PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON.

SERIES A

CONTAINING PAPERS OF A MATHEMATICAL OR PHYSICAL CHARACTER.

FOR THE YEAR 1897.

VOL. 190.

LONDON:

PRINTED BY HARRISON AND SONS, ST. MARTIN'S LANE, W.C.,

Printers in Ordinary to Her Majesty.

1898.

WORKS PUBLISHED BY THE ROYAL SOCIETY.

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Adjudication of the Medals of the Royal Society for the year 1897, by the President and Council.

The Copley Medal to Albert von Kölliker, For.Mem.R.S., in recognition of his important work in Embryology, Comparative Anatomy, and Physiology, and especially for his eminence as a Histologist.

A Royal Medal to Andrew Russell Forsyth, F.R.S., for his Contributions to the progress of Pure Mathematics, and especially for his work in Differential Equations, and the Theory of Functions.

A Royal Medal to Lieut.-General Sir Richard Strachey, F.R.S., for his Researches in Geographical, Meteorological, and Botanical Science.

The Davy Medal to John Hall Gladstone, F.R.S., for his numerous Contributions to Chemical Science, and especially for his important work in the Application of Optical Methods to Chemistry.

The Buchanan Medal to Sir John Simon, F.R.S., for his Distinguished Services as an Organiser of Medical Sanitary Administration in this country, and as a promoter of Scientific Research relating to Public Health.

The Bakerian Lecture, "On the Mechanical Equivalent of Heat," was delivered by Osborne Reynolds, M.A., F.R.S., and W. H. Moorby.

The Croonian Lecture, "The Mammalian Spinal Cord as an Organ of Reflex Action," was delivered by Charles S. Sherrington, M.D., F.R.S.

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(A)

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I. *The Total Solar Eclipse of August* 9, 1896.—*Report on the Expedition to Kiö Island.*

By Professor J. Norman Lockyer, *C.B.*, *F.R.S.*

Presented by the Joint Permanent Eclipse Committee.

Received May 15,—Read June 17, 1897.

Contents.

I. Preparations for the Eclipse.

Although this expedition failed in its main objects, because of unfavourable weather, I think it desirable to place on record an account of the arrangements which had been made to secure observations, more especially as a new feature was introduced in the training of a large number of observers.

The weather chances in Japan and elsewhere, from accounts which I had received, were not very promising, and it was determined, therefore, to occupy a station in Lapland, where the chances were certainly better. One of the most convenient places of observation was Vadsö, but as another section of the expedition had selected this station, it seemed desirable to observe from some other point, so as to multiply the chances of obtaining results. I accordingly made representations to this effect to the Admiralty, and H.M.S. "Volage" was placed at my disposal, with instructions to select a station on the south side of the Varanger Fjord. The final choice of a place for observation was to depend upon examinations of suitable sites near the central line of eclipse, which as a result of local inquiries seemed to show the greatest probability of a fine morning on August 9th.

The Norwegian Government readily granted permission to land, and instructed the authorities at Vadsö to render assistance.

The party consisted of Mr. FOWLER, Dr. W. J. S. LOCKYER, and myself. Being a delegate to the International Conference on Bibliography, I could not leave London before July 22nd. Mr. FOWLER and Dr. LOCKYER, therefore, went on in advance, and joined H.M.S. "Volage" at Hammerfest on July 22nd.

Objects of Expedition.

The success which attended the use of the large scale prismatic camera during the total eclipse of 1893, indicated that spectroscopes of this form were the most important instruments which could be employed on an eclipsed sun. In the report on the results obtained in 1893* I have given a full account of the appearances shown on photographs taken with prismatic cameras, and have indicated the special points to be considered in interpreting them. It may be remarked, however, that among the special services rendered by the instrument are (1) the separation of the true coronal spectrum from the apparent one produced by reflected chromospheric light when a slit spectroscope is employed; (2) localisation of the different radiations, so that we get a complete separation of coronal from chromospheric light, and information relating to the distribution of any particular radiation; (3) the special facilities for photographing the phenomena in the lower parts of the sun's atmosphere, in the region of the so-called "reversing layer."

Profiting by previous experience, prismatic cameras of the highest available power formed the chief part of the instrumental equipment. These were the 6-inch prismatic camera employed in Africa in 1893, and a 9-inch which had recently been purchased for the Solar Physics Observatory. The work of each of these instruments was arranged to supplement that of the other in as many particulars as possible.

As long ago as 1871 I had occasion to lay stress upon the importance of securing a record of the integrated spectrum of the light proceeding from every part of the eclipsed sun. The use of an integrating spectroscope is now more necessary than ever, for the reason that the prismatic cameras define what part of the total light proceeds from the chromosphere and prominences, so that a simple subtraction gives us the spectrum of the corona. A large spectroscope, which will be described later, was accordingly prepared as an integrating spectroscope for use during the eclipse.

* 'Phil. Trans.,' A, 1896, vol. 187, pp. 551–618.

Another observation, which it was very important to make, was to note the presence or absence of indications of carbon and other substances in the corona. For this observation a 6-inch telescope, giving a small bright image, and a spectroscope with a silvered glass grating, were provided.

To supplement the work of the photographic spectroscopes, a number of prisms and small slit spectroscopes were taken out for use by such assistants as might be available.

When it was ascertained that an almost unlimited number of helpers was forthcoming, the programme of the expedition was extended so as to include records of as many as possible of the attendant phenomena, and sketches of the corona, with or without the disks introduced by Professor NEWCOMB in 1878 to protect the eye from the glare of the inner corona.

Selection of a Station.

Leaving Hammerfest on July 23rd, the "Volage" proceeded to the Varanger Fjord, and, after twelve hours' delay on account of fog, arrived off Kiö Island on the evening of July 24th. The observers, and a surveying party of officers and men under the command of Lieut. MARTIN, R.N., were landed here, with tents and other requisites for camping out, while the ship went on to Vadsö for mails and provisions, and to make inquiries of the Governor as to the local meteorological conditions.

The exploring party first landed on an island in Bras Havn. On July 25th the weather was so bad that little progress was made, but towards evening the island of Kiö was visited and several possible sites were marked out. On the following morning another visit to Kiö Island resulted in the final selection of a site, and the places for the various instruments were provisionally prepared.

H.M.S. "Volage" returned on the evening of July 26th, and operations were seriously commenced on the following morning, leaving very nearly a clear fortnight for preparations.

The island of Kiö lies nearly north of Bras Havn, at a distance of about a mile and a quarter. It is but small, and consists chiefly of *moutonnéed* gneiss rocks, which are in many places covered with peat to the average depth of a foot. With the willing help of bluejackets, the actual site was levelled and covered with pebbles from the beach, to minimise the ill effects of the soddened peat.

There was a perfectly clear horizon in all necessary directions, and a sea horizon to the north of east, where the eclipsed sun would be visible.

The nearest safe anchorage for the ship was Bras Havn, so that the observers and working parties had to travel by boats between Bras Havn and Kiö Island every day. The inconvenience of this was greatly reduced by the tents which had been lent by the War Department, in which the observers could rest and take their meals as occasion required.

The latitude of the observing station was 69° 54′ 55″ N., and the longitude

29° 46′ 10″ E., so that it was a little less than five miles south-east of the nearest part of the central line of eclipse.

Local Conditions of Eclipse.

The apparent semi-diameters of the sun and moon at the time of eclipse were respectively 15′ 48·4″ and 16′ 17·8″, while the duration of totality was 105 seconds. Assuming the chromosphere to be 10″ deep, a portion of it would thus be visible for 18 seconds after the commencement of totality, and another portion for 18 seconds before the end of totality.

The sun's altitude at the time of totality was a little over 14°, and its amplitude about 7° N. of E.

The calculated Greenwich time of the commencement of the total eclipse was August 8d. 15h. 57m. 42s. First contact took place at the approximate position angle 70° west of the north point, and the last at 111° east of the north point.

Erection of Huts and Instruments.

To secure proper foundations for each of the instruments, the surface peat was removed, and concrete bases laid down on the solid rock. The wood framing for the huts had been brought out from England, each piece carefully marked so that no time was lost in erecting them. The large hut for the 6-inch prismatic camera, as well as that for the siderostat, were almost completed and covered with Willesden canvas at the end of the first day, owing to the zeal of the chief carpenter, Mr. Martin, and his assistants.

July 28th, like the preceding day, was fortunately fine, and the huts were then completed and made to safely withstand the violent wind experienced. The photographic dark-room was also finished.

The following two days were wet and cold, but the huts being erected, it was possible to continue the erection and adjustments of the instruments, unpack and attend to various small matters. A tent, consisting of spars and sail cloth, was also fitted up as a shelter for the large integrating spectroscope.

During the next few days all the instruments brought from England by the "Volage" were completely mounted, and by the time of my arrival on August 2nd, they were in adjustment and ready for the eclipse.

The remaining instruments which I took out with me on the s.s. "Garonne" were also put in position without delay, so that almost a whole week was available for rehearsals and general training of those members of the ship's company who volunteered their assistance.

Trial photographs with all three instruments, taken on August 6th, at the same time in the morning as the eclipse would occur, showed all to be in perfect readiness and adjustment.

Organisation of "Volage" Observers.

In response to a general call for volunteers made by Captain KING HALL, R.N., officers and men to the number of 74 volunteered their assistance for securing observations of the eclipse. To utilize this help to the best advantage, demonstrations and lectures were given by Mr. FOWLER and myself in the Captain's cabin, which was willingly placed at my disposal for the purpose, and on deck. As a result of these, selections were made for the different branches of observations. All the volunteers were well prepared for the eclipse, and fully capable of using the instruments entrusted to them, assisting with the large instruments, or recording the phenomena for which they were told off.

The complete muster roll of observers and their division into parties was drawn up and handed to me by the Rev. E. J. VAUGHAN as follows :—

Personal Assistant to Mr. NORMAN LOCKYER.
Mr. HUGH B. MULLENEUX, Midshipman.

1. *6-inch Prismatic Camera.*
Mr. FOWLER.
Lieut. BEAL.
GEO. ROBERTS, A.B.
Private FRAS. HUSKISSON, R.M.L.I.
Private JOSEPH BRIGGS, R.M.L.I.

2. *9-inch Prismatic Camera.*
Dr. LOCKYER.
Mr. FITZWILLIAMS, Mid.
Mr. BRUCE, Mid.
PATRICK SULLIVAN, Lg. Shipwrt.
HARRY FROUD, A.B.
ALFRED WOOLLARD, A.B.

3. *Integrating Spectroscope.*
Lieut. MARTIN.
Mr. SILVERTOP, Mid.
Mr. BRENDON, Mid.
Mr. WOODBRIDGE, Mid.

4. *Disks 3′ and 5′.*
Capt. KING HALL. }
Mr. PARKER, Mid. } 3′ Eclipse Island.
FREDK. HESCH, P.O. 2 Cl. }
WILLIAM BOWDEN, P.O. 1 Cl. } 3′ Signal Island.
THOS. BRIDGEMAN, P.O. 2 Cl. }
JOHN HILYARD, Qual. Sig. } 5′ Eclipse Island.

5. *Sketches of Corona.*
Mr. CONSTABLE, Mid.
Mr. GREENE, Mid.
Mr. WARTON, Mid.
THOS. SUTHERLAND, S. Corpl.
ARCHD. WRIGHT, 2 Yeo. Sig.
EDWD. MARSHALL, Boy 1 Cl.
ROBERT ROBERTS, Boy 1 Cl.
WILLIAM HICKS, Qual. Sig.
CHAS. MILES, P.O. 1 Class.
CHAS. BENNETT, S.B. Attendant.
EDWARD MILLER, P.O. 1 Class.
JAMES BISS, Boy 1 Class.
WILLM. BARTON, Ord.
JOSEPH GALE, P.O. 1 Class.
EDWARD PEGLER, Lg. Sig.
CUTHBERT DAVIS, Lg. Sea.

6. *Colours of Landscape.*
Lieut. SINCLAIR. On board "Volage."
Staff Surgeon WHELAN. Signal Island.
Lieut. YELVERTON. }
HARRY BERESFORD, Yeo. Sigs. }
HARRY WHITE, Ch. Stoker. }
GEO. BENNETT, Lg. Seaman. } Eclipse Island.
HERBERT GAMBLER, A.B. }
JAMES HARDING, Stoker. }

7. *Shadow Phenomena.*
Private GEO. ALLEN, R.M.L.I.
Private FRANK BLANCHARD, R.M.L.I.
Private THOS. GAUNTLETT, R.M.L.I.
RICHARD COLLINGS, Blacksmith.

8. *6-inch Equatorial with grating.*
Mr. FOWLER. }
Lieut. CLINTON BROWN. } First and last contacts.
Mr. NORMAN LOCKYER. }
Lieut. CLINTON BROWN. }
Mr. BROOKS, Asst. Paymr. } During totality.
RODNEY MUNDAY, Sig. Boy. }

9. *$3\frac{3}{4}$-inch Telescope.*
Mr. NORMAN LOCKYER.
Lieut. HODGES.
HENRY LEWIS, 3rd Writer.

10. *Slit Spectroscopes.*
Lieut. LAW. }
CHAS. SMITH, Boy Writer. }
THOS. MAKEPEACE, Ch.E.R.A.
WILLM. WESTACOTT, Sig.

11. *Prisms for Rings.*
ALEX. DUNCAN, E.R.A.
THOMAS BROWN, E.R.A.

12. *Timekeepers.*
WILLIAM SMITH, P.O. 1 Cl.
ALFRED SAUNDERS, P.O. 1 Cl.
Mr. C. E. LLOYD THOMAS, Mid. (Chronometer).

13. *Contacts.*
1–4. Staff Paymaster RAMSAY. }
Staff Engineer UNDERHILL. } Signal Island.
1–4. Mr. FOWLER. }
Lieut. CLINTON BROWN. } Eclipse Island.
2nd. Mr. NORMAN LOCKYER.
3rd. Mr. NORMAN LOCKYER.

14. *Polariscope.*
Rev. E. J. VAUGHAN, M.A., Chaplain.
ROWLAND ALLISON, Armourer's Crew.

15. *Meteorology* (Thermometers).
JOHN YARDLEY, P.O. 2 Cl. }
CHAS. SYMES, P.O. 1 Cl. }
FRED FAIZELL, Stoker. }
WILLM. THRIPP, Stoker. } Eclipse Island.
ERNEST HURST, P.O. 1 Cl. }
T. CANNON, Lg. Sig. } Signal Island.

16. *Stars.*
Rev. E. J. VAUGHAN, M.A.
Lieut. B. YELVERTON (Dup.).
Lieut. HUGH F. SINCLAIR (Dup.).

17. *Landscape Camera.*
Marquis of GRAHAM. Eclipse Island.

18. *Observations of Shadow Bands.*
Staff Surgeon WHELAN, M.D. Signal Island.

The following were the intructions to the observers :—

"One volunteer in each subject to collect results and see that everything is signed. Also these must see Mr. LOCKYER in the course of this morning (August 9) to report all ready, &c."

Disks (F. HESCH, P.O., 2nd Cl., to report).

Disk observers, besides dictating what they see, should afterwards give a sketch if possible (small sun).

Sketches of Corona (Mid. CONSTABLE to report).

Sketchers, besides making drawings, should hand in a written statement of what they have seen. Both this and the sketches should be signed.

Colours of Landscapes (H. BERESFORD, Yeo. Sig., to report).

Besides filling up the form, each observer should hand in a signed statement of his impressions.

Shadow Phenomena (Private G. ALLEN, and R. COLLINGS, Blacksmith, to report).

(1.) Note shadow approaching from westward.
(2.) Give an idea of the apparent velocity.
(3.) Give an idea of the effects on colours, &c.
(4.) Give an idea of the effects on birds, &c.
(5.) How long visible before totality ?

Slit Spectroscopes (Lieut. LAW, and T. MAKEPEACE, Ch.E.R.A., to report).

(1.) What was the nature of the spectrum ?
(2.) Was it a continuous, or a bright line one ?
(3.) What were the colours of the bright lines ?
(4.) What were the colours of the brightest lines ?
(5.) General remarks.

Prisms for Observations of Rings (A. DUNCAN, E.R.A., to report).

(1.) Were there any bright rings ?
(2.) If so, what colours were they ?
(3.) Which ring was brightest ?
(4.) Which ring was broadest ?
(5.) General remarks.

Timekeepers (Mid. THOMAS to take charge of Admiralty chronometer).

(1.) Warn Mr. LOCKYER 5 minutes before G.M.T. of first contact.
(2.) Note times of first and last contacts as called by Mr. FOWLER.
(3.) Warn Mr. LOCKYER 10 minutes before G.M.T. of commencement of totality.

(4.) Note the time when Mr. LOCKYER calls "180°," and call "1 minute" after the lapse of 6 m. 50 s.

(5.) Note the times of beginning and end of totality, as signalled by Mr. LOCKYER.

W. SMITH and A. SAUNDERS (1st Cl. P.O.'s) to take charge of stop-watch.

(1.) At signal "go" from Mr. LOCKYER, SMITH, with back to the sun, to announce "105 seconds" and afterwards give the time remaining every 5 seconds by such calls as "95 seconds more."

SAUNDERS meanwhile to observe general phenomena.

(2.) At "65 seconds more," SAUNDERS to turn his attention to stop-watch and to call "60" simultaneously with SMITH, afterwards continuing the calls every 5 seconds. During the last 45 seconds SMITH to note the general appearances.

Contact Observations (Staff Paymaster RAMSAY, and Staff Engineer UNDERHILL).

(1.) Select coloured glasses to enable you to comfortably observe the sun before the eclipse begins.

(2.) Note time of first contact, as indicated by the chronometer provided.

(3.) Similarly note times of 2nd, 3rd, and 4th contacts.

(4.) Compare chronometer with ship's chronometers.

(5.) Note general impressions of phenomena.

Thermometers (C. SYMES, P.O., 1st Cl., to report).

(1.) Prepare forms for entering observations.

(2.) Set up screens 3 ft. 6 in. high to shield thermometers from direct rays of sun, the thermometer to be 3 ft. from the ground and 1 ft. from screen.

(3.) Begin readings at first contact.

(4.) End readings at last contact.

(5.) Read every 5 minutes, or oftener, if rapid changes are noted.

Observations of Stars (Rev. E. J. VAUGHAN to report).

(1.) Give list of first magnitude stars seen close to sun.

(2.) State whether any second or third magnitude stars were seen.

(3.) Was the number of stars about equal to that usually seen at full moon, or was it darker and more stars visible?

Landscape Camera (The Marquis of GRAHAM).

(1.) Before totality, see that the sun's image is near the centre of the plate.

(2.) Expose a plate for 1 second when the timekeeper calls "95 seconds left."

(3.) Expose a plate for 5 seconds when "55 seconds" is called.

(4.) Note your general impressions of the phenomena.

The 6-inch Prismatic Camera.

The 6-inch prismatic camera, intrusted to Mr. FOWLER, was essentially the same instrument as that employed for the Eclipse of April 16th, 1893, in West Africa.* Instead of the mounting of my 6-inch Cooke telescope, however, the equatorial head of a Dallmeyer photoheliograph was adapted for the occasion, this resting upon a wooden stand which was afterwards filled up with concrete. The wooden tube of the instrument was square in section and was firmly attached to the declination axis by a strong iron plate. A consideration of the position angles of the points of contact indicated that dispersion in an east and west direction would better show the chromospheric arcs, and the prism was placed accordingly.

In 1893, ten dark slides, each holding three dry plates, were provided. The experience then gained showed that narrower plates would meet all requirements, so five compartments were made to replace the three in each slide, and in this way fifty plates became available.

Guided by the results obtained in 1893, the following table of exposures was drawn up for this instrument :—

* 'Phil. Trans.,' A, 1896, vol. 187, p. 559.

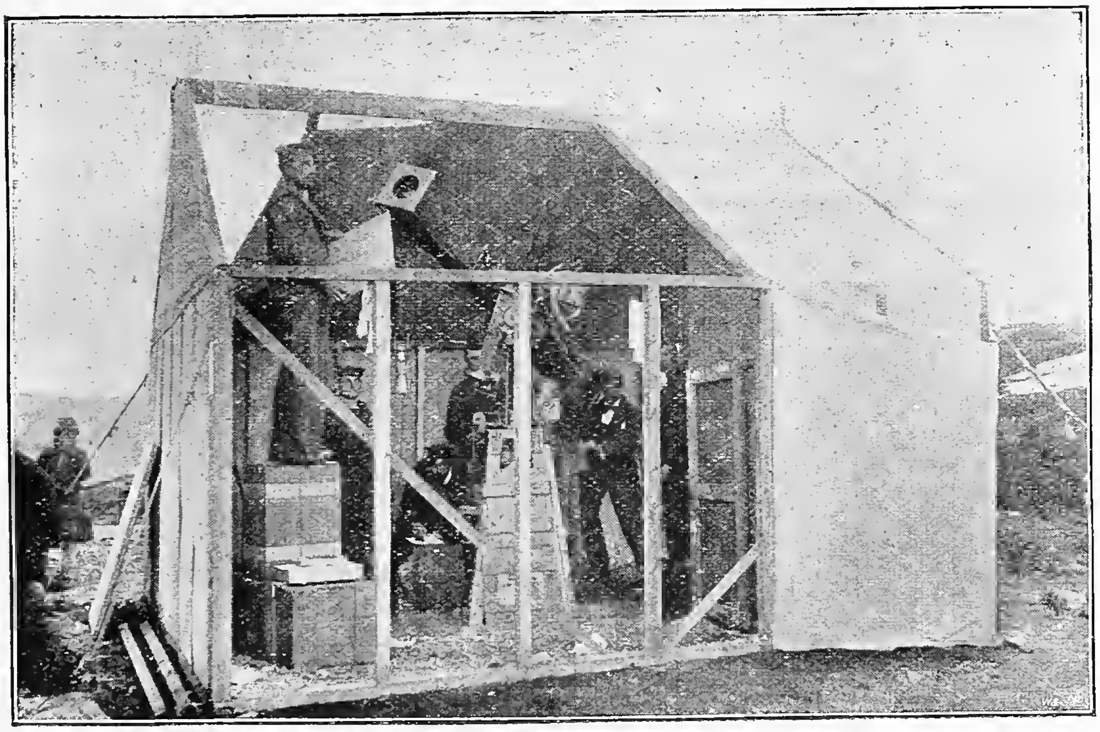
Exposures for 6-inch Prismatic Camera.

Number.	Exposure.	Time.	Remarks.
1	Instantaneous	..	1 minute before totality
2	,,	..	40 seconds ,, ,,
3	,,	..	30 ,, ,, ,,
4	,,	..	20 ,, ,, ,,
5	,,	..	10 ,, ,, ,,
6	,,	105	Totality begins. Wait signal
7	,,	103	
8	,,	101	
9	,,	99	
10	,,	97	
11	,,	92	
12	,,	90	
13	,,	88	
14	,,	86	
15	,,	84	Chromosphere disappears
16	20 seconds	79–59	
17	12 ,,	57–45	
18	30 ,,	43–13	Chromosphere reappears
19	Instantaneous	11	
20	,,	9	
21	,,	4	
22	,,	2	
23	,,	0	Totality ends
24	,,	..	2 seconds after totality
25	,,	..	4 ,, ,, ,,
26	,,	..	9 ,, ,, ,,
27	,,	..	11 ,, ,, ,,
28	,,	..	13 ,, ,, ,,
29	,,	..	15 ,, ,, ,,
30	,,	..	17 ,, ,, ,,
31	,,	..	22 ,, ,, ,,
32	,,	..	24 ,, ,, ,,
33	,,	..	26 ,, ,, ,,
34	,,	..	28 ,, ,, ,,
35	,,	..	30 ,, ,, ,,
36	,,	..	35 ,, ,, ,,
37	,,	..	37 ,, ,, ,,
38	,,	..	39 ,, ,, ,,
39	,,	..	41 ,, ,, ,,
40	,,	..	43 ,, ,, ,,
41	,,	..	48 ,, ,, ,,
42	,,	..	50 ,, ,, ,,
43	,,	..	52 ,, ,, ,,
44	,,	..	54 ,, ,, ,,
45	,,	..	56 ,, ,, ,,
46	,,	..	61 ,, ,, ,,
47	,,	..	63 ,, ,, ,,
48	,,	..	65 ,, ,, ,,
49	,,	..	67 ,, ,, ,,
50	,,	..	69 ,, ,, ,,

Edwards's Isochromatic plates were employed throughout.

