Out	Scattered, many cells apical, but basal in parts.
			In	Much scattered. (Not very distinct difference between "out" and "in.")

Thus out of six experiments there was only one in which there was no difference between the starch in the seedlings pointing outwards and inwards. In one (0·05 g.) the difference was not distinct, while in the other four experiments the difference was distinct. This proves that the starch moves under the influence of a centrifugal force of from 0·03—0·04 g. From the prevalence in our notes of such phrases as "scattered and apical," "scattered and basal," it is evident that with 0·03 and 0·04 g. the starch does not move easily, and this is precisely what we should have expected from the small amount of curvature produced with centrifugal forces of about this magnitude.

Centrifugal Machine with Vertical Axis.—We made some use of a centrifugal apparatus with a vertical axis, in the belief that the results would be more easily observed than in the case of a horizontal axis. Supposing that the seedlings are fixed radially on the horizontal disc of the machine, the starch grains will, if the wheel is at rest, be "lateral," *i.e.*, will lie on the longitudinal cell walls which are now horizontal. When the wheel is set in motion there will be no tendency to general diffusion of the statoliths, which will remain "lateral" but will tend to accumulate at the outward extremities of the cells; the starch will be what we call "apically cornered" or "basally cornered," according as the apices or bases of the seedlings are outwards.

With regard to curvature, if the seedlings are placed horizontally and tangentially, they will of course curve upwards in response to the force of gravity, and at the same time inwards in response to centrifugal force. In a similar way, if the seedlings are placed vertically they will not grow straight upwards as they would if gravity alone was in question, but again slightly towards the centre. In those experiments in which the centrifugal force varied between 0·02 and 0·04 g., only 48 per cent. of the seedlings curved to the centre, and the average curvature was only 10°. This, perhaps, was to be expected when the centrifugal force was no more than from 2—4 per cent. of gravity.

The effect on the starch grains is shown in Table III.

Table III.

Plant.	Centrifugal force.	Temp.	Apex of seedling.	Position of starch.
	g.	° C.		
Setaria.......	0·02	18	Out	Scattered, apical-cornered, and some basal-cornered.
Sorghum.....	0·02	19	,,	Lateral and apical-cornered; some basal-cornered.
Setaria.......	0·03	16—18	,,	Lateral; some basal and apical-cornered.

On the whole, with 0·02 and 0·03 g. the movement is irregular and slight. But the existence of a considerable number of apically-cornered cells can, we think, only be accounted for as the result of centrifugal force. Without centrifugal force the starch should have been lateral or basally cornered.

In the remaining experiments (Table IV) two sets of seedlings were used one with the apices radially outwards, the other radially inward. Here the result was more decided.

Table IV.

Plant.	Centrifugal force.	Temp.	Apex of seedling.	Position of starch.
	g.	° C.		
Setaria and Sorghum	0·03	18	Out In	Scattered and lateral; a few apical in both.
Setaria......	0·03	16	Out	Apically cornered and scattered.
			In	Basal.
,, ...	0·04	20	Out	Apical and lateral.
			In	Basal.
,, ...	0·06	20	Out	Apical, lateral; some basal.
			In	Basal, scattered.
Sorghum	0·07	17·5—20	Out	Scattered, apical.
			In	Basal.

It is clear from these results that a force of 0·03—0·04 g. has a distinct effect on the position of the starch grains. The effect of 0·06 and 0·07 g. was not materially greater than that of 0·04, and we are inclined to think that this is also true in the experiments with a horizontal axis of rotation.

Inclined Plane.—We made a few experiments, in which seedlings were fixed on plane surfaces inclined at angles varying from 2—5°. If the plane be inclined at an angle θ, the component of gravity acting in the line of the plane will be $\sin\theta$ g. Thus, if the seedlings be placed apex downwards, on a plane inclined at 2°, there will be a force equal to 0·035 g., tending to move the starch grains to the apices of the cells. Our experiments were not very numerous, but, as far as they go, they confirm the above results. We find that the statoliths are displaced by 0·035 g., but not by 0·017 g.

The Behaviour of the Statoliths on the Klinostat.—When a plant is kept slowly rotating on a horizontal axis, the practical result is that it does not bend geotropically. The result has been explained by the supposition that the plant never remains in one position long enough for the perception of gravity. But there are experiments which show that (at any rate, in certain cases) the gravitational stimulus is continuously perceived, but fails to produce a curvature, because of its symmetrical distribution.* Our experiments tend to confirm this point of view, since they show that the statoliths are not evenly dispersed through the cells,† but that they take up more or less definite positions in different parts of the rotation. The experimental plants were, as before, seedlings of *Setaria* and *Sorghum*, cemented into boxes, or fixed between cork and damp blotting-paper, and so arranged that the axis of the plants were at right angles to the axis of rotation. The klinostat, which made one revolution in 17, 20, or 30 minutes, was placed in a dark room, where the temperature varied from 17 to 24° C. The number of seedlings used in each experiment was four, six, or eight, and, in all cases, half the number pointed in one direction, and the others in the opposite direction. Thus, at the moment at which the seedlings are vertical, half of them are "apex upward," the rest being apex downwards. The method was to leave them rotating on the klinostat, and to remove them the moment they reach the vertical, when the starch in the two lots (up and down) was rapidly compared. With a rotation of once in 30 minutes, the apex-upward seedlings will have been for $7\frac{1}{2}$ minutes passing from the horizontal to the vertical. With the 17 and 20 minute klinostats, the periods will be respectively $4\frac{1}{4}$ and 5 minutes. It will be seen in Table V that, in the first three experiments in which the duration of the rotation was short, no definite result was obtained; and the same absence of difference in the starch was noted in two other experiments in which the plants were examined, after two rotations of 17 minutes. In the remaining experiments a distinct result was obtained.

In a few experiments given in Table VI, the klinostat was stopped when the seedlings were horizontal. In this case half the seedlings ("apex last down") had for $4\frac{1}{4}$ minutes been changing from apex vertically down to horizontal, while the other lot ("apex last up") had changed from apex up to horizontal. These experiments, taken with those in Table V, show that a complete reversal of the starch from the apical to the basal ends of the cells may take place on the klinostat in $8\frac{1}{2}$ minutes.

Table VI also gives the results of three experiments, in which the seedlings were parallel to the axis of rotation, in two of which the starch was, as was expected, on the longitudinal cell walls ("lateral").

On the whole, the results of Tables V and VI show that there is no inconsistency between the statolith theory and the view above referred to, that continuous gravitational stimulation occurs on the klinostat.

The question whether $8\frac{1}{2}$ minutes, the period during which gravi-

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* See Elfving 'Öfversigt af Finska Vetenskaps Förhandlingar,' 1884, and F. Darwin on "Cucurbitous Seedlings," 'Practical Physiology of Plants,' 1st edition, 1894, p. 176.

† As Jost, *loc. cit.*, p. 176, seems to believe to be the case.

Table V.—Change and Position of Starch produced by Rotation on a Klinostat.

Period of rotation of klinostat...... Expt. 1—6, 30 mins.
" " " " 7, 20 "
" " " " 8—10, 17 "

No.	Date.		Plant.	Position when stopped.	Time on klinostat.	Temp.	Seedlings, apex up, position of starch.	Seedlings apex down, position of starch.
	1901.				h. m.			
1	December	12	Sorghum	Vert. up and down	1 20	17°	Scattered	Scattered.
2	"	16	"	"	0 45	17	Basal	Basal; scattered.
	1902.							
3	January	9	Setaria	"	1 50	..	Scattered	Scattered; perhaps more apical.
4	"	10	"	"	2 20	20	Scattered; some cells basal	Scattered; some cells apical.
5	"	13	Sorghum	"	1 5	21	Scattered; many basal	Scattered; many apical.
6	"	14	"	"	19 15	..	On the whole basal	Many cells apical and apically cornered.
7	"	18	"	"	2 0	..	Basal and basally cornered; scattered	Apically cornered and scattered.
8	November	18	Setaria	"	21 41	17	Basal, basal-cornered; scattered	Scattered and apical and apically cornered.
9	"	19	"	"	21 20	20	Scattered; basal and lateral	Scattered; many apical and lateral.
10	"	20	"	"	22 11	16	Scattered; basal and lateral	Scattered; some apical.

Table VI.—Klinostat Experiments.

One rotation in 17 minutes.

Nos. 11—15, seedlings at right angles to axis of rotation, examined when horizontal.
„ 16—18, „ parallel to axis of rotation.

No.	Date.	Plant.	Position when stopped.	Time on klinostat.	Temp.	Starch, apex last up.	Starch, apex last down.
	1902.			h. m.			
11	November 21	Setaria	Horizontal	23 22	17°·5	Basal and scattered	Scattered, basal and apical.
12	„ 24	„	„	22 45	22·0	Lateral and basal	Lateral ; apical and basally cornered.
14	„ 25	„	„	22 39	22·0	?	Scattered and apical.
15	„ 26	„	„	22 55	24·0	Basal and basally cornered and scattered	Apically cornered ; apical and scattered.
Seedlings parallel to axis of Klinostat.							
16	November 28	Setaria	..	24 10	20·0	Scattered and lateral	—
17	December 1	„	..	24 20	18·5	Scattered	—
18	„ 2	„	..	22 25	..	Scattered and lateral	—

tational stimulus remains unchanged in direction (though not in intensity) is long enough to be effective in producing curvature, does not concern us, since it is a question which is applicable to all theories of gravitational sensibility.

II.

On the Presence of Statoliths in Tertiary Roots.

Early in our work we noticed the existence of movable starch in the tertiary roots of *Vicia faba*, and since tertiary roots have been hitherto believed to be devoid of gravitational sensitiveness, we saw that here was a question for investigation. In Jost's above quoted paper (p. 173) this difficulty is referred to in the following words:—

"Further anatomical investigation is also needed in the case of roots. My own work has shown us that lateral roots of secondary or tertiary rank, which are not gravitationally sensitive, have just the same movable starch as primary roots."

We do not know to what secondary roots Jost refers. In the case of *Vicia faba* there is no difficulty in finding a function for the statoliths of the secondary roots, since these are well known to react to gravity. In regard to the tertiary roots the difficulty is a real one.

It is well known that, if the primary root is cut off, one or more of the secondary roots ceases to grow diageotropically, and turns vertically downward. It occurred to us that the tertiary roots springing from such a secondary might acquire gravitational sensitiveness, and behave like normal secondaries. This proves to be the case, and in this fact undoubtedly lies the explanation of the presence of statoliths in tertiary roots.

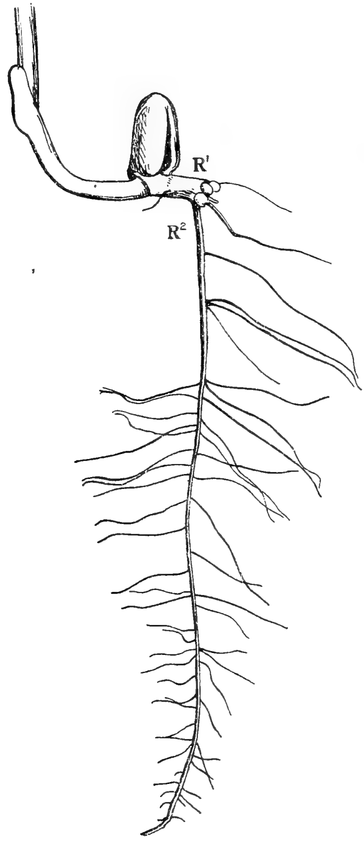
Our method of procedure is to wait until the secondary roots of a bean (*Vicia faba*) have begun to show themselves, when the primary root is cut near its base, and the secondary roots, with the exception of one, are at the same time removed. The seedling is then planted with the stump of the primary root roughly horizontal, and the single secondary root pointing vertically downwards, as shown in fig. 1. It will be seen that the secondary root R^2 grows vertically for the greater part of its length, and that the general direction of growth of the tertiary roots is strikingly like that of normal secondaries; and this preparation alone would strongly suggest, to anyone accustomed to the subject, that under these conditions the tertiaries were regulated by gravitation.

We grow our bean roots (in a dark room) in Sachs's troughs, having oblique glass sides, so that the secondary root, in its attempt to grow vertically, is forced to follow the glass wall. This insures that the secondary root, and a fair proportion of tertiaries, shall be visible, and thus their course can be recorded by painting lines on the glass.

Care is necessary, at the beginning of the experiment, to prevent more than one secondary root developing; the primary root should at first be uncovered every day, and any young secondaries pinched off.

The preliminary arrangement takes some time; thus, for instance, a primary root was amputated February 6, and the tertiary roots were not ready for observation until February 16.

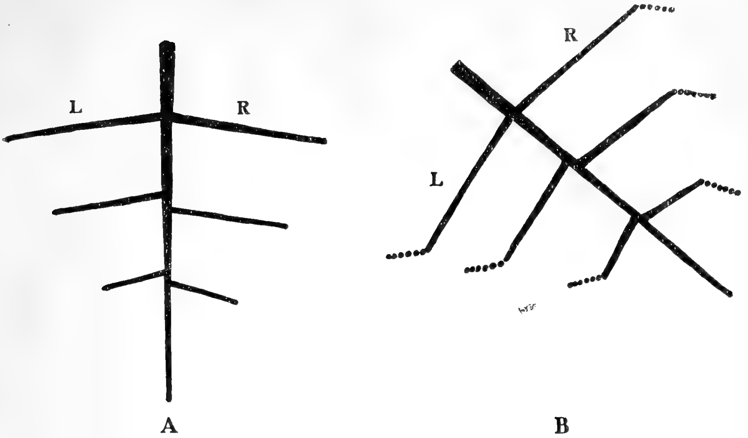
Fig. 1.



The method was that of Sachs, which may be illustrated by the following diagram (p. 487).

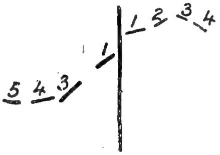
Fig. 2, A shows a primary root growing vertically, and giving off, to the left and right, two sets of secondaries, L and R, which have assumed their usual lines of growth somewhat below the horizontal. The trough is now raised at one end, as in fig. B; the roots L and R are affected in opposite ways by this change, the L roots are now 45° below, while the R's are 45° above their normal line of growth, the consequence is that the L's bend up and the R's down, as shown by the

FIG. 2.



dotted lines in fig. B. Fig. 3 shows the similar behaviour of tertiary roots.

FIG. 3.



The line representing the secondary root is vertical and unbroken, the curved lines springing from it on the left and right are tertiaries, and are made up of separate sections, each of which represent the growth on one day. The marks 1 and 1 were painted on May 12, 1903. They show that the R and L roots grew out at very different angles with the secondary root. This is accounted for by the fact that while sections 1 and 1 were developing the left end of the trough was raised (25°). If the figure is turned to this position it will be seen that 1 and 1 make roughly equal angles with the horizon.

After tracing sections 1 and 1 the trough was raised so as to make 40° with the horizon, the left end being still the higher.

On May 13, section 2 was painted on the R side, section 2 on the L side could not be clearly seen. The trough was now placed at an angle of 32°, being, however, reversed, so that the R end was higher.

May 14, sections 3 painted on both sides. It will be seen that root R has begun to assume its proper angle below the horizon, whereas L has continued to grow nearly in line with section 1. On the following days (sections 4 and 5) L bent upwards. This is a striking instance of what we have observed in other cases, viz., that tertiary roots bend downwards more readily than upwards. It is interesting because it is a point of resemblance between them and diageotropic secondary roots. The slowness of the growth of the tertiaries in this experiment was probably due to the development of more than one secondary root.

Fig. 4.

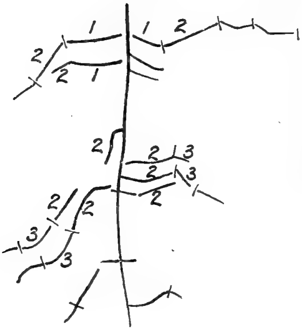


Fig. 4 shows the same state of things, namely, that the direction of growth of the sections marked 2 is different on the two sides. Thus section 2 is directed obliquely upwards in the right-hand tertiary roots, and downwards on the left. A similar contrast exists between the sections marked 3. These changes of direction are due, as in fig. 3, to changes in the position of the trough.

In the later experiments, of which fig. 5 is an example, the tracings were made each day in a different coloured paint, so that a complete record of all tertiaries was obtained. This was not the case in fig. 4, so that some of the sections are necessarily left without numbers.

In fig. 5, the numbers 1, 2, 3, etc., give the tracings made on successive days. It will be seen that in the younger part of the root, near the lower end of the figure, the sections of the tertiary roots do not begin with the numerals 1 or 2, but with 5, 6, or 7; this is because when sections 1 and 2 were traced, the lower (younger) tertiaries had not yet appeared.

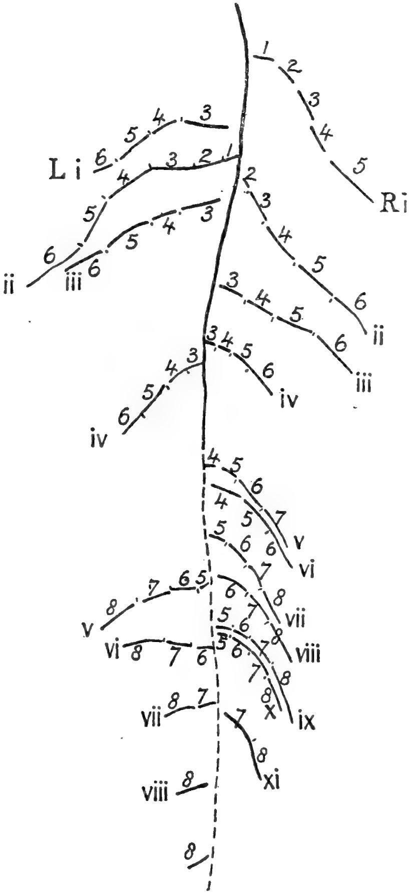
After section 1 was traced, the trough was tipped up at an angle of 39° (right side highest), and remained in that position while 2 and 3 developed. This accounts for the bend downwards in the oldest root on the right (R i), and the slight upward bend in L ii (section 3).

The trough was now reversed, being placed at 19° (left side highest) and being allowed to remain in that position while sections 4 and 5 developed. It will be seen that the curvature of the sections 3, 4, 5 is upward on the right (except in root R iv) and downwards on the left.

The trough was again reversed after a tracing of 5 had been made, the right side being now higher, and the angle about 30°. Here again the roots on the right bend down and on the left up, as will be seen by comparing sections 5 and 6.

The roots, therefore, show clearly a power of curving up or down so as to assume their proper angle with the horizontal just as normal secondary roots do in similar circumstances. The only obvious exceptions to the rule are in roots R iv, v, vi, where a downward bend occurs between sections 3 and 5 or 4 and 5, as the case may be; here the curve should either have been absent or in the upward direction.

Fig. 5.



After section 6 was traced the trough was exposed to light and sections 7 and 8 were traced. It will be seen by comparing 6 and 8, that in both sets of tertiaries (right and left) there is a tendency to curve downward—and this is well known to occur in normal secondary roots exposed to light, and is another instance of the assumption by the tertiaries of characters hitherto associated only with secondary roots.

The facts above given prove that when the primary root is removed and a secondary root assumes its place, the tertiary roots take on the character of normal secondaries. It may be believed, therefore, that the existence of statoliths in normal tertiary roots is a provision enabling them to assume diageotropic growth in case of injury to the primary root. This, though appearing a bold conclusion, does not involve an adaptive action different in principle from the well-known assumption by secondary roots of the characters of the primary root.

We desire to express our thanks to Mr. T. Elborn, Assistant in the Cambridge Botanical Laboratory, for the help he has given us during the course of our research.

"On the Electric Effect of Rotating a Dielectric in a Magnetic Field." By HAROLD A. WILSON, M.A., D.Sc., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 18,—Read June 2, 1904.

(Abstract.)

It was shown by Faraday in 1831 that an electromotive force is induced in a conductor when it moves in a magnetic field so as to cut the lines of force. The object of the experiments described in this paper was to see if a similar electromotive force is induced in a dielectric when it moves in a magnetic field.

According to Maxwell's electromagnetic theory, as developed by H. A. Lorentz and Larmor, such an electromotive force should be induced in a dielectric, and should be equal to that in a conductor multiplied by the factor $1 - K^{-1}$, where K is the specific inductive capacity of the dielectric.

The method employed was to rotate a hollow cylinder of ebonite in a magnetic field parallel to the axis of the cylinder. The inside and outside surfaces of the cylinder were provided with metal coatings with which electrical contact was made by sliding brushes. The inside coating was connected to earth, and the outside coating to one pair of the quadrants of a sensitive quadrant electrometer, the other pair of quadrants being connected to earth. The magnetic field was then reversed, so reversing the induced electromotive force in the ebonite. The resulting electric displacement was measured by means of the

electrometer; the quantity of electricity required to produce a given deflection of the electrometer needle being determined by means of a small parallel plate guard ring condenser.

It is shown that if E is the induced E.M.F. in the ebonite, and V the potential difference indicated by the electrometer, then

$$2\mathrm{E} = \mathrm{V}\frac{c+c'}{c}$$

where c is the capacity of the ebonite cylinder and c' the capacity of the electrometer. If the electrometer gives a deflection D due to V, and a deflection d due to a known quantity of electricity q, then $2\mathrm{E}c = \mathrm{V}(c+c') = \mathrm{D}q/d$. Consequently E can be determined in terms of c, D, d and q.

If the outside radius of the ebonite cylinder is r_2, and the inside radius r_1, then according to the electromagnetic theory $\mathrm{E} = n\pi\mathrm{H}\ (r_2^2 - r_1^2)(1 - \mathrm{K}^{-1})$ where H is the strength of the magnetic field and n the number of revolutions per second.

The cylinder used was 10 cm. long, and $2r_2 = 4{\cdot}15$ cm., $2r_1 = 2{\cdot}01$ cm. It was mounted in a solenoid having 95 turns per cm., by which a magnetic field of strength 1500 could be produced. The cylinder was driven by a $\frac{1}{2}$ horse-power motor, and could be run at 200 revolutions per second. The following table contains the results obtained with various rates of revolution and magnetic fields.

Revolutions per second.	Magnetic field reversed.	Deflection of Electrometer due to one E.S. unit. (Millimetres.)	Deflection. (Millimetres observed.)	Deflection. (Calculated).
192	1480	6015	26·2	26·0
192	750	6015	13·6	13·2
192	380	6015	6·8	6·7
192	320	6015	5·3	5·6
183	1420	4780	18·0	18·9
182	710	4780	9·2	9·4
182	1160	6040	19·9	19 4
100	1400	6320	14·0	13·5
100	1100	6320	10·5	10·6
100	670	6320	7·0	6·4
100	540	6320	5·0	5·2
93	1100	6100	9·5	9·5
92	1460	4770	10·0	9·7
92	1260	4770	8·0	8·4
92	650	4770	4·5	4·3
49·2	1200	6320	6·0	5·7
49·2	600	6320	3·0	2·8

The mean result obtained for the quantity of electricity set free on the outside coating of the cylinder, on reversing the magnetic field, only differs from the amount calculated theoretically by 1 per cent. The specific inductive capacity of the ebonite, as determined by measuring the capacity of the cylinder, was 3·54, while the value calculated from the results obtained, using the formula $E = n\pi H (r_2^2 - r_1^2)(1 - K^{-1})$, was 3·64. As the result of these experiments, it may be concluded that—

1. A radial electric displacement is produced in a dielectric, such as ebonite, when it is rotated in a magnetic field parallel to the axis of revolution.
2. The direction of the displacement is the same as is produced in a conductor.
3. The displacement is proportional to the magnetic field, and to the rate of revolution.
4. The amount of the displacement agrees with that calculated on the assumption that the induced E.M.F. in the dielectric is equal to that in a conductor multiplied by $1 - K^{-1}$.

The results obtained are thus in complete agreement with the theories of Lorentz and Larmor, and may be regarded as a confirmation of these theories.

The experiments were done in the Cavendish Laboratory, and the cost of the special apparatus used has been defrayed by a grant from the Royal Society.

BAKERIAN LECTURE.—"The Succession of Changes in Radio-active Bodies." By E. RUTHERFORD, F.R.S., Macdonald Professor of Physics, McGill University, Montreal. Lecture delivered May 19, 1904.

(Abstract, Received May 27, 1904.)

It has been shown by Rutherford and Soddy that the radio-activity of the radio-elements is always accompanied by the production of a series of new substances, possessing some distinctive physical and chemical properties. These new substances are not produced simultaneously, but arise in consequence of a succession of changes originating in the radio-elements. The radio-activity of these products is not permanent, but diminishes, in most cases according to an exponential law, with the time. Each product has a distinctive rate of decay of activity, which has not, so far, been altered by any physical or chemical agency. The law of decay has been explained on the supposition that the product undergoes change according to the same law as a mono-molecular change in chemistry. The change occurs in consequence of the expulsion of an α or β particle, or both, and the activity of a product is thus a measure of its rate of change. While the products like the emanations, and UrX, lose their activity according to an exponential law, the matter emanation X, which gives rise to the phenomena of excited activity, does not lose its activity according to a simple law. The experiments of Miss Brooks and the author, and of Curie and Danne, have shown that the decay of the excited activity of radium is very complicated, and depends upon the time of exposure to the exciting cause, viz., the emanation. The author has shown that the excited activity produced in a body by a short exposure in the presence of the thorium emanation increases at first for a few hours, passes through a maximum value, and then decays with the time according to an exponential law.

In the paper the curves of decay of excited activity of radium and thorium are given for both short and long exposures to the emanations, and it is shown that the law of change of activity with time can be completely explained on the theory that emanation X of thorium and radium is complex and undergoes a series of successive changes.

The mathematical theory of successive changes is given in detail, and a comparison is made of the theoretical and experimental curves obtained for the variation with time of the excited activity. In the case of thorium, two changes are found to occur in emanation X. The first change is a "rayless" one, *i.e.*, the transformation is not accompanied by the appearance of α, β, or γ rays. The second change gives rise to all three kinds of rays.

The decay of activity of emanation X of radium depends greatly on whether the α or β rays are used as a means of measurements. The curves obtained by the β rays are always identical with those obtained by the γ rays, showing that the β and γ rays always occur together and in the same proportion. The complicated decay curves obtained for the different types of rays, and for different times of exposure, can be completely explained on the supposition that there are three rapid successive changes in the matter deposited by the emanation, viz.:—

(1) A rapid change, giving rise only to α rays, in which half the matter is transformed in about 3 minutes.

(2) A "rayless" change in which half the matter is transformed in 21 minutes.

(3) A change giving rise to α, β and γ rays together, in which half the matter is transformed in 28 minutes.*

A similar rayless change is shown to occur in the "emanating substance" of Giesel.

The occurrence of a rayless change in the three radio-active bodies is of considerable interest. Since the change is not accompanied by rays, it can only be detected by its effect in the change or changes which follow. The matter of the rayless change is transformed according to the same law as the other changes. The rayless change may be supposed to consist either of a rearrangement of the components of the atom or a disintegration of the atom, in which the products of the disintegration are not set in sufficiently rapid motion to ionise the gas or to affect a photographic plate. The significance of the rayless changes is discussed, and the possibility is pointed out that similar rayless changes may occur in ordinary matter; for the changes taking place in the radio-active bodies would probably not have been detected if a part of the atom had not been expelled with great velocity.

The radiations from the different active products have been examined and it is shown that the β and γ rays appear only in the last rapid change of each of the radio-elements. The other changes are accompanied by the emission of α particles alone.

Evidence is given that the last rapid change in uranium, radium, and thorium, which gives rise to β and γ rays, is far more violent and explosive in character than the preceding changes. There is some evidence for supposing that, in addition to the expelled α and β particles, more than one substance is produced as a result of the disintegration.

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* A statement of the nature of the three changes occurring in emanation X of radium was first given in a paper by Rutherford and Barnes ('Phil. Mag.,' Feb., 1904). A brief account of the theory from which the results were deduced has been given in my book "Radioactivity" (Cambridge University Press). Later, Curie and Danne ('Comptes Rendus,' March 14, 1904) arrived, in a similar way, at the same conclusions.

After the three rapid changes have taken place in emanation X of radium, there remains another product, which loses its activity extremely slowly. Mme Curie showed that a body, which had been exposed for some time in the presence of the radium emanation, always manifested a residual activity which did not appreciably diminish in the course of 6 months. A similar result has been obtained by Giesel. Some experiments are described, in which the matter of slow decay, deposited on the walls of a glass tube containing the emanation, was dissolved in acid. The active matter was found to emit both α and β rays, and the latter were present in unusually large proportion. The activity measured by the β rays diminished in the course of 3 months, while the activity measured by the α rays was unaltered. The active matter was complex, for a part which gave out only α rays was removed by placing a bismuth plate in the solution. The radio-active matter deposited on the bismuth is closely allied in chemical and radio-active properties to the active constituent contained in the radio-tellurium of Marckwald. The evidence, as a whole, is strongly in support of the view that the active substance present in radio-tellurium is a disintegration product of the radium atom. Since the radium emanation is known to exist in the atmosphere, the active matter of slow dissipation produced from the emanation must be deposited on the surface of all bodies exposed to the open air. The radio-activity observed in ordinary materials is thus probably, in part, due to a thin surface film of radio-active matter deposited from the atmosphere.

A review is given of methods of calculation of the magnitude of the changes occurring in the radio-elements. It is shown that the amount of energy liberated in each radio-active change, which is accompanied by the emission of α particles, is about 100,000 times as great as the energy liberated by the union of hydrogen and oxygen to form an equal weight of water. This energy is, for the most part, carried off in the form of kinetic energy by the α particles.

A description is given of some experiments to see if the α rays carried a positive charge of electricity, with the view of experimentally determining the number of α particles projected from one gramme of radium per second. Not the slightest evidence was obtained that the α rays carried a charge at all, although it should readily have been detected. Since there is no doubt that the α rays are deflected in magnetic and electric fields as if they carried a positive charge, it seems probable that the α particles must in some way gain a positive charge after their expulsion from the atom.

Since on the disintegration theory, the average life of a given quantity of radium cannot be more than a few thousand years, it is necessary to suppose that radium is being continuously produced in the earth. The simplest hypothesis to make is that radium is a disintegration product of the slowly changing elements uranium, thorium, or actinium present in pitchblende. It was arranged that Mr. Soddy should examine whether radium is produced from uranium, but the results so far obtained have been negative.

I have taken solutions of thorium nitrate and the "emanating substance" of Giesel (probably identical with the actinium of Debierne) freed from radium by chemical treatment, and placed them in closed vessels. The amount of radium present is experimentally determined by drawing off the emanation at regular intervals into an electroscope. A sufficient interval of time has not yet elapsed to settle with certainty whether radium is being produced or not, but the indications so far obtained are of a promising character.

"On the Electric Equilibrium of the Sun." By SVANTE ARRHENIUS. Communicated by Sir WILLIAM HUGGINS, Pres. R.S. Received and read June 2, 1904.

In recent years many attempts have been made to apply the pressure of radiation, that is a consequence of the theories of Maxwell and Bartoli, to the explanation of cosmical phenomena. Especially the enigma of the nature of comets' tails has been elucidated from this new point of view.

In a memoir presented to the Swedish Academy of Sciences in 1899, I pointed out that several electric and magnetic phenomena, especially auroras and magnetic storms, might also be connected with the pressure of radiation. C. T. R. Wilson found that the negative ions condense vapours more easily than do positive ions. Without doubt the gases in the atmosphere of the sun are practically ionised by the ultra-violet radiation. Therefore we have to suppose, that among the little drops formed by condensation in the sun's atmosphere far more are negatively charged than are positively charged. As these drops are driven away by the pressure of radiation they charge with negative electricity the atmospheres of celestial bodies, *e.g.*, the earth, which they meet, till the charge is so great that discharges occur, and cathode rays are formed, which carry the charge back to the universe.

A calculation of the speed, with which these particles move through space, will not be without interest. Suppose first, for simplicity, that the pressure of radiation is double that of the weight of the particles in the neighbourhood of the sun. It is not difficult to calculate, that in this case the time, necessary for the particle's passage from the surface of the sun to the earth, amounts to 68·7 hours. The specific weight is supposed to be that of water.