The method of working the instrument was that adopted by Mr. FOWLER in 1893. He himself being stationed at the camera, 2nd Class Petty Officer G. M. ROBERTS made the exposures by withdrawing a card from the front of the prism, while Sub-Lieutenant BEAL made careful records of the times at which each plate was exposed, and warned Mr. FOWLER of the termination of the three long exposures. Privates J. BRIGGS and F. HUSKISSON stood one on each side and respectively handed the slides to and received them from Mr. FOWLER.

Fig. 1.



The 6-inch Hut, showing Mr. FOWLER and his Assistants at drill.

Actual rehearsals with the splendid assistance thus available showed that the programme could be thoroughly carried out; in fact a few seconds were saved, so that the attempt to secure a photograph exactly at the end of totality was the more likely to be successful.

The 9-inch Prismatic Camera.

This instrument was intrusted to Dr. W. J. S. LOCKYER.

The tube carrying the prism, lens, and camera of the 9-inch prismatic camera was fixed horizontally on loaded packing cases resting on concrete foundations, and the sun's rays were reflected into it by the mirror of a 12-inch siderostat. Three dark slides, each holding three plates, were provided, the change from one plate to the next in each slide being made by a rack and pinion. An additional slide, carrying a plate $8\frac{1}{2}'' \times 6\frac{1}{2}''$, was so arranged that the light passing through a narrow slit running the whole length of the spectrum was exposed at any instant. The position of this slit was adjusted so as to fall at the point where the photosphere would reappear after the end of totality. This "dropping plate" was intended to be exposed from as near as possible ten seconds before the end of totality, to 15 seconds after, the spectrum being moved slowly in the direction at right angles to the length of the spectrum.

Fig. 2.

The 9-inch Prismatic Camera.

With this instrument the exposures were intended to be longer than with the 6-inch, so that there might be a greater chance of obtaining impressions of the fainter coronal rings. The complete scheme of exposures was as follows :—

EXPOSURES for 9-inch Prismatic Camera.

Number.	Exposure.	Time.	Remarks.
1	Instantaneous	10 seconds before totality	
2	"	105	Totality begins. Wait signal.
3	10 seconds	103–93	
4	5 "	88–83	Chromosphere disappears.
5	10 "	81–71	
6	30 "	69–39	
7	5 "	34–29	
8	10 "	27–17	
			Chromosphere reappears.
9	Instantaneous	15	
10	25 seconds	10 seconds before totality to 15 seconds after	Dropping plate.

In working the instrument Dr. LOCKYER had five assistants. Midshipman FITZ-WILLIAMS was in charge of the siderostat, to attend to clock-winding, &c.; Midshipman BRUCE acted as special timekeeper to note the times by a deck watch at which each plate was exposed and to announce the termination of the longer exposures; Shipwright P. SULLIVAN stood by the prism to remove the cap at a signal from Dr. LOCKYER and to replace it when Midshipman BRUCE gave the signal "over"; Able Seamen H. FROUD and A. WOOLLARD respectively handed the dark slides to Dr. LOCKYER and replaced them in the box from which they had been taken.

The Integrating Spectroscope.

This instrument was intrusted to Lieutenant MARTIN, R.N.

The dispersive parts of the integrating spectroscope consisted of two dense flint glass prisms of 60°, having an effective aperture of very nearly 3 inches. As collimator, a 4-inch Cooke object-glass (TAYLOR'S patent triplet) of 72 inches focus was employed, while the camera was fitted with a portrait lens of 19 inches focus. The optical parts were mounted on a board 7 feet by 2 feet 6 inches. This was hinged to another board of the same size, which was to serve as a base, and the boards could be inclined at an angle equal to the sun's altitude by the use of blocks.

The base rested upon loaded packing cases which were carefully levelled with cement upon a solid rocky foundation. As a siderostat was not available for use with this instrument, a simple arrangement was provided for keeping the collimator approximately directed to the centre of the dark moon during totality. A 2-inch object-glass of 30 inches focus was fixed to the inclined board so as to throw an image of the sun on the centre of a small screen when the collimator was pointed at the sun. The whole spectroscope could be moved in azimuth by means of a milled headed driving screw, 4 inches in length, which was turned by hand, and in this way the sun's image could be kept sufficiently near to the centre of the screen to ensure a large proportion of the direct rays of the corona entering the collimator.

The instrument was housed in a tent made of spars and sails, loaded with rocks to prevent it being blown away by the wind. The only opening was towards the east, in the direction of the eclipse, but on the morning of the eclipse one side was removed in order that the workers might be able to view the phenomena as opportunity offered.

Three plates were exposed during totality with the following exposures, but no results were obtained :—

Number.	Exposure.	Time.	Remarks.
1	15 seconds	105–90	
2	65 ,,	85–20	
3	10 ,,	15– 5	5 seconds allowed to prevent over-running

The times indicated in the table are, as before, those announced by the general timekeepers as measuring the number of the remaining seconds of totality.

Lieutenant MARTIN, R.N., was assisted by Midshipman SILVERTOP as exposer, Midshipman BRENDON as special timekeeper, while Midshipman WOODBRIDGE attended to the screw for keeping the collimator directed to the sun.

The 3¾-Inch Refractor.

The 3¾ inch Cooke telescope was mounted on a small equatorial head without clamps or adjustments. In this case the tripod stand was dispensed with, and the head was screwed to the top of a loaded packing-case. A diagonal eyepiece, which could be rapidly changed, by a simple operation, from total deflection at a silvered surface to reflection from plane glass, was provided.

With this instrument it was my intention to observe the times of commencement and ending of totality, and the structure of the inner corona. Observations of the corona were also to be made by Lieutenant HODGES during the time I was engaged with other instruments, H. LEWIS, 3rd Writer, was to act as recorder. It was calculated by Dr. LOCKYER, that 7 minutes 50 seconds before totality the visible crescent would extend over 180°, and I hoped to utilise this fact as a check on the accuracy of the chronometers.

Sketches of Corona.

As no photographic telescope was available for the work at Kiö Island, I had to rely upon sketches to indicate the general appearance of the corona. Experience has shown that although drawings usually represent the feebler extensions more clearly than photographs, the drawings made by different observers frequently have little resemblance to each other. An experiment I had made during the passage outwards on s.s. "Garonne," however, convinced me that these large differences could be almost entirely eliminated after a little practice in sketching from photographs projected upon a screen by a magic-lantern, and that drawings made under good conditions might be very valuable.

The form which the rehearsals took was as follows :—

Each observer was supplied with the necessary drawing-paper, Morris-tube targets being found to serve this purpose capitally.

By means of a magic-lantern, enlarged views of several different coronas were thrown on a screen for inspection, and attention was drawn to the dissimilarity between any two of these. Instruction was then given as to the important features to be noted and recorded, special stress being laid on noting accurately the exact orientation of the features in question. This was facilitated to a great extent by the concentric circles and radial lines surrounding the bullseye, and passing through the centre respectively. It was suggested, also, to those making these drawings to consider the image of the corona on the screen, or the actual eclipsed sun in the sky as an ordinary compass card, with the north at the top and the east on the right-hand side. The question of the orientation of any point was by this means an easy matter to determine.

After these preliminary trials the coronas were withdrawn, and a previously unseen one substituted. This corona was then exhibited on the screen for 105 seconds, this being the duration in time of the approaching eclipse. The time was called out in exactly the same manner as was adopted during the actual eclipse. Commencing with 105, the number of seconds remaining was called out every 10 seconds until the final 10 seconds were left, when each second was counted. The image of the corona was then immediately withdrawn from the screen, the observers were allowed 15 minutes, to fill in any extra details which they had observed and not had time to record, and to make any notes of the phenomena in general.

A subsequent examination of the drawings thus made showed a remarkable similarity, and by means of a few rehearsals, the observers became quite expert in inserting most of the details shown on the screen, both as regards form and colour.

During the competition that took place nearly every evening, marks were awarded, 10 being allowed for form and 10 for colour. In most cases the marks given were high, 18 marks out of 20 was by no means uncommon.

The selection of the sketching party was accordingly based upon the results of these competitions.

For the actual eclipse, each observer was provided with a piece of drawing-paper, 12 inches square, in the centre of which was a blackened disc representing the dark moon ; passing through the centre of the disc were lines at angles of 45°, which were taken to represent points of the compass.

Some of the observers were provided with wooden disks to cut off the light of the lower corona, so that the long extensions might be the better visible. Two of the disks were arranged to cover the corona to a distance of 3 feet from the moon's limb, and one to a distance of 5 feet.

Lieutenant MARTIN, R.N., very kindly superintended the setting up of the disks. The disks themselves were of thin wood, about 5 inches in diameter, and were attached by iron rods to long spars standing vertically. The place for the eye was determined, in the first instance, with the aid of a theodolite and measuring tape, the altitude and azimuth of the sun at the time of eclipse, and the proper distance of the disk from the eye having been previously calculated.

To guard against error, the pointers indicating the position of the eye were provided with horizontal and vertical movements. A horizontal bar was supported between two uprights about 18 inches apart, and from this was suspended a piece of wood about 10 inches long, which could slide along it. A piece of brass, the end of which marked the place of the eye, was free to slide up and down the vertical piece.

Ten minutes before totality the disk observers were to be blindfolded, and, meanwhile, an amanuensis was to keep the pointer in the proper place; at the commencement of totality the observer would take his place and dictate the directions and lengths in diameters of the most conspicuous streamers.

II. RESULTS OBTAINED.

Forty-five plates were exposed in the 6-inch prismatic camera, and three in the integrating spectroscope, but no images were obtained in consequence of the thick clouds. There was no opportunity of adjusting the 9-inch, so that the plates were not even exposed.

Lord GRAHAM secured during totality an excellent photograph with the $7\frac{1}{2}'' \times 5''$ camera, which showed that the sun was completely blotted out from view by the dense clouds at the time the exposure was made.

The only other observations were those secured by the "Volage" observers. These are appended.

Meteorology (*Temperatures*).

As already stated, six observers were told off to take thermometer readings at intervals of five minutes throughout the entire eclipse. Two observers were stationed at each thermometer, one to take the readings and another to note the times.

The thermometer on Bras Havn was graduated on Fahrenheit's scale and was fully exposed. Those on Kiö Island were Centigrade thermometers, and they were supported at a height of 3 feet from the ground, at a distance of a foot from a piece of sail cloth, which shielded them from the direct rays of the sun.

The readings observed are shown in the following table :—

TEMPERATURE Observations During Eclipse.

Time.	Thermometer in open. Island in Bras Havn. Petty Officer, 1st Class, E. W. Hurst; Leading Signalman, T. Cannon.		Shielded thermometer, No. 1. Kiö Island. Petty Officer, 1st Class, C. Symes; Petty Officer, 2nd Class, J. Yardley.		Shielded thermometer, No. 2. Kiö Island. Stokers, F. Faizell and W. Thripp.		Remarks.
A.M.	° F.	° C.	° F.	° C.	° F.	° C.	
5. 0	52	11·1	46·4	8	47·3	8·5	1st contact
3	50	10					
5	49·75	9·9	46·4	8	47·3	8·5	
7	48·75	9·3					
8	48	8·9					
10	47	8·3	46·4	8	47·3	8·5	
15	46·5	8	46·4	8	47·3	8·5	
20	46	7·8	46·4	8	47·3	8·5	
25	46	7·8	46·4	8	47·3	8·5	
30	46	7·8	46·4	8	47·3	8·5	
35	46	7·8	46·4	8	47·3	8·5	
40	46	7·8	46·4	8	47·3	8·5	
45	45·75	7·6	45·95	7·75	46·85	8·25	
50	45·75	7·6	45·95	7·75	46·85	8·25	
							Totality
55	45·5	7·5	45·5	7·5	46·4	8	
6. 0	45·5	7·5	45·5	7·5	46·4	8	
5	45·5	7·5	45·95	7·75	46·4	8	
10	45·5	7·5	45·95	7·75	46·85	8·25	
15	45·5	7·5	45·95	7·75	46·85	8·25	
20	45·5	7·5	46·4	8	47·3	8·5	
25	45·5	7·5	46·4	8	47·3	8·5	
30	46	7·8	46·4	8	47·3	8·5	
35	47	8·3	46·4	8	47·3	8·5	
40	47	8·3					
45	47	8·3					
50	47	8·3					
55	47	8·3					
							Last contact
7. 0	47	8·3					
	Temperature remained at 45°·5 F. (7°·5 C.) during totality.						

It will be seen from the foregoing that the exposed thermometer fell $6\frac{1}{2}$° F. between first contact and totality, but only rose $1\frac{1}{2}$° F. from totality to near the time of last contact.

The variations of the two shielded thermometers on Kiö Island were exactly equal, although the actual readings differed by half a degree Centigrade. From first contact to totality the fall of temperature was 0°·5 C. (0°·9 F.), while there was an equal rise of 0°·5 C. from totality to near the end of the eclipse.

Landscape Colours.

The observations of the colours of sky, clouds, land, and water before, during, and after totality, are indicated in the following table, and by the additional remarks appended :—

Landscape Colours, &c.

Direction.	Sky.			Clouds.			Land.			Water.			Observer.
	Before totality.	During totality.	After totality.	Before totality.	During totality.	After totality.	Before totality.	During totality.	After totality.	Before totality.	During totality.	After totality.	
East	Streaks of light in horizon. A little blue sky	...	...	Heavy, in bands	...	...	...	...	...	Grey; green	...	...	Lieut. Yelverton, R.N.
	...	...	...	Dull grey, slate	...	...	...	Black	...	...	...	...	Chief Stoker H. E. White.
	Blue	Violet	Blue	Dirty grey	Dark	Grey	Black	Blue black	...	Blue	Yellowish brown	...	Leading Signalman G. A. Bennett.
	...	...	...	Dark, with bright streak	...	...	...	...	...	...	Dirty	Blue	Yeoman of Signals H. Beresford.
	...	Purple blue	...	...	Sepia	...	...	Black	...	...	Light streaks	...	H. Gambler, A.B.
	Blue	Blue	...	Dark slate	Slate	...	Grey	Dark brown	...	Dirty	Dirty	...	J. Harding, Stoker.
South	Light near horizon. Greenish tinge	...	...	Heavy and black	...	...	...	...	...	...	...	...	Lieut. Yelverton, R.N.
	...	...	...	...	...	...	...	Black	...	...	...	...	Chief Stoker H. E. White.
	Blue	Light blue	Grey	Grey	Dark	Dark	Violet	Bluish	...	Brownish	Light blue	...	Leading Signalman G. A. Bennett.
	Grey	Grey	Grey, with red streaks	Grey, with bluish tip	Dark	...	...	Dark	...	...	Dirty	Blue	Yeoman of Signals H. Beresford.
	...	Blue	...	Dark	Dark and light	...	Brown	Brown	...	Dirty	Dirty	...	J. Harding, Stoker.

Landscape Colours, &c. (continued).

Direction.	Sky.			Clouds.			Land.			Water.			Observer.
	Before totality.	During totality.	After totality.	Before totality.	During totality.	After totality.	Before totality.	During totality.	After totality.	Before totality.	During totality.	After totality.	
West	A little blue sky	...	...	Neutral and dark Slate blue	...	...	Greys and green	...	...	Dark grey	...	...	Lieut. Yelverton, R.N.
	...	...	...		...	...	...	Black	...	...	...	...	Chief Stoker H. E. White.
	Dark and light blue, streaky	...	...	Grey	...	...	Dark	...	...	Yellowish	...	...	Leading Signalman G. A. Bennett.
	Small patch of blue	...	...	...	...	...	...	Dark	...	...	Dirty	Blue	Yeoman of Signals H. Beresford.
	Light sepia streaked with greyish blue	...	...	Dirty white	...	...	Natural	...	...	Grey	...	...	H. Gambler, A.B.
	Blue	...	...	Slate	Light	...	Brown	...	...	Dirty	...	...	J. Harding, Stoker.
North	Heavy clouds	Light; very little blue green	...	Grey	Dark and heavy	...	Band of reddish light on hill, slate blue elsewhere	...	...	Dark grey	...	...	Lieut. Yelverton, R.N.
	...	...	...	...	Black, streaked with yellow	...	...	Black	...	...	Streak of light colour	...	Chief Stoker H. E. White.
	Blue	...	...	Grey	...	...	Dark	...	...	Black	...	...	Leading Signalman G. A. Bennett.
	Grey and blue capped with white	Grey blue, capped with white	Dark blue, with reddish streaks	Grey, with silver tops	...	...	...	Dark	...	...	Dirty	Pale streak from E. to W.	Yeoman of Signals H. Beresford
	Dark sepia	...	...	...	...	...	...	...	...	...	...	...	H. Gambler, A.B.
	Blue	...	...	Dark	...	...	Brown	...	...	Dirty	...	...	J. Harding, Stoker.

Additional notes of colours, &c. were as follows:—

Chief Stoker H. E. WHITE:—"Before totality there was hardly any change from usual colours. During totality the land went black, streaks in clouds assumed a reddish-yellow tint, and a streak of light colour appeared on the sea towards the north, otherwise no change."

Yeoman of Signals H. BERESFORD:—"Before totality the sun was shining only on a patch of land to the westward of Vadsö, which appeared like a sandhill with a reddish tint; no change noted elsewhere, either in land, sky, or water. During totality the land and water appeared to be of an inky blackness; no change was observed in the sky; the air seemed to go suddenly cold. Directly after the total eclipse a slight change appeared in sky to the south; the land came back to its ordinary colour, the sun shining on the same place as before total eclipse; the water underwent a slight change, to the northward a pale blue streak running from east to west, and the air seemed to feel a little warmer."

General Observations.

After the eclipse the following notes as to the general phenomena observed were received from the officers named:—

Staff-Surgeon J. H. WHELAN, M.D.:—"The sudden rush of darkness at totality caused a feeling of dread; an instinctive feeling of fright lest the source of our being had gradually been extinguished, it seemed to me.

"The gulls started a discordant calling which seemed to distinctly change to one of rejoicing when the light began to increase again.

"During totality, the sky being nearly entirely overcast by clouds, a brightness appeared to the north, and another to the south, west of the horizon, as if the sun had set there, tingeing the clouds."

Lieutenant YELVERTON, R.N., noted the bleating of sheep and screaming of gulls during the darkness of totality.

Lieutenant W. H. B. LAW, R.N.:—"For 20 minutes before totality the sky darkened appreciably. The clouds to the south took an ashy grey colour, the land also took a dark grey tint, and looked, if possible, even more desolate and barren than before. In fact, the whole appearance of land and sky to the south looked as if a heavy thunderstorm was imminent.

"About 4 seconds before the signal for totality was given, a rift in the clouds allowed the sun to be seen. The moon had covered all except a thin crescent at the left hand bottom corner; the inner edge of the crescent through binoculars appeared somewhat rough; then the clouds came over again.

"Suddenly it became much darker—somewhat like the darkness of a London fog (medium); the moon's shadow swept across the earth and sea, moving in a north by westerly direction. The air became appreciably colder to the feelings, and the sea birds began screaming. A few seconds before mid-totality a faint glare or reflection became visible on the clouds about the sun's position, lasting several seconds.

"Just about 5 seconds before the end of totality, the sky became clearer, the shadow could be seen sweeping across the fjord, looking like a huge catspaw moving rapidly to leeward.

"Directly after totality was over, land, sea, and sky gradually regained their normal appearances, the lightening process being much quicker than the darkening."

T. MAKEPIECE (*Chief Engine-room Artificer*):—"The left limb of the sun was visible for the space of 5 seconds (before totality). At '75 seconds left,' a luminous body was visible, having at the right upper side a smaller body of the same nature, both appearing as through a haze.

"At '35 seconds left' there occurred a great commotion among the sea birds settled on the surrounding rocks."

Mr. MAKEPIECE afterwards expressed the opinion that the larger of the bright cloudy patches was in the position of the sun, and appeared as if it were the sun seen through several thicknesses of muslin. It may be remarked that the two patches in question are very distinctly shown in a photograph taken by Lord GRAHAM during totality with the $7\frac{1}{2}'' \times 5''$ camera.

A. DUNCAN *and* T. BROWN (*Engine-room Artificers*):—"At 5.52 the left limb of the sun was visible for 5 seconds; colour, very faint white. At '35 seconds to go,' birds fly off screaming."

A. WRIGHT (*Second Yeoman of Signals*):—"Observed a silvery-white crescent from about N. by W. to S. by W., with a horn pointing in a direction S.E. on the S.W. part of dark moon. The part of the crescent visible was about one-eighth the diameter of the dark moon."

Immediately on my return to England, as the "Volage" was practically going out of commission, I reported to the Royal Society that the arrangements made by the Admiralty to assist the observing party had been carried out in the most admirable manner, and I suggested that the President and Council of the Society should mark its appreciation of the attempt on the part of a large ship's company to further the cause of science. I am glad to say that a letter of thanks was sent by the Society to the Admiralty, and another by the Admiralty to the officers and men of H.M.S. "Volage."

In the erection of the huts and instruments, no one could have wished for more help than was given. Everyone in the ship showed the keenest interest in the work, and help was afforded in every possible manner. Had the weather been fine, I am convinced that the results obtained by the "Volage" observers would have been far more complete than any previously obtained by a single party.

I am anxious, also, to state my entire satisfaction with the manner in which Mr. FOWLER and Dr. LOCKYER conducted the operations before my arrival.

INDEX SLIP.

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Phil. Trans., A, 1897, vol. 190, pp. 23–41.

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II. *On Boomerangs.*

By G. T. Walker, *M.A., B.Sc., Fellow of Trinity College, Cambridge.*

Communicated by Professor J. J. Thomson, *F.R.S.*

Received March 15,—Read April 8, 1897.

The attempts that have hitherto been made to explain the flight of a boomerang have in general been of a somewhat fanciful nature.

Exception must be made in the case of such papers as those of Werner Stille, "Versuche und Rechnungen zur Bestimmung der Bahnen des Bumerangs" (Poggendorff, 'Annalen der Physik,' Bd. 147, 1872), and of Edmund Gerlach, "Ableitung gewisser Bewegungsformen geworfener Scheiben aus dem Luftwiderstandsgesetze" ('Zeitschrift des Deutschen Vereins zur Förderung der Luftschifffahrt,' Heft 3, 1886). In the latter, which is the most noticeable contribution to the subject with which I am acquainted, the author gives an explanation in general terms of some of the effects of the air-resistance upon a symmetrical boomerang: he introduces, however, no analytical treatment of the dynamics of the rotating body and neglects entirely all consequences of the important deviations from symmetry which I have subsequently described as "twisting" and "rounding." Without one of these a return flight is, I believe, impossible.

For an account of the native Australian weapons, and in particular those of Victoria, reference should be made to the very complete descriptions given in Brough Smyth's book, 'The Aborigines of Victoria,' vol. 1, pp. 311–318; shorter notices are to be found in books of travel, such as that of Karl Lumholtz, 'Among Cannibals,' p. 50.

Boomerangs may at the outset be divided into two classes—returning and non-returning; it is rather on weapons of the latter of these types that the natives of Australia rely when engaged in war or the chase. A typical returning boomerang (see fig. 1) resembles in general outline an arc of a hyperbola, and is about 80 centims. in length measured along the curve. At the centre, where the dimensions of the cross section (fig. 1′) are greatest, the width is about 7 centims., and the thickness 1 centim.; these dimensions become smaller as the ends are approached.

As a rule two properties are present. In the first place, the transverse section at any point would show that one surface possesses distinctly greater curvature than the other; secondly, the arms of the implement must be slightly twisted (from coincidence with the plane through each of them) after the fashion of the blades of a screw propeller or a windmill. The direction of the twist is such that rotation about a normal to the plane tends to set up linear velocity of the boomerang in the direction of the vector representing that rotation. These two peculiarities will in future be referred to as the "rounding" and the "twisting."

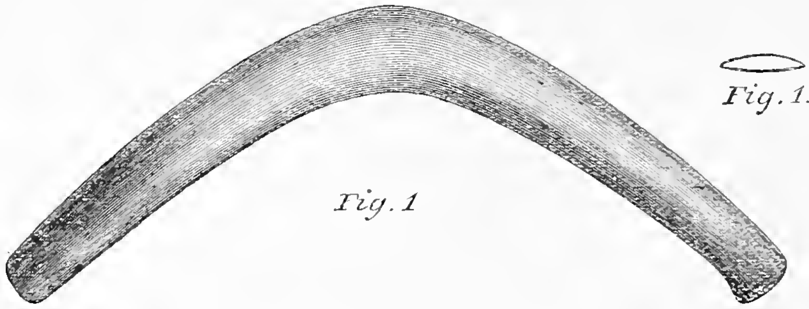


Fig. 1

Fig. 1'

A weapon of this type is thrown in a horizontal direction in such a way as to impart considerable rotation in the vertical plane containing its initial direction of motion; the more convex surface is towards the thrower. The plane of rotation leans slowly over to the right (*i.e.*, the vector representing the spin begins to point slightly upwards) and the path curls to the left. The projectile proceeds to describe a loop whose longer diameter is about fifty yards; it gradually rises until it reaches a height which is usually about thirty feet from the ground, travels horizontally for a time, and then gradually sinks to the earth.

The change in the angular motion has throughout the flight continued unaltered in character; the inclination of the plane of rotation to the horizon has steadily diminished from a right angle to zero, and the axis of the spin has veered continually to the left (as seen from above) in such a manner that as long as the linear velocity remains large, the angle between the direction of motion and the plane of rotation is small.

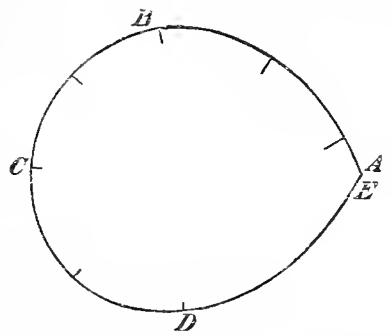
In the accompanying diagram (figs. 2, 3) a plan and elevation of this, the simplest form of path, is given. An attempt is made to indicate the inclination of the axis of rotation by representing at intervals the projection of a line of constant length drawn along that axis.

If it be not desired to make so large a loop as that described, it is fairly easy to get the boomerang to describe a circle of thirty-five yards in diameter, without ever rising to more than twelve feet from the earth.

In the more complicated paths, as long as the velocity remains considerable, the manner in which the plane of rotation and the direction of motion change is precisely the same as in the simpler cases; it is the rates of change that differ. The graceful gyrations that a boomerang performs on its downward course, if the linear velocity dies out while it is high in the air, present little or nothing that is new in principle. It is in the explanation of the earlier motion that the problem really lies, and the observation of actual flights makes it clear that their character is deducible when the two components of angular velocity (denoted subsequently by Ω_1, Ω_2), whose axes lie in the plane of the boomerang, are determined.

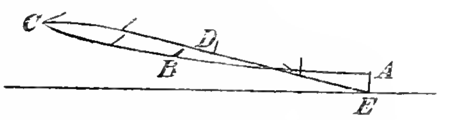
The flight may be regarded as a case of steady motion of which the circumstances gradually vary. It is only with very badly made instruments that small oscillations are at times perceptible; with ordinary boomerangs, the accident of grazing the ground or meeting a sudden puff of wind will not cause visible vibrations.

Fig. 2. Plan.



The scale of this and the following diagrams is 1 : 1000, or 28 yards to 1 inch, approximately.

Fig. 3.



Elevation upon a vertical plane through AC.

Let the plane containing the arms of the boomerang (in future called the primary plane) be taken as that of XY, with the centre of gravity as the origin, and the projection upon this plane of the resultant velocity as OX; OZ is drawn on the more convex side. If then the rectangular components of linear and angular velocity of the body be U, O, W, and Ω_1, Ω_2, Ω_3, it may be observed that W is always small compared with U, and Ω_1, Ω_2 compared with Ω_3. Throughout the motion Ω_1 is positive, Ω_2 negative, and Ω_3 positive. The time of flight is about nine seconds, and the greatest distance fifty yards; the mean values of Ω_1 and Ω_2 may be estimated at one-sixth and *minus* one-third respectively, while in C.G.S. units U is two thousand and Ω_3 is thirty.

The angular velocity θ_3 of the axes is small and positive throughout the motion, except near the conclusion, when it sometimes vanishes and becomes negative.

For theoretical purposes I have regarded the body as replaced by one of extremely thin material with the same general shape and twist; the transverse section will be a circular arc with its convex surface on the same side as the more rounded surface of the wooden weapon.

Experiment shows that if a thin rectangular plate be advancing with velocity v in a direction that is inclined at a small angle α to its plane, the air-pressure produces a force and a couple about the centre; the mean normal pressure per unit area may be denoted by $\lambda v^{\mu} \alpha$, where λ is a constant that depends on the proportions of the rectangle and μ is a number which has been often assumed to be equal to 2. In his "Experiments in Aerodynamics" ('Smithsonian Contributions to Knowledge,' 1891), S. P. LANGLEY makes this assumption, but if from Table XIV. the value of μ be calculated by comparing the soaring velocities of 24 × 6 planes, weighing 250 and 1,000 grms. at inclinations of 10° (the smallest inclination quoted), it proves to be 2·7; for square planes of the same weight, inclined at 5°, the index is 2·5. From comparison of the cases of inclination of 2° and 5° of Table XII., the value 3·3 of μ may be deduced. In the course of the following analysis it will be seen that progress is attended with extreme difficulty unless $\mu = 3$, and inasmuch as the constant λ is at our disposal, we shall be justified in taking $\mu = 3$ and choosing λ so as to agree with LANGLEY's experiments at the mean value of the velocity under discussion. Any error introduced by an incorrect value of μ will be quantitative rather than qualitative.

In addition to the uniform pressure acting on the rectangular plate, there will be a couple whose amount may be taken as $\kappa v^{\nu} \alpha$ per unit area, κ being a constant depending on the dimensions of the plate.* The assumption of a velocity potential would lead to the value $\nu = 2$,† while $\nu = 3$ is suggested by the previous assumption. In order to simplify subsequent proceedings we shall choose the smaller value and deduce the value of κ from LANGLEY's experiments.

We have now to consider the effect of the air on the slightly distorted thin surface which represents the boomerang, and in order to surmount the difficulties introduced by the fact that the velocities at different points vary, as well as the directions of the normals to the surface, we are driven to make some hypothesis.

Now the effect of the air-pressure upon a plane surface in uniform motion may be obtained by integrating over it, provided that we regard the effect due to any small portion as proportional to the area of that portion.

We therefore assume, as a first approximation, that the contribution from any element of the distorted surface is the same as if the rest of the surface were in the same plane as the element and had the same velocity; that this assumption, in the case of simple distortions, leads to results of the right character, is easily verified.

The determination of κ depends on the fact that if the width of an arm measured in the direction of the velocity of the point in question be c, and if f stand for the

* See THOMSON and TAIT, 'Natural Philosophy,' § 325. The existence of this couple is often stated in the form that the resultant thrust on the plate does not act at the centre of figure. LANGLEY finds (chap. viii., pp. 89–93), that in the case of a square plate the point of application of the resultant pressure, when α does not exceed ten degrees, is at a distance from the centre of figure equal to about one-sixth of the length of the side. He quotes JOËSSEL and KUMMER as having obtained a fifth and a sixth respectively as the value of this ratio.

† LAMB's 'Hydrodynamics,' p. 185 (3); BASSET's 'Hydrodynamics,' vol. 1, § 190.

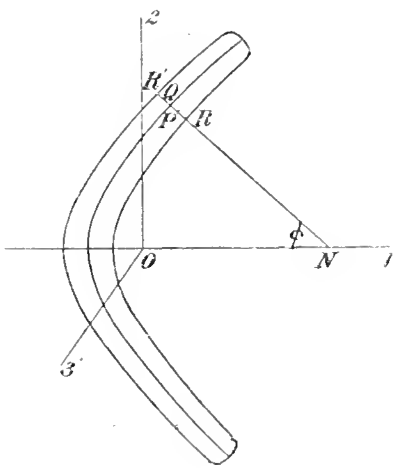
ratio 1 : 5 or 1 : 6 as deduced from experiment, then the couples $\lambda U^3 . cf$ and κU^2 should be the same. This would give a value of κ varying from point to point. We therefore, for convenience, treat κ as constant, and give it the magnitude corresponding to the mean value of c.

It must be realised at the outset that the following analysis does not claim to be more than a first approximation, in which the quantities neglected may be of a tenth of the magnitude of those retained. Our knowledge of the laws of the resistance of the air is not at present great enough for accurate results to be attainable, and I have accordingly not hesitated to neglect small terms in order to effect a material simplication in the mathematical analysis.

It may appear that such processes reduce the method to little more than a qualitative one, but though much may be done by qualitative methods applied to this subject, and all the chief terms may be traced to their source without the use of algebraical symbols, yet, as will soon become clear, the effects of the forces in action are conflicting. It is therefore necessary, in order to obtain results which are qualitatively right, to adopt methods which, although not accurate, have at any rate some approach to quantitative correctness.

We now take axes fixed in the body, 1 and 2 being along and perpendicular to the axis of symmetry in the primary plane.

Fig. 4.



If the velocity at any point xyz have components u, v, w, and the direction cosines of the normal on the convex side there be l, m, n, then the normal pressure in the direction $-l$, $-m$, $-n$ will be

$$\lambda q^{\mu-1}(lu + mv + nw),$$

where l, m, w are small quantities and $q^2 = u^2 + v^2 + w^2$.

The couple per unit area will have moment

$$\kappa q^{\nu-1}(lu + mv + nw)$$

about an axis whose direction cosines are

$$mw - nv, \quad nu - lw, \quad lv - mu,$$

each divided by a quantity differing from q by squares of small quantities. Hence, if $\mu = 3$, $\nu = 2$, the normal force at any point will be

$$\lambda q^2 (lu + mv + w),$$

and the component couples

$$- \kappa v (lu + mv + w), \quad \kappa u (lu + mv + w), \quad 0,$$

where squares of small quantities are omitted.

Now if, due to the "rounding," a transverse line RR′ (fig. 4) through any point P be a circular arc of radius ρ, and if its middle point Q have co-ordinates x, y, then, denoting QP as measured along the inward normal QN by s, the direction cosines of the normal at

$$x + s \cos \phi, \quad y - s \sin \phi$$

will be

$$\frac{s \cos \phi}{\rho}, \quad - \frac{s \sin \phi}{\rho}, \quad 1.$$

In addition to this the line RR′ will, owing to the twisting, be turned about the tangent at Q through an angle which may be taken as $\frac{y}{\tau}$, where τ is a constant length. The superposition of this small distortion on the former will add to the direction cosines terms

$$\frac{y \cos \phi}{\tau}, \quad - \frac{y \sin \phi}{\tau}, \quad 0.$$

Let the linear and angular velocities of the body referred to the axes 1, 2, 3 be

$$u, \ v, \ w, \ \omega_1, \ \omega_2, \ \omega_3,$$

where w is small compared with $(u^2 + v^2)^{\frac{1}{2}}$ and ω_1, ω_2 compared with ω_3. The component velocities u_1, v_1, w_1 of P will then be

$$\begin{aligned} & u - (y - s \sin \phi)\, \omega_3, \\ & v + (x + s \cos \phi)\, \omega_3, \\ & w - (x + s \cos \phi)\, \omega_2 + (y - s \sin \phi)\, \omega_1. \end{aligned}$$

The resultant force due to the pressures will have negligible components parallel to the axes 1, 2, and the force Z along the third axis is the integral over the surface of

$$- \lambda [u^2 + v^2 + 2(x + s\cos\phi) v\omega_3 - 2(y - s\sin\phi) u\omega_3 + \{x^2 + y^2 + 2s(x\cos\phi - y\sin\phi) + s^2\} \omega_3^2]$$

$$\times \left[\left(\frac{s}{\rho} + \frac{y}{\tau}\right)\cos\phi\,\{u - (y - s\sin\phi)\,\omega_3\} - \left(\frac{s}{\rho} + \frac{y}{\tau}\right)\sin\phi\,\{v + (x + s\cos\phi)\,\omega_3\} + w - (x + s\cos\phi)\,\omega_2 + (y - s\sin\phi)\,\omega_1\right].$$

This may be regarded as the sum of three forces: (1) Z_0 the force which is exerted on a boomerang without distortion, (2) Z_1 due to the rounding, (3) Z_2 due to the twisting.

Denoting by brackets ( ) the operation of taking the mean value over the area of the boomerang, we shall adopt the notation

$$(x^2) = \kappa_2^2, \quad (y^2) = \kappa_1^2, \quad (x^3) = \kappa_3^3, \quad (xy^2) = \kappa_4^3,$$
$$(x^4) = \kappa_5^4, \quad (x^2y^2) = \kappa_6^4, \quad (y^4) = \kappa_7^4,$$
$$(y\sin\phi) = l_1, \quad (xy\sin\phi) = l_2^2, \quad (y^2\cos\phi) = l_3^2,$$
$$(x^2y\sin\phi) = l_4^3, \quad (xy^2\cos\phi) = l_5^3, \quad (y^3\sin\phi) = l_6^3,$$
$$(x^3y\sin\phi) = l_7^4, \quad (x^2y^2\cos\phi) = l_8^4, \quad (xy^3\sin\phi) = l_9^4, \quad (y^4\cos\phi) = l_{10}^4,$$
$$(x^4y\sin\phi + x^3y^2\cos\phi + x^2y^3\sin\phi + xy^4\cos\phi) = l_{11}^5,$$
$$(s^2\cos^2\phi) = m_0^2, \quad (s^2\sin^2\phi) = m'^2_0,$$
$$(s^2x\cos^2\phi) = m_1^3, \quad (s^2x\sin^2\phi) = m'^3_1, \quad (s^2y\sin\phi\cos\phi) = m_2^3,$$
$$(s^2x^2\cos^2\phi) = m_3^4, \quad (s^2x^2\sin^2\phi) = m'^4_3, \quad (s^2xy\sin\phi\cos\phi) = m_4^4,$$
$$(s^2y^2\cos^2\phi) = m_5^4, \quad (s^2y^2\sin^2\phi) = m'^4_5,$$
$$(s^2x^3\sin^2\phi) = m_6^5, \quad (s^2x^2y\sin\phi\cos\phi) = m_7^5, \quad (s^2xy^2\cos^2\phi) = m_8^5,$$
$$(s^2xy^2\sin^2\phi) = m'^5_8, \quad (s^2y^3\sin\phi\cos\phi) = m_9^5.$$

Then for a plane surface we find at once

$$Z_0 = -\lambda S[(u^2 + v^2)w - 2(\kappa_1^2 u\omega_1 + \kappa_2^2 v\omega_2)\,\omega_3 + \{(\kappa_1^2 + \kappa_2^2)\,w - (\kappa_3^3 + \kappa_4^3)\,\omega_2\}\,\omega_3^2]$$

where S is the area, and terms in s^2 have been omitted since they are multiplied by the small terms w, ω_1, ω_2.

The rounding produces a force

$$Z_1 = -2\lambda\int dS\,\frac{s^2}{\rho}\,\{(u - y\omega_3)\cos\phi - (v + x\omega_3)\sin\phi\}\,\{u\omega_3\sin\phi + v\omega_3\cos\phi + (x\cos\phi - y\sin\phi)\,\omega_3^2\}$$

$$= \frac{2\lambda}{\rho}\,Su\omega_3\,\{(m'^2_0 - m_0^2)\,v + (m'^3_1 - m_1^3 + 2m_2^3)\,\omega_3\}.$$

Due to the twisting we have

$$Z_2 = -\frac{\lambda}{\tau}\int dS\, y\,\{u^2 + v^2 + 2(xv - yu)\,\omega_3 + (x^2 + y^2)\,\omega_3^2\}\,\{(u - y\omega_3)\cos\phi - (v + x\omega_3)\sin\phi\}$$

$$= \frac{\lambda}{\tau}\,S\Big[\{l_1 v + (l_2^2 + l_3^2)\,\omega_3\}\,(u^2 + v^2) + (3l_4^3 + 2l_5^3 + l_6^3)\,\omega_3^2 v + 2\,(l_2^2 v^2 + l_3^2 u^2)\,\omega_3 + (l_7^4 + l_8^4 + l_9^4 + l_{10}^4)\,\omega_3^3\Big].$$

The resultant couple about the first axis will be the integral over the surface of

$$-\kappa v_1\,(lu_1 + mv_1 + w_1) - (y - s\sin\phi)\,\lambda\,(u_1^2 + v_1^2)\,(lu_1 + mv_1 + w_1).$$

As before this may be divided into three portions, of which the first, on a plane boomerang, is

$$F_0 = -\int dS\,(w - x\omega_2 + y\omega_1)\,[\kappa\,(v + x\omega_3) + \lambda y\,\{u^2 + v^2 + 2\,(xv - yu)\,\omega_3 + (x^2 + y^2)\,\omega_3^2\}],$$

in which terms in s^2 have been omitted as before.

Therefore,

$$F_0 = Sw\,(-\kappa v + 2\lambda\kappa_1^2\omega_3 u) + S\omega_2\omega_3\,(\kappa\kappa_2^2 - 2\lambda\kappa_4^3 u) - \lambda S\omega_1\,\{\kappa_1^2\,(u^2 + v^2) + 2\kappa_4^3\omega_3 v + (\kappa_6^4 + \kappa_7^4)\,\omega_3^2\}.$$

$$F_1 = -\int dS\,\frac{s^2}{\rho}\Big[[\kappa\omega_3\cos\phi + \lambda\,\{2y\omega_3\,(u\sin\phi + v\cos\phi) + 2\omega_3^2 y\,(x\cos\phi - y\sin\phi) - \sin\phi\,(u^2 + v^2 + 2\omega_3 vx - 2\omega_3 uy + x^2\omega_3^2 + y^2\omega_3^2)\}] \times [(u - y\omega_3)\cos\phi - (v + x\omega_3)\sin\phi]\Big]$$

$$= -\frac{S}{\rho}\,[\kappa m_0^2 u\omega_3 + 2\lambda\omega_3\{2m_2^3 u^2 + (m_1'^3 - m_2^3)v^2\} + \lambda(u^2+v^2)\{(m_1'^3 + m_2^3)\omega_3 + m_0'^2 v\} + \lambda v\omega_3^2(3m_3'^4 - 2m_4^4 - 2m_5^4 + 3m_5'^4) + \lambda\omega_3^3(m_6^5 - m_7^5 - 2m_8^5 + 3m_8'^5 + 3m_9^5)],$$

while

$$F_2 = \frac{1}{\tau}\int dS\,[-uy\cos\phi\,\{\lambda y\,(u^2 + v^2 + 2\omega_3 vx + x^2\omega_3^2 + y^2\omega_3^2)\} + y\,(\omega_3 y\cos\phi + v\sin\phi + \omega_3 x\sin\phi)\,\{\kappa\,(v + \omega_3 x) - 2\lambda\omega_3 uy^2\}]$$

$$= \frac{\kappa S}{\tau}\,\{l_1 v^2 + (2l_2^2 + l_3^2)\,\omega_3 v + (l_4^3 + l_5^3)\,\omega_3^2\} - \frac{\lambda S u}{\tau}\,\{l_3^2\,(u^2 + v^2) + 2\,(l_5^3 + l_6^3)\,\omega_3 v + (l_8^4 + 2l_9^4 + 3l_{10}^4)\,\omega_3^2\}.$$

The couple about the axis 2 is the integral of

$$(lu_1 + mv_1 + w_1)\,[\kappa u_1 + (x + s\cos\phi)\,\lambda\,(u_1^2 + v_1^2)],$$

leading to

$$G_0 = \mathrm{S}w\,[\kappa u + \lambda\omega_3\,\{2\kappa_2^{\,2}v + (\kappa_3^{\,3} + \kappa_4^{\,3})\,\omega_3\}] - \mathrm{S}\omega_1\omega_3\,(\kappa\kappa_1^{\,2} + 2\lambda\kappa_4^{\,3}u)$$
$$- \mathrm{S}\omega_2\lambda\,\{\kappa_2^{\,2}(u^2 + v^2) + 2\kappa_3^{\,3}\omega_3 v + (\kappa_5^{\,4} + \kappa_6^{\,4})\,\omega_3^{\,2}\},$$

$$G_1 = \frac{\mathrm{S}}{\rho}\,[-\kappa\omega_3\,\{m'^{\,2}_0 v + (m'^{\,3}_1 + m_2^{\,3})\,\omega_3\} + \lambda u\,\{m_0^{\,2}(u^2 + v^2) + 2\omega_3 v\,(2m_1^{\,3} + m_2^{\,3} - m'^{\,3}_1)$$
$$+ 3\omega_3^{\,2}(m_3^{\,4} + m_5^{\,4}) - 2\,(m_4^{\,4} + m'^{\,4}_3)\,\omega_3^{\,2}\}],$$

$$G_2 = -\frac{\kappa\mathrm{S}}{\tau}\,\{l_1 uv + (l_2^{\,2} + 2l_3^{\,2})\,\omega_3 u\} - \frac{\lambda\mathrm{S}}{\tau}\,\{(u^2 + v^2)(l_2^{\,2}v + l_4^{\,3}\omega_3 + l_5^{\,3}\omega_3)$$
$$+ 2\omega_3\,(l_4^{\,3}v^2 + l_5^{\,3}u^2) + (3l_7^{\,4} + 2l_8^{\,4} + l_9^{\,4})\,\omega_3^{\,2}v + l_{11}^{\,5}\omega_3^{\,3}\}.$$

The equations of angular motion are

$$\mathrm{A}\dot{\omega}_1 - (\mathrm{B} - \mathrm{C})\,\omega_2\omega_3 = \mathrm{F},$$
$$\mathrm{B}\dot{\omega}_2 - (\mathrm{C} - \mathrm{A})\,\omega_3\omega_1 = \mathrm{G},$$
$$\mathrm{C}\dot{\omega}_3 - (\mathrm{A} - \mathrm{B})\,\omega_1\omega_2 = 0.$$

Neglecting the product $\omega_1\omega_2$, we see that ω_3 may be replaced by n, a constant. Also, for a thin flat body, C is sensibly equal to A + B, and if m be the mass per unit area, we have

$$\mathrm{A} = \mathrm{S}m\kappa_1^{\,2}, \qquad \mathrm{B} = \mathrm{S}m\kappa_2^{\,2},$$

so that our equations become

$$\mathrm{S}m\kappa_1^{\,2}\,(\dot{\omega}_1 + n\omega_2) = \mathrm{F},$$
$$\mathrm{S}m\kappa_2^{\,2}\,(\dot{\omega}_2 - n\omega_1) = \mathrm{G}.$$

We shall first of all discuss the motion of an undistorted boomerang free from the action of gravity. If we revert to our former axes OX, OY, OZ, of which OX is the projection on the primary plane of the direction of motion, we shall obtain as the equations of translation,

$$\dot{\mathrm{U}} + w\Omega_2 = 0,$$
$$-w\Omega_1 + \mathrm{U}\theta_3 = 0,$$
$$m\,(\dot{w} - \mathrm{U}\Omega_2) = \frac{\mathrm{Z}_0}{\mathrm{S}}\,.$$

Hence, neglecting squares, U is constant, and θ_3, the angular velocity of the axes, is zero. Thus we are justified in replacing u, v by $U \sin nt$, $U \cos nt$. We shall then have

$$\left.\begin{aligned} \omega_1 &= \Omega_1 \sin nt - \Omega_2 \cos nt \\ \omega_2 &= \Omega_1 \cos nt + \Omega_2 \sin nt \end{aligned}\right\} \quad \ldots \ldots \quad (1),$$

and on multiplying the former rotation equations by

$$\frac{\sin nt}{\kappa_1^2}, \quad \frac{\cos nt}{\kappa_2^2},$$

and adding, we get

$$m\,(\dot{\Omega}_1 + 2n\Omega_2) = F_0 \frac{\sin nt}{S\kappa_1^2} + G_0 \frac{\cos nt}{S\kappa_2^2} = P_0 \text{ say},$$

$$m\,(\dot{\Omega}_2 - 2n\Omega_1) = - F_0 \frac{\cos nt}{S\kappa_1^2} + G_0 \frac{\sin nt}{S\kappa_2^2} = Q_0,$$

$$m\,(\dot{w} - U\Omega_2) = \frac{Z_0}{S} = R_0.$$

Now

$$\begin{aligned} P_0 = {} & 2\lambda n U w - \lambda U^2 \Omega_1 + \frac{\kappa}{2}\left(\frac{\kappa_2^2}{\kappa_1^2} + \frac{\kappa_1^2}{\kappa_2^2}\right) n\Omega_2 - \frac{\lambda}{2}\left(\frac{\kappa_6^4 + \kappa_7^4}{\kappa_1^2} + \frac{\kappa_5^4 + \kappa_6^4}{\kappa_2^2}\right) n^2 \Omega_1 \\ & + \lambda \frac{\kappa_3^3 + \kappa_4^3}{\kappa_2^2} n^2 w \cos nt + \frac{\kappa}{2}\left(\frac{1}{\kappa_2^2} - \frac{1}{\kappa_1^2}\right) wU \sin 2nt \\ & + \frac{\kappa n}{2}\left(\frac{\kappa_2^2}{\kappa_1^2} - \frac{\kappa_1^2}{\kappa_2^2}\right)(\Omega_1 \sin 2nt - \Omega_2 \cos 2nt) \\ & - \frac{\lambda}{2} \kappa_4^3 n U \left[\Omega_1 (\cos nt - \cos 3nt)\left(\frac{2}{\kappa_1^2} + \frac{1}{\kappa_2^2}\right) + \Omega_2 \sin nt \left(\frac{3}{\kappa_1^2} - \frac{1}{\kappa_2^2}\right)\right. \\ & \qquad \left. - \Omega_2 \sin 3nt \left(\frac{1}{\kappa_1^2} + \frac{1}{\kappa_2^2}\right)\right] \\ & - \frac{\lambda}{2} \frac{\kappa_3^3}{\kappa_2^2} n U \left[\Omega_1 (3 \cos nt + \cos 3nt) + \Omega_2 (\sin nt + \sin 3nt)\right] \\ & + \frac{\lambda n^2}{2}\left(\frac{\kappa_6^4 + \kappa_7^4}{\kappa_1^2} - \frac{\kappa_5^4 + \kappa_6^4}{\kappa_2^2}\right)(\Omega_1 \cos 2nt + \Omega_2 \sin 2nt). \end{aligned}$$

Similarly

$$Q_0 = - \lambda U^2 \Omega_2 + \frac{\kappa U w}{2}\left(\frac{1}{\kappa_1^2} + \frac{1}{\kappa_2^2}\right) - \frac{\kappa}{2}\left(\frac{\kappa_2^2}{\kappa_1^2} + \frac{\kappa_1^2}{\kappa_2^2}\right) n\Omega_1 - \frac{\lambda}{2}\left(\frac{\kappa_6^4 + \kappa_7^4}{\kappa_1^2} + \frac{\kappa_5^4 + \kappa_6^4}{\kappa_2^2}\right) n^2 \Omega_2,$$

together with terms whose coefficients involve circular functions of nt.