Now, after Schwarzschild's calculations, a perfectly reflecting drop will be driven away with the greatest force from the sun if its circumference is just as great as the wave-length of the radiation. The wave-length of the maximal radiation of the sun is about 0·5 μ. Therefore the optimal dimension for a drop, that is driven away by the pressure of radiation, will be about 0·06 μ. If we suppose a specific weight of the drop like that of water, the repulsive force for a perfectly reflecting drop amounts to about ten times its weight. For a perfectly black drop it is half as great. Now, most drops are neither perfectly reflecting nor perfectly black. Most fluids absorb nearly completely the non-luminous radiation, and reflect a part of the other. An appreciation of these two factors leads to the estimation that the effect for the translucent fluids will be about half as great as for a perfectly black body, *i.e.*, about 2·5 times greater than the gravity against the sun. Such a particle will move away from the sun with 1·5 times greater speed than that calculated above, *i.e.*, it will reach the earth in about 46 hours.

Of course, there may be represented speeds that are more than the double this for drops of low specific weight (compounds of carbon and hydrogen). On the other hand, the speed may be extremely little (or negative), for drops of high specific weight (*e.g.*, gold). This will also be the case for great or very small drops, as Schwarzschild has shown.

These figures have recently acquired a great interest through the discussion by Ellis, Maunder, and Riccò of the connection between sunspots and magnetic storms. Riccò had already, in 1892, stated that in six cases of very strong magnetic storms, these appeared in mean 45·5 hours after the passage of a great sunspot over the central meridian of the sun. In one case the difference of time was only 20 hours.

From the researches of Ellis and Maunder it appears that the magnetic storms *commence* in mean 26 hours after the great groups of sunspots, which probably caused them, had passed the central meridian of the sun. Riccò applies a correction to these figures. He says that in mean the great magnetic storms, quoted by Ellis and Maunder, lasted for 33 hours, and therefore it is natural to assume that the maximum of the magnetic storm, which will probably fall near its middle, arrives 16·5 hours after its commencement. It will, therefore, be nearly true that the maximum of the magnetic storms observed by Ellis, came $26+16{\cdot}5 = 42{\cdot}5$ hours after the passage of the corresponding spot through the central meridian of the sun. The figure very nearly coincides with those of Riccò and also with that calculated above. Riccò also makes the observation that the velocity of the small particles, which in my opinion cause the auroras and the magnetic storms, is of the same order of magnitude as the observed velocity with which the cause of these perturbations moves from the sun.

If the sun only emitted negatively electrified particles on all sides, it would soon assume so great an electric charge of positive sign, that the electric forces would hold the negative particles back in the neighbourhood of the sun. There must, therefore, be some cause that carries back as much negative electricity to the sun as it loses through the emission of negative particles. In supposing the least negative charge to be the same as the positive one of a hydrogen atom, weighing 8×10^{-25} grammes, it may be calculated that the force with which this is drawn back to the sun by a potential slope of 3000 volt per cm., amounts to 23·2 dynes. A drop of radius 0·08 μ, of the specific weight of water, has the weight 59×10^{-10} dynes at the surface of the sun. Its repulsion by the pressure of radiation is about 2·5 times greater, 148×10^{-10} dynes. The electric attraction is therefore only about the fourth part of the total force by which the drop is driven away from the sun; therefore its speed is only three-fourths of that calculated before. It is evident that if the electric charge of the sun, or rather of its upper atmosphere, is much greater (about four times) than that supposed, no negatively charged particles can be emitted from the sun. On the other hand, if the sun's charge is less, the particles will move with a speed that is nearly independent of the magnitude of the charge. Probably the charge of the sun in times of great emission, *i.e.*, at sun-spot maxima, will be of this order of magnitude, and in times of sun-spot minima somewhat less.

The charged particles are driven out to all sides from the sun. It might, perhaps, be expected that they would lose their electric charge under the influence of the strong ultra-violet radiation from the sun. But the circumstances must be other for these small particles than for great pieces that are examined in our laboratories. Otherwise it would be impossible to conceive that drops are condensed at all on the negatively charged electrons under the influence of ultra-violet light.*

But if many drops agglomerate together, the potential increases and greater pieces are formed, which can lose their charge gradually. According to the experiments of Elster and Geitel, and Lenard, these charged bodies part slowly with their negative charge in the form of electrons that traverse space.

The path of these electrons is now influenced by the strongly positively charged suns. Their paths become by this influence curved, and they describe hyperbolas round the suns. If their perihelial distance is less than the sun's radius, they fall down on the sun, and diminish its positive charge.

If we now suppose the electric charge of the sun to be just as great

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* As the first drops contain only one elementary charge of electricity, they would lose their whole charge at once at a discharge. Perhaps this circumstance causes the difference for elementary and great charges.

as assumed above, we find that electrons moving with the velocity of light are caught by the sun, if the asymptote of their hyperbolic path is less distant from the sun than 2420 times the mean distance of the earth from the sun (or one twenty-fourth of a light-year). This limit distance is inversely proportional to the velocity of the electrons, and nearly proportional to the square-root of the charge of the sun. As now, according to the researches of Lenard, the electrons from a negatively electrified body possess a much less velocity than light, this distance is really much greater than that just calculated.

If, for instance, the velocity of the electrons is the thirtieth part of that of light, a number that is in good agreement with Lenard's measurements, all electrons from space which came along a path that is less distant than 1·25 light-year from the sun, will be caught by the sun. Of course the electrons move with different velocity, so that the said distance may only be regarded as a mean, or as representing the order of magnitude.

Now our nearest star (α Centauri) is distant from us by about 4 light-years, and other stars lie within less than 10 light-years. Thus it is evident that the negative electrons, which are sent off from aggregates of negatively charged drops (these aggregates are probably identical with what we call cosmic dust or meteorites), can in general not pass by many suns without being caught by them. And on the other hand the suns recover in mean from space as much negative electricity as they lose. The electric charges of the suns are in this respect very effective regulators. If the charge is quadrupled the mean distance of the caught electrons is doubled, or, in other words, as they are uniformly disseminated in space, their quantity is quadrupled. Therefore the supply of negative electricity to the suns is proportional to their defect thereof.

From these considerations we see that a very effective balance of gains and losses of negative electricity is maintained. Evidently this balance depends upon the supposition that for the particles that drive away from the sun, other forces than the electric, viz., the pressure of radiation, are preponderating, whilst for the negative electrons caught by the sun, other forces than the electric are wholly insignificant compared with these.

If one supposed, as some authors do, that the negative electricity was carried away from the sun by means of cathode rays, an effective circulation like that described above would be wholly impossible.

*[D 7050, 8010; Q 1230.]

"Studies on Enzyme Action. II.—The Rate of the Change, conditioned by Sucroclastic† Enzymes, and its Bearing on the Law of Mass Action."

By Edward Frankland Armstrong, Ph.D., Salters' Company's Research Fellow, Chemical Department City and Guilds of London Institute, Central Technical College. Communicated by Professor H. E. Armstrong, F.R.S.

Received April 5,—Read April 28, 1904.

*D 0070 Q 0070	Nomenclature of enzymes.
D 1820	Milk sugar and maltose, hydrolysis by enzymes.
D 8010	Emulsin, lactase, maltase—kinetics of their action.

Although it is now universally recognised that enzymes play a most important part in animal and plant metabolism and that they even condition synthetic changes, most of the work done has been of a qualitative character; in only a few cases has the nature of the action been precisely determined—and the results arrived at in these few cases, if not discrepant, cannot easily be harmonised at first sight.

C. O'Sullivan and Tompson‡ were the first to study the action of enzymes quantitatively. They came to the conclusion that the action of invertase on cane sugar takes place in accordance with the ordinary theory of mass action, a theory which involves the assumption that, throughout an interaction, the amount of change taking place in a given interval of time is always the same proportion of the material remaining unchanged—so that the course of change is expressible by a logarithmic curve.

Duclaux§ subsequently contended that the rate of change during the early period is directly proportional to the time, although afterwards, under the influence of the products of hydrolysis, it follows the logarithmic law. If this suggestion that the action follows the law

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* [Index supplied by communicator, classified according to the schedule of the International Catalogue of Scientific Literature.]

† [Attention was directed by me in 1890 to the fact that "the terms amylolytic, proteolytic, etc., are confusing to the student who has learnt that electrolysis signifies splitting up by means of electricity and hydrolysis splitting up by means of water—not the splitting up of electricity or of water." ("The Terminology of Hydrolysis, especially as affected by Ferments," 'Chem. Soc. Trans.,' 1890, p. 528.) Unfortunately such terms are still generally used. To avoid the difficulty, I would suggest that enzymes should be spoken of as sucroclastic, glucosidoclastic, amyloclastic, proteoclastic, lipoclastic, etc., according as they condition the hydrolysis of disaccharides, glucosides, starch, proteids, fats, etc.—H. E. A.]

‡ 'Chem. Soc. Trans.,' 1890, vol. 57, p. 843.

§ 'Ann. Inst. Pasteur,' 1898, vol. 12, p. 96.

only when it is modified by the products of change be accepted, it follows that the interaction does not take place at any time in accordance with the law of mass action.

Somewhat later, an elaborate investigation of the action of enzymes was carried out by Victor Henri, whose work is summarised in his 'Lois générales des diastases,' Paris, 1903. Henri found that the velocity coefficient K, calculated on the assumption that the logarithmic law was applicable, steadily increased in value as the action proceeded. He attributed the increase to the influence of the products of change $\left(\frac{x}{S}\right)$ and took these into account by writing $K_1\left(1+\epsilon\frac{x}{S}\right)$ in place of K in the equation of mass action, which thus becomes

$$\frac{dx}{dt} = K_1\left(1+\epsilon\frac{x}{S}\right)(S-x).$$

This expression was derived from Ostwald's 'Lehrbuch.'[*] It may be pointed out that it is there put forward as applicable to cases of change in which the products may be assumed to have an *accelerating influence*, and that, as a matter of fact, it is the equation to a curve showing a change in direction corresponding to a rise to a maximum velocity and a subsequent fall in the rate. Henri deduced from his results the value $+1$ for ϵ, so that the equation became

$$2K_1 = \frac{1}{t}\log\frac{S+x}{S-x}.$$

In 1902, Adrian Brown,[†] besides arriving at the conclusion that hydrolysis is effected at a more rapid rate than is indicated by the law of mass action, also showed that on varying the concentration an approximately *constant weight*—not as the law of mass action requires a *constant proportion*—of sugar is hydrolysed in a given time. It is necessary to bear in mind, however, that in his experiments, the proportion of the total sugar hydrolysed in no case exceeded one-fifth, so that in reality the comparison was made only during the earlier stage of the hydrolysis. To explain the somewhat remarkable result to which he was led, Adrian Brown assumed that not only is a compound of enzyme and sugar formed but that this persists during a really appreciable interval of time: consequently a molecule of the enzyme can effect only a limited number of complete molecular changes in unit time; whatever the available mass may be, no increase in the amount of substance changed is possible. On the other hand, in dilute solutions in which the proportion of sugar to enzyme falls below a certain maximum, the amount of sugar hydrolysed should be directly proportional to the amount present—and this Adrian Brown proved to be the case.

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[*] 2nd edition, vol. 2, p. 264.

[†] 'Chem. Soc. Trans.,' vol. 81, p. 373.

Similar results were obtained by Horace Brown and Glendinning* in the case of the hydrolysis of starch under the influence of diastase. Stress was laid by these observers on the fact that, at first, equal amounts of starch are hydrolysed in equal intervals of time and that subsequently the change follows the usual logarithmic law.

It is noteworthy, however, that when this logarithmic law is applied either to Henri's results or to those of Adrian Brown for invertase or to those of Horace Brown and Glendinning for diastase, reckoning from the commencement of the change, a series of increasing values is obtained for K, whereas when Henri's equation is used K_1 is a constant in each case. It would thus appear that the action of the two enzymes follows the same fundamental law.

The nature of the change effected by enzymes has been fully discussed by Horace Brown and Glendinning. Starting from the conception that hydrolysis is preceded by a combination of the hydrolyte with the enzyme, on the assumption that the concentration of the added enzyme is very small in relation to the initial concentration of the sugar, they point out that, in the earlier stages of the hydrolysis, the amount of sugar in unit volume will be very large compared with the amount of the combination of sugar with enzyme: consequently, so long as the concentration of the unaltered sugar remains very large compared with that of the combination, this latter will remain almost constant in amount and equal amounts of sugar will be hydrolysed in equal times: the time curve will, in fact, be approximately a straight line. When, however, the amount of sugar present is materially reduced, the combination will more nearly follow the ordinary law of mass action. They point out that this explanation is in accord with all the known facts.

A considerable body of evidence is put forward in this communication which, in the main, confirms the conclusion arrived at by Horace Brown and Glendinning; but it will be shown that it is necessary somewhat to extend their argument.

A novel conception was introduced by Croft Hill in 1898,† who studied the action of maltase on maltose advisedly from the point of view that the change might be reversible. He not only showed that the product of hydrolysis (glucose) exercised a marked retarding effect but also that a change was producible in the concentrated solutions of glucose by maltase; in his opinion, this retardation was due to reversion and he suggested that maltose was reproduced. Subsequently, in 1903,‡ while upholding the view that the change was a reversible one, he came to the conclusion that the main product, at all events, was an isomeride

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* 'Chem. Soc. Trans.,' 1902, vol. 81, p. 388.

† 'Chem. Soc. Trans.,' vol. 73, p. 634.

‡ 'Chem. Soc. Trans.,' vol. 83, p. 578.

of maltose. E. Fischer and the author* had meanwhile shown that the enzymes lactase and emulsin both acted reversibly.

English workers appear to have overlooked the work done by Tamman in 1892,† who studied with great care the action of emulsin on the two glucosides salicin and amygdalin, as well as that of invertase on cane sugar. He came to the conclusion that the rate of change was retarded by the products of degradation but could detect no sign of reversion; he even went so far as to express the opinion that enzymes did not act reversibly. He insisted, however, that their action was incomplete.

In dealing with problems of enzyme action, the probability that the extract used contains several enzymes cannot be lost sight of, since it is known that in yeast extract, for example, at least three sucroclastic enzymes are present together with a proteoclast in amounts which vary in different yeasts. It is at least conceivable that, in those cases in which reversion has been observed, the hydrolysis is conditioned by one enzyme and the synthesis by another. It will be obvious, therefore, that the field for investigation is a very wide one and that our knowledge of enzymes is of a most incomplete and unsatisfactory character.

The present communication deals with the problem of the rate at which change proceeds during the earlier period, when the products of hydrolysis are present in relatively small proportions.

The results obtained not only justify the extension to enzymes generally of the view put forward by Horace Brown and Glendinning in explanation of the action of diastase and invertase, but also make it possible to explain cases in which the departure from the law of mass action is in a direction contrary to that considered by these authors; the influence exercised by the products of change, which was not taken into account by them, will also be considered. The action of acids is compared with that of enzymes in a separate communication.

Preparation of Enzyme.—The enzymes considered are lactase, emulsin and maltase; the first two of these both condition the hydrolysis of milk sugar but the last affects only maltose. Unfortunately no way has been devised hitherto of working with a known amount of enzyme; the nearest approach to a satisfactory method is to prepare a fresh extract for each series of experiments under conditions as nearly uniform as possible. Far too little care is sometimes given to this operation, the importance of working rapidly and at a suitable temperature being commonly neglected. The following is a description of the methods adopted in the experiments referred to in this communication.

Lactase.—Ten grammes of Kephir grains were very vigorously

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* 'Ber.,' 1902, vol. 35, p. 3144.

† 'Zeit. Physiol. Chem.,' vol. 16, p. 271.

agitated with 200 c.c. water, using the modified ice-cream mixer described by Moody:* after 4—6 hours, when the grains were reduced to a fine pulp, 5 c.c. of toluene was added and the closed vessel set aside at about 20° over night. The milky liquid was then decanted and rapidly filtered, filtration being repeated, if necessary, until a clear liquid was obtained. The extract thus prepared was a clear yellow liquid: as changes took place in it on standing, it was used without delay. It is probable that a nearly saturated solution of the enzyme is obtained by operating in the manner described, as the residue is still very active; thus on extracting it for a further period of 24 hours with 200 c.c. water, an extract was obtained of about one-fourth the activity of the first.