Also

$$\begin{aligned} R_0 = {} & - \lambda \{U^2 + (\kappa_1^2 + \kappa_2^2) n^2\} w + \lambda (\kappa_1^2 + \kappa_2^2) n U \Omega_1 \\ & - \lambda (\kappa_1^2 - \kappa_2^2) n U (\Omega_1 \cos 2nt + \Omega_2 \sin 2nt) \\ & + \lambda (\kappa_3^3 + \kappa_4^3) n^2 (\Omega_1 \cos nt + \Omega_2 \sin nt). \end{aligned}$$

If all the terms be collected on the same side there result three equations of the form

$$a_1\Omega_1 + b_1\Omega_2 + c_1 w = 0,$$
$$a_2\Omega_1 + b_2\Omega_2 + c_2 w = 0,$$
$$a_3\Omega_1 + b_3\Omega_2 + c_3 w = 0,$$

in which the coefficients may contain the operator d/dt, or circular functions of the time.

The equations giving the motion under gravity of a boomerang with the two distortions will differ from these in three ways.

First of all, if $l'm'n'$ be the direction cosines of a line drawn vertically downwards, the equations of translation will be

$$m(\dot{U} + w\Omega_2) = mgl',$$
$$m(-w\Omega_1 + U\theta_3) = mgm',$$
$$m(\dot{w} - U\Omega_2) = R_0 + R_1 + R_2 + mgn',$$

P_1, P_2, Q_1, Q_2, R_1, R_2 bearing to Z_1, Z_2 the same relations as P_0, Q_0, R_0 to Z_0. From the second equation

$$\theta_3 = \frac{gm'}{U},$$

and from the first

$$\dot{U} = gl.$$

Now θ_3 in numerical value is comparable with $\frac{1}{4}$, and n with 30, so that the additional terms in (1) due to the consideration of θ_3 will be of negligible magnitude. Again, l' is small when the path is nearly in a horizontal plane; hence, in considering the steady motion corresponding to a particular portion of the path, the change in U need not trouble us.

When the forces due to the distortions—and these will not involve Ω_1, Ω_2, w—are introduced, we obtain

$$a_1\Omega_1 + b_1\Omega_2 + c_1 w = P_1 + P_2,$$
$$a_2\Omega_1 + b_2\Omega_2 + c_2 w = Q_1 + Q_2,$$
$$a_3\Omega_1 + b_3\Omega_2 + c_3 w = R_1 + R_2 - mg\cos\theta,$$

where θ is the angle between the axis of revolution and the upward vertical, and $\dot{\theta}/n$ being small, we shall treat θ in the third equation as constant.

We next regard the right-hand sides of these equations as expanded in the form

$$A_1 + B_1 \sin nt + C_1 \cos nt + D_1 \sin 2nt + E_1 \cos 2nt + F'_1 \sin 3nt + G'_1 \cos 3nt,$$
$$A_2 + B_2 \sin nt + C_2 \cos nt + D_2 \sin 2nt + E_2 \cos 2nt + F'_2 \sin 3nt + G'_2 \cos 3nt,$$
$$A_3 + B_3 \sin nt + C_3 \cos nt + D_3 \sin 2nt + E_3 \cos 2nt,$$

in which A, B, C . . . are constants.

On substituting the values of F_1, F_2, we find

$$A_1 = -\frac{\kappa n U}{2\rho}\left(\frac{m_0^2}{\kappa_1^2} + \frac{m'^2_0}{\kappa_2^2}\right) - \frac{\lambda U}{2\tau}\left\{\left(\frac{l_3^2}{\kappa_1^2} + \frac{l_2^2}{\kappa_2^2}\right)U^2 + \left(\frac{l_8^4 + 2l_9^4 + 3l_{10}^4}{\kappa_1^2} + \frac{3l_7^4 + 2l_8^4 + l_9^4}{\kappa_2^2}\right)n^2\right\},$$

$$A_2 = \frac{\lambda U}{2\rho}\left\{\left(\frac{m'^2_0}{\kappa_1^2} + \frac{m_0^2}{\kappa_2^2}\right)U^2 + \left(\frac{3m'^4_3 - 2m_4^4 - 2m_5^4 + 3m'^4_5}{\kappa_1^2} + \frac{3m_3^4 - 2m'^4_3 - 2m_4^4 + 3m_5^4}{\kappa_2^2}\right)n^2\right\} - \frac{\kappa}{2\tau}\left\{\frac{2l_2^2 + l_3^2}{\kappa_1^2} + \frac{l_2^2 + 2l_3^2}{\kappa_2^2}\right\} nU,$$

$$A_3 = \frac{\lambda n}{\tau}\left\{2\left(l_2^2 + l_3^2\right)U^2 + \left(l_7^4 + l_8^4 + l_9^4 + l_{10}^4\right)n^2\right\} - mg\cos\theta.$$

Our equations may now be satisfied by the infinite series

$$\Omega_1 = \alpha_1 + \beta_1 \sin nt + \gamma_1 \cos nt + \delta_1 \sin 2nt + \ldots$$
$$\Omega_2 = \alpha_2 + \beta_2 \sin nt + \gamma_2 \cos nt + \ldots$$
$$w = \alpha_3 + \beta_3 \sin nt + \gamma_3 \cos nt + \ldots$$

which are convergent, since the ratio of the coefficients of $\sin \overline{r+1}\, nt$ and $\cos \overline{r+1}\, nt$ to those of $\sin rnt$ and $\cos rnt$ proves to be ultimately comparable with κ/mr.

If we adopt the notation

$$U^2 + \left(\frac{\kappa_6^4 + \kappa_7^4}{\kappa_1^2} + \frac{\kappa_5^4 + \kappa_6^4}{\kappa_2^2}\right)\frac{n^2}{2} = U_1^2,$$
$$U^2 + \left(\kappa_1^2 + \kappa_2^2\right)n^2 = U_2^2,$$
$$m - \frac{\kappa}{4}\left(\frac{\kappa_2^2}{\kappa_1^2} + \frac{\kappa_1^2}{\kappa_2^2}\right) = m_1,$$
$$\frac{1}{\kappa_1^2} + \frac{1}{\kappa_2^2} = \frac{1}{\kappa'^2},$$

the non-circular terms on the left-hand sides become

$$m\dot{\Omega}_1 + \lambda U_1^2\Omega_1 + 2m_1 n\Omega_2 - 2\lambda n U w,$$
$$-2m_1 n\Omega_1 + m\dot{\Omega}_2 + \lambda U_1^2\Omega_2 - \frac{\kappa U w}{\kappa'^2},$$
$$-\lambda\left(\kappa_1^2 + \kappa_2^2\right)nU\Omega_1 - mU\Omega_2 + m\dot{w} + \lambda U_2^2 w.$$

Hence the substitution of the series for Ω_1, Ω_2, w gives as the equations for $\alpha_1, \alpha_2, \alpha_3$

$$\left.\begin{aligned} &\lambda U_1^2\alpha_1 + 2m_1n\alpha_2 - 2\lambda nU\alpha_3 + f_1 = A_1 \\ &- 2m_1n\alpha_1 + \lambda U_1^2\alpha_2 - \frac{\kappa U\alpha_3}{\kappa'^2} + f_2 = A_2 \\ &- \lambda(\kappa_1^2 + \kappa_2^2) nU\alpha_1 - mU\alpha_2 + \lambda U_2^2\alpha_3 + f_3 = A_3 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (2),$$

where f_1, f_2, f_3 are linear functions of α, β, γ . . . in which the constant coefficients all contain λ or κ, but not the numerically more important quantity m, as a factor.

In order, then, to obtain a steady motion about which minute oscillations are going on, we neglect the terms f_1, f_2, f_3 in our first approximation. This is equivalent to taking two points on the path at an interval corresponding to a number of complete revolutions (say twelve), and asserting that the angular change in the axis of rotation is that due to the non-periodic portion or mean of the couples in action during that period.

Stability.

For steady motion to be possible it is necessary that the values of Ω_1, Ω_2, w, given by the equations

$$\begin{aligned} &m\dot{\Omega}_1 + \lambda U_1^2\Omega_1 + 2m_1n\Omega_2 - 2\lambda nUw = 0 \\ &- ln_1n\Omega_1 + m\dot{\Omega}_2 + \lambda U_1^2\Omega_2 - \frac{\kappa Uw}{\kappa'^2} = 0 \\ &- \lambda(\kappa_1^2 + \kappa_2^2) nU\Omega_1 - mU\Omega_2 + m\dot{w} + \lambda U_2^2 w = 0 \end{aligned}$$

shall be always small.

On inserting numerical values, it appears that this condition is satisfied if the ratio of n to U be large enough to give the determinant

$$\begin{vmatrix} \lambda U_1^2, & 2m_1n, & -2\lambda nU \\ -2m_1n, & \lambda U_1^2, & -\frac{\kappa U}{\kappa'^2} \\ -\lambda(\kappa_1^2 + \kappa_2^2) nU, & -mU, & \lambda U_2^2 \end{vmatrix}$$

a positive value.

If $2\alpha = \pi - \beta$ where β is small, the motion is unstable unless with actual values, $n > 270$.

When $2\alpha = 120°$ the critical value of n is 26, and when the arms are at right angles, stability is secured when $n = 22$.

These values are rather larger than those found necessary in practice, but their mutual relations are correct. The first time that a beginner attempts it, he can make a boomerang whose arms are at right angles travel steadily, but the more obtuse the angle, the more difficult is the throwing of the implement, and when $2a = 150°$ or upwards, and the material of which the boomerang is made is light, throwing it against a wind requires skill of a high order.

The values of the constants l_2, m_3, κ_1 . . . have been calculated for boomerangs whose arms are 36 centims. in length and 5 centims. in width, the mass per unit area being five-eighths of a gramme.

When these constants are substituted in the equations of steady motion, it is found that for a boomerang whose arms are at right angles, corresponding to the values

$$U = 2000, \qquad n = 40, \qquad \kappa U = 7, \qquad \lambda U^2 = 5,$$

are the velocities

$$\left.\begin{aligned} \Omega_1 &= -\frac{3.6}{\rho} + \frac{610}{\tau} + 1.9 \cos\theta \\ \Omega_2 &= \frac{4}{\rho} - \frac{2200}{\tau} - 6.8 \cos\theta \\ w &= \frac{680}{\rho} - \frac{480000}{\tau} - 1600 \cos\theta \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (3).$$

If we make $n = 30$, the values of Ω_1, Ω_2, w given by the equations are too large; this is due to the fact that the theoretical limit of stability ($n = 22$) is not sufficiently exceeded.

If the value of κU be taken as 5 instead of 7 (these being estimated inferior and superior limits of κU corresponding to $c = 6$, $f = 1/6$, and $c = 7$, $f = 1/5$) there appear

$$\left.\begin{aligned} \Omega_1 &= -\frac{3.2}{\rho} + \frac{270}{\tau} + \cos\theta \\ \Omega_2 &= \frac{2.9}{\rho} - \frac{2100}{\tau} - 5.7 \cos\theta \\ w &= \frac{630}{\rho} - \frac{460000}{\tau} - 1400 \cos\theta \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (4).$$

The velocities corresponding to a larger spin

$$U = 2000, \qquad n = 50, \qquad \kappa U = 5, \qquad \lambda U^2 = 5,$$

are

$$\left.\begin{aligned} \Omega_1 &= -\frac{2.8}{\rho} + \frac{100}{\tau} + .4 \cos\theta \\ \Omega_2 &= \frac{1.2}{\rho} - \frac{1200}{\tau} - 3.2 \cos\theta \\ w &- \frac{240}{\rho} - \frac{215000}{\tau} - 790 \cos\theta \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \quad (5).$$

It is interesting to compare these results with those belonging to a boomerang whose arms include an angle 120°.

Thus, taking

$$2\alpha = 120°, \quad U = 2000, \quad n = 50, \quad \kappa U = 7, \quad \lambda U^2 = 5,$$

we obtain

$$\left.\begin{aligned} \Omega_1 &= -\frac{8.9}{\rho} + \frac{700}{\tau} + 1.3\cos\theta \\ \Omega_2 &= \frac{3.8}{\rho} - \frac{2000}{\tau} - 3.1\cos\theta \\ w &= \frac{720}{\rho} - \frac{380000}{\tau} - 720\cos\theta \end{aligned}\right\} \quad \ldots \ldots \quad (6),$$

while the second estimate of κU, namely 5, yields

$$\left.\begin{aligned} \Omega_1 &= -\frac{10.7}{\rho} + \frac{510}{\tau} + \cos\theta \\ \Omega_2 &= \frac{4.6}{\rho} - \frac{2300}{\tau} - 3.5\cos\theta \\ w &= \frac{860}{\rho} - \frac{440000}{\tau} - 850\cos\theta \end{aligned}\right\} \quad \ldots \ldots \quad (7).$$

The values of ρ and τ in practice are usually comparable with 20 and 800 respectively.

That the form of the equations is correct, at any rate as regards a first approximation, is confirmed by the experience gained in making and throwing upwards of seventy boomerangs of different weights, shapes, and sizes.

If, for example, one of these does not curl sharply enough to the left (*i.e.*, Ω_2 is negative, but not numerically large enough), it is found that increasing the twist (*i.e.*, diminishing τ) will produce the desired effect. A further result will be an increase in Ω_1 and a consequent tendency to "sky;" this may be corrected by making the difference of curvature of the two surfaces more pronounced; a diminution in ρ will thus bring about a diminution of Ω_1.

Some of these implements were made with the express object of verifying particular terms. If there be no twist, and $\theta = \pi/2$, while ρ is not extremely large, Ω_1 is negative and Ω_2 positive; if, on the other hand, ρ is infinite, but τ is finite and positive, Ω_1 is positive and Ω_2 is negative.

From experiments made in this manner, with a somewhat smaller spin than that assumed above, I have deduced the formulæ

$$\left.\begin{aligned} \Omega_1 &= -\frac{5}{\rho} + \frac{200}{\tau} + 2\cos\theta \\ \Omega_2 &= \frac{2}{\rho} - \frac{1200}{\tau} - \frac{\cos\theta}{2} \end{aligned}\right\} \quad \ldots \ldots \quad (8),$$

in which, owing to the experimental difficulties, the numerical values of the coefficients may be regarded as lacking in accuracy; they may however be relied upon as, at any rate, of the correct order of magnitude. Of w I have observed nothing except that it does not exceed 500, and is probably smaller and negative.

A comparison of the theoretical results (3) and (4), or (6) and (7), obtained with different data for κU will show that the formulæ, as calculated, must be looked upon as giving only a rough estimate of the motion regarded quantitatively; but, in spite of the calculated value of w being excessive (between 600 and 1200 when $\cos\theta = \frac{1}{3}$), it will be seen that the discrepancies are of the kind that might be anticipated, and that the theoretical equations are qualitatively consistent with the experimental results given in (8).

Another piece of evidence is that furnished by non-returning boomerangs. If it be desired to make an efficient missile that shall travel in as straight a path as possible, it is natural to manufacture a boomerang without twist and with the curvature of the two faces the same. It is this form that many of the cruder Australian weapons possess.

Experiment and theory alike show, however, that if initially θ have a positive value less than a right angle (*i.e.*, the natural method of throwing be adopted), then Ω_1 will be positive and Ω_2 will be negative as long as θ is less than a right angle: when the plane of rotation has reached and passed through the horizontal position Ω_2 remains negative. The shape of the path is indicated in fig. 5, and it will be seen that it is far from straight.

Fig. 5.

Plan.

A path in one vertical plane could be secured by throwing an undistorted weapon with its plane of rotation accurately vertical; the least inclination, however, would grow, and the plane of initial motion be departed from; in any case, except for the reduction in the resistance of the air, the path in a vertical plane would yield no greater range than would be afforded by a spherical missile of the same weight.

We might attain the same end by designing the shape so as to make Ω_1, Ω_2 small when $\theta = 0$, and throwing the boomerang with its plane approximately horizontal. In that case the plane would remain horizontal, and the axis OX in it would soon be pointing in a direction slightly above the tangent to the path; a much longer flight would then be maintained, as the effect of gravity would be balanced by the upward pressure of the air on the lower surface of the projectile.

It is interesting to notice that this is the method that experience has taught the blacks to adopt. Their best non-returning weapons always have strongly developed positive rounding (the more curved surface is uppermost when thrown) and often a small negative twist; examination of the equations will show that these distortions will combine to produce the required results. An estimate of the efficiency of the shape may be made from the fact that as far as my experience goes, a boomerang of this type may be thrown more than twice as far as a spherical object of the same weight.

In figs. 6, 7 are given the plan and elevation of the path obtained with a boomerang

Fig. 6.

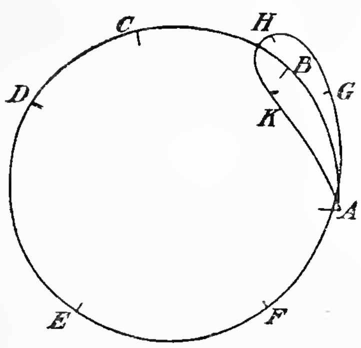
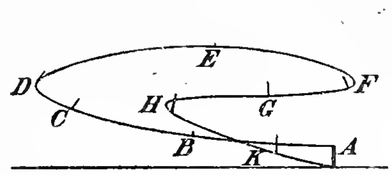


Fig. 7.



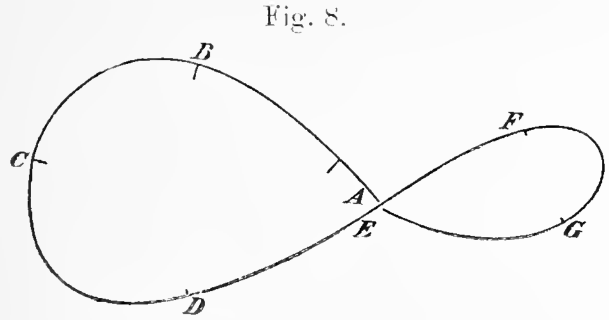
Elevation upon a vertical plane through AC.

designed to continue in its circular route as long as possible. The arms of the implement are at right angles, and the twist and rounding exaggerated a little; the initial plane of rotation is vertical, and as much energy as possible is imparted in the act of throwing, while the aim is slightly uphill.

The numerical value of Ω_2 is somewhat increased and that of Ω_1 diminished, so that when the weapon in its return journey is over the thrower's head, its axis of rotation, instead of being vertical, is inclined a little towards the inner side of the curve that it has described; the forward velocity, though reduced, being still unexpended, the original curve is continued, and the existence of Ω_3 implies that the plane of rotation will tilt slightly upwards and the tendency to fall be overcome.

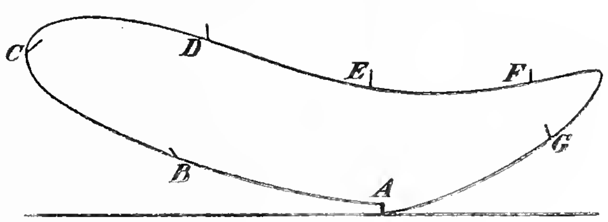
After the end H of this second loop has been reached, the forward velocity has still further diminished, and gravity brings the boomerang, still spinning fast in a nearly horizontal plane, to the ground near the starting-point. I have obtained second loops, which were thirty yards in length when measured horizontally, while, if the point H be high enough in the air, a third loop will be described before the boomerang alights.

Fig. 8.



Plan.

Fig. 9.



Elevation along CAG.

In figs. 8, 9 is represented the flight of a boomerang, of which the arms form an angle which is larger by about thirty degrees than that of the previous case. The axis

Fig. 10.

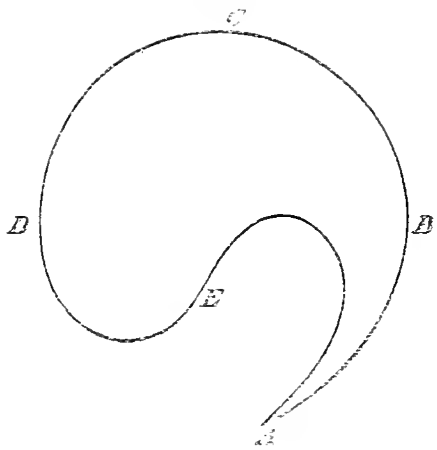
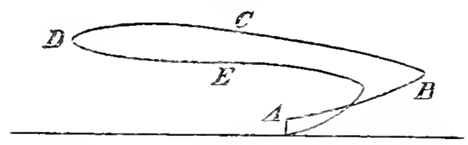
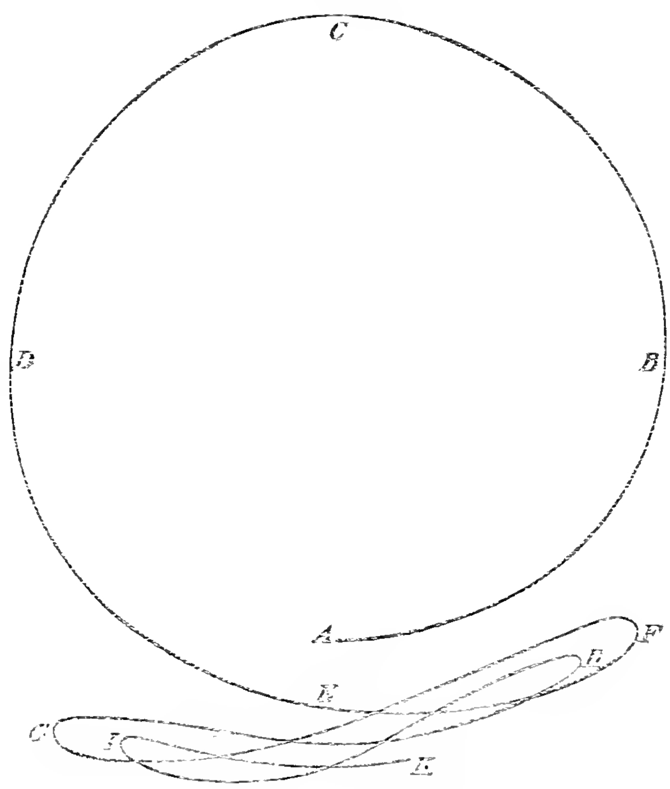


Fig. 11.



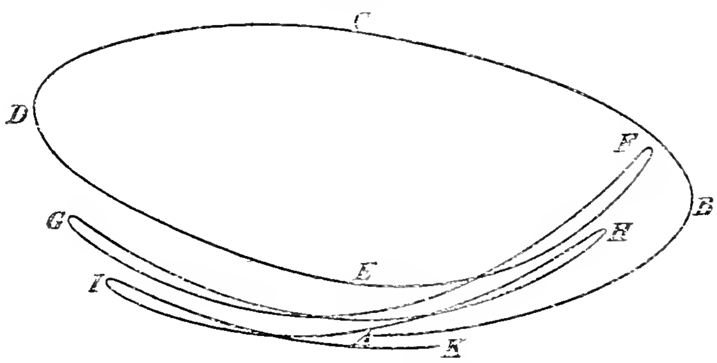
Elevation on the plane through BD.

Fig. 12



Plan. Scale $\frac{1}{1200}$.

Fig. 13.



Elevation on a plane parallel to DB.

of rotation may point at the outset rather upwards, and the initial direction of motion is slightly uphill.

As the theoretical angular velocities indicate, there will be an increase in the value of Ω_1, and this leads to the plane of rotation being horizontal when the implement passes over the thrower's head at E. The angular velocity along the axis OX will then turn the path to the right along EF, while Ω_2 implies that the body rises from E to F. After a short time the forward velocity has diminished so far that the descent from F to the ground is made quite slowly, under the influence of gravity checked by the rotation of the boomerang in a nearly horizontal plane.

Figs. 10, 11 illustrate the magnitude of the changes in the trajectory that are rendered possible by small variations in the shape of the missile. This path was traced by a boomerang which subsequently warped to a slight extent in such a way as to increase the twisting: the natural flight then became the figure of eight of the two previous diagrams.

Through the kindness of Mr. O. ECKENSTEIN, I have recently had the opportunity of seeing and throwing some boomerangs made by him, in which rounding was present, but no twisting; the angle between the arms was considerably more obtuse, the size increased, and the weight doubled.

An examination of the equations (3–7) will show that if the value of $\cos\theta$ be increased, the term due to gravity might be expected to replace for the most part that due to the twisting; further, as the angle between the arms is larger, a given amount of rounding will produce a greater effect in diminishing Ω_1.

When the proportions are rightly chosen, I have not found it difficult to obtain a return path; the plane of rotation is initially inclined at 15° instead of 90° to the horizon, and with a decidedly smaller forward velocity as much spin as possible must be imparted.

In the hands of one accustomed to its use, a boomerang of this type is capable of extremely interesting flights. For the remarkable diagrams (figs. 12, 13) which illustrate one of these, I am indebted to Mr. ECKENSTEIN.

INDEX SLIP.

III. *On the Orientation of certain Greek Temples and the Dates of their Foundation derived from Astronomical Considerations, being a Supplement to a Paper on the same subject published in the Transactions of the Royal Society in* 1893.

By F. C. PENROSE, *F.R.S.*

Received February 24,—Read March 11,—Revised June 28, 1897.

THE paper now presented to the Royal Society is a sequel to one on the same subject read here on April 27, 1893, and published in the Transactions for that year. In that paper the subject was explained at some length; it will, therefore, be unnecessary in this to repeat more than a very few explanatory observations.

The aim of this inquiry is to deduce the date of the foundation of a Greek (or Egyptian) temple from its orientation, but I confine myself entirely to Greek temples, in which, however, the same practice was followed which had previously been reduced to a system in Egypt (*vide* 'Dawn of Astronomy,' by Sir J. N. LOCKYER). Almost all the temples in Greece and its Colonies had an Easterly frontage, and the principal religious function in each temple took place on the morning of the day when the sun, as it rose above the visible horizon, shone through the open Eastern door directly upon the sanctuary, where there was usually a statue of the deity in the centre. As some time was requisite for the priests to prepare for the ceremony, the orientation of the temple was so directed as to combine with the sunrise the previous heliacal rising or setting of some conspicuous star which could also be observed from the sanctuary. In the absence of clocks the heliacal rising or setting of stars was very greatly observed by the ancients—the meaning of the term being that the star, when very slightly above the horizon, should just be visible in the twilight, before being extinguished by the dawn. The angle of the orientation depended primarily on the time of year chosen for the principal festival, but it would be liable to a slight modification for the sake of combining an heliacal star with the sunrise, and it is the latter consideration which offers the means of determining the date of foundation, because the stars, owing to the precession of the equinoxes, are affected by a slow, but steady movement, which alters the amplitude, as it is called, of their rising or setting—viz., the angular distance from the true East or West as the case may be, and which is reckoned positive if towards the North, and negative if towards the South.

A journey to Greece and my return by way of Calabria and Sicily during the spring of 1896 has enabled me to add the orientations of a considerable number of Greek temples to the list presented to the Society in 1893. The majority of these new examples are not indeed from Greece proper but from its Colonies in Italy and Sicily, but they conform to the same general law.

Whilst at Athens I occupied a room commanding an uninterrupted view of the Eastern sky, over the ridge of Mount Hymettus, which, although the weather was by no means exceptionally fine, enabled me to obtain a good many observations of heliacally rising stars; from which I select the following as best worth recording.

Sunrise stars seen at Athens in 1896.

Date.	Name of star.	Magnitude.	Altitude.	Depression of sun below true horizon.	Difference of azimuth between star and sunrise point.	Remarks.
March 19	Mars	= 1·3	8° 30′	13° 0′	30°	Planet brilliant, but θ Capricorni (4th mag.) about 3° distant from ♂, not seen with the naked eye.
„ 23	„	„	5 56	16 53	29	
„ 23	α Aquarii	3·2	10 15	14 59	9	Seen distinctly.
„ 25	„	„	14 37	11 42	10	Seen without doubt, but near the limit of *my* vision.
„ 25	Mars	= 1·3	13 5	8 45	35	An easy object.
April 12	γ Pegasi	3	7 30	10 57	0	Doubtful.

Of this last I must observe that after finding it with the help of an opera glass I could only say that I fancied I glimpsed it with the naked eye, but it was so bright in the glass that I was of opinion that a younger sight than mine might have seen it. At the same time it ought, I think, to be judged too near the limit of visibility to warrant the use of third magnitude stars *at rising* with less than twelve degrees of solar depression. This star does occur among the orientation stars, but always when rising heliacally, with the sun at least as deeply depressed as that.

Sunset Stars.

On April 21, at sea between Corfu and Brindisi, I saw the Pleiades. Altitude of η Tauri, 10° 5′. Depression of sun, 10° 50′, and 7° difference in azimuth from the sunset point. The constellation was undoubtedly seen, but not easily.

It is obvious that in average weather the visibility of rising or setting stars in twilight must depend upon their intrinsic brightness, and on the depression of the sun, as well as on the altitude of the star; and I have invariably found that when the heliacal rising of a star of decidedly less than the first magnitude has been one of the elements of an orientation, the conditions require that the sun must have been depressed to the extent of at least 11°. This view of the matter agrees very well with such actual observation as I have been able to make. In 1892 I saw Rigel setting after sunset at 2° 40′ altitude, the sun being 9° 48′ below the horizon. In March last I saw Mars easily at his rising before sunrise, altitude 13° 5′, the sun being 8° 45′ depressed. The star magnitude of Mars at that time was about 1·3 In 1892 I had seen Antares in the morning, at an altitude of about 16°, when the sun was not more than 7° 40′ below the horizon. On an evening of the same year, I saw γ Andromedæ, second magnitude, at an altitude of 9°, when the sun had sunk to 12°. The conclusion I have come to is that (1) a first magnitude star in fair average weather in Greece or Italy could be seen, when rising heliacally, at an altitude of 3°, the sun being 10° below the horizon; (2) that a second magnitude star should require an altitude of 3° 30′ with the sun 11° depressed; but that for a third magnitude star the sun's depression should not be less than 13°, and consequently I feel that in the elements of orientations in my former paper, I had over-estimated the *heliacal* visibility of the Pleiades in treating them as equivalent to a first magnitude star. I have, since then, given a good deal of attention to this constellation It is true that I have recorded one instance when it was seen with the sun depressed 10° 50′, but the considerable altitude of the Pleiades, 10°, was then in their favour, and I have not had the opportunity of observing them heliacally at a lower altitude. But, for the following reasons, I cannot assign to this constellation for the purpose of this inquiry, a greater heliacal value than that of a second magnitude star. When the twilight is at all luminous, or when in the neighbourhood of the moon, η Tauri seems to be shorn of the glory which surrounds it in a clear sky, when the constellation catches the eye as readily as a first magnitude star; and again, if viewed with strongly magnifying spectacles, the dispersed light seems very much inferior to that given out by Aldebaran. I am, therefore, fully persuaded that a second magnitude value for orientation purposes is the right value. I have, therefore, recalculated the elements of orientation of those temples in the first list, into which the *rising* of the Pleiades has entered, with the following results, viz.:—

Athens.—The archaic Temple of Minerva	B.C. 2020	instead of	1830.*
The Hecatompedon	,, 1495	,,	1150.
The earlier Temple of Bacchus	,, 1180	,,	1030.
Epidaurus.—The Asclepieium	,, 1370	,,	1275.†
And in the case of a second magnitude star (α Arietis) to be observed heliacally at an altitude of 3° 30′ instead of 3°:—			
Tegea.—The older temple	,, 1660	,,	1580.
The later ,,	,, 1140	,,	1080.

* In the first paper this date is given as 1530. It should have been 1830.

† As the star's altitude, owing to the mountain opposite, was considerable, viz., 7°, the sun's depression is taken at 10° 30′ instead of 11°.

With respect to the temple near Thebes, published in the former list, p. 831, some magnetic observations have been sent me which show an orientation differing by about 90° from that which I had deduced from my observations. The latter are perfectly consistent amongst themselves, but as I cannot affirm the impossibility of some accidental error, it seems best to withdraw that temple from the list, at least for the present.

The order in which the examples given below are placed, is simply that in which they were examined.

In the elements which follow, the rules which have been observed with respect to the sun's depression and the altitude of the stars, when heliacal, are generally in accordance with the remarks made above on the visibility under such circumstances of the different magnitudes. In some cases the altitudes are affected by the height of the visible horizon. The propriety of these rules appears to be strengthened by the statement of BIOT, as derived from PTOLEMY, in his 'Recherches sur l'Année Vague des Égyptiens,' p. 53, namely, that in Egypt the sun's depression for heliacal purposes was considered to be 11°. And as it has been shown by Sir J. N. LOCKYER in 'The Dawn of Astronomy' that such stars as α Columbæ and γ Draconis were included among those used for the Egyptian temples, 11° would be very suitable for a general rule; although so deep a depression would be unnecessary for a star of the first magnitude, as I have personally noticed in Greece. In a great many cases I have added the effect produced upon the dates by the variation of some of the elements. But the dates which follow the rule I believe to be the most probable, as well as the most systematic. The calculations have all been carried out to seconds of arc and time, but the results as entered, are restricted to the nearest minute. The years of the dates of the temples can, of course, be only considered as approximate; relatively, however, they may be more implicitly trusted. The days of the month given are less uncertain, as they depend upon the sun's place, which results immediately from the orientation. It is an important element, as marking the time of year of the principal festivals.

As respects the identification of the stars there is seldom much uncertainty. In the first place, for solar temples the possible stars are so few. Thirteen stars and two constellations, the Pleiades and Aquarius, make up the whole list of those that are both bright enough and near enough to the ecliptic to be seen heliacally in connection with the sun through the narrow eastern or western openings; and six of the thirteen stars, including two of the brightest, viz., Aldebaran and Regulus, do not appear to have been used.

In the former paper, p. 819, I described briefly some of the methods which may be followed in the search for the star. That which I have myself adopted is here rather more fully explained. On a stereographic projection of the sphere, taken on the pole of the ecliptic, using a mean obliquity, the pole of the equator is also shown, with R.A. hour-lines and parallels of declination, and upon it also the principal available stars, taking 1850 as the standard year, are plotted down. On this I lay a sheet of tracing paper with a straight line drawn upon it, and in the first position placed so as to coincide with the solstitial colure of the projection. Then, having found for any particular temple, by calculation from the orientation measures, the sun's declination and the corresponding vernal and autumnal places, I mark two points on the tracing paper having the same parallel of declination as the sun, but with their right ascensions less by the difference of the hour angles of the two bodies—for a first trial one hour's difference may generally be taken. I then turn the tracing paper round upon the pole of the ecliptic as a centre, following the direction of the hours of R.A. on the projection, until the straight line above-mentioned having started from the solstitial colure, makes an angle with it, corresponding to a suitable time within archæological limits. If, during that operation, one of the points marked as above, falls very near to one of the stars on the projection, it may be presumed that that star now occupies the place both in R.A. and declination of the heliacal star sought for, and has then to be examined more minutely. If the coincidence is so close that an adjustment of the amplitude within the narrow limits of the field of view will make it exact, it will give a solution of the problem for an heliacally rising star. Should, however, no star be found within range, search must be made in an almost identical manner for a setting star. (I have been, however, accustomed to search for a setting star in all cases, even after finding a suitable rising one.) The difference between the two cases will be, that when a setting star has been used its R.A. will differ from that of the sun by the sum of the sun's hour angle added to that of the star, and the declination will, in general, have a different sign from that of the sun.

Although, in working out the former list with these four lines of trial, in no case more than one solution was to be found—in the trials for the present list, in the case of two temples (viz., temple A at Selinus (p. 62), and that attributed to Minerva at Syracuse), the claims of more than one star, found in the preliminary search, have had to be considered. The former temple could have agreed either with the setting of Spica, the setting of α Arietis, or the rising of γ Pegasi. The first, however, was found to be quite inadmissible from its date, 1400 B.C., which would reach back far beyond any other Sicilian example. α Arietis would be acceptable for date, but I give the preference to γ Pegasi; on the ground that the two temples, called C and D, close adjoining, are evidently following the movement of α Arietis and are adapted for autumnal festivals; whereas γ Pegasi would provide for one in the spring, and is quite as acceptable in respect of date. In the case of the temple at Syracuse, α Arietis had to be considered as well as Spica—both rising—but the derived date (the former about 400 years earlier than the latter) as well as the greater brilliancy of the star, give Spica decidedly the preference. Besides these two temples I have met with no other uncertainties of this nature, and the two lists contain all the temples of which I have obtained sufficiently complete particulars, with the exception of three, namely, the Theseum, and the later temple of Bacchus at Athens, and the temple, as re-built, of Jupiter Olympius, and the last re-building at Ephesus, which will be mentioned in a group of temples of late foundation at the end.

The elements of orientation of four very small temples at Athens, additional to the former list, are given below. Two of them—both in the precincts of Dionysus ἐν λίμναις, recently explored by the German archæologists—are so placed that they could have had no connexion with the rising sun. The orientation of one of them is extra solstitial, and both are interfered with by high ground towards the east. It is, therefore, reasonable to inquire whether they might belong to the class of temples in which the midnight appearance of one of the brighter stars at their rising or setting at a north-westerly or south-westerly door was looked for. The first on the list was well provided for in this respect by Arcturus. The second by Antares. The date of the latter seems early, but not earlier than some of the other sanctuaries at Athens.

Athens.—Latitude 37° 58′ 20″.

Name of temple.	Orientation angle.			Elements of star.	Name of star.
Lower temple Dionysus ἐν λίμναις	317° 28′	A	Amplitude	+ 44° 28′ N.W.	Arcturus setting
		B	Corresponding altitude	3°	
		C	Declination	+ 35° 43′	
		D	Hour angle	$7^h\ 35^m$	
		E	R. A.	$12^h\ 5^m$	
		F	Approximate date .	850 B.C., July 19	
Upper temple Dionysus ἐν λίμναις	250° 30′	A	Amplitude	− 16° 39′ S.W.	Antares, setting
		B	Corresponding altitude	3°	
		C	Declination	− 11° 2′	
		D	Hour angle	$5^h\ 10^m$	
		E	R. A.	$13^h\ 2^m$	
		F	Approximate date .	1700 B.C., June 20	

The other two Athenian examples are ordinary solar temples. The first on the list is a small temple near the Olympieium and a little to the south of it, which has been very recently discovered.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Dedication unknown	274° 27′	A	Amplitude of star or sun	+ 1° 14′ E.	− 4° 27′ E.	Spica, rising
		B	Corresponding altitude	5° 20′	4° 42′	
		C	Declination . . .	+ 4° 17′	− 0° 36′	
		D	Hour angles . . .	$5^h\ 46^m$	$6^h\ 49^m$	
		E	Depression of sun when star heliacal	. .	10°	
		F	R. A.	$11^h\ 3^m$	$12^h\ 6^m$	
		G	Approximate date .	780 B.C.,	Sept. 23	

The ancient Asclepieium.—There are remains of two temples very near each other in the same precinct. The foundations only remain. Those of the later temple are insufficient to supply the angle, but it was probably parallel to the adjacent stoa—if so the angle was 263° 33′, and the axis of the temple would have followed the precessional change of α Arietis at a date of about 140 years later than the other, which would agree with the architecture of the stoa.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Ancient temple of Esculapius.	264° 27′	A	Amplitude of star or sun	+ 9° 24′ E.	+ 5° 33 E.	α Arietis, rising
		B	Corresponding altitude	4°	3° 25′	
		C	Declination . . .	+ 9° 52′	+ 6° 28′	
		D	Hour angles . . .	$6^h\ 10^m$	$7^h\ 18^m$	
		E	Depression of sun when star heliacal	. .	11	
		F	R. A.	$23^h\ 52^m$	$0^h\ 59^m$	
		G	Approximate date .	560 B.C.,	Apr. 5	

If the sun's depression had been 12°, the derived date would be 720 B.C.

On revisiting the Heræum of Argos, I was enabled to measure the orientation of the older temple from the foundations of the actual cella wall, and found that the angle differed only slightly from what I had deduced for it in 1893 (see p. 833 of the former paper).

Near Argos.—Latitude 37° 41′ 10″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
The ancient Heræum	287° 50′	A	Amplitude of star or sun	– 15° 33′ E.	– 17° 50′ E.	Antares, rising
		B	Corresponding altitude	3°	2° 49′	
		C	Declination . . .	– 10° 22′	– 12° 14′	
		D	Hour angles . . .	$5^h\ 12^m$	$6^h\ 13^m$	
		E	Depression of sun when star heliacal	..	10°	
		F	R. A.	$12^h\ 56^m$	$13^h\ 57^m$	
		G	Approximate date .	1830 B.C.,	Oct. 24	

Variations { Star's altitude 3°, sun's depression 11°, date 1900
,, ,, 4°, ,, ,, 10°, ,, 1900
,, ,, 4°, ,, ,, 11°, ,, 1980

The foundations of the Temple of Apollo at Delphi have now been fully explored by the French archæologists, and there is evidence, both historical and structural, of the temple having been rebuilt—and, as it appears, rebuilt nearly, but not exactly, on the same site as before. Many fragments of an older structure have been used in the existing basement, but they are built on a line differing by about 3° from what seems to have been the original orientation, which was presumably parallel to the terrace wall—namely, the well-known wall of polygonal masonry covered with inscriptions. The orientation angle of this wall is 231° 17′, and that of the present temple 227° 53′.

The peculiar situation, a narrow ledge of moderately sloping ground on a mountain side, in a nook formed by two spurs of Parnassus, evidently determined the orientation of the temple; but this is so completely extra-solstitial, that at no period of the year could the rising sun shine along the axis. Moreover, one of the two poetic summits of the mountain, together with an eminence on the left bank of the Pleistus, preclude any sunrise illumination upon the temple for considerably more than half the year, and a favourable gap does not occur till about 12° of south amplitude, where the rising sun can surmount the hills at an altitude of 3°. The western view is less impeded: a sloping line of ground opposes itself to the axis of the present temple, at an altitude of about 3°, or, if looking parallel to the inscribed wall, about 3° 30′. It is evident, therefore, that for any solar or stellar theory on the orientation of this temple the conditions are unusually complicated. At Bassæ (*vide* p. 815 of the former paper on this subject) the temple lies very nearly north and south, but there was an eastern door to the sanctuary to admit the sunrise at right angles to the axis. In a few instances in Greece (more frequently in Egypt), when the orientation is extra-solstitial, it can be traced to the rising or setting of a first magnitude star. None of these, however, are available in this case. Sirius would, indeed, have transited the western axis, but at too great an altitude to lend any probability to the hypothesis; and it seems almost self-evident that no satisfactory explanation could be made for Apollo's temple without the sun, even though the rule may have to be somewhat exceptionally treated.

At Bassæ, as we have seen, the eastern door admitted the sunrise at right angles to the axis of the temple: at Delphi, assuming the case of an earlier temple parallel to the inscribed wall, the sunrise would strike the flank at an angle of 51° instead of 90°. At that angle seven-ninths of any opening prepared to receive it would still be available, and the oblique light so thrown would be quite as effective as, or more so than, the direct. Assuming, then, from the abundance of evidence drawn from ordinary cases, that this exceptional temple would have followed in the main the general rule, we may proceed to examine whether any suitable star can be found which, at its setting in the south-west in the direction of the axis of the temple, would have given the proper warning of the sun's approach. β Lupi, of the third magnitude, is such a star, conspicuous enough by itself as a setting star, but the more so on account of its neighbour, κ Centauri, less than a degree apart, and of not much inferior brightness. The elements would be as follows :—

Delphi.—Latitude 38° 27′ 33″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Ancient temple of Apollo	231° 17′	A	Amplitude of star or sun	− 40° 29′ S.W.	− 12° E.	β Lupi, setting
		B	Corresponding altitude	3° 30′	3°	
		C	Declination . . .	− 28°	− 7° 39′	
		D	Hour angles . . .	$3^h\ 57^m$	$6^h\ 43^m$	
		E	Depression of sun when star heliacal	. .	13° 4′	
		F	R. A.	$12^h\ 9^m$	$22^h\ 49^m$	
		G	Approximate date .	970 B.C.,	March 1.	
Later temple of Apollo	227° 53′	A	Amplitude of star or sun	− 42° 55′ S.W.	− 12° S.W.	β Lupi, setting
		B	Corresponding altitude	3°	3°	
		C	Declination . . .	− 30°	− 7° 39′	
		D	Hour angles . . .	$3^h\ 50^m$	$6^h\ 34^m$	
		E	Depression of sun when star heliacal	. .	11° 23′	
		F	R. A.	$12^h\ 24^m\ 30^s$	$22^h\ 49^m$	
		G	Approximate date .	630 B.C,	March 1	

It will be seen that the star's amplitude agrees very closely with the orientation angle of the existing foundations, and the divergence of line in the two temples is accounted for, as in so many instances, by the movement of the star.

Calabrian Greek Temples.

At Taranto there is a fragment of a Doric temple of which two columns only are known to exist with their foundations, but they are in sufficient preservation to give measurements for orientation, and from these I deduce the following elements. The remains themselves have an archaic appearance suitable to the date arrived at below.

Taranto.—Latitude 40° 28′.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Dedication unknown	294° 25′	A	Amplitude of star or sun	−25° 9′ E.	−24° 25′ E.	Antares, rising
		B	Corresponding altitude	3°	1°	
		C	Declination . . .	−16° 46′	−17° 39′	
		D	Hour angles . . .	4h 43m		
		E	Depression of sun when star heliacal	. .	10°	
		F	R. A.	13h 55m	15h 4m	
		G	Approximate date .	640 B.C.,	Nov. 10	
If the sun's depression had been 11° the derived date would be 730 B.C.						

Metapontum.—Latitude 40° 23′.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Foundations near San-Sansoni. Dedication unknown	306° 39′	A	Amplitude of star or sun	+36° 18′ W.	−35° 27′ E.	β Geminorum, setting
		B	Corresponding altitude	4°	3°	
		C	Declination . . .	+29° 38′	−23° 46′	
		D	Hour angles . . .	7h 29m	5h 31m	
		E	Depression of sun when star heliacal	. .	10°	
		F	R. A.	5h 2m	18h 2m	
		G	Approximate date .	610 B.C..	Dec. 21	
If the sun's depression had been 11° the derived date would be 760 B.C.						

Metapontum.—Latitude 40° 27′.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
The temple with 15 columns	276° 57′	A	Amplitude of star or sun	−1° 17′ E.	−6° 57′ E.	γ Pegasi, rising
		B	Corresponding altitude	4°	0	
		C	Declination . . .	+1° 37′	−5° 17′	
		D	Hour angles . . .	$5^h\ 44^m$	$6^h\ 50^m$	
		E	Depression of sun when star heliacal	. .	13°	
		F	R. A.	$22^h\ 7^m$	$23^h\ 12^m$	
		G	Approximate date .	580 B.C.,	March 6–7	

With solar depression 14° the derived date would be 680 B.C.

Of the celebrated temple of Juno Lacinia, on Cape Colonna, near Croton, some foundations of the cella wall remain, and one Doric column of fine proportion is still standing, but very precariously, on the very edge of the sea cliff, which is continually falling away.