The unfiltered milky extract is much more active than the filtered, probably because it contains some enzyme in suspension; it may also be mentioned that repeated filtration seems to diminish the activity even of the clear liquid. Although lactase prepared by the ordinary methods, involving precipitation and subsequent washing with alcohol, possesses some power of inducing the hydrolysis of milk sugar, it is only feebly active in comparison with an extract prepared as above described. The following figures, showing the parts per hundred of sugar hydrolysed during the intervals indicated in 50 c.c. of a 5 per cent. solution of milk sugar, may be quoted in illustration:—

	1 hr.	4 hr.	24 hr.
Solution containing 20 c.c. filtered extract	4·8	16	45
,, ,, 20 c.c. unfiltered extract ...	15	38	80
,, ,, 40 c.c. ,, ,, ...	29	57	86
,, ,, Kephir residues..............	17	45	74

As the extract affects cane sugar, it may be supposed that it contains invertase as well as lactase; there is also reason to believe that one or more proteoclasts are present.

Maltase.—The extract was prepared by merely grinding up with water, in presence of toluene at about 21°, yeast which had been dried as rapidly as possible—by spreading it in a thin layer on biscuit ware and exposing it to the atmosphere; the clear filtrate was used. Experience seems to show that the activity of such an extract depends on the temperature at which it is made and that different yeasts require to be extracted at different temperatures.

Toluene has been used as an antiseptic throughout the experiments, as it was found not to influence the activity of the enzymes, whereas chloroform had a distinctly retarding effect.

Rough experiments made to ascertain the temperature at which lactase exercises its maximum activity show this to be about 35°:

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* 'Chem. News,' 1902 vol. 86, p. 230.

although the extract is still very active at 15°, above 38° the activity begins to lessen. The experiments with lactase and emulsin were made at 35—37°, those with maltase at about 30°. In making the experiments, a mixture in the desired proportions of the solution of the sugar and of the enzyme, in a closed flask, was kept at a known temperature in an incubator; samples were withdrawn from time to time: these were diluted so that they contained 0·2 per cent. of the sugar and their cupric-reducing power was then determined by the modification of Pavy's method described by Croft Hill. A simple calculation gave the proportion in which monosaccharide and disaccharide were present. It may be claimed that the results are comparable; probably they are affected by an error amounting to at least from 1 to 2 per cent. when the amount of sugar hydrolysed does not exceed about 70 per cent.

The proportion or weight of disaccharide hydrolysed during successive intervals having been determined, the results were plotted graphically; the coefficient of velocity K was also calculated for each result, on the assumption that the rate of change was proportional to the amount of hydrolyte which undergoes change.

The actions of lactase and emulsin on milk sugar and of maltase on maltose have been studied in the manner described, using solutions containing 2, 5 and 10 grammes of the hydrated disaccharide in 100 c.c. Typical series of results selected from a large number of a similar character are quoted in the following tables. In these, t is time from the commencement of the experiment, x the percentage of substance hydrolysed and K the velocity coefficient calculated from the equation $K = \frac{1}{t} \log_{10} \frac{100}{100 - x}$.

Lactase.—On contrasting the results given in the two following tables it will at once be obvious that the character of the change is dependent on the proportion which the enzyme present bears to the sugar. When, as in the case of Experiment I, the proportion of enzyme is relatively large, K falls throughout the whole period of change; whereas, when the proportion of enzyme is considerably reduced, equal amounts of sugar are changed in successive equal intervals of time until about 10 per cent. has been hydrolysed, the value of K increasing at first but afterwards falling rapidly as in the previous case.

2 grammes Milk Sugar per 100 *c.c.*

Table I.

100 c.c. Enzyme Extract.

t.	*x.*	*K.*
1 hr.	22·1	0·1085
2 hrs.	31·2	0·0812
3 ,,	38·9	0·0713
4 ,,	45·8	0·0665
5 ,,	51·5	0·0629
6 ,,	56·6	0·0604
10 ,,	69·0	0·0509
17 ,,	84·2	0·0471
23 ,,	92·4	0·0461
29 ,,	95·3	0·0457
38 ,,	98·0	0·0447

Table II.

40 c.c. Enzyme Extract.

t.	*x.*	*K.*
$\frac{1}{3}$ hr.	3·2	0·0423
$\frac{2}{3}$,,	6·4	0·0430
1 ,,	9·6	0·0438
$1\frac{1}{2}$ hrs.	13·2	0·0410
2 ,,	16·4	0·0389
3 ,,	20·8	0·0338
5 ,,	25·2	0·0252
23 ,,	47·6	0·0122
100 ,,	89·6	0·0082

The same result is brought out in Tables III and IV, which apply to somewhat more concentrated solutions of milk sugar.

5 *grammes Milk Sugar per* 100 *c.c.*

Table III.

100 c.c. Enzyme Extract.

t.	*x.*	*K.*
1 hr.	13·7	0·0640
2 hrs.	22·1	0·0543
3 ,,	27·2	0·0460
5 ,,	30·0	0·0310
24 ,,	51·0	0·0129

Table IV.

20 c.c. Enzyme Extract.

t.	*x.*	*K.*
1 hr.	1·6	0·00700
2 hrs.	3·2	0·00705
4 ,,	5·4	0·00602
23 ,,	21·4	0·00455
29 ,,	25·0	0·00430
47 ,,	32·7	0·00370
71 ,,	45·0	0·00365
144 ,,	49·6	0·00207

That similar results are obtained on working with different materials at different times is shown in Tables V and VI, which refer to experiments made at times 4 months apart.

10 *grammes Milk Sugar per* 100 *c.c.*

Strong Enzyme Extracts.

Table V.

t.	*x.*	*K.*
1 hr.	13·0	0·0605
2 hrs.	22·1	0·0543
4 ,,	33·7	0·0446
6 ,,	38·0	0·0346
24 ,,	55·5	0·0146
48 ,,	64·5	0·0094

Table VI.

t.	*x.*	*K.*
1 hr.	12·1	0·0560
2 hrs.	18·0	0·0431
3 ,,	22·0	0·0360
4 ,,	25·1	0·0314
23 ,,	46·7	0·0119
28 ,,	53·0	0·0117
47 ,,	61·0	0·0087

Emulsin.—Tables VII—X refer to experiments made with emulsin; the conclusions to be drawn from them are precisely similar to those deduced from the experiments with lactase. It is to be noted that emulsin acts much less rapidly than lactase.

2 *grammes Milk Sugar per* 100 *c.c.*

Table VII.

0·2 gramme Emulsin.

t.	*x.*	*K.*
½ hr.	3·2	0·0282
1 ,,	4·8	0·0214
2 hrs.	6·4	0·0143
3 ,,	7·6	0·0114
4½ ,,	9·0	0·0091
6 ,,	10·0	0·0091
23 ,,	19·7	0·0041
29 ,,	22·0	0·0037
48 ,,	29·0	0·0031
53 ,,	30·7	0·0030
144 ,,	62·2	0·0029
264 ,,	77·5	0·0024

Table VIII.

0·4 gramme Emulsin.

t.	*x.*	*K.*
1 hr.	4·9	0·0218
2 hrs.	7·5	0·0169
4½ ,,	9·4	0·0095
6 ,,	10·6	0·0081
23 ,,	30·5	0·0069
29 ,,	35·0	0·0064
48 ,,	47·8	0·0059
53 ,,	50·0	0·0057
144 ,,	84·0	0·0055

5 *grammes Milk Sugar per* 100 *c.c.*

Table IX.

0·4 gramme Emulsin.

t.	*x.*	*K.*
1 hr.	1·0	0·00440
2 hrs.	1·8	0·00395
4 ,,	3·2	0·00352
6 ,,	4·5	0·00333
22 ,,	15·0	0·00320
46 ,,	25·5	0·00277
70 ,,	35·6	0·00271
167 ,,	54·7	0·00206

Table X.

0·1 gramme Emulsin.

t.	*x.*	*K.*
1 hr.	3·1	0·0137
2 hrs.	4·6	0·0102
3 ,,	6·0	0·00896
4 ,,	7·0	0·00790
6 ,,	8·8	0·00667
22 ,,	21·5	0·00478
46 ,,	34·7	0·00404
70 ,,	46·0	0·00382

Maltase.—Tables XI and XII refer to experiments with maltase and maltose and show that here again the same conclusions hold. K decreases steadily: when the proportion of enzyme is small the change is at first linear. It is of interest as confirming these results that when K is calculated from the value given in Croft Hill's tables* a similar series of decreasing values is obtained.

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* 'Chem. Soc. Trans.,' 1898, p. 634.

Table XI.

Maltose, 5 per cent.

t.	*x.*	*K.*
1 hr.	7·3	0·0329
2 hrs.	13·9	0·0325
4 ,,	24·4	0·0304
7¼ ,,	31·7	0·0229
23 ,,	35·2	0·0082

Table XII.

Maltose, 10 per cent.

t.	*x.*	*K.*
1 hr.	4·7	0·0209
3 hrs.	11·7	0·0180
5 ,,	17·8	0·0170
23 ,,	23·9	0·0052
28 ,,	25·0	0·0045
47 ,,	31·4	0·0035

Concentration of Hydrolyte.—Although the experiments recorded in Tables I—XII furnish evidence that the rate of change decreases as the concentration of the sugar solution is increased, while the actual weight of sugar hydrolysed increases; as the experiments were made at different times and with different materials, it was necessary to carry out a strictly comparable series in which the amount of sugar present was the only factor varied. Table XIII shows the results obtained in experiments with relatively concentrated solutions of milk sugar and lactase in which the amount of enzyme present was quite small. As the concentration of sugar was increased, the rate of change diminished so that the fraction of sugar hydrolysed in a given time was inversely proportional to the amount of sugar present—so that a constant weight of sugar was changed independently of the concentration—a result which is in agreement with Adrian Brown's observations with invertase.

Table XIII.—Amount of Sugar Hydrolysed.

Solutions containing—	24 hours.		46 hours.		144 hours.	
	Proportion.	Weight.	Proportion.	Weight.	Proportion.	Weight.
10 per cent.	14·2	1·42	22·2	2·22	33·4	3·34
20 ,, ...	7·0	1·40	10·9	2·18	16·9	3·38
30 ,, ...	4·8	1·44	7·7	2·21	11·0	3·30

Table XIV relates to a series of experiments made with emulsin and milk sugar in which, however, the proportion of emulsin taken was not particularly small. Although the rate of change decreased as the concentration increased, the experiments afforded no evidence that a constant weight of sugar was hydrolysed in a given time independently of the concentration. In a second series, however, in which a considerably smaller proportion of enzyme (see Table XV) was used, it was found that an approximately constant weight of sugar was hydrolysed whatever the concentration of the sugar.

Table XIV.—Amount of Sugar Hydrolysed.

Solution containing—	22 hours.		46 hours.		70 hours.	
	Proportion.	Weight.	Proportion.	Weight.	Proportion.	Weight.
10 per cent.......	22	2·2	29	2·9	39·6	3·96
20 "	13	2·6	20	4·0	25·1	5·0
30 "	10	3·0	15	4·5	19·0	4·7

Table XV.—Amount of Sugar Hydrolysed.

Solution containing—	23 hours.		48 hours.		92 hours.	
	Proportion.	Weight.	Proportion.	Weight.	Proportion.	Weight.
10 per cent.......	19·7	1·97	29·8	2·98	35·7	3·57
20 "	10·6	2·12	15·3	3·06	23·3	4·66
30 "	7·0	2·10	10·2	3·06	16·3	4·89

The conclusions drawn from the experiments recorded in the three preceding tables apply only to the concentrated solutions in which the proportion of enzyme present was small; in very dilute solutions, on the other hand, quite another effect is produced by changing the concentration. Table XVI relates to experiments in which the proportion of enzyme present was large relatively to that of sugar. It will be seen that on increasing the amount of sugar present there was nearly a proportionate increase in the amount hydrolysed, though the proportion hydrolysed, as well as the value of K, remained constant, a result in agreement with the law of mass action. It should be mentioned, however, that the error affecting titration in these experiments is somewhat large, owing to the influence of the proteids on the determination of the end point.

Table XVI.

Milk sugar per 100 c.c.	Amount changed in 3 hours.	K.
1·0 gms.	0·185	0·0296
0·5 "	0·098	0·0298
0·2 "	0·0416	0·0337

Concentration of Enzyme.—Lastly, Table XVII shows the effect of varying the amount of enzyme present. Experiments in this direction are limited by the uncertain nature of the material, as well as by the fact that it is the concentration of enzyme relatively to that of sugar which must be studied. The solution contained 5 per cent. of sugar. Solutions containing varying amounts of enzyme were obtained by diluting portions of one and the same stronger solution. The weight hydrolysed in a given time by varying amounts of enzyme was approximately proportional to the amount of enzyme, provided that the amount was not too large and also that the comparison was made during the earlier stages of hydrolysis before the secondary products began to exert a marked influence.

Table XVII.—Proportions Hydrolysed in 100 c.c. of a 5 per cent. Solution.

Solution containing	1·5 hours.	20 hours.	25 hours.	45 hours.	68 hours.
1 c.c. lactase......	0·15	2·2	2·6	3·9	4·8
2·5 c.c. lactase....	0·4	5·8	6·8	10·2	12·6
10 " "	1·6	23·3	—	38·6	48·5
20 " "	3·2	45·8	54·5	—	—

Very small quantities of the enzymes lactase and emulsin were found to be capable of hydrolysing only a small amount of sugar: their action then ceased. This result affords very definite evidence in favour of the view that the products of hydrolysis—in this case glucose and galactose—are capable of combining with enzymes and of removing them from the sphere of action.

Table XVIII.—Proportions Hydrolysed in 100 c.c. of a 5 per cent. Solution.

Solution containing	24 hours.	144 hours.
0·66 c.c. lactase....	2·3	2·3
1·0 " "	3·2	3·5
2·0 " "	6·3	7·4
5·0 " "	15·4	34·0

Summary and Discussion of Results—Nature of Enzyme Action.

From the results recorded in the foregoing tables, it is clear that two periods may be distinguished in the course of change—an earlier period, during which the change is a linear function of the time; and a later period, during which change proceeds at another rate (Tables

II, IV, VII, XI). The duration of these periods is conditioned by the relation which the amount of enzyme present bears to the sugar, the linear period disappearing when the amount of enzyme present is relatively considerable (Tables I, III, VIII). It will be noted that in the cases studied in which the action proceeds comparatively slowly, *e.g.*, hydrolysis by lactase, emulsin or maltase—K falls rapidly in value; whereas when the action proceeds rapidly, *e.g.*, hydrolysis by invertase or diastase—K increases.

To understand the origin of these differences, it is desirable to consider the subject of mass action somewhat in detail, even at the risk of repetition.

Changes such as that which cane sugar undergoes in an aqueous solution under the influence of a catalyst, in which, on account of its large relative mass, the part played by the solvent may be disregarded—known as changes of the first order—are assumed to follow the logarithmic law of mass action: it is supposed that the one substance only undergoes change and that the same fraction of the residue is changed in successive equal intervals of time, *i.e.*, the factor K deduced rom the equation $K = \frac{1}{t} \log \frac{S}{S - x}$ is a constant.

But this is an ideal conception, as it involves the assumption that the amount of catalyst functioning is so small as to be negligible. In actual practice, not only is the amount of enzyme used not inconsiderable but it is known that the rate of change is affected both by alteration in the concentration of the hydrolyte and in the proportion which the hydrolyte bears to the catalyst; moreover, the products of change are known to exercise an influence and in concentrated solutions reversion may take place.

In view of the proof afforded by E. Fischer's researches that a close relationship exists in configuration between the hydrolyte and the enzyme which alone affects it, there can scarcely be any doubt that hydrolysis by enzymes, in the first instance, involves the combination of the enzyme with the hydrolyte, as Horace Brown and Glendinning have assumed to be the case.

On this assumption, the rate at which the change proceeds will depend on the extent to which the combination of enzyme with sugar takes place as well as on the degree of readiness with which the compound breaks down.

The proportion of the total sugar present in solution combined with the enzyme and undergoing change at any one moment may be regarded as the *active mass* of the sugar. The conception of an active mass has already been introduced by Arrhenius* but in another connection—to account for the very rapid rise in the value of K, when cane sugar is hydrolysed by acids, occasioned by a rise in temperature, the increase

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* 'Zeit. Phys. Chem.,' 1889, vol. 4, p. 226.

being far greater than can be accounted for by the increased "ionisation" or by the greater mobility of the molecules at the higher temperature. Arrhenius supposed that only a part of the total hydrolyte present was really concerned in the hydrolysis: this he termed the "active part." It was arbitrarily assumed that this was a fixed proportion of the total mass at any temperature and that the proportion increased rapidly as temperature rose.

On the hypothesis that the enzyme combines with the sugar, the active mass of the sugar will be that portion s of the whole S which is in combination with an amount of enzyme e: it will be convenient to speak of the combination $s+e$ as the *active system.*

It is to be supposed that several influences are at work in a solution containing enzyme and sugar: on the one hand, enzyme and sugar molecules seek to combine; but on the other, the water molecules also tend to unite with the sugar molecules—so that there is, so to speak, competition between the enzyme and the water for the sugar molecules, which results in the establishment of an equilibrium depending to some, though probably a very limited extent, on the proportion relatively to enzyme in which the water and sugar molecules are present.* The possibility that the products of change also compete for the enzyme must, however, not be left out of consideration.

It is necessary to consider separately four sets of conditions, viz.:—

Case I, in which, whatever the amount of sugar present, the quantity of enzyme is relatively small.

Case II, in which there is a difference from Case I inasmuch as the quantity of enzyme is relatively considerable.

Case III, in which the amount of enzyme diminishes as the action proceeds.

Case IV, in which the amount of sugar present is varied.

Case I.—As hydrolysis proceeds, assuming that the enzyme itself is not affected by the work it does, since the magnitude of the active system depends on the amount of enzyme present, it is obvious that in the initial stages, if the total amount of sugar present S be large compared with s, the enzyme will be in presence of enough sugar molecules to establish the maximum possible number of effective combinations: or in other words the magnitude of the active system will remain constant and the change will be expressible, as Brown and Glendinning have pointed out, as a linear function of the time. As hydrolysis

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* The enzymes, it is to be supposed, are colloids, *i.e.*, substances which have but little affinity for water: the stability of the combination $s + e$ will therefore be but to a slight extent dependent on the proportion in which water is present; whereas, probably, in the case of a combination of a crystalloid, such as an acid, with sugar, the proportion in which the components of the system are in equilibrium would vary to a considerable extent with the concentration. This is known to be the case. The effect of acids is dealt with in a separate note.

proceeds, the amount S of sugar present decreases until it is no longer negligible compared with that of the active part *s* and hence the enzyme will no longer effect the maximum possible number of combinations: the proportion of sugar *s* undergoing change will then be a function of the total mass and the formation of active systems will be governed by the law of mass action. The rate of change will be a logarithmic function of the time.

This explanation is fairly in accordance with the observed facts in the case of invertase and diastase, the only enzymes hitherto experimented with, which have always been used in very small quantities.

Case II.—If, on the other hand, the quantity of enzyme used be relatively large, the active mass will be a function of the total mass from the very beginning of the experiment, so that the linear part of the change will escape notice. O'Sullivan and Tompson seem to have used a relatively large proportion of enzyme, and therefore it is easy to understand why they found the action of invertase to follow the logarithmic law, whilst subsequent observers using relatively small quantities of enzyme have noted departures from this law.

Case III.—When the amount of the enzyme does not remain constant but for some reason decreases, the magnitude of the active system will not only be a function of the amount of sugar but also of that of the enzyme; it will therefore be represented by an equation of the second order, in which both of two interacting substances decrease—as, for example, is the case in the interaction of an alkali and methylic acetate. Such an expression corresponds to a curve falling off from a logarithmic curve and therefore giving a series of decreasing values for K when this is calculated for the simple logarithmic law. In such a case, the change in its early stages will still be a linear function of the time, as the diminution in the amount of enzyme will not at first materially influence the magnitude of the active system.

Stated shortly, the ordinary equation of mass action

$$\frac{dx}{dt} = \mathrm{K}(\mathrm{S} - x),$$

where S is the total sugar and x the amount changed in time t, is applicable only to the period during which a constant relatively large proportion of enzyme is present together with a continually decreasing amount of sugar but uninfluenced by the products of change.

During the final period, when the products of change exercise an influence by withdrawing enzyme from the sphere of action,

$$\frac{dx}{dt} = \mathrm{K}(\mathrm{S} - x)(\mathrm{E} - y),$$

where E is the total enzyme, y the amount withdrawn in combination with the products in time t.

During the period when the proportion of sugar present is very large, x becomes negligible compared with S, so that

$$\frac{dx}{dt} = \text{KS} = k,$$

where k is a constant.

The apparent duration of the linear period must be affected not only by x becoming no longer negligible compared with S but also by the extent to which the products of change make their influence felt.

It may here be pointed out that Henri's formula combines in a single expression the linear and logarithmic periods, but does not take into account the last period during which the products of the change exercise a retarding influence.

All these points are brought out not only in the results given in this paper but also in those tabulated by Brown and Glendinning. Although these observers have called attention to the existence of a linear period followed by a "logarithmic" period, they have failed to point out the meaning which may be attached to the decrease in the values of K observed during the latter stages of hydrolysis, when the products exercise a marked influence.

Their experiments afford an example of such an influence. Thus in Table IV,* referring to the hydrolysis of a 3 per cent. solution of starch by diastase at 21°, the value of K will be seen to increase until about half the starch was hydrolysed and then to decrease. If the value after 60 minutes be made the new starting point and the values be recalculated in the manner adopted by Brown and Glendinning,† the following figures are obtained :—

Old time units.	New time units.	K_0.
60 minutes	—	—
80 ,,	20 minutes	458
100 ,,	40 ,,	461
120 ,,	60 ,,	448
140 ,,	80 ,,	410

It is evident that whereas K_0 should be a constant if the period were "logarithmic," it begins to decrease after about 66 per cent. of the starch has been hydrolysed: showing that the influence of the products in removing enzyme has begun to make itself felt.

The action of invertase appears to be much less affected by invert

* *Loc. cit.*, p. 400.

† *Loc. cit.*, p. 393.

sugar and that of diastase by maltose than is that of lactase, emulsin or maltase by the products to which they respectively give rise. Consequently, for these latter enzymes, the linear period is of short duration (Tables II, IV, VII, XI) and the logarithmic period is barely perceptible, owing to the rapid reduction in the rate.

Case IV.—When the amount of enzyme and water is kept constant whilst that of sugar is increased, it may be supposed that the magnitude of the active system will increase until $s+e$ reaches a maximum, a definite equilibrium being established between enzyme, sugar, and water, the whole of the enzyme, perhaps, becoming combined with the sugar. It may be assumed that if the amount of sugar be further increased, the equilibrium will remain unaffected, notwithstanding that an addition of sugar is practically equivalent to a withdrawal of water.

But if $s+e$ remain unaltered, whatever the proportion of sugar present beyond a certain minimum, a constant amount of hydrolyte will undergo change in a given time, although the proportion changed as also the value of K will decrease as the concentration is increased. This conclusion is entirely in agreement with the facts elucidated, especially by Adrian Brown, and with my own observations.

Lastly, it should be pointed out that in discussing the action of enzymes, besides taking into account the conditions affecting the formation of an active system and the part which such a system plays in hydrolysis, it is necessary to consider the relative stability or tendency to break down under the influence of water of the combination of enzyme and sugar in connection with the very different rates at which different enzymes condition hydrolysis. Generally speaking, the observed differences in the rate at which hydrolysis is effected may be conditioned by—

(*a*) Specific differences in the enzymes, *e.g.*, lactase as compared with emulsin, which both hydrolyse milk sugar.

(*b*) Differences in the configuration of the hydrolyte, *e.g.*, β-methyl glucoside and β-methyl galactoside, which are both hydrolysed by emulsin.

(*c*) Differences in the stability of the hydrolytes, *e.g.*, cane sugar as compared with milk sugar.

But it is very difficult to institute just comparisons, for whereas, in the case of acids, the effect of only a single substance on the variety of sugars may be contrasted, in studying the hydrolysis of sugars under the influence of enzymes, it is necessary in most cases to use a different enzyme for each sugar, so that positive data are not easily obtained. Experiments made under comparable conditions with acids to test their action on different sugars show that these are hydrolysed at very different rates: thus, for instance, whilst cane sugar is hydrolysed nearly 1000 times as rapidly as maltose, this latter undergoes change about 1·3 times as rapidly as milk sugar. It is scarcely possible to doubt that even greater differences exist both in the affinity of the enzymes for the sugar and in the degrees of readiness with which the enzyme sugar systems break down than are known to hold in the case of acid sugar systems. It is important to keep these considerations in view in discussing the rate at which different enzymes effect hydrolysis.

It will be apparent from what has been said that there is no reason to suppose that the action of enzymes follows any other than a normal course; the difficulties which have been met with in interpreting such changes may be ascribed to the incomplete consideration of the numerous factors involved.

[D 1810, 8010; Q 1230.]

" Studies on Enzyme Action. III.—The Influence of the Products of Change on the Rate of Change conditioned by Sucroclastic Enzymes."

By EDWARD FRANKLAND ARMSTRONG, Ph.D., Salters' Company's Research Fellow, Chemical Department, City and Guilds of London Institute, Central Technical College. Communicated by Professor H. E ARMSTRONG, F.R.S. Received April 5,—Read April 28, 1904.

D 1810	Hexoses—power of sucroclastic enzymes to combine with.
D 8010	Enzymes—activity of correlated with configuration of hydrolyte.
D 8010 Q 1240	Emulsin, lactase, maltase—varying influence of glucoses and glucosides on their activity.

In the previous paper, it has been shown that, in order to explain the action of sucroclastic enzymes, it is necessary to assume not only that the enzyme combines with the hydrolyte but that it is also more or less affected by—and presumably combines with—the product of change.

At present there is but little information available bearing on this latter contention. The experiments to be described have been made with the object of ascertaining by direct observation whether and to what extent the action of a given enzyme is affected by one or more of the products formed under its influence. They establish very clearly the existence of a close relationship between the configuration of the hexose and the enzyme in those cases in which a retarding influence is apparent: it is difficult to explain such a result except on the assumption that the enzyme and hexose combine together in some intimate manner.

Historical.—The observation made by Würtz in 1879,* when studying the action of Papaïn on fibrin, that the enzyme was in some way retained, probably first gave rise to the conception that the primary action of enzymes was additive in character. The first definite evidence of combination, however, appears to be that advanced by O'Sullivan and Thomson in 1890.† It was shown by these observers that in the presence of cane sugar invertase will withstand without injury a temperature fully 25° higher than it will in its absence. They pointed to this as a very striking fact which was difficult to explain except on the assumption that the invertase enters into combination with the sugar; they further supposed that it was capable of combining with the *invert* sugar.

Systematic experiments were made by Tamman in 1892,‡ who showed that the hydrolysis of amygdalin and salicin by emulsin is materially retarded in either case on adding any one of the products of their change in advance.

The effect of glucose on the hydrolysis of maltose by maltase was studied by Croft Hill in 1898.§ The retardation observed was attributed by him, perhaps not quite logically, merely to a reverse action by which the glucose underwent conversion into maltose: it may be added that the investigation was undertaken by him from this point of view.

A more definite step forward was taken by Henri in 1901, who showed that the retarding effect of invert sugar was mainly due to the fructose, glucose having little or no effect on the action of invertase on cane sugar. Evidence that the effect was due to a specific action of the invert sugar was adduced in the same year by Adrian Brown,‖ who showed that whilst the rate of hydrolysis of cane sugar was materially reduced by invert sugar, the corresponding amount of milk sugar had little or no effect, thus precluding the conclusion that the retardation was due to increased viscosity of the liquid.

In the course of his classic researches on the action of enzymes on the stereochemically related glucosides commenced by Emil Fischer¶ in 1894, it was clearly established that the closest relationship exists between the configuration of the hydrolyte and that of the particular enzyme which can affect it.

In a course of lectures delivered at the Pharmaceutical Society in 1892,** my father discussed fermentation phenomena generally from the point of view that hydrolysis was conditioned by the association of

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* 'C. R.,' vol. 81, p. 425 and vol. 91, p. 787.

† 'Chem. Soc. Trans.,' vol. 57, p. 919.

‡ 'Zeit. physiol. Chem.,' vol. 16, p. 291.

§ 'Chem. Soc. Trans.,' 1898, vol. 23, p. 634.

‖ 'Chem Soc. Trans.,' 1902, vol. 81, p. 373

¶ Summary in 'Zeit. physiol. Chem.,' vol. 26, p. 60.

** 'Pharmaceutical Journal,' vol., 22, pp. 495, 596, 659, and 757.

the enzyme with both the sugar and water; this theme was further developed in his Presidential Address to the Chemical Society in 1895.*

Hydrolysis of Milk Sugar by Lactase.

Table 1.—Solutions were compared containing equal amounts of milk sugar to one of which an equal weight of a mixture of equal parts of glucose and galactose was added: it will be seen that this addition reduced the rate to nearly half its value. In this and all subsequent tables the figures indicate the percentage of biose hydrolysed:—

Time in hours.	10 grammes milk sugar per 100 c.c.	+ 5 grammes glucose and 5 grammes galactose.
1	1·3	0·88
2	2·2	1·34
4	3·37	1·82
6	3·8	2·22
24	5·55	3·19
48	6·45	3·64

Table 2.—The effect of glucose, of galactose and of an equal weight of a mixture of both on a 10-per-cent. solution of milk sugar are contrasted in the following table:—

Time in hours.	+ 10 grammes galactose.	+ 5 grammes galactose and 5 grammes glucose.	+ 10 grammes glucose.
19	18·2	22·8	22·9
24	21·0	25·0	25·6
44	25·6	29·5	29·5
67	30·9	34·8	34·8
190	38·7	44·8	44·8

Table 3.—That the retardation is produced almost entirely by the galactose and that glucose or fructose are almost without influence is shown by the following comparisons:—

Time in hours.	5 grammes milk sugar per 100 c.c.	+ 5 grammes glucose.	+ 5 grammes fructose.	+ 5 grammes galactose.
4	18·0	17·6	18·0	16·0
22	59·2	59·6	59·6	47·4
28	65·6	65·4	65·4	52·0
69	81·4	78·4	80·2	61·6

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* 'Trans.,' vol. 67, p. 1136.

Table 4.—The effect of increasing the amount of galactose is illustrated by the following figures :—

Time in hours.	5 grammes milk sugar per 100 c.c.	+ 5 grammes fructose.	+ 5 grammes galactose.	+ 10 grammes galactose.	+ 15 grammes galactose.
5	18·8	18·8	16·0	15·2	9·2
23	59·6	56·0	38·0	28·4	18·0
48	67·2	68·0	47·4	37·0	19·2

Hydrolysis of Milk Sugar by Emulsin.

Table 5.—A comparison of the relative influence of glucose and of galactose, as well as of an equal weight of a mixture of these in equal proportions, on the rate of hydrolysis shows that the glucose has the greater effect on this enzyme. Each solution contained 10 grammes of milk sugar in 100 c.c. :—

Time in hours.	+ 10 grammes glucose.	+ 5 grammes glucose and 5 grammes galactose.	+ 10 grammes galactose.
1	1·0	1·0	1·3
2	2·8	3·2	3·5
3	3·8	4·0	4·3
23	7·4	8·7	8·7
46	9·3	11·1	12·0
70	17·7	20·7	21·3
140	34·4	39·0	42·7
380	45·8	50·1	54·0

Table 6.—The following results, representing the influence of glucose, galactose and fructose, show that whilst glucose most retards hydrolysis, galactose also exercises a retarding influence, fructose having little if any effect, the differences noticed being within the limits of error :—

Time in hours.	5 grammes milk sugar per 100 c.c.	+ 5 grammes fructose.	+ 5 grammes galactose.	+ 5 grammes glucose.
22	17·0	17·0	16·0	13·6
46	32·0	30·4	23·2	20·0
70	46·0	43·8	31·2	27·0
94	58·0	54·6	42·0	34·8

Hydrolysis of Maltose by Maltase.