Near Croton.—Latitude 39° 1′ 48″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Juno Lacinia	267° 26′	A	Amplitude of star or sun	+6° 45′ E.	+4° 34′ E.	α Arietis, rising
		B	Corresponding altitude	3° 30′	0	
		C	Declination . . .	+7° 27′	+3° 32′	
		D	Hour angles . . .	$6^h\ 6^m$	$7^h\ 9^m$	
		E	Depression of sun when star heliacal	. .	11°	
		F	R. A.	$23^h\ 29^m$	$0^h\ 32^m$	
		G	Approximate date .	1000 B.C.,	March 28	

In the above elements the sun's amplitude is that of the northern edge of the eastern opening, as appears to have been used in some other cases. With the same amplitude, and depression 11° 44′, the date would be 1120 B.C.

If the sun had the amplitude of +2° 34′, viz., that of the temple's axis, the depression being 11°, the derived date would be 1280 B.C.

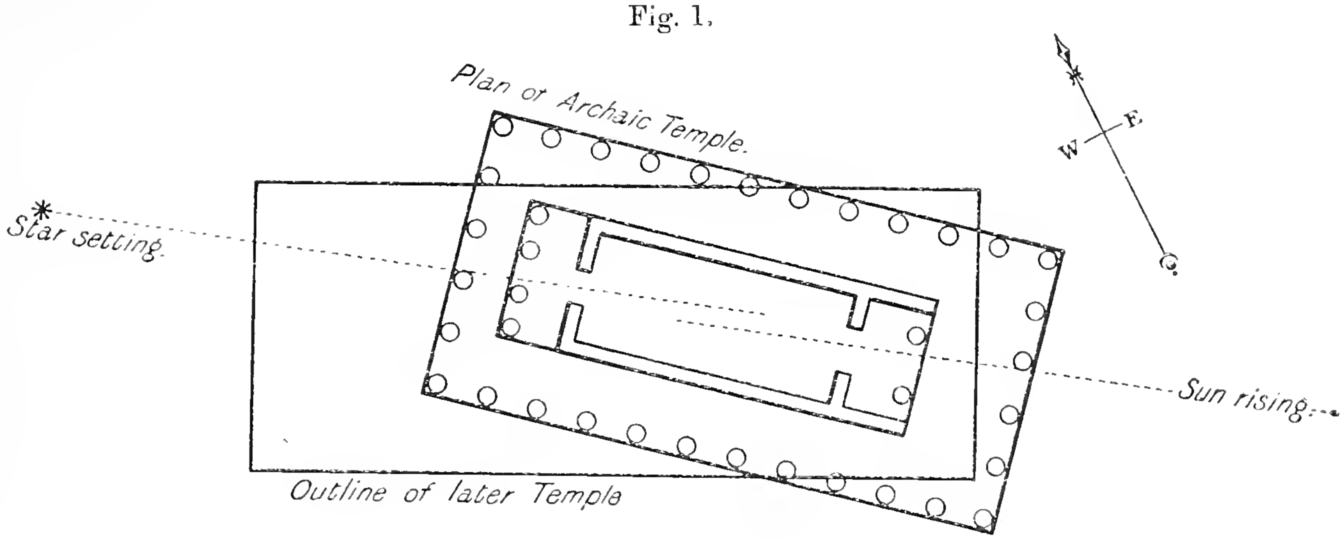
Near Gerace, amongst the remains of the ancient city of the Locri, there are two temple sites. Of these I visited one only, which is remarkable for having two temples (both Ionic) built obliquely one over the other (fig. 1), the divergence being too great to be accounted for by the movement of the same star, the orientation angle shown by the foundations of the older structure being 309° 37′ and of the later temple 296° 56′. The former is turned so much to the south of east that the sun, at its absolute rising from the sea-level towards which the front is turned, could not have shone centrally into the sanctuary, but the axis lies so near to the limiting angle of sunrise at the winter solstice that it could hardly have had any other intention than the admission of an early sunbeam. All that is necessary to assume is that in this case, as in some others, it was the northern jamb of the eastern doorway instead of the axis that was offered to the sunrise, and that also a small amount of provision was made for the very probable case of the sea horizon at the winter solstice being partially blocked by clouds, and that the effect of sunrise was looked for when it had attained a moderate altitude, which I have assumed to have been 4°. (I had supposed an analogous amount of solar altitude in the very similar case of extreme southern orientation at the temple near San Sansoni, at Metapontum.) In this case the wider intercolumniations of an Ionic temple permit a greater extension of the amplitude than would have been allowable in a Doric temple.

Locri—Latitude 38° 12′ 21″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
The ancient temple	309° 36′	A	Amplitude of star or sun	+35° 12′ W.	−34° 44′ E.	β Geminorum, setting
		B	Corresponding altitude	4°	4°	
		C	Declination . . .	+29° 40′	−23° 47′	
		D	Hour angles . . .	$7^h\ 21^m$	$5^h\ 36^m$	
		E	Depression of sun when star heliacal	. .	10°	
		F	R. A.	$5^h\ 3^m$	18^h	
		G	Approximate date .	610 B.C.,	Dec. 21	
If the sun's depression had been 11° 9′, the date would be 770 B.C.						
Locri—The later temple	296° 56′	A	Amplitude of star or sun	+26° 57′ W.	−26° 56′ E.	β Tauri, setting
		B	Corresponding altitude	3°	0°	
		C	Declination . . .	+22° 50′	−20° 51′	
		D	Hour angles . . .	$7^h\ 0^m$	$5^h\ 57^m$	
		E	Depression of sun when star heliacal	. .	12° 11′	
		F	R. A.	$3^h\ 2^m$	$15^h\ 59^m$	
		G	Approximate date .	430 B.C.,	Nov. 23	

It is possible that, considering the lateness of the year, the sunrise might here

Fig. 1.



Locri Temples, Calabria.

In the older temple there does not seem to have been a large western doorway, but the evidence does not exclude a smaller one.

also have been looked for a little above the sea level, and, assuming an altitude of 1° on this account, the date would have been about 500 B.C.; but in ordinary fine weather the sun would cast a strong shadow the moment any noticeable part of the orb had appeared above the sea.

Sicilian Temples.

Girgenti.—Latitude 37° 18′ 36″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple attributed to Juno Lacinia	264° 0′	A	Amplitude of star or sun	+7° 52′ E.	+6° E.	α Arietis, rising
		B	Corresponding altitude	3° 30′	0° 30′	
		C	Declination . . .	+9° 13′	+5° 4′	
		D	Hour angles . . .	$6^h\ 11^m$	$7^h\ 12^m$	
		E	Depression of sun when star heliacal	. .	11°	
		F	R. A.	$23^h\ 45^m$	$0^h\ 46^m$	
		G	Approximate date .	690 B.C., April 1		

The archæological evidence for the dedication of the above temple is not very strong, but its connection with α Arietis gives it very considerable support.

The two following examples at Girgenti, namely the temple of Hercules and the temple of Concord, have very nearly the same orientation angle.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple of Hercules	269° 56′	A	Amplitude of star or sun	+0° 39′ W.	+0° 4′ E.	Spica, setting
		B	Corresponding altitude	3°	0° 30′	
		C	Declination . . .	+2° 30′	+0° 22′	
		D	Hour angles . . .	$5^h\ 53^m$	$6^h\ 52^m$	
		E	Depression of sun when star heliaeal	..	10°	
		F	R. A.	$11^h\ 10^m$	$0^h\ 3^m$	
		G	Approximate date .	470 B.C.,	March 20	

The temple of Concord, of which the orientation angle is 270° 4′, would have the same star and the date about 18 years later.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
The Olympieium, sometimes called Temple of Giants	257° 35′	A	Amplitude of star or sun	+10° 44′ E.	+12° 25′ E.	α Arietis, rising
		B	Corresponding altitude	3° 30′	+0° 30′	
		C	Declination . . .	+10° 39′	+10° 9′	
		D	Hour angles . . .	$6^h\ 15^m$	$7^h\ 51^m$	
		E	Depression of sun when star heliaeal	..	15°	
		F	R. A.	$23^h\ 59^m$	$1^h\ 36^m$	
		G	Approximate date .	430 B.C.,	April 14	

See a subsequent remark on the deep solar depressions of temples of late date.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple of Castor	266° 0′	A	Amplitude of star or sun	+0° 14′ W.	+4° E.	Spica, setting
		B	Corresponding altitude	3°	+0° 30′	
		C	Declination . . .	+2° 0′	+3° 29′	
		D	Hour angles . . .	$5^h\ 51^m$	$7^h\ 18^m$	
		E	Depression of sun when star heliaeal	. .	13° 9′	
		F	R. A.	$11^h\ 23^m$	$0^h\ 32^m$	
		G	Approximate date .	400 B.C.,	Sept. 13	

In this case, with a solar depression of 10°, the correspondence with the heliaeal star would not take place till about 400 years later.

Segesta.—Latitude 37° 56′ 18″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Dedication not known historically	264° 36′	A	Amplitude of star or sun	+ 9° 21′ E.	+ 5° 24′ E.	α Arietis, rising
		B	Corresponding altitude	4°	3° 40′	
		C	Declination . . .	+ 10°	+ 6° 31′	
		D	Hour angles . . .	$6^h\ 11^m$	$7^h\ 18^m$	
		E	Depression of sun when star heliacal	. .	11°	
		F	R. A.	$23^h\ 53^m$	1^h	
		G	Approximate date .	550 B.C.,	April 5	
With solar depression 13° the date would have worked out 830 B.C.						

*Selinus**.—Latitude 37° 35′.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple C	274° 52′	A	Amplitude of star or sun	+ 8° 38′ W.	− 4° 52′ E.	α Arietis, setting
		B	Corresponding altitude	3°	0° 35′	
		C	Declination . . .	+ 8° 40′	− 3° 30′	
		D	Hour angles . . .	$6^h\ 12^m$	$6^h\ 40^m$	
		E	Depression of sun when star heliacal	. .	10°	
		F	R. A.	$23^h\ 40^m$	$12^h\ 32^m$	
		G	Approximate date .	795 B.C.,	Sept. 30	
With solar depression 11° 15′ the date would be 870 B.C.						

* See the diagram of temples at Selinus on p. 62.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple D	276° 18′	A	Amplitude of star or sun	+ 9° 56′ W.	− 6° 18′ E.	α Arietis, setting
		B	Corresponding altitude	3°	0° 35′	
		C	Declination . . .	+ 9° 42′	− 4° 38′	
		D	Hour angles . . .	$6^h 16^m$	$6^h 36^m$	
		E	Depression of sun when star heliacal	..	11°	
		F	R. A.	$23^h 50^m$	$12^h 42^m$	
		G	Approximate date .	610 B.C.,	Oct. 4	
Temple A	277° 21′	A	Amplitude of star or sun	− 0° 32′ E.	− 7° 21′ E.	γ Pegasi, rising
		B	Corrèsponding altitude	3° 30	0° 35′	
		C	Declination . . .	+ 1° 38′	− 5° 28′	
		D	Hour angles . . .	$5^h 47^m$	$6^h 50^m$	
		E	Depression of sun when star heliacal	..	13° 13′	
		F	R. A.	$22° 5^m$	$23^h 8^m$	
		G	Approximate date	550 B.C.,	March 5	

Temple B, a very small building, appears to be exactly parallel to Temple C.

Syracuse.—Latitude 37° 3′ 30″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple (forming part of the Cathedral) attributed to Minerva	269° 18′	A	Amplitude of star or sun	+ 3° 14′ E.	+ 0° 42′ E.	Spica, rising
		B	Corresponding altitude	3°	0°	
		C	Declination . . .	+ 4° 30′	+ 0° 34′	
		D	Hour angles . . .	$5^h 59^m$	$6^h 52^m$	
		E	Depression of sun when star heliacal	..	10	
		F	R. A.	$11^h 1^m$	$11^h 55^m$	
		G	Approximate date .	815 B.C.,	Sept. 20	

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
The Olympieium	277° 26′	A	Amplitude of star or sun	+ 9° 51′ W.	− 5° 30′ E.	α Arietis, setting
		B	Corresponding altitude	3°	0°	
		C	Declination . . .	+ 9° 40′	− 4° 23′	
		D	Hour angles . . .	6h 14m	6h 37m	
		E	Depression of sun when star heliacal	..	10°	
		F	R. A.	23h 49m	12h 40m	
		G	Approximate date .	610 B.C..	Oct. 3	

With solar depression 11° 7′, the date works out 695 B.C.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
The so-called Temple of Diana	271° 45′	A	Amplitude of star or sun	+ 1° 30′ E.	− 1° 45′ E.	Spica, rising
		B	Corresponding altitude	3°	0°	
		C	Declination . . .	+ 2° 22′	− 1° 24′	
		D	Hour angles . . .	5h 22m	6h 47m	
		E	Depression of sun when star heliacal	..	10°	
		F	R. A.	11h 19m	12h 13m	
		G	Approximate date .	450 B.C.,	Sept. 26	

South Italian Temples.

At Pæstum there are two temples—the great temple, presumably of Neptune, and a smaller temple, attributed to Ceres. There is also a large columnar structure, named the Basilica, of which the purpose has not been established. These three buildings are practically parallel with each other. The elements of orientation of one only are given.

Pæstum.—Latitude 40° 25′ 0″.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple of Neptune	273° 9′	A	Amplitude of star or sun	+ 1° 30′ W.	− 3° 9′ E.	Spica, setting
		B	Corresponding altitude	3°	5° 20′	
		C	Declination . . .	+ 3° 5′	+ 1° 4′	
		D	Hour angles . . .	$5^h\ 55^m$	$7^h\ 2^m$	
		E	Depression of sun when star heliacal	. .	11°	
		F	R. A.	$11^h\ 13^m$	$0^h\ 10^m$	
		G	Approximate date .	535 B.C.,	March 22–23	

The star could have been well seen *setting* with less solar depression; had this been calculated at 10°, the date would be 400 B.C., which is archæologically impossible. The only effect of the deeper depression would be to add about five minutes to the time given for preparation.

Pompeii.—Latitude 40° 45′.

Here there are two temples which can be referred to the Greek period. Of one, namely, that occupying part of the Triangular Forum, little remains but the foundation, but it was evidently a Greek Doric temple.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple in the Triangular Forum	301° 1′	A	Amplitude of star or sun	−26° 51′ E.	−28° 32′ E.	Antares, rising
		B	Corresponding altitude	4° 40′	4° 12′	
		C	Declination . . .	−16° 44′	−18° 15′	
		D	Hour angles . . .	$4^h\ 33^m$	$5^h\ 50^m$	
		E	Depression of sun when star heliacal	. .	10°	
		F	R.A.	$13^h\ 56^m$	$15^h\ 14^m$	
		G	Approximate date .	640 B.C.,	Nov. 12	

The other is the Temple of Isis, which seems to have been repaired, and probably considerably altered in external appearance during the interval which occurred between the earthquake, in A.D. 63, and the eruption of Vesuvius, in A.D. 79. A very noticeable point is that originally a large window-opening had been formed in the north-eastern inclosing wall of the Temenos, opposite the axis of the temple; which could have had no other purpose than that of permitting the rising sun to shine into the sanctuary. This opening, however, had been filled up with brickwork at some subsequent period. The orientation of this temple belongs to the summer solstice.

Name of temple.	Orientation angle.			Stellar elements.	Solar elements.	Name of star.
Temple of Isis	238° 39′	A	Amplitude of star or sun	+37° 18′ N.E.	+31° 21′ N.E.	β Geminorum, rising
		B	Corresponding altitude	3°	0° 49′	
		C	Declination . . .	+29° 33′	+23° 48′	
		D	Hour angles . . .	$7^h\ 37^m$	$8^h\ 36^m$	
		E	Depression of sun when star heliacal	. .	10°	
		F	R.A.	$4^h\ 56^m$	$5^h\ 56^m$	
		G	Approximate date .	750 B.C.	June 19	

The examples from Greece proper, which formed the first series of these studies, in by far the greater number of instances, demanded the hypothesis that although there was nothing inconsistent with archæological probability, yet that the date of foundation given by the orientations was much earlier than could be assigned to the existing remains on the spot, and that the walls, &c., which we are enabled to see, are those of a restoration generally on the same parallels as the original building.

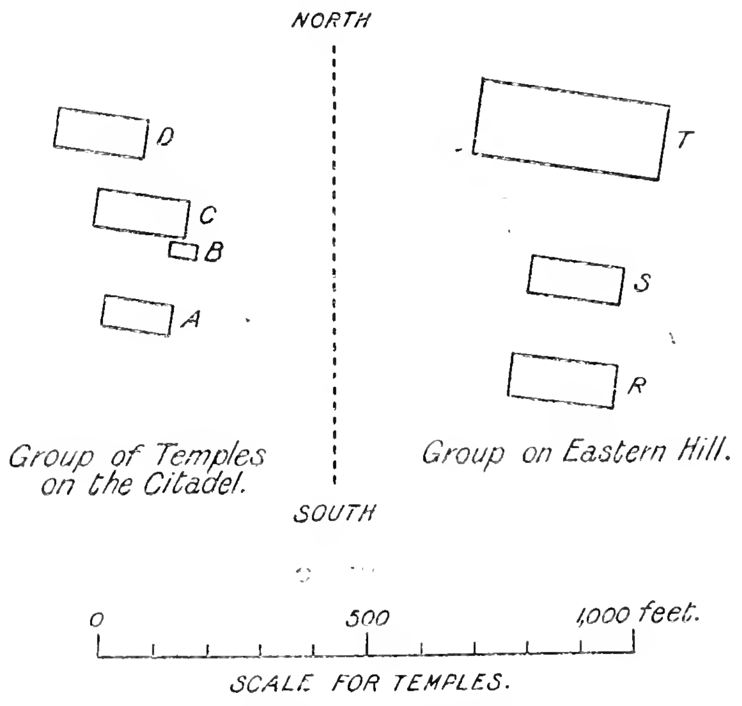
This is very much less the case in the list now produced from the Greek colonies in Magna Græcia and Sicily. In these, without the necessity of calling for a restoration, there are some very remarkable agreements between the deductions from the astronomic theory and ordinary historical data. In a few cases such as the Temple of Juno Lacinia, near Croton, the date, though still pre-historic, is nearly 900 years later than that which the same method of investigation assigns to the earliest example in Greece, and is by no means surprisingly early for a sanctuary of such celebrity. The most remarkable coincidences with the dates which might be derived from history are found in Sicily, where nearly two-thirds of the number of the examples which I have examined fall within periods of which Thucydides has preserved the tradition. In his sixth book* the great historian gives a short summary of the Hellenic colonization of the island, from which historians have arrived at the following conclusions with respect to the colonization of some of the principal cities, namely :—

Naxos	735 B.C.
Syracuse	734 ,,
Thapsus	728 ,,
Gela	690 ,,
Selinus	628 ,,
Agrigentum . . .	582 ,,

* Thucydides, vi., 2.

The temples of three of these towns are represented in the elements of orientations above given, and it will be seen that much more than half of the number of examples fall in with the dates derived from THUCYDIDES. As respects those which seem to require an earlier foundation, it must be borne in mind that THUCYDIDES, in naming those particular cities, is not speaking of the earliest occupation of the island, but of its colonization by expeditions from certain particular Greek states which took possession of different parts of Sicily. In the same book THUCYDIDES refers to a much more ancient colonization, in which he mentions exiles from Troy, who, after settling in Sicily, combined forces with certain Greeks from Phocis. Segesta seems to date from that period and occupation, and not only at Segesta, but elsewhere the earliest inhabitants of the country would naturally have possessed themselves of the sites most suitable for habitation and defence, and it would follow that when, in the eighth century B.C., the Greek colonists mentioned by THUCYDIDES possessed themselves of the country they would have found in some of the cities temples dedicated to the same Olympian gods that they themselves acknowledged.

Fig. 2.—Diagram of the Temples at Selinus.



The citadel group is about 1000 yards west and 600 yards south of the group on the Eastern Hill.

We learn, also, from Egyptian sources that even at the end of the fourteenth century B.C. there were alliances between the Greeks and the Sicilians. In the 5th year of RAMESES III., *i.e.*, about 1300 B.C., a combined attack was made upon Egypt by the Etruscans, Sardinians, Sicilians, Lycians, and Achæans. This account makes it extremely probable that these allies had a certain affinity with each other, and that, therefore, these Sicilians worshipped the same gods as the Achæans and

Lycians. Consequently, there is nothing archæologically inconsistent when we find a temple on a commanding site at Agrigentum, with a date as early as about 700 B.C., although the Hellenic colony, mentioned by THUCYDIDES, may not have gone there until 582. At Selinus the earliest astronomic date is about 800 B.C. This temple is that called for convenience temple C in fig. 2. It was here that the extremely archaic sculptures, now in the museum at Palermo, were found, representing Hercules carrying off the Cercopian giants, to which the date 795 seems as consistent as anything subsequent to 628 would be.

The orientation of this temple C shows that about an hour before the sun rose upon the axis, α Arietis was setting heliacally towards the West, nearly two centuries before the date of the Hellenic colonization of the city. When, however, the newcomers took possession, they would have found that the star had ceased to serve the purpose it was intended to fulfil, and accordingly it would appear that a new temple, D, was built closely adjoining it to the North at an angle sufficiently inclined to follow the star. The calculated date of this new work is eighteen years after the coming of the Hellenes. From the star α Arietis being thus connected with the orientation, it may be inferred that these two temples were dedicated to Jupiter. The fact that the achievements of Hercules had formed the subject of the metopes of the earlier temple by no means invalidates the supposition that the temple itself may have been so dedicated. The temple A falls well in the Hellenic period, and the architectural character, both of this and of temple D, appears to be in keeping with the astronomic dates. Temple B is extremely small, and is apparently parallel to the neighbouring temple C, but I did not examine it particularly. Of the group of temples on the Eastern hill I was unable from want of time to secure the orientations with sufficient exactness to justify my giving final elements. The angles, however, are approximately as below :—

	°	′
T, the great temple	276	40.
S	275	35.
R	275	40.

From these it may be inferred that the dates accord with the time of the Hellenic colonization.

At Segesta the star, α, Arietis, seems to favour the supposition that the original temple was dedicated to Jupiter. The existing structure, which from the refined character of its architecture seems to require a date of about the middle of the fifth century B.C. (which was also the epoch of a flourishing period among the Greek cities of Sicily), appears never to have been completely finished. As for the original foundation, if we accept the tradition countenanced by THUCYDIDES that the city was founded by refugees from Troy, the probability of the foundation of a temple in honour of Jupiter a century or more earlier does not require much argument.

At Agrigentum, with the exception of the temple attributed to Juno Lacinia, which has been already referred to, the astronomic and historical dates of the temples are in very close correspondence. DIODORUS informs us that the spoils obtained by the Agrigentines in their victory over the Carthaginians at Himera, B.C. 480,* enabled the inhabitants to embellish their city, but that afterwards they were in turn defeated, their city taken, and their power destroyed in 406, and that the great Temple of Jupiter, which had been completed with the exception of its roof, remained henceforth unfinished.† From these facts, in his contribution to the 'Supplementary Volume' of STUART and REVETT, Professor COCKERELL argues that that temple would probably have been commenced twenty years earlier. The orientation dates found for the Temples of Hercules, Concord, and Jupiter are respectively 470, 452, and 430 B.C. These dates, therefore, accord as strictly with the dates derived from historical probability as they do with the architectural character of the remains. Of three temples at Syracuse two fall within the historic period, and the other might be brought within it by a slight adjustment of the elements.

The orientation date derived for the great temple at Pæstum is 535 B.C. This is not only extremely accordant with the architectural character of the temple, but also with the mention by HERODOTUS of a Posidonian architect who was in repute about that time.‡

On page 825 of the former paper on this subject, I said that there were five temples of late foundation, of which I had measured the orientation, which lay within the solstitial limits, but for which I had been unable to find heliacal stars, but that the elements of two others, also of late foundation, had been included in the list. The occurrence of three temples of late foundation in the present list, which have been associated with stars, but invariably combined with a deeper depression of the sun than is found applicable to the older temples, and in this particular agreeing with the two temples of the former list just referred to, has led to a further examination of the five alluded to above, and in every case with an analogous result, namely, association with stars at a deeper depression of the sun. The following list includes all the temples of which I have the requisite data, and of which the foundation evidently falls later than the beginning of the fifth century B.C. Of fully half the number the date is accurately known.

In the case of those which are marked with an asterisk, the sun rises along the axis; of the others, in the direction of the north side of the opening.

* DIODORUS, xi., 25.

† *Ibid.*, xiii., 82.

‡ HERODOTUS, i., 167.