Table 7.—In this case, glucose retards the hydrolysis considerably galactose also has a slight retarding influence, but the effect of fructose is not appreciable :—

Time in hours.	5 grammes maltose per 100 c.c.	+ 5 grammes fructose.	+ 5 grammes galactose.	+ 5 grammes glucose.
3	25·6	25·8	25·2	14·0
5	34·0	34·8	28·8	18·0
24	73·8	75·2	64·0	23·0

Correlation of Differential Action of Enzymes with Configuration of Hydrolyte.

Combining my results with Emil Fischer's earlier observations the following table is arrived at:—

Enzyme.	Corresponding hydrolyte.	Effect of hextose on rate of change.		
		Glucose.	Galactose.	Fructose.
Lactase ..	β-Galactosides (*i.e.*, milk sugar, β-alkyl galactosides).	No influence.	Retards.	No influence.
Emulsin .	β-Glucosides (*i.e.*, most natural glucosides, β-alkyl glucosides). β-Galactosides (as above).	Retards considerably.	Retards slightly.	No influence.
Maltase ..	α-Glucosides (*i.e.*, maltose, α-alkyl glucosides). α-Galactosides (*i.e.*, α-alkyl galactosides).	Retards considerably.	Retards slightly.	No influence.
Invertase	Fructosides* (*i.e.*, cane-sugar, raffinose, gentianose, manneotetrose).	No influence.	—	Retards.

The compounds entered in the second column of the table are those which, according to Emil Fischer, are alone hydrolysed by the particular enzymes indicated in the first column. My own observations on the specific retarding influence of the various hexoses are entered in the remaining columns. It is clear that the only hexoses which retard hydrolysis by any given enzyme are those derived from

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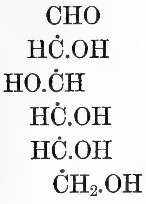
* It is probable that these compounds are not derivatives of fructose of the —OR type corresponding to the simple glucosides; probably the linkage is of a peculiar character, two centres being concerned. In this connection, it may be mentioned that invertase has no action on methyl fructoside, a substance in every way analogous to methylglucoside. This point will be more fully dealt with in a later communication.

the hexosides* which undergo hydrolysis under the influence of that enzyme.

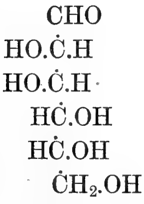
A more absolute proof of the close correlation in configuration between enzyme and hydrolyte cannot well be imagined: it is difficult to interpret such behaviour in any other way than as evidence that the enzyme combines with the hexose in some special, peculiarly intimate manner and is thereby withdrawn from the sphere of action. The retardation cannot well be due to reversion, as in the case of milk sugar the retardation is effected chiefly by glucose when emulsin is the active agent but by galactose alone when lactase is used to effect hydrolysis.

Emil Fischer's researches have brought to light the remarkable fact that the naturally occurring hexoses or derivatives of these, *i.e.*, glucose, mannose, galactose and fructose, are the only compounds affected either by the organisms which condition alcoholic fermentation or by any of the sucroclastic enzymes. The stereoisomeric hexoses produced by artificial means cannot be fermented; this is true also of the "lower" and "higher" sugars, whether derived from natural products (arabinose, xylose, etc.) or prepared artificially from the natural products by reducing or adding to the number of carbon atoms in the chain. The fact that resistant materials such as straw, the gums, etc., are pentose derivatives is of interest in this latter connection. Three of the four hexose sugars referred to are fermented readily and apparently with equal ease; the fourth, galactose, is only slowly fermented: such being the case, it is noteworthy that the formulæ ordinarily assigned to glucose, mannose and fructose are reducible to one common enolic form. It is conceivable that this enolic form is the substance actually fermented to carbon dioxide and alcohol.



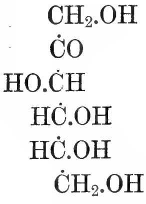
$$\begin{array}{c} \mathrm{CHO} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{HO.\dot{C}H} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{\dot{C}H_2.OH} \end{array}$$

Glucose.



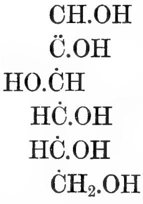
$$\begin{array}{c} \mathrm{CHO} \\ \mathrm{HO.\dot{C}.H} \\ \mathrm{HO.\dot{C}.H} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{\dot{C}H_2.OH} \end{array}$$

Mannose.



$$\begin{array}{c} \mathrm{CH_2.OH} \\ \mathrm{\dot{C}O} \\ \mathrm{HO.\dot{C}H} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{\dot{C}H_2.OH} \end{array}$$

Fructose.



$$\begin{array}{c} \mathrm{CH.OH} \\ \mathrm{\ddot{C}.OH} \\ \mathrm{HO.\dot{C}H} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{H\dot{C}.OH} \\ \mathrm{\dot{C}H_2.OH} \end{array}$$

Enolic form.

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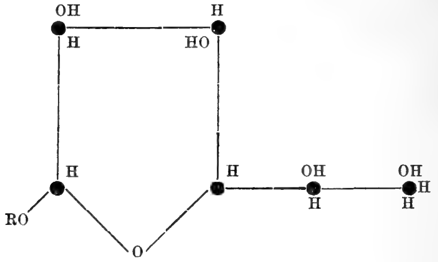
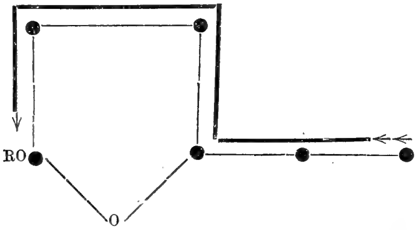
* The term "hexoside" is used as a general expression to include all compounds of a glucosidic character derived from a hexose.

But it is necessary to attribute a γ-oxide formula to glucose and its derivatives,* thus:—



When R = H, the formula is that of glucose itself; when R is an alkyl radicle, it represents one of the alkyl glucosides; when R is a hexose residue ($C_6H_{12}O_6 - H$), it represents a biose such as maltose. The carbon atom to which the group RO is attached in this formula is that which is figured as the superior carbon atom in the formula previously given.

Attachment of enzyme to hydrolyte.—The effect of hydrolysis, whether by acids or enzymes, is to remove the radicle R and displace it by hydrogen; apparently, this change need in no way affect the oxygen linkage in the ring. In the case of acids, although the attack may be regarded as located on the OR radicle, it probably proceeds from the neighbouring oxygen atom of the ring;† but there is every reason to suppose that, in the case of enzymes, the enzyme becomes in some way attached *along the line of carbon atoms*, thus:



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* It is to be borne in mind that ordinary glucose in solution consists almost wholly of two stereoisomeric compounds in equilibrium, together with a very small proportion, at the most, of the enolic form, the presence of which must be assumed in order, among other reasons, to explain the results arrived at by Lobry de Bruyn, which establish a reciprocal relationship between glucose, fructose and mannose in solution. The argument which makes it necessary to attribute the γ-oxide formula to glucose and allied compounds is fully stated in the first of this series of papers ('Chem. Soc. Trans.,' 1903, vol. 85, p. 1305) and in a paper by Dr. Lowry (*loc. cit.*, p. 314). The conclusions at which Dr. Lowry and I arrived have received independent confirmation from the recently published work of Behrend and Roth ('Annalen,' 1904, vol. 331, p. 361).

† See Part I, *loc. cit.*

In proof of this contention the following facts may be adduced:—

It is to be remembered that each hexose can give rise to two stereoisomeric hexosides (*e.g.*, α- and β-methyl glucoside), which differ only in the fact that the RO group and an atom of hydrogen are attached to the carbon atom in different relative positions: nevertheless, these require different enzymes to effect their hydrolysis. It is obvious, therefore, that but a slight shifting of the one radicle in space is sufficient to throw the enzyme out of action.

Yet it would seem that the enzyme is only out of harmony with the glucoside at the terminal point, inasmuch as the action of emulsin on milk sugar is hindered not only by glucose but also, although to a less extent, by α-methyl glucoside, which is not in the least attacked by emulsin, whereas the corresponding β-glucoside is readily hydrolysed.

Time in hours.	10 grammes milk sugar per 100 c.c.	+ 10 grammes α-methyl glucoside.
3	11·2	8·3
6	15·2	12·1
25	32·7	24·5

[*Note added May* 30, 1904.—Since this paper was presented a series of observations have been made with glucosides. A second experiment with the α-glucoside confirms the results arrived at in the first.

Time in hours.	10 grammes milk sugar per 100 c.c.	+ 10 grammes α-methyl glucoside.
5	7·9	5·7
23	23·3	16·9
28	27·1	19·0

In a similar manner, α-methyl galactoside, which is itself unaffected by the enzyme, hinders the hydrolysis of milk sugar by lactase practically to the same extent as galactose itself does.

Time in hours.	10 grammes milk sugar per 100 c.c.	+ 10 grammes α-methyl galactoside.	+ 10 grammes galactose.
2	7·9	5·3	4·8
5	14·9	9·7	9·6
26	27·8	20·2	20·0

On the other hand α-methyl glucoside like glucose has no retarding influence on the hydrolysis of milk sugar by lactase.

Lastly, β-methyl glucoside was found to retard the action on maltose of maltase, which easily hydrolyses the α-glucoside though it is entirely without action on the β-glucoside.

Time in hours.	10 grammes maltose per 100 c.c.	+ 10 grammes β-methyl glucoside.
2	10·2	7·7
4	14·4	10·7
6	21·0	17·0]

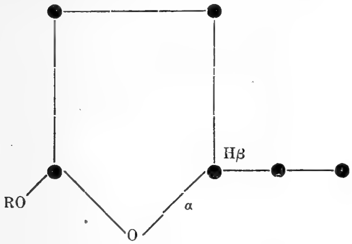
One of the most remarkable conclusions to be derived from Fischer's researches is that glucose and galactose are the only hexoses which afford glucosidic derivatives attackable by sucroclastic enzymes: any alteration in configuration other than that involved in the passage from the one of these hexoses into the other or any shortening or lengthening of the chain is sufficient to confer on the derivative complete immunity from attack. Thus the methyl mannosides, the methyl arabinosides and xylosides and the methyl glucoheptosides cannot be hydrolysed by enzymes. Inasmuch as hydrolysis affects only the OR radicle, this inhibition of hydrolysis by the very slightest change in configuration must mean that the enzyme not only becomes attached but that the attachment takes place along the entire chain of the hexosides: in other words, *enzyme and hydrolyte must be in complete correlation.* Probably this union is effected through the agency of the hydroxyl groups and it may be of basic groups in the enzyme—perhaps their union takes place not immediately but through the intervention of water molecules.

The only case in which there is evidence of partial or incomplete attachment is that afforded by the inhibiting influence of α-methyl glucoside on the hydrolysis of milk sugar by emulsin: in this case it would seem that there is no interruption along the chain, the correlation of hydrolyte and enzyme being incomplete only at the terminal point. This argument is applicable not only to hydrolyte but also to enzyme. Inasmuch as glucose inhibits the action of emulsin and of maltase, it is to be supposed that the general configuration of these enzymes is comparable with that of glucose: the departure must be of such a character that the terminal element of the enzyme is deflected in the one case in the direction which brings it into harmony with the α-OR radicle whilst in the other it coincides in its space orientation with the β-OR radicle. It may be anticipated that β-methyl glucoside will be found to inhibit the action of maltase on maltose.

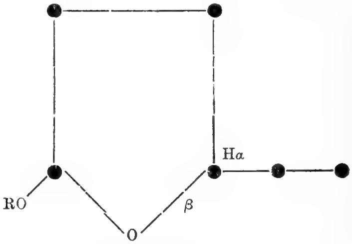
[*Note added May* 30, 1904.—This has since been found to be the case, and as before mentioned whilst α-methyl galactoside hinders the action of lactase, α-methyl glucoside is without influence on this enzyme.]

Extent to which Configuration may be modified.—Galactose differs from glucose merely in having the radicles attached to the fourth carbon atom in the reversed order: as galactose is fermented less readily than glucose and galactosides are less easily hydrolysed by enzymes than are the corresponding glucosides, the change in configuration, although sufficient to hinder action, does not prevent it.

Writing the skeleton formulæ of the two compounds side by side, it would seem that they differ but slightly in configuration, the alteration being one which only concerns the ring structure.



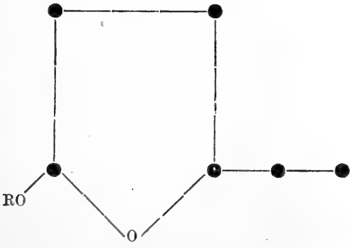
Glucoside.



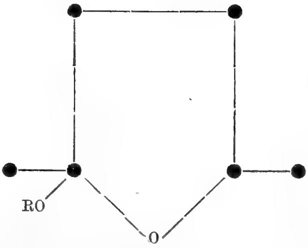
Galactoside.

Assuming that the attachment of the enzyme is in some way dependent on the hydroxyl groups, assuming also that the enzyme is provided with points of attachment which bring it into relation with the hydroxyl groups rather than with the oxygen atom in the ring, it may be supposed that the alteration in the configuration of the ring which is involved in the passage from glucose to galactose—following the establishment of a β- instead of an α-linkage in the ring—would be of less consequence than any shifting of the hydroxyl groups relatively to the ring plane. It is proposed to discuss the question of the relation of glucose to galactose more fully in a separate communication dealing with the relative stability of derivatives of the two compounds in presence of acids.

It remains only to consider why fructose, which is so closely related to glucose, should have no inhibiting influence except on invertase. Inasmuch as this enzyme is the only one at present known to us which can effect the separation of fructose from higher carbohydrates and inasmuch as it is influenced by fructose alone, it must be supposed that the structure of invertase is in close correlation with that of fructose. Assuming that fructose is also a γ-oxide, its structure is essentially different from that of glucose, as will be seen on comparing their formulæ.



Glucose.



Fructose.

If acceptance can be accorded to the arguments put forward in this communication, some progress will have been made towards unravelling the very complex phenomena presented by fermentative changes.

[D 1820, 7050, 8010.]

"Studies on Enzyme Action. IV.—The Sucroclastic Action of Acids as contrasted with that of Enzymes." By EDWARD FRANKLAND ARMSTRONG, Ph.D., Salters' Company's Research Fellow, and ROBERT JOHN CALDWELL, B.Sc., Clothworkers' Scholar, Chemical Department, City and Guilds of London Institute, Central Technical College. Communicated by Professor H. E. ARMSTRONG, F.R.S.. Received April 5,—Read April 28, 1904.

D 1820	Milk sugar, hydrolysis by acids.
D 7050	Hydrolysis of sugars by acids and by enzymes contrasted. Nature of active system and explanation of influence of concentration and of temperature.
D 8010	Enzyme action contrasted with that of acids.

Not only are the various bioses hydrolysed at very different rates by enzymes but they are also known to differ in their behaviour towards acids: cane sugar being hydrolysed with the greatest facility, whilst maltose is acted upon but slowly. The experiments described in this communication were instituted primarily with the object of ascertaining the behaviour of milk sugar, of which nothing was known.

The hydrolysis of cane sugar under the influence of acid was carefully investigated by Wilhelmy as far back as 1850, with the aid of the polariscope, then a new instrument. It was shown by him that nversion follows the logarithmic law

$$K = \frac{1}{t} \log \frac{a}{a-x},$$

which at a later date became regarded as the general law of mass action. Subsequent workers have studied the action more in detail, but the object of several of the latter inquiries has been rather to determine the relative activities of different acids and to study the application of the ionic hypothesis in such a case of hydrolysis.

Maltose has been shown by Sigmond* to exhibit the same general behaviour as cane sugar towards acids, whilst affording very different constants.

The only other glucose derivative which has been studied is salicin.†

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* 'Zeit. Phys. Chem.,' vol. 27, p. 385.

† Noyes and Hall, 'Zeit. Phys. Chem.,' vol. 18, p. 240.

Since the above was written, a paper has appeared by Noyes and others* on the hydrolysis of maltose and dextrin by dilute acids, which in the main confirms Sigmond's results. The hydrolysis of starch by dilute acids under pressure at temperatures about 100°, was studied in detail by Rolfe and Defren in 1896.†

Although the behaviour of cane sugar in any particular solution is fairly in accordance with the law of mass action, marked departures from the law are observed in contrasting the effects produced by varying the concentration whether of sugar or of acid. Not only does the value of K increase as the concentration of the hydrolyte is increased; it also increases beyond the proportionate value when the concentration of the acid is increased.