	Date B.C.	Name of temple.	Orientation angle.	Sun's depression.	Name of star.	Day of month.
1†	470	Theseum, Athens	283° 6′	17° 10′	Spica, rising . .	Oct. 5
2‡	445	Later Erechtheum, Athens .	265 9	12 0	α Arietis, rising .	April 9
3	About 430	*Later Temple, Locri	296 56	12 11	β Tauri, setting .	Nov. 23
4	About 430	*Girgenti, Temple of Jupiter .	257 35	15 0	α Arietis, rising	April 14
5	425	*New Heræum, Argos . . .	285 59	19 34	Aquarius, rising	Feb. 21
6	About 400	*Girgenti, Temple of Castor .	266 0	13 9	Spica, setting .	Sept. 13
7	About 360	*Olympia, the Metroum . . .	281 47	14 6	α Arietis, setting	Oct. 9
8§	355	Ephesus, the last rebuilding .	284 35	15 30	Spica, rising . .	Oct. 6
9‖	340	*Athens, new Temple of Bacchus	255 49	17 46	α Arietis, rising	April 23
10	174	*Athens, new Jupiter Olympius	270 0	12 0	Spica, setting .	March 27

In all the above cases, excepting No. 2, the depressions are quite unnecessarily deep for the purpose merely of seeing the stars distinctly. Spica or β Tauri could have been seen setting in the morning twilight with a solar depression of 8°.

Two explanations may be offered with respect to this alteration of the element of the sun's depression. One is, that attention had been called, as it hardly could help being called, to the fact that the heliacal star failed to keep its original connexion with sunrise, and that there would be a better chance of permanence if the interval between the two bodies were increased ; the other is, that the temple service had become more complicated, and that more time was required by the priests for preparation. Every additional degree of sun's depression would add about five minutes for this purpose.

† The festival of the Thesea is supposed to have been held on October 8 and 9.

‡ The autumnal return of the sun to the same point of the Erechtheum would take place on September 2. There does not appear to be an heliacal star available for that occasion, but the great festival of the Niceteria in honour of the victory at Marathon is considered to have been held on the 3rd of that month, when the sun would shine fully along the axis of the temple.

§ The foundations of this great temple of Ephesus show that at the last rebuilding the orientation had been changed about 9° from the original line, for the purpose of following the movement of the star.

‖ The date of this temple is considered to be that of the alterations made in the adjoining Dionysiac theatre, under the direction of Lycurgus.

INDEX SLIP.

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Phil. Trans., A. 1897, vol. 190, pp. 67–88.

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IV. *On the Passage of Heat between Metal Surfaces and Liquids in contact with them.*

By T. E. Stanton, *M.Sc., Assistant Lecturer in Engineering at University College, Liverpool.*

Communicated by Professor Osborne Reynolds, *F.R.S.*

Received April 7,—Read May 13, 1897.

Introduction.

The determination of the rate of transmission of heat from the surface of a heated metal to water in contact with it, or from hot water to a colder surface, is a problem of some difficulty, but is of such great importance in the theory of boilers and surface condensers, that further investigation would seem to be justified, especially as, although the theory of the subject has been much studied, yet owing to practical difficulties, the constants involved, so far as the author is aware, have not been definitely determined for cases which occur in practice.*

Peclet's experiments on the rate of transmission of heat from *water to water* across a metal plate,† throw much light on the question. His experiments were made to determine the thermal conductivities of various metals by measuring the heat passed through a metal plate, one side of which was exposed to steam and the other to water which was kept agitated by a stirrer. In these experiments, Peclet found that the heat transmitted was sensibly independent of the nature and thickness of the metal used, the conclusion being that on each side of the plate there was a film of water through which the heat was transmitted by conduction, and that compared to these, the thermal resistance of the plate was small.

This difficulty was overcome by an arrangement consisting of revolving brushes in contact with each side of the plate, so as to prevent the formation of a film on the surfaces, and by keeping the water in a violent state of agitation. In this way it was possible to keep the surfaces of the plate at the same temperature as the water in contact with them, and the conductivities of metals determined by this method agree with carefully determined conductivities obtained by other methods.

Peclet further pointed out, that before the brushes were used to prevent the

* Rankine's 'Steam Engine,' p. 266.

† 'Traité de la Chaleur,' p. 388.

formation of a film, the rapidity of the agitation of the water by the stirrer had a marked effect on the amount of heat transmitted. This fact has been often observed in experiments on steam boilers, in which the rate of evaporation has been shown to depend upon the rapidity of the convection of the water in the boiler.

Hence, in order to determine the rate of transmission for the case of the ordinary heating surfaces of boilers, it would be necessary to first determine the rate of convection of the water to and from the surface. The difficulty of measuring the convection in such a case is very great, and seemed an effectual bar to all experiment.

A means, however, of measuring the rate of convection appeared in the case of water flowing through metal pipes, at fairly high velocities, which could be determined, and it is to this case that the experiments described are confined.

The theory of the transfer of heat under these conditions has been stated by Professor OSBORNE REYNOLDS.* According to this theory, the heat carried off by any fluid from a surface is proportional to the internal diffusion of the fluid at or near the surface, that is, for a given difference of temperature between the fluid and the surface.

Professor REYNOLDS further points out that the rate of this diffusion will depend on—

(1.) The natural internal diffusion of the fluid when at rest.

(2.) The eddies caused by visible motion which mix the fluid up and continually bring fresh particles into contact with the surface, and that the combined effect of these two causes may be expressed as follows :—

$$H = At + B\rho vt \quad \quad (1),$$

where t is the difference of temperature between the surface and the fluid, ρ is the density of the fluid, v its velocity, and A and B constants depending on the nature of the fluid; H being the heat transmitted per unit of surface in unit time. In the same paper experiments were described giving evidence in favour of the truth of the above theory.

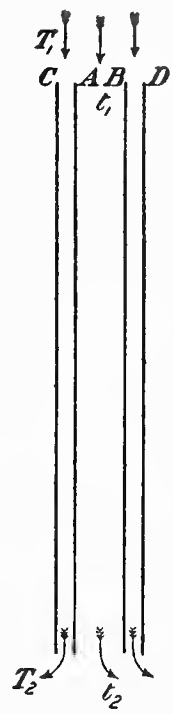
The chief difficulty in any experimental determination of the rate of transmission in metal pipes, lies in the fact that the temperature of the surface of the pipe varies from point to point along the pipe, and again tends to adjust itself by lateral conduction along the pipe. Hence, in order that any definite results may be obtained, it is necessary that the temperature of the pipe shall be constant throughout its length. It occurred to the author that this result might be obtained in the following way :—

In fig. 1, AB represents a tube placed vertically, and surrounded by a second tube CD, the annular space between them being used as a water-jacket. Now, if hot water at a temperature T_1 initially, flow down the water-jacket, and cold water, at a temperature t_1 flow down the pipe, then heat will be transmitted through the

* 'Proceedings, Manchester Lit. and Phil. Soc.,' 1874, p. 9.

walls of the pipe to the cold water, and if the *quantities* of water be the same in each case, the fall of temperature of the jacket water will be equal to the rise of temperature of the water flowing through the pipe, if means are taken to prevent the escape of heat from the jacket water to the outer walls.

Fig. 1.



In this way, although the range of temperature from water to water is diminishing, yet the *mean* value of $(T + t)$ is the same at all cross sections.

Now the temperature of the wall of the pipe at any cross section will not necessarily be a mean between the values of T and t at that section, but if the total fall of temperature from one end of the pipe to the other is small, say not more than 6° C., then *under certain conditions of flow which will be stated*, we may fairly assume that the ratio of the differences of temperature between (jacket water and wall) and between (wall and water flowing through pipe) is constant for the whole length of the pipe, and hence that the temperature of the pipe is constant throughout its length.

As regards the conditions of flow it is necessary to point out that if the motion in the pipe or the jacket is "steady," *i.e.*, the water flows in stream-lines parallel to the axis of the pipe, then the temperature of the water cannot be considered as uniform across any section of the pipe, and might vary considerably.

In order to avoid this condition of flow, all the experiments were made at velocities considerably higher than the critical velocity of water for the pipe in question; this "critical" velocity, as determined by Professor REYNOLDS' experiments,* being given by the expression

$$V_c = \frac{1}{278}\frac{P}{D} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

where

D = diameter of pipe in metres,
T = temperature of the water,
$P = (1 + \cdot 0336\,T + \cdot 000221\,T^2)^{-1}$,
V_c = critical velocity in metres per second.

Equation (2) gives the critical velocity for the smooth lead pipes used in those experiments, and it may be assumed that the critical value of the velocity for smooth copper pipes does not vary greatly from this.

Under these conditions, and using an apparatus as described above, it seemed possible to study experimentally the transmission of heat from metal to water, and water to metal, at varying velocities and ranges of temperature, by careful observations of the initial and final temperatures of the water and the temperatures of the surface.

* 'Phil. Trans.,' 1883, p. 976.

The apparatus being made, before more complicated experiments were tried, it was necessary to test the truth of the above assumption that, at velocities greater than the critical value for the pipe, the temperature at any cross section was constant at all points in the section, not immediately adjacent to the surface of the hot pipe.

To test this, the pipe and jacket were fixed vertically, with hot water at about 70° C. flowing up the jacket, and water at 20° initially flowing up the pipe. A cap was fitted to the end of the pipe, having a small pipe, of half the diameter of the outer one, projecting about 1 centim. into it, as shown in fig. 2.

In this way the water at the section AB was divided, the inner portion passing into the chamber C, and the outer portion into the chamber D, in which the temperatures were taken by thermometers.

It was found impossible to detect any difference in temperature between the water taken from the centre of the pipe and that from the outside, and this even in cases when the velocity was considerably below the critical value, thus showing that when the temperature of water flowing through a pipe is continually changing the motion is unsteady, even although the velocity is *below* the critical value.

Fig. 2.

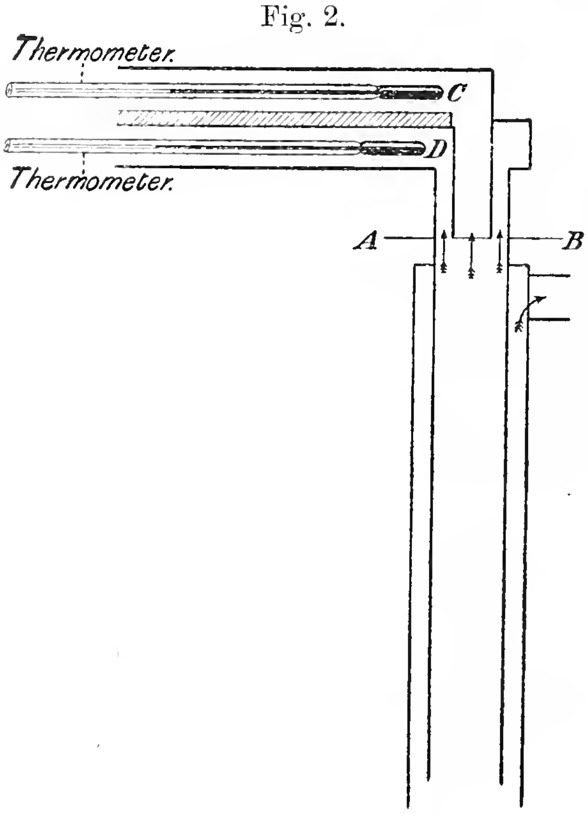
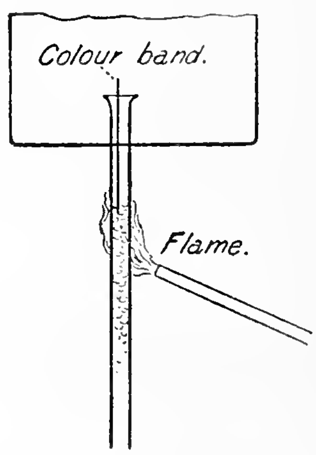


Fig. 3.



This may be illustrated by the following experiment. A glass jar, filled with water, and having a glass pipe fixed to it, as shown in fig. 3, and through which water may be drained, is allowed to stand until the eddies in the water have died out.

A tap is then opened at the bottom, and the water allowed to flow down the pipe. If now a streak of highly-coloured water be allowed to pass into the pipe from the tank, then, if the velocity is sufficiently low, this extends in a straight line down the tube. (This method of showing the stream-lines was used by Professor REYNOLDS in his experiments,* and has been fully described.) If now the flame of a Bunsen be applied to the outside of the glass pipe, it will be noticed that the streak of colour soon begins to flicker, and finally breaks up into eddies, and this for a very small rise of temperature of the water.

This experiment seems to explain the equality of temperatures observed in the case of the water taken from the heated copper tubes. It was found, however, that experiments made at velocities below the critical value were unreliable, owing to the rapid alternations between steady and unsteady motion which went on in the pipe. This was shown in the following way. When the velocity was above the critical value, then for the given temperature of the surface, the final temperature of the water was perfectly steady. If now the velocity was reduced until near its critical value, it was observed that the mercury in the thermometer stem suddenly began to oscillate rapidly through a range of one, or sometimes two degrees, and that the time of these oscillations was about two seconds.

The table shows at what velocity the unsteadiness comes in.

Diameter of pipe.	Temperature of surface.	Initial Temperature of water.	Final Temperature of water.	Velocity. In centims. per second.
1·39	39·6	18·00	22·6	69·0
1·39	39·6	18·00	22·75	58·1
1·39	39·6	18·00	22·91	43·6
1·39	39·6	18·00	23·2—23·15	28·6
			(Unsteadiness beginning.)	
1·39	39·6	18·00	23·4—23·8	18·00
			(Mercury oscillating rapidly.)	

Now assuming that the critical velocity for the pipe used is given by

$$V_c = \frac{P}{278D} \text{ metres per second,}$$

then for a temperature of 23°

$$V_c = 0{\cdot}137,$$

so that the unsteadiness observed from the thermometer readings becomes marked at about a velocity of

$$1{\cdot}3\ V_c \text{ to } 1{\cdot}4\ V_c.$$

* 'Phil. Trans.,' 1883, p. 942.

This result agrees remarkably with the results of Professor REYNOLDS' experiments on the change of the law of resistance in pipes, in which he found a range of unsteadiness in the *pressure* lying between

$$V_c \text{ and } 1\cdot 3\ V_c.$$

Description of the Apparatus.

For the purpose of the experiments, three drawn copper tubes were obtained, the thickness of the metal being ·08 centim., the length 48 centims., and the internal diameters being 1·39, 1·07, and ·736 centims. respectively.

As it was necessary that the velocity of the jacket water should be as high as possible, in order that the motion in the jacket might be "eddying" (the critical velocity for such a case not having been yet determined), the distances between the outer surfaces of the pipe and the inner surface of the jacket was made as small as possible, consistent with the possibility of getting the required amount of water to flow through under the available pressure.

The jacket pipes were of brass, the width of the jacket space being

·165, ·065, ·16 centim.

for the three cases.

To insure a uniform density of the water at any cross section, it was necessary to place the pipes in a vertical position, and, to make the motion of the water as unstable as possible, the water flowed downwards in each case.

The water used was obtained from a large tank in the tower of the College buildings, the head available being about 100 feet, which remained practically constant throughout the experiments. It was found that the supply from the Town's main was useless, owing to the varying pressure in the mains causing the flow to be unsteady.

Measurement of the Water.

To estimate the quantities of water passing through the pipe and jacket, two meters were required which would give correct values of the amount of water passing through them at any instant, and which should be sensitive, *i.e.*, would indicate *at once*, the change in the flow due to an adjustment of the water valves.

In the early experiments, the water ran into cylindrical vessels with open tops, and thin lipped orifices, through which the water was discharged; the discharge being estimated from the head of water in the vessel.

The objection to this form of meter is that the change in the "head," due to an alteration of the valve, takes place slowly, in some cases nearly a minute elapsing before the head of water in the vessel attains its new position. Consequently the adjustment of the valves to obtain exactly equal amounts of water through the pipe and jacket became very difficult and tedious.

To remedy this defect, two meters were made according to suggestions made by Mr. Foster of Owens College, and these proved very successful. The form of the meter is shown in fig. 5, and consists of a cylindrical tin box, 14 centims. diameter by 7 centims. deep. The water enters at the centre A, and flows radially over the flat plate BC, then radially inwards, guided by the radial vanes to the central orifice, which is re-entrant. When in use the meter is always full, and the motion is very steady.

The head is measured by the column of water in the small glass tube EF, and the flow read off on the scale GH, which is calibrated at intervals by experiment. It was found that on any re-adjustment of the water valves, the level of the water in EF almost instantly took up its proper position for the altered flow, so that it became an easy matter to set the two valves to give an equal flow of the desired amount.

The circular orifices were made of the thinnest sheet brass procurable, soldered to brass plugs, which could be screwed into the bottom plate of the meter. A set of three pairs of orifices was made, and the scale calibrated for each by experiment.

Method of Heating the Water.

As it was necessary to be able to adjust the initial temperature of the jacket water and pipe water to any required value, two copper coils were made, out of $\frac{3}{8}''$ tubing, the length used being about 8 feet for each coil. These were contained in cast-iron cylinders, which were connected to a steam boiler, and were provided with suitable cocks and drains.

During every experiment it was found necessary to maintain the pressure in the boiler constant, as a variation of two pounds on the square inch in the boiler pressure had a considerable effect on the final temperature of the water passing through the coils.

By using steam in the cylinders surrounding the coils at 60 lbs. per square inch pressure, it was possible to raise as much as 25 lbs. of water per minute through a range of 50° C.

The arrangement of the heating coils is shown in fig. 4.

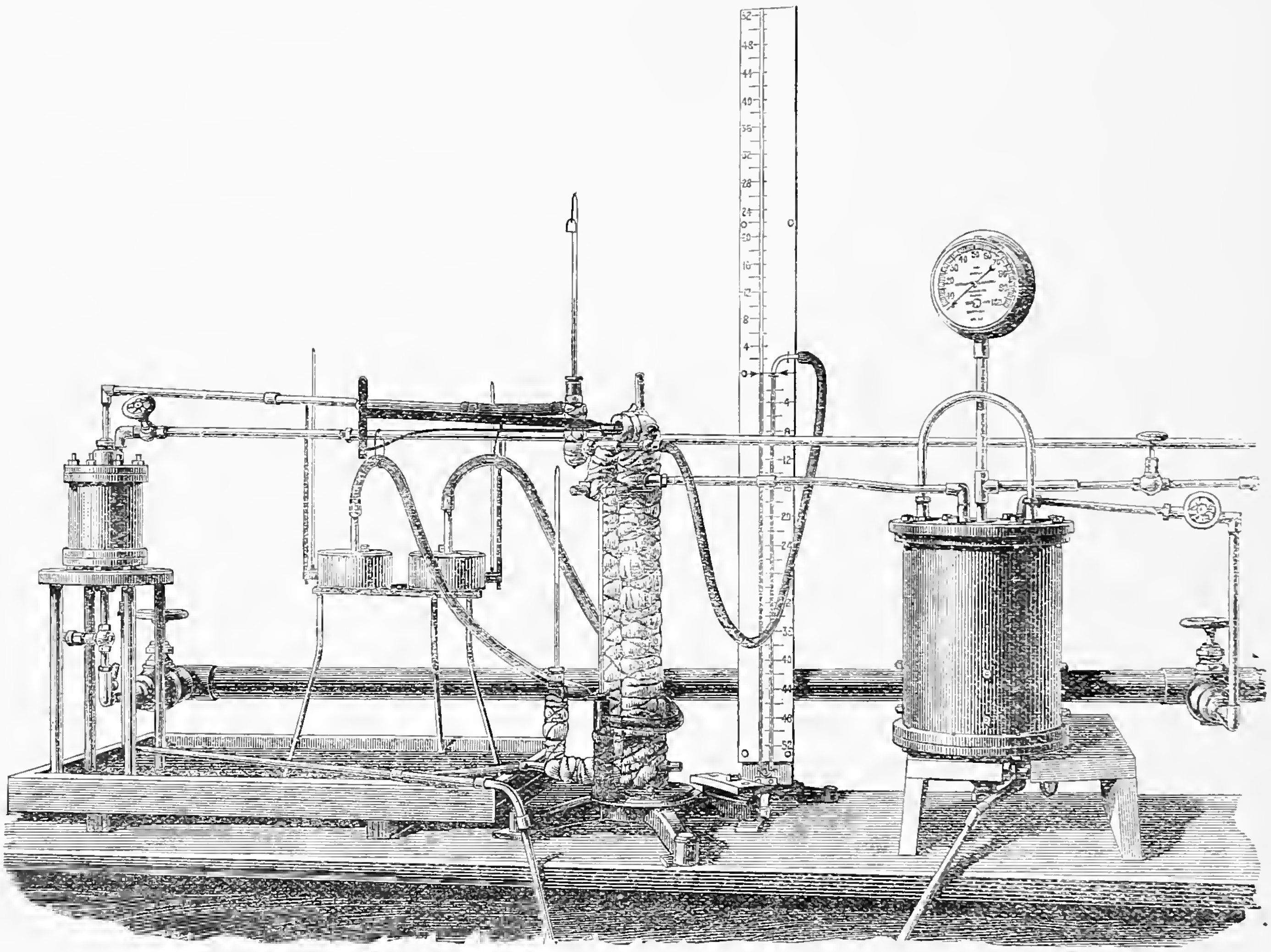
Measurement of the Surface Temperature of the Pipes.

In the preliminary experiments it was attempted to measure the temperature of the surface by means of a thermo-electric couple. As the wires had to be taken through the hot water in the jackets, considerable difficulty was experienced in insulating them, and although several attempts were made, consistent results could not be obtained, so that the method was abandoned.

A satisfactory means of measuring the surface temperature was found in the following way.

Since the thickness of the metal was only 0·08 centim., and the heat transmitted per square centim. per second was not in any case greater than 10 thermal units, and in the majority of the experiments was less than 5 thermal units, then, from the known conductivity of copper, the fall of temperature from one side of the wall to the

Fig. 4.

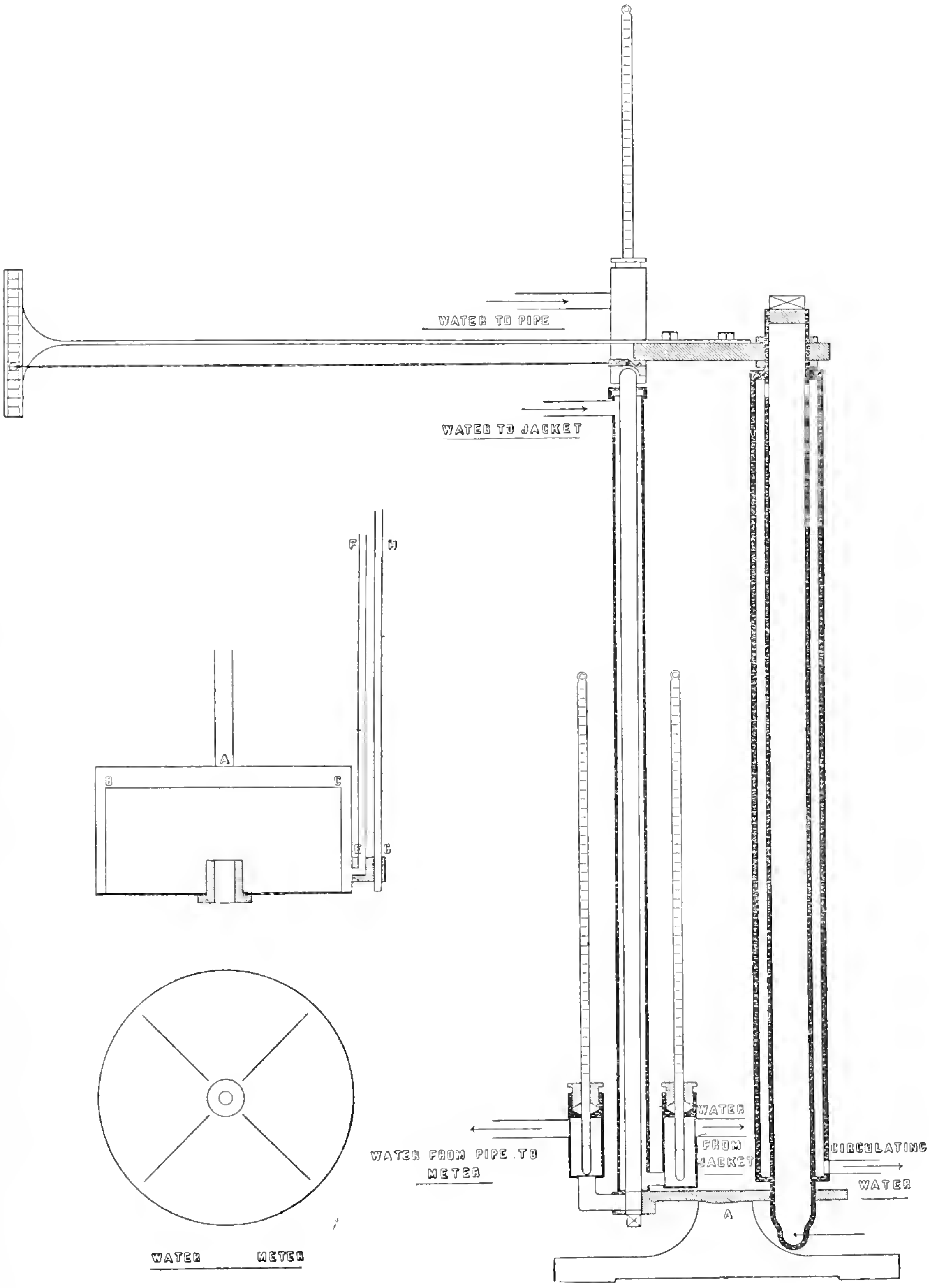


other would in most cases be less than 0·5° and would never exceed 1°. Thus, if the mean temperature of the pipe be determined, then by applying a small correction to this, depending on the amount of heat transmitted, the surface temperature can be found.