The outcome of the experiments described in the following pages is, briefly stated, as follows: In the first place, it is shown that, within certain limits, milk sugar is hydrolysed in accordance with the logarithmic law; but that in somewhat concentrated solutions there is a marked tendency for "reversion" to take place, so that the course of change in the later stages of hydrolysis departs from this law. The rate at which milk sugar undergoes hydrolysis compared with that at which cane sugar is affected is shown to be remarkably slow: yet the effect of increasing the concentration is found to resemble that produced by changes of concentration in the case of cane sugar and maltose; moreover, the products of change are found to exercise an influence on the rate of change comparable with that exercised by milk sugar itself. Finally, it is shown that an increase in temperature has even more influence on the rate of change of milk sugar than on that of cane sugar.

Experimental Method.—The method adopted in determining the rate of change was practically that used by previous workers; but, as milk sugar is hydrolysed very slowly at ordinary temperatures, the experiments were carried out at 60° C. On account of the small change in the specific rotatory power which milk-sugar solutions exhibit when hydrolysed, it was necessary to work with strong solutions—the solution most frequently used contained 18 per cent. of sugar, *i.e.*, 0·5 gramme molecular proportion per litre; this was hydrolysed by an equivalent solution of chlorhydric acid. The solutions of acid and sugar, heated to 60° and then mixed, and the mixture maintained at 60°. Portions were taken out at stated intervals of time: each sample was quickly cooled and its optical rotatory power determined in a 2 dcm. tube. As the heating was often extended over several days, it was necessary to cover the solution in the flask with a layer of molten paraffin. In order to make sure that no evaporation had taken place, the solution was titrated with alkali at the end of

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* 'J. Am. Chem. Soc.,' 1904, vol. 26 p. 266.

† 'J. Am. Chem. Soc., vol. 18, p. 869.

each experiment. In order to determine the end value, a portion of the solution was heated on the water bath during 2 hours, a period which sufficed to effect complete hydrolysis without giving rise to any marked secondary change such as always occurs on prolonged heating at the temperatures at which the rate of change was studied. The ratio between the rotatory power at complete change and the initial value was found, as the mean of three accordant observations, to be 1·285.

The results of a complete series of observations are recorded in the following table. It is to be noted that in calculating the constant from the logarithmic equation, *t* is taken in minutes. To make the results comparable with those of other workers, the velocity constant K is expressed in terms of a gramme molecular proportion of acid per litre; this merely involves the multiplication of the constant deduced from the results approximately by two.

Table I.—18 per cent. (0·5 gramme molecule) Milk Sugar, 0·5007 gramme molecule HCl. Temperature 60°·1.

Time in hours.	α_D.	Per cent. hydrolysed.	$\frac{10^4}{t}\log_{10}\frac{a}{a-x}$.
0	19°·55	0·0	—
5	20·22	12·04	1·85
9	20·62	19·2	1·71
15	21·27	30·8	1·78
20	21·83	40·9	1·90
25	22·17	47·0	1·84
30	22·47	52·4	1·79
40	23·23	66·0	1·95
62	24·23	84·0	[2·14]
87	24·93	96·5	[2·81]
134	25·15		——
184	25·28	Mean	1·83
216	25·56		
Complete change	25·12		

K = 3·65.

In a second series of experiments, the value deduced from the first eight successive observations—during which about two-thirds of the sugar had undergone change—was K = 3·42.

In the interests of space, as the quotation of long series of values serves no useful purpose, in discussing our remaining observations it will suffice, as a rule, to give the mean value of K obtained in the manner described. It is, of course, to be understood that K is derived from not less than six accordant readings taken at regular intervals during a period of 30—40 hours.

It will be seen that after about 40 hours, when hydrolysis is half completed, the values in the last column are no longer constant but increase; after 134 hours, the value of α_D actually rises above that corresponding to complete conversion into glucose and galactose. This increase was presumably to be ascribed to the formation of compounds of high specific rotatory power from the *products of change*: to test the correctness of this conclusion, experiments were made in which solutions of glucose and galactose were heated at 60° during several hours with chlorhydric acid of the strength used in the previous experiments. As Tables II and III show, in each case, a rise in α_D was observed but the rotatory power did not become constant until after about 250 hours, equilibrium being only slowly established between the monosaccharide and its reversion products. The departure during the later part of hydrolysis from the logarithmic law may therefore with some degree of probability be ascribed to some form of reversible change. It may be pointed out that Wohl* has observed a similar change in highly concentrated solutions of glucose and fructose when these are heated with small quantities of chlorhydric acid on the water bath; and that Fischer† prepared the biose isomaltose by the action of cold strong chlorhydric acid on glucose.

Table II.

27 per cent. Glucose.

Time in hours.	α_D.	Successive increase per hour.
0	28°·67	—
8	28·85	0·022
16	29·03	0·022
22	29·13	0·016
44	29·40	0·012
96	29·93	0·010
166	30·42	0·007
200	30·58	0·005

Table III.

27 per cent. Galactose.

Time in hours.	α_D.	Successive increase per hour.
0	42°·75	—
8	42·70	—
16	43·03	0·041
22	43·23	0·033
44	43·50	0·012
72	43·90	0·014
96	44·07	0·007
166	44·28	0·003

Hydrolysis of Maltose.—Owing to the great variation in the value of K produced by alterations in concentration and temperature, it was impossible to compare our results for milk sugar directly with those of Sigmond‡ for maltose. Accordingly, the value of K was determined for maltose under the conditions observed in our experiments with milk sugar.

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* 'Ber.,' 1890, vol. 23, p. 2084.

† 'Ber,' 1890, vol. 23, p. 3687.

‡ *Loc. cit.*

Table IV.—18 per cent. (0·5 gramme molecule) maltose, 0·5007 gramme molecule HCl. Temperature 60°·1.

Time in hours.	α_D.	Per cent. hydrolysed.	$\frac{10^4}{t}\log_{10}\frac{a}{a-x}$.
0	23°·28	0·0	—
8	20·02	23·5	2·43
16	17·92	38·7	2·22
24	16·12	51·7	2·20
50	12·58	77·3	2·15
72	11·22	87·1	[2·06]
96	10·45	92·7	[1·97]
144	9·85		
168	9·95		
216	9·94		
Complete change	9·44		
		Mean	2·25

K = 4·49.

It will be noted that the values in the last column begin to fall after 50 hours, when about three-quarters has undergone change. As the hydrolysis of maltose is carried to an end, the value of α_D falls to a minimum, then rises slightly, finally remaining steady; the theoretical minimum rotation, however, is never attained. This is easily explicable as the consequence of the secondary changes brought about by the action of the acid on the glucose.

The following table shows at a glance the vast difference in the values of K for milk sugar and maltose as compared with that for cane sugar. This last value has been calculated with the aid of Arrhenius's temperature equation from the 47·6 value at 25° given by Ostwald.

Table V.

Sugar.	K at 60°·1.	Ratio.
Milk sugar	3·53	1·0
Maltose..................	4·49	1·27
Cane sugar	4378·0	1240·0

Although it is not yet possible to explain this very striking difference, it must be remembered that cane sugar has a peculiar structure: maltose and milk sugar are closely related in structure, one-half of the molecule still showing the properties of glucose; but cane sugar has none of the properties either of glucose or of fructose.

Influence of Concentration of Hydrolyte.—Whereas, in the case of enzymes, an increase in the concentration of the sugar never increases the rate of hydrolysis, in the case of acids an increase in the concentration of the hydrolyte actually hastens the rate of change. This is known to be true both of cane sugar and of maltose. Our experiments show that it is equally true of milk sugar. Using a half normal ($\frac{1}{2}$ gramme molecular) solution of HCl, we have found that K has the following values:—

Table VI.

Concentration of sugar.	K.
9 per cent.	2·88
18 "	3·53
27 "	4·10

From these figures the following empirical law may be deduced connecting K with p, the concentration in grammes per 100 c.c.:

$$K = 2{\cdot}27\,(1 + 0{\cdot}03\,p).$$

Arrhenius has calculated a similar expression from Spohr's results for cane sugar, viz., $K = 19{\cdot}26\,(1 + 0{\cdot}0131\,p)$.

It would, therefore, appear that the rate at which hydrolysis takes place is more than twice as much influenced by changes in concentration in the case of milk sugar than in that of cane sugar.

Influence of the Products of Change.—The influence of the products of change on the rate of hydrolysis is of particular interest in view of the results obtained with enzymes. In the case of acids the products accelerate instead of retarding the change but they exercise no selective influence; moreover, about the same effect is produced by the addition of equal weights of glucose, of galactose or of milk sugar or even of the equivalent quantity of a neutral salt.

Table VII.

Amount added to 18 per cent. solution of milk sugar (K = 3·53).	K.
9·0 grammes milk sugar	4·10
9·0 " glucose, 1st expt.	3·89
9·0 " " 2nd "	4·06
9·0 " galactose	4·14
3·7 " potassium chloride	3·79

Hydrolysis by Sulphuric Acid.—Several experiments were made, with an acid equivalent in strength to the chlorhydric acid used in the experiments previously referred to.

Table VIII.—18 per cent. (0·5 gramme molecule) Milk Sugar, 0·25 gramme molecule H_2SO_4. Temperature 60°·1.

Time in hours.	α_D.	Per cent. hydrolysed.	$\frac{10^4}{t}\log_{10}\frac{a}{a-x}$.
0	19°·37	0·0	—
6	19·73	6·5	0·81
17	29·22	15·4	0·71
42	21·50	38·5	0·84
66	22·38	54·5	0·86
90	23·00	65·7	0·86
140	24·02	84·2	0·96
Complete change	24·89		
		Mean	0·84

K = 1·68.

In a second experiment a value K = 1·70 was obtained.

The value for K thus arrived at is much smaller than that deduced from the experiments with chlorhydric acid; but if the comparison be made for acids of the same molecular strength the results are nearly identical, since 2K = 2*x* 1·69 = 3·38.

It will be noticed that in this instance there is little or no evidence of reversion.

Influence of Temperature.—We have determined the rate at which milk sugar undergoes hydrolysis at different temperatures in order to be able to contrast its behaviour in this respect with that of other sugars. The following table gives the values of K at three temperatures obtained on hydrolysing a solution containing 0·5 gramme-molecular proportion of milk sugar by means of a 0·5 gramme-molecular proportion chlorhydric acid.

Temperature.	K.
60°·1	3·53
74·1	25·05
99·0 (approx.)	334·2

It is apparent from these numbers that an increase of about 14 per cent. in the value of K takes place per degree and that the rate of hydrolysis of milk sugar is even more influenced by temperature than is that of maltose. The difference is better seen on comparing the constants calculated for each sugar from Arrhenius's temperature equation.

Sugar.	Acid.	q/2.
Cane sugar	HCl	12,820*
Maltose	HCl	17,127†
Milk sugar	HCl	19,105
”	H_2SO_4	18,424

* Arrhenius, *loc. cit.*

† Sigmond, *loc. cit.*

It will be seen that the value of the temperature constant for milk sugar is nearly half as great again as that for cane sugar.

That temperature changes have a similar effect on the hydrolysis of milk sugar by sulphuric acid is shown by the following figures, obtained by hydrolysing a 0·5 gramme-molecular proportion solution of the sugar by means of 0·25 gramme-molecular proportion acid.

Temperature.	K.
60°·1	3·36
74°·1	22·5

Lastly, the effect of adding a 0·5 gramme-molecular proportion of potassium chloride at the two temperatures was determined; it will be seen that this salt has a considerably greater accelerating influence at the higher temperature.

Temperature.	K with KCl.	K without KCl.
60°·1	3·79	3·53
74° 1	29·0	25·05

It is to be noted that maltose (K = 4·49) and milk sugar (K = 3·53) are hydrolysed at similar rates at 60°·1; as the rate of hydrolysis is affected to different extents by a rise of temperature in the two cases, there will be a temperature at which the two should be hydrolysed at the same rate; and above this temperature milk sugar should be hydrolysed more rapidly than maltose. The temperature at which the two sugars should behave alike, deduced from Arrhenius's equation, is about 77°; we intend comparing the hydrolysis of the two sugars at this temperature.

Theory of Hydrolysis by Acids.—When the action of enzymes on the sugars is contrasted with that of acids, the enormous difference in the rates at which enzyme and acid effect hydrolysis is very striking; thus, whereas an extract of lactase prepared as described on pp. 503 to 504, to which sufficient milk sugar has been added to give a 5-per-cent. solution, will hydrolyse about one-fourth of the sugar at 35° in about an hour, it takes twice normal hydrogen chloride at the same temperature about 5 weeks to effect the same amount of hydrolysis.*

Nevertheless, a very simple explanation of the action of acids may be given if the problem be considered from a point of view similar to that applied to enzyme action. In the first place, it may be assumed that *an active system* is formed by the combination of a part of the sugar with a part of the acid; and as the water molecules in the solu-

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* Although no information is at present forthcoming admitting of a definite conclusion, it cannot be doubted that the amount of enzyme made use of in hydrolysing sugars, regarded as a molecular proportion, must be extraordinarily small. Any such conclusion as this makes their action appear all the more remarkable.

tion are attracting both sugar and acid molecules, that there is so to speak competition between the water and the sugar for the acid : there will, therefore, be at any given temperature an equilibrium between water, sugar and acid, depending on the relative proportions of these three constituents; a change in any one of them will necessarily also change the position of the equilibrium and therefore also the proportion of the combination of acid and sugar present.

At any moment during hydrolysis, the sugar is not susceptible to change as a whole: only the active system, formed by the combination of acid and sugar, including some limited number of water molecules, is concerned.

In the experiments hitherto made the proportion of acid molecules present has always been large, so that the magnitude of the active system present cannot have been negligible compared with the total amount of sugar: in such cases it was not to be expected that the change would prove to be a linear function of the time. If, however, a proportion of acid be used in some degree corresponding to the proportion of enzyme which is commonly used, it is to be expected that a linear period will be apparent.

To test this point experiments were made in which N/100 hydrogen chloride was used to hydrolyse a gramme-molecular proportion of cane sugar at 20° : in every case distinct indication was obtained that, at first, equal amounts of sugar were changed in successive equal intervals of time, change proceeding according to the logarithmic law only during the later stages.

It does not appear desirable to put forward our results relating to this question at present, as we are not satisfied with the degree of refinement we have reached in our experiments: to determine the law for very small proportions of acid, it will be necessary to make exact observations and especially to regulate the temperature with excessive care, as slight differences in temperature produce relatively great changes.

Effect of Altering the Amount of Acid.—It is well known in the case of cane sugar that an increase in the proportion of acid used to effect hydrolysis is followed by a more than proportionate increase in the rate of change. The effect of an increase in the amount of acid must obviously be to disturb the equilibrium in the direction of increasing the magnitude of the active system; it may be supposed that, on the whole, when the amount of acid is increased, the sugar is the greater gainer and that the increase in the active system is, therefore, more than proportionate to the increase in the amount of acid.

Effect of Altering the Amount of Hydrolyte.—Any considerable increase in the amount of sugar present must have the effect of diminishing the attraction exercised by the water upon the acid and consequently must increase the stability of the combination of sugar with acid—in other words, it must materially increase the magnitude of the active system: this would lead to an increase in the rate of change.

Any substance having an attraction for water should exercise a similar influence: it is easy to understand that, in the case of milk sugar, as we have shown, the rate of change is as much accelerated by either glucose or galactose as it is by milk sugar and that the neutral salt, potassium chloride, exercises about the same influence as one of these sugars when used in equivalent amount. It is well known that the hydrolysis of cane sugar is hastened by neutral salts,* but it has not been noticed previously that the products of change exercise an accelerating influence.

Influence of Temperature.—The effect of temperature on the rate at which chemical changes generally take place has been discussed at length by Arrhenius.† Frankly recognising that the rate at which change increases as temperature rises is too great to be ascribed to an increase of the "ionisation" or to a diminution of the viscosity of the solution, Arrhenius has introduced the conception of an active part or mass but without in any way defining the nature of this mass. In applying this conception specially to the hydrolysis of cane sugar, he assumes that the formation of the active mass involves an absorption of heat, and, further, that although it is present in very small proportion relatively to the total sugar, the proportion increases rapidly as the temperature rises. Adopting the ordinary convention he supposes that the active mass undergoes hydrolysis at the expense of the hydrogen ions of the acid, at a rate, however, that is almost independent of the temperature. Availing himself of the equation which van't Hoff had put forward to express the alteration of equilibrium with temperature in the case of reversible interactions, he deduced the expression

$$K_{t_1} = K_{t_0} . e^{\frac{q}{2} \cdot \frac{T_1 - T_0}{T_1 T_0}} ,$$

where K_{t_1} and K_{t_0} are the velocities at two temperatures and q is the heat absorbed in the formation of the active mass—this being an expression from which it is apparent that a rise in temperature should favour the formation of the active mass and *vice versâ*. Arrhenius applied this expression to a large number of simple chemical changes, especially to cases of hydrolysis, for which the rate had been measured, and showed that it was in agreement with experimental facts.

The idea of an active mass was extended to explain the influence of salts: it was arbitrarily assumed by Arrhenius that these increased the proportion of the active mass, notwithstanding that they decreased the number of hydrogen ions.

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* Spohr, 'Zeit. Phys. Chem.,' 1888, vol. 2, p. 194.

† 'Zeit. Phys. Chem.,' 1889, vol. 4, p. 226.

In a lengthy paper published 10 years later,[*] in which the results obtained in the meantime by other workers were discussed, he made an important addition to his theory by showing that the increase in K with concentration is in exact correspondence with the increase of osmotic pressure of the solution. He vaguely referred to the influence of neutral salts as being of the same order as that exercised by an increase in the concentration of the hydrolyte.

Exception was taken by Lippmann,[†] in 1900, to the explanation put forward by Arrhenius, on the ground that it was impossible, in the case of sugar, to understand how any change could take place which would render one part more active than another. Lippmann also sharply criticised the attempt made by Euler[‡] to interpret the hydrolysis of sugar from a purely ionic standpoint. Euler's[§] answer to these criticisms in no way served to remove the difficulties raised by Lippmann,[||] but rather the contrary.

There can be no doubt that in discussing hydrolytic changes generally the conception of an active mass cannot be dispensed with; it remains only to give precision to the conception by defining the precise character of such a mass. On the assumption made in this and the two preceding papers, the active mass postulated by Arrhenius is neither more nor less than the system composed of the hydrolyte, the enzyme or acid and a certain limited number of water molecules. Although the formation of such a system is probably attended with a slight evolution of heat, its immediate breakdown should involve the absorption of heat and this change should be reversible. Etherification phenomena generally and the change of isodynamic systems into one another all satisfy these conditions. There can be little doubt that the breakdown of complex systems, such as are here contemplated, would be most materially influenced by changes in temperature, and that the rate of change would advance rapidly as temperature rose. The tendency to form such systems would obviously diminish somewhat as temperature rose.

In conclusion, it is desirable to lay emphasis on the differences noticeable in the behaviour of enzymes and acids respectively as hydrolytic agents: it appears not improbable that this difference is due mainly, if not wholly, (1) to the superior affinity of the enzymes for the carbohydrates, (2) to the very different behaviour of the two classes of hydrolysts towards water—which is a consequence of the colloid nature of the one and the crystalloid nature of the other. It appears possible to explain such differences as are apparent in their

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[*] 'Zeit. Phys. Chem.,' 1899, vol. 28, p. 317.

[†] 'Ber.,' vol. 33, p. 3560.

[‡] 'Ber.,' vol. 33, p. 3202, and 'Zeit. Phys. Chem.,' vol. 36, p. 641.

[§] 'Ber.,' vol. 34, p. 1568.

[||] 'Ber.,' vol. 34, p. 3747.

behaviour to these circumstances alone. If osmotic pressure be regarded as the outcome of some reciprocal interaction between solvent and dissolved substance rather than as a mechanical effect, the fact that the variation in osmotic pressure and in the value of K with concentration follow the same law may be taken as evidence that the increase in K with concentration when a sugar is hydrolysed with the aid of an acid is the consequence of a diminution in the influence exercised by the water.

*[D 7050, 8020; Q 1230, M 3010.]

"Enzyme Action as bearing on the Validity of the Ionic-Dissociation Hypothesis and on the Phenomena of Vital Change." By Henry E. Armstrong, Ph.D., F.R.S.

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*D 8030 Formation of carbohydrates in protoplasm.

D 1810, Q 1440 } Glucosamine, significance of, in enzymes.

On several occasions of late years, I have protested against the dogmatic attitude assumed by the advocates of the ionic-dissociation hypothesis of chemical change and have remarked on the danger of allowing a purely mathematical treatment to supersede a careful, unbiassed consideration of the facts as these present themselves to the chemist. I have insisted on the limited application of the hypothesis—especially in explanation of the behaviour of the large majority of organic compounds; and have contended that an association hypothesis is preferable and of far wider application: yet, in so doing, I have always recognised that the dissociation hypothesis is often susceptible of numerical treatment in a way which places it at a great advantage.

Twenty years ago, I contended that the solvent played as important a part as the dissolved substance in electrolytic changes; then and for some time afterwards the dissociationists regarded the solvent as a mere screen. Gradually they have been led to recognise that the solvent plays an active part and "ionising" solvents are now freely spoken of: the admission has been made, however, tacitly and without recognition of the fact that the difference of opinion is now reduced to the one question—whether ions enjoy separate existence in solution: whether, for example, in a solution of hydrogen chloride, free hydrogen

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* [Index supplied by author, classified according to the schedule of the International Catalogue of Scientific Literature.]

and chlorine ions are present to the extent of 70 odd per cent. I have ventured to characterise such an assumption as not merely unnecessary but eminently improbable: the contradictions involved in the conception being extraordinary—for while we are asked to believe this of hydrogen chloride, we are assured that a substance of such very inferior stability as mercuric chloride exists in solution practically "unionised." There is danger that under the guidance of mathematicians anxious to negotiate numerical agreements, we may lose our sense of proportion as chemists; I believe the danger to be a very serious one, against which it is necessary to make some protest. We shall fail in securing the object we aim at if we allow the element of authority to intrude in any way into our work: it will cease to be scientific; it will be impossible to claim for it any special educational value; an attitude of doubt rather than one of conscious certainty is essential to progress.

Although the solvent is now regarded as of importance, its peculiar importance and functions are far from being sufficiently recognised. It is necessary, in fact, to urge that the nature of the correlated processes of electrolysis and chemical change need to be considered most carefully, from every point of view, especially with reference to the functions of the solvent and the part played by residual affinity. Results such as have been obtained by Brereton Baker—proving that interactions occur only when a somewhat complex conducting circuit is established—must be fully taken into account: the observations of this chemist on the formation of ammonium chloride, for example, are of the utmost consequence; yet the advocates of the ionic hypotheses simply disregard all such evidence.

In my paper presented to the Society in 1885, I contended that the special influence exercised by water is to be attributed to the residual affinity possessed by the oxygen atom of the water molecule; this view has gradually grown in popularity of late years in consequence of the discovery by Collie and Tickle, von Baeyer and others of compounds in which oxygen apparently functions as a tetrad. There has naturally been a tendency to extend the view to other elements and to attribute the "ionising" power of liquid ammonia, hydrogen cyanide, etc., to residual affinity. But it is necessary to be cautious in accepting the conclusion that these solvents are strictly comparable with water. Water is perhaps peculiar in the extent to which it conditions the electrolysis of substances which are not electrolytes *per se.* In at least a large proportion of the cases in which conducting solutions in solvents other than water have been obtained, substances have been used which are conductors *per se* in the liquid (fused) state: it may be, therefore, that the conductivity of such solutions is but a consequence of the presence of the liquid substances and is largely if not wholly independent of the solvent, the relatively high conductivity of some of the solutions being perhaps conditioned by the slight viscosity of the solvents. On the other hand, it is highly probable that in many cases in which slight conductivity has been observed insufficient care has been taken in the purification of the materials used.

In discussing the nature of chemical change in my Presidential Address to the Chemical Society in 1895,* I specially drew attention to the difficulty of explaining the behaviour of enzymes—particularly their selective action—as hydrolytic agents by the ionic-dissociation hypothesis: the view was then advocated that their function consists in bringing water into conjunction with the carbohydrate *by combining with both.* The evidence brought forward in the two previous papers† on enzyme action appears to me to be of consequence in this connection: and I venture to think that the case is very materially strengthened by the considerations advanced by E. F. Armstrong and R. J. Caldwell in discussing the action of acids on the sugars.† The selective character of the effect produced by enzymes, which has now been demonstrated in several ways, may almost be regarded as final proof that action is determined by association—not by dissociation. And if in such a case we are led to admit that change is the immediate consequence of effective association, there can be no difficulty in regarding chemical changes generally as of this character.

In their behaviour towards acids, the sugars exhibit peculiarities which are so little in accordance with the ionic-dissociation hypothesis that we are almost compelled to admit that in this case also hydrolysis is dependent on association. The explanation which this latter view affords of the great influence which temperature changes have on the rate of change is in itself striking evidence in its favour—especially as the explanation set forth in their paper by E. F. Armstrong and R. J. Caldwell is applicable to chemical changes generally.

In advancing proof that the enzymes are built of dimensions which enable the molecule to become associated *at several points* with the carbohydrate molecule, E. F. Armstrong has carried a step further the argument which I advanced in explanation of fermentation proper in 1895.‡ The statement then made was as follows:—"Supposing that the protoplasmic hydrolyst were to condition the formation of a conducting circuit in which any two of the carbon systems (CH_2OH, CHOH or COH) of the glucose molecule and water molecules were included, if the total hydrolytic change which could take place in such a circuit were exothermic, even if the change affecting the one group involved an expenditure of energy, water could be electrolysed and its hydrogen would effect the withdrawal of OH from the one group and its displacement by hydrogen, while oxygen would be added to the other

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* 'Chem. Soc. Trans.,' vol. 67, p. 1122.

† *Supra,* pp. 526—537.

‡ *Loc. cit.,* p. 1137.

group, it might be either directly or in consequence of the displacement of H by OH. The different effects produced by different organisms, on this hypothesis, would be the consequence of the hydrolysis affecting different systems. As most, if not all, fermentable compounds are asymmetric and the enzymes are undoubtedly also asymmetric bodies, the direction of attack would depend on the character of the asymmetry of both hydrolyte and hydrolyst; moreover only compatible hydrolysts, *i.e.*, those compatible with the hydrolyte, would condition hydrolysis and fermentation."

I then considered that fermentative changes were "presumably functions of the protoplasm." Buchner's researches, however, would seem to necessitate a non-vitalistic interpretation of the phenomena. He has unquestionably proved that fermentation can take place to some extent outside the living cell, but the effect observed has always been of a very transient character, unlike enzyme action in general; this is the more remarkable, as the amount of energy liberated during fermentation is by no means inconsiderable; and also because Buchner's observations would lead us to correlate alcoholic fermentation more closely than we have hitherto done with the ordinary sucroclastic action of enzymes. A system such is pictured of enzyme and hexose might well break down into the ordinary products of fermentation, if energy from outside were in some way initially impressed upon the system, as might well happen if it were a part of the protoplasmic complex. It is conceivable that Buchner may have dealt with systems intermediate in complexity between the enzymes proper and the protoplasmic complex in which exothermic change is still in progress; but ephemeral systems only. On the other hand, the presence of proteoclastic enzymes in the expressed fluid may be a cause of the decay of the fermentative activity. But I venture to think the case is not yet fully proved in Buchner's favour; and however willing we may be to regard alcoholic and lactic fermentations as enzymic changes, such an explanation appears to be altogether inapplicable, for example, to butyric fermentation or to fat formation: we have no reason at present to suppose that the various kinds of fermentation are brought about in essentially different ways.

In discussing the changes which attend fermentation, I pointed out, in 1895, that "Such changes are known to occur entirely within the cell and are presumably functions of the protoplasm; in other words, they probably occur within very complex molecular systems of extreme instability, perhaps under the influence of, in contact with, the very same hydrolyst (enzyme) which is so active, when separated from the cell, in promoting the hydrolysis of cane sugar; or if not, of substances of a similar nature." This explanation has gained greatly in probability now that it is established that the sucroclastic enzymes are very closely related to the alcoholic ferments.

If invertase were present as a branch of the protoplasmic complex, any hexose compatible with it would be attracted by it; and when once in association with the complex, the sugar molecule might undergo changes such as are contemplated in Baeyer's explanation of fermentation, the energy required to initiate these changes being derived from exothermic changes proceeding simultaneously in some other part of the complex.

But it is possible to extend the conceptions which we owe to van't Hoff and Emil Fischer still further. My son and I agree in thinking that the assumption that enzyme and hydrolyte become associated will make it possible to understand a variety of phenomena which have not yet received any satisfactory explanation. For example, when condensed under laboratory conditions, formaldehyde gives rise to an inactive mixture of *d*- and *l*-hexose; but under natural conditions only *d*-fructose and *d*-glucose are produced. *If condensation took place in immediate contact with a compatible enzyme, it is conceivable that a bias would be given to the synthesis sufficient to determine change wholly in the one of the two possible directions.*

It is a striking fact that yeast, which so readily ferments the hydroschists of cane sugar, contains the two enzymes—invertase and maltase—which are respectively compatible with the two hydroschists. Assuming that these enzymes are present as branches of the protoplasmic complex, it is easy to understand, from the point of view now set forth, why yeast should be able to ferment both fructose and glucose easily.

On the other hand, the existence of contiguous maltase and invertase branches in the protoplasmic complex might determine the formation of glucose and fructose in contiguity; and these might then unite—thus giving rise to cane sugar. This conclusion is of special interest, bearing in mind the observations of Horace Brown and Morris* that cane sugar rather than maltose is a primary product of plant metabolism. Both invertase and maltase are known to be present in plants.†

The formation of starch may be looked at from a somewhat similar point of view. It may be supposed that glucose is produced initially as an open chain compound and that the formation of the γ-oxide ring confers stability on the molecule; it may well be that starch is formed by condensation of a number of such merely potential glucose

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* 'Chem. Soc. Trans.,' 1893, vol. 63, p. 604.

† I am still inclined, however, to favour the view which I expressed in 1890 ('Chem. Soc. Proc.,' p. 56), that maltose is the precursor of cane sugar in the plant: it is difficult to avoid this conclusion in view of the fact that cane sugar is formed when barley embryos are fed on maltose but not when they are fed on glucose, although in the latter case the plantlet is found to contain invert sugar.—[*Note added May* 28, 1904.]

molecules prior to the closure of the oxide ring. The formation in this manner of starch rather than of the far simpler glucose or maltose might be conditioned by the production at protoplasmic surfaces in close contiguity and in suitable orientation of the necessary number of glucose or it may be maltose elements to form the starch molecule.

The origin of albuminoids may be regarded from a similar point of view; and if carbohydrate elements were associated with the protoplasmic complex, they might serve to determine the formation of compatible enzymes in the same way that the enzyme elements may be supposed to determine the formation of compatible carbohydrates.* *In short, the protoplasmic complex may be regarded as built up of a series of associated templates which serve as patterns to determine change in the various directions necessary for the maintenance of vital processes and of growth.*

There are many directions in which such a principle can be applied so as to form the basis of experimental inquiry. Its bearing on the formation of anti-toxins, for example, is obvious; and it may be worth while to ascertain, if possible, whether in cases of acute diabetes the failure of the organism to utilise glucose may not be the consequence of the disappearance of the maltase element from the protoplasm of the organs which ordinarily condition its utilisation.

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* This assumption almost necessarily involves the conclusion that a carbohydrate in some way enters into the composition of the sucroclastic enzyme. From this point of view, it is perhaps significant that glucosamine is obtainable from many albuminoids especially as mucin from human saliva is said to afford 30 per cent. of its weight of this substance. The production of levulinic acid from yeast nuclein is equally noteworthy. But if the activity, for example, of the amyloclastic enzyme should prove to be referable to the presence of a carbohydrate component and this be glucosamine, it will follow that the influence is exerted by a compound in some measure similar to the carbohydrate in configuration—not by one opposed to it in symmetry.—[*Note added May* 28, 1904.]

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