The mean temperature of the pipe was found by observing the actual elongation of the copper pipe by means of an extensometer, the arrangement being shown in fig. 5.

A is a cast-iron standard, the upper face of which is planed, and into which the copper tube is screwed, the connexion being as thin as possible, in order to prevent loss of heat from the pipe to the metal of the standard. At a distance of $3\frac{1}{2}$ inches from the copper pipe a wrought-iron pipe is screwed into the casting, which is also surrounded by a water-jacket as shown.

Fig. 5.



During a set of experiments, water at a constant temperature is circulated up through the inner pipe passing into the jacket through the holes shown at the top, and then down the jacket. In this way, the length of the wrought-iron pipe remains constant. To the upper end of this pipe a steel plate is attached, on the lower side of which the knife-edge of the extensometer bears, and which also carries the scale.

The extensometer consists of a long light lever working in a small bearing, at the top of the copper tube, the short end carrying a thin steel knife-edge, and the long end extending to the scale, by means of which the extension of the copper pipe could be measured.

The extension was magnified by this means, so that a movement of 1 millim. of the pointer represented a change in the temperature of the pipe of 0·5°, with the result that the temperature of the pipe could be estimated to $\frac{1}{10}$th of a degree. Before and after any experiment, the temperature corresponding to the given scale reading was determined by actual trial. All the pipes and the exposed parts of the apparatus were carefully lagged with cotton wool and sheet cork to prevent loss or reception of heat.

Measurement of the Temperature of the Water.

For this purpose carefully calibrated thermometers were used, of such a scale that readings within $\frac{1}{100}$th of a degree could be estimated. These were fixed in small brass chambers through which the water passed on entering and leaving the pipes, the chambers being carefully lagged and placed as near the pipe as possible. The chambers were fitted with small stuffing boxes and screw glands to prevent leakage, and are shown in section in fig. 5.

As the difference in pressure between the water entering the pipe and leaving it was small, no correction was found necessary for the observed readings.

In the experiments, at a pressure of about two atmospheres, it was found that the pressure had a small effect on the thermometer readings, due to compression of the bulb, but this was not more than 0·15° C.

To measure the pressure, a "Tee" joint was connected to the top of the copper pipe, as shown in fig. 4. This "tee" carried the extensometer lever in the centre, one branch being connected to the water supply, and the other branch being connected to a mercury pressure gauge in the form of a U-tube.

To regulate the pressure to any desired amount, brass cocks were attached to the waste pipes leading from the pipe and jacket, which could be adjusted to produce the pressure.

The joint between the copper tube and the jacket pipe at the upper end was made by an india-rubber washer and screwed cap, so as to allow free expansion of the pipe. At the lower ends, where the pipe was fixed to the standard, the jacket pipe and inner pipe were soldered together.

Method of Making an Experiment.

In the first place, the inside of the pipe was cleaned by a small brush, then the valves were set so that the required amount of water was passing through the pipe and the jackets. The water in each coil being raised to the same temperature, T_0 say, the scale reading was taken. The initial temperature of the water flowing through the pipe was then set to a given value, say t_1, the temperature of the jacket water being regulated so that the scale reading remained the same as before. When the final temperature of the water flowing through the pipe became steady, its value t_2 was taken. The temperatures of the pipe and jacket water were then again brought to the common value they had initially, and the scale reading again taken. If this agreed with the first reading, the experiment was taken as correct; if not, it was rejected and another made.

The observations would be as follows :—

EXPERIMENT I.—Water, 148 grms. per second.

(a.) *Measurement of Surface Temperature* :—

Temperature of jacket water = 47·8° } Scale reading, 6·57 centims.
" pipe " = 47·8° }

(b.) *Measurement of Heat transmitted* :—

Initial temperature of water in pipe = 17·98°.
" " " jacket = 67·60°.
Final " " pipe = 24·45°.
" " " jacket = 61·10°.

(c.) *Checking Surface Temperature* :—

Temperature of jacket water = 47·9° } Scale reading, 6·56 centims.
" pipe " = 47·7° }

Results of the Experiments.

Let

T_0 = temperature of the inner surface of the pipe in degrees Centigrade.
t = mean temperature of the water in the pipe at any cross section.
v = velocity of the water through the pipe in centims. per second.
p = pressure of the water.
r = radius of the pipe in centims.
L = length of the pipe in centims.

Then, for a small element of the internal surface of the pipe = $2\pi r\,dl$, we may write, for the case of transmission of heat from metal to water,

$$\text{Heat transmitted} = dH,$$
$$= K \, . \, 2\pi r dl \; \phi \, (T_0, \, t, \, (T_0 - t) \, . \, p \, . \, v \, . \, r),$$

where K and the function ϕ are to be determined by experiment.

From the description of the apparatus given above, it will be seen that the effect of each of the quantities T_0, t, $(T_0 - t)$ p, v and r, on the rate of transmission, can be studied separately.

Thus, in determining the effect of the velocity of the water, a series of experiments were made at varying velocities, but in which the values of T_0, t_1, $(T_0 - t_1)$, p and r remained constant.

I.—EFFECT of the Varying Pressures of the Water, T_0, $(T_0 - t_1)$, v, and r constant.

The following table gives the results obtained :—

Diameter of pipes.	Temperature of surface.	Initial temperature of water.	Final temperature of water.	Velocity of water.	Pressure in centims. of mercury.	Rise of temperature.
1·07	60°·4	25°·6	34°·6	116·0	94·0	9°·0
1·07	60·4	25·4	34·3	116·0	113·0	8°·9
1·07	60·4	25·3	34·4	116·0	152·0	9°·1
1·07	60·4	25·3	34·5	116·0	191·0	9°·2

These results show that for the given range of pressure, *i.e.*, from one to two atmospheres, the transmission is practically independent of the pressure.

II.—EFFECT of Variation of Velocity, T_0, $(T_0 - t_1)$, t_1, and r constant.

About 50 experiments of this kind were made. As an example of the results the following table may be given :—

Diameter of pipes.	T_0.	t_1.	t_2.	v.	Rise of temperature.
1·39	47·55	17·98	24·45	69·0	6°·47
1·39	47·55	17·99	24·08	98·0	6°·09
1·39	47·55	18·00	23·92	123·2	5°·92

These results show that the increase in temperature of the water for the given range of temperature, &c., is nearly independent of the velocity. Thus, for an increase in the velocity of 80 per cent., the rise of temperature is only 8·5 per cent. less, or in other words, the heat transmitted for the given range of temperature is nearly proportional to the velocity of the water. Those experiments were repeated, and then tried with a different value of the range $(T_0 - t_1)$, but with the same results, the ranges of temperature varying from 5° to 40°.

III.—EFFECT of Varying Ranges of Temperature t_1, v and r remaining constant.

As an example, the following set of results may be quoted :—

Diameter of pipes.	T_0	t_1.	t_2.	v.	Rise of temperature.
1·39	47·55	18·00	23·92	186·0	5°·92
1·39	39·35	18·01	22·22	186·0	4°·21
1·39	32·45	18·05	20·83	186·0	2°·78

Now if the heat transmitted under these conditions varied simply in the range of temperature, we should have had for the whole surface of the pipe

$$C\,(T_0 - t)\,ds = W dt,$$

or,

$$C_1 S = W \log \frac{T_0 - t_1}{T_0 - t_2},$$

where

S = whole surface of the pipe,

C_1 = a constant,

W = weight of water flowing through the pipe per second.

Now the values of $\log \frac{T_0 - t_1}{T_0 - t_2}$ for the experiments quoted are ·222, ·218, ·213 respectively, which show that the heat transmitted is proportional to $(T_0 - t)$ multiplied by a function of the temperature T_0.

IV.—EFFECT of Varying the Initial Temperature t_1, the range $(T_0 - t_1)$ v and r being constant.

A set of experiments at 69·0 centims. per second gave :—

Diameter of pipes.	T_0.	t_1.	t_2.	v.	Rise of temperature.
1·39	35·00	18·80	22·18	69·0	3°·38
1·39	53·60	37·40	41·25	69·0	3°·85

These results show a considerable increase in the heat transmitted for the same range and velocity; this increase, as will be seen by comparing Experiments III. and

IV., being greater than would be accounted for by the difference in the value of T_0. Other experiments carefully made gave similar results.

Thus, for a pipe of given diameter these experiments indicate that the transmission of heat from the surface of the pipe to water flowing through it, at velocities above the critical value for the pipe used, is given by an expression of the form

$$dH = K 2\pi r \, dl \, (T_0 - t) \, f(v) \, F(T_0) \, \Phi(t) \quad \ldots \ldots \quad (1).$$

It is also seen that the values of $F(T_0)$ and $\Phi(t)$ do not vary very much from unity, and may probably be put in the form

$$F(T_0) = 1 + \alpha T_0,$$
$$\Phi(t) = 1 + \beta t,$$

where α and β are constants to be determined by experiment.

Again, from Experiments II., it is seen that the heat transmitted is nearly proportional to the velocity, thus indicating the probable form of the velocity function, as

$$f(v) = V^n,$$

where n is a number a little less than unity, and to be determined by experiments at varying velocity.

It may be noticed that in equation (1) the value of K may depend on the diameter of the pipe and the nature of the surface.

The experiments made on the three drawn copper pipes, of diameter 1·39, 1·07, and ·736 centim., did not clearly indicate what the relation between H and r was, beyond showing that the effect of the variation in diameter of these pipes was not great, the supply of hot water from the heating coils not being sufficient to enable experiments to be made on pipes of larger diameter.

It is shown in the theory that the heat transmitted is proportional to the value of

$$r^{n-2}$$

where n has the value 1·84 approximately.

This would make the heat transmitted across unit area of the surface of the smallest pipe (·736 centim. diameter) about 10 per cent. greater than that transmitted through unit area of the surface of the largest pipe (1·39 centim. diameter) under the same conditions of flow and temperature.

General Theory.

The experiments described in this paper were originally made in order to determine, if possible, an expression for the rate of transmission of heat from metal surfaces to water, without reference to the theory, and which expression has been shown to be of the form given in equation (1).

The Author is indebted to Professor OSBORNE REYNOLDS, who kindly offered to look through the paper before publication, for the following theory of the subject.

The outline of this theory, as has been previously stated, was published in 1874.*

The discovery of the law of resistance in parallel channels, made by Professor REYNOLDS, in 1883,† enables this theory to be definitely stated.

According to this theory, the motion of heat from the surface of the pipe follows the same laws as the motion of momentum to the surface, whether by conduction or convection (though not by radiation and absorption, through the material, which unquestionably plays an important part in the so-called conduction of water).

Taking x as the direction of motion,

r = radius of pipe,
t = temperature of the water,
T_0 = temperature of surface of pipe,
D = weight of unit volume of water,
p = pressure of water per unit area,
W = weight of water discharged per second,
w = velocity of the water flowing through the pipe,
$P = (1 + \cdot 0336t + \cdot 000221t^2)^{-1}$,

A, B, and n constants depending on the nature of the surface.

Then, above the *critical* velocity, the loss of pressure is given by the equation

$$\frac{dp}{dx} = \frac{P^{2-n}}{(2r)^{3-n}} w^n \cdot \frac{B^n}{A}.\ddagger$$

Writing this in the form

$$\pi r^2 \frac{dp}{dx} = \pi r^2 \frac{g}{W} \cdot \frac{P^{2-n}}{(2r)^{3-n}} w^{n-1} \frac{B^n}{A} \cdot \left(\frac{W}{g} w\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

then, in (1), $\pi r^2 (dp/dx)$ is the loss of momentum due to diffusion and convection, so that, according to the above theory, substituting

$$W \frac{dt}{dx} \text{ for } \pi r^2 \frac{dp}{dx},$$

and

$$W (T_0 - t) \text{ for } \frac{W}{g} w,$$

* 'Proc. Manchester Lit. and Phil. Society,' 1874, p. 8.
† 'Phil. Trans.,' 1883, p. 976.
‡ 'Phil. Trans.,' 1883, p. 976.

the equation for the passage of heat will be

$$\frac{W\,dt}{dx} = \pi r^2 g \frac{P^{2-n}}{(2r)^{3-n}} w^{n-1} . \frac{B^n}{A} . (T_0 - t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

or writing

$$W = Dw\pi r^2,$$

the slope of temperature along the pipe is given by

$$\frac{dt}{dx} = \frac{B^n}{A} . \frac{g}{D} . \frac{P^{2-n}}{(2r)^{3-n}} w^{n-2} (T_0 - t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

This is supposing that the conductivity of the water, as compared with the viscosity, does not enter; but as it probably does, for ultimately it is conductivity by which the heat passes from the walls of the pipe to the water, there will probably be a coefficient

$$f(c/P),$$

the form of which can be determined by experiment.

Application of Professor REYNOLDS' *Theory to the Experiments.*

Assuming the variation in the value of t to be small, say, not greater than 6° in the whole length of the pipe, then, integrating equation (3),

$$\log \frac{T_0 - t_1}{T_0 - t_2} = \frac{B^n}{A} . \frac{g}{D} . \frac{\overline{P^{2-n}}}{(2r)^{3-n}} w^{n-2} . L \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

where

t_1 = initial temperature of the water in the pipe,
t_2 = final " " " "
L = length of the pipe,
$\overline{P^{2-n}}$ = mean value of P^{2-n}, for the water in the pipe.

Now, from equation (4) the value of n can be determined by a set of experiments, in which

$$P^{2-n}$$

has the same value in each.

This was done by plotting the logarithmic homologues of

$$\log \frac{T_0 - t_1}{T_0 - t_2} \quad \text{and} \quad w,$$

when it was found that the points plotted all lay approximately on a straight line, there being no systematic deviation.

For the three pipes used in the experiments the slopes of the logarithmic homologues were found to be :—

For pipe No. 1, 1·39 centim. diameter; slope $= -0·14$, or $n = 1·86$.
,, No. 2, 1·07 ,, ,, ,, $= -0·175$, or $n = 1·825$.
,, No. 3, ·736 ,, ,, ,, $= -0·170$, or $n = 1·83$.

In pipe No. 1, the velocities of the water had values between 28·7 and 123·2 centims. per second.

In pipe No. 3, the velocities of the water had values between 60 and 393·7 centims. per second.

It will be seen that the values of n given above correspond with the values we should expect to find for smooth copper pipes from the law of the resistances, the value for glass being about 1·73, and for smooth metal rather higher, rising to a value of 2 for rough metal surfaces.

Effect of Viscosity and Conductivity.

If the conductivity of the water at the bounding surface be neglected, then for experiments at constant velocity equation (4) gives the value of

$$\frac{\log \frac{T_0 - t_1}{T_0 - t_2}}{P^{2-n}}$$

constant for different values of t.

Referring to the results given in the tables, it is seen that the value of this expression *rises* with the value of the mean temperature (t_m) of the water, which seems to show that the conductivity of the water at the boundary has an effect.

It was also seen, in the experiments quoted above, p. 79, that the heat transmitted depended on the values of T_0 and t, and that this effect would be represented by coefficients of the form

$$(1 + \alpha T_0) \text{ and } (1 + \beta t).$$

From experiments at constant values of w and t_m, the value of α is found to be

$$\alpha = ·004,$$

and that of β is

$$\beta = ·01.$$

The slope of temperature of the water in the pipe is then given by

$$\frac{dt}{dx} = \frac{B^n}{A} \cdot \frac{g}{D} \cdot \frac{P^{2-n}}{(2r)^{3-n}} w^{n-2} (T_0 - t)(1 + \alpha T_0)(1 + \beta t) \quad . \quad . \quad . \quad (5).$$

Now for smooth metal pipes the value of B^a may be assumed nearly constant, so for a pipe of given length and diameter in which the surface temperature is constant,

$$kL = \frac{(2r)^{3-n} w^{2-n} \log \frac{T_0 - t_1}{T_0 - t_2}}{P^{2-n} (1 + \alpha T_0)(1 + \beta t_m)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where k is a constant depending on the nature of the surface of the pipe.

For the ranges of temperature obtained in these experiments no sensible error is introduced by taking the *mean* value of

$$P^{2-n} \text{ and } t$$

for the experiment and substituting them in equation (6).

When the variation in t is considerable, equation (5) must be integrated more exactly.

Applying equation (6) to the results of the experiments on the three copper pipes used, the values of k are found to be :—

Pipe.	Diameter.	Length.	Value of k.			Number of experiments.
			Maximum.	Minimum.	Mean.	
	centims.	centims.				
I.	1·39	47·0	·0108	·0104	·0106	22
II.	1·07	44·5	·0104	·0100	·0102	13
III.	·736	46·0	·0103	·0099	·0100	15

If W = weight of water flowing through the pipe in grammes per second, equation (5) may be written

$$W \frac{dt}{dx} = k\pi r^2 \frac{P^{2-n}}{(2r)^{3-n}} w^{n-1} (T_0 - t)(1 + \alpha T_0)(1 + \beta t) \quad . \quad . \quad . \quad . \quad (7),$$

which gives for the transmission of heat *from metal to water* per square centim. of the surface of the pipe

$$H = \frac{k}{4} \frac{P^{2-n}}{(2r)^{2-n}} (T_0 - t)(1 + \alpha T_0)(1 + \beta t) w^{n-1} \quad . \quad . \quad . \quad . \quad . \quad (8),$$

gramme-degrees per second.

Case of Transmission of Heat from Water to Metal.

The theory for this case is the same as in the transmission of heat from the surface to the water, so far as the *convection* of the heat is concerned. It seemed probable that the conductivity coefficients would also be the same, but on experiment this was found not to be so, the viscosity in this case having a much greater effect, the results of the experiments being that the heat transmitted was *nearly* inversely proportional to the mean viscosity of the film of liquid at the surface, so that the conductivity coefficient can be put in the form

$$k/\mathrm{P}_m,$$

where P_m is the mean value of P for the experiment, and

$$\mathrm{P} = \left\{1 + \cdot0336\left(\frac{\mathrm{T}_0 + t}{2}\right) + \cdot000221\left(\frac{\mathrm{T}_0 + t}{2}\right)^2\right\}^{-1}.$$

For this case equation (5) now takes the form

$$\frac{dt}{dx} = -\frac{\mathrm{B}^n}{\mathrm{A}} \cdot \frac{g}{\mathrm{D}} \cdot \frac{\mathrm{P}^{2-n}}{(2r)^{3-n}} \cdot \frac{w^{n-2}(t - \mathrm{T}_0)}{\mathrm{P}_m} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

The results of experiments made under these conditions are given in Table V., and the values of k calculated. These values do not show quite such a high degree of consistency as the values of k deduced from the other experiments, but this is probably due to the difficulty of working under the given conditions. Other experiments in which the heat flowed from water to metal were made, but always with the result that the heat transmitted was sensibly inversely proportional to the mean viscosity of the film at the surface, the deviation being never greater than 7 per cent.

The Transmission of Heat from the Jacket Water to the External Surface of the Pipe.

During all the experiments the temperatures of the jacket water were taken, so that the rate of heat transmission from the jacket water to the surface of the pipe could be calculated.

It was found in this case also that the transmission of heat was sensibly inversely proportional to the mean viscosity of the film of water at the surface of the pipe, the value of the conductivity coefficient being always slightly less than the mean value of P^{-1} for the film of water.

This is shown in Table VI., in which the product in the last term is calculated on the assumption that the effect of velocity and range of temperature is the same as in the previous cases.

It was found from these experiments on the jacket water that the value of n is practically the same as in the case of the pipe, the value for the jacket of No. 1 pipe being 1·855.

The results stated in Tables V. and VI. would, therefore, seem to show that the conductivity coefficients in the passage of heat between a metal surface and water in contact with it, will depend, in their value, on the *direction* of the flow of heat, the viscosity of the film of water at the surface having much less effect when the heat flows from metal to water than when the flow is in the opposite direction.

The experiments were made in the Whitworth Engineering Laboratory, Owens College, in 1895 and 1896, the author being at that time Demonstrator in Engineering in Owens College.

TABLE I.—Copper Pipe, No. I. 47·0 centims. long, 1·39 centim. diameter. Value of $n = 1·86$.

T_0.	t_1.	t_2.	w.	w^{2-n}.	$\log \frac{T_0 - t_1}{T_0 - t_2}$.	$\overline{P^{n-2}}$.	$1 + \alpha T_0$.	$1 + \beta t_m$.	k.
47·20	18·00	23·92	123·2	1·96	·2263	1·086	1·189	1·209	·0105
39·10	18·01	22·22	123·2	1·96	·2224	1·082	1·156	1·201	·0106
32·28	18·05	20·83	123·2	1·96	·2171	1·079	1·129	1·194	·0105
50·55	33·25	37·00	123·2	1·96	·2440	1·134	1·202	1·351	·0104
40·25	25·10	28·20	123·2	1·96	·2286	1·106	1·161	1·266	·0105
34·50	18·15	21·39	123·2	1·96	·2206	1·080	1·137	1.198	·0107
41·55	18·18	22·85	123·2	1·96	·2227	1·083	1·166	1·205	·0105
47·26	17·99	24·14	98·0	1·90	·2356	1·086	1·189	1·211	·0105
39·15	18·00	22·38	98·0	1·90	·2318	1·082	1·157	1·202	·0107
32·31	18·03	20·90	98·0	1·90	·2241	1·079	1·129	1·195	·0106
45·00	18·79	24·18	98·0	1·90	·2300	1·088	1·180	1·215	·0103
50·25	33·25	37·03	98·0	1·90	·2512	1·134	1·201	1·351	·0104
40·25	25·20	28·40	98·0	1·90	·2387	1·107	1·161	1·268	·0106

TABLE II.—Copper Pipe, No. I. 47·0 centims. long, 1·39 centim. diameter. Value of $n = 1·86$

T_0.	t_1.	t_2.	w.	w^{2-n}.	$\log \frac{T_0 - t_1}{T_0 - t_2}$.	$\overline{P^{n-2}}$.	$1 + \alpha T_0$.	$1 + \beta t_m$.	k.
47·33	17·98	24·45	69·0	1·80	·2487	1·087	1·189	1·212	·0105
39·20	18·00	22·60	69·0	1·80	·2443	1·083	1·157	1·203	·0107
32·35	18·03	21·03	69·0	1·80	·2348	1·080	1·129	1·195	·0105
45·00	18·77	24·28	83·6	1·86	·2358	1·088	1·180	1·215	·0104
39·65	18·82	23·15	83·6	1·86	·2325	1·086	1·158	1·210	·0104
54·25	42·20	45·10	83·6	1·86	·2750	1·160	1·217	1·436	·0106
39·52	18·32	23·02	58·1	1·767	·2504	1·085	1·158	1·206	·0107
39·55	18·35	23·26	43·6	1·696	·2632	1·085	1·158	1·208	·0108
40·30	18·43	23·65	28·7	1·600	·2725	1·086	1·160	1·210	·0105

TABLE III.—Copper Pipe, No. II. 44·5 centims. long, 1·07 centims. diameter.
Value of $n = 1·825$.

T_0.	t_1.	t_2.	w.	w^{2-n}.	$\log \frac{T_0-t_1}{T_0-t_2}$.	$\overline{P^{n-2}}$.	$1+\alpha T_0$.	$1+\beta t_m$.	k.
63·35	23·28	32·20	116·0	2·30	·2515	1·139	1·253	1·277	·0100
54·60	23·30	30·12	116·0	2·30	·2455	1·134	1·220	1·267	·0101
47·35	23·30	28·37	116·0	2·30	·2365	1·130	1·189	1·258	·0100
50·85	36·60	39·88	116·0	2·30	·2613	1·184	1·203	1·382	·0104
35·70	21·38	24·35	103·8	2·25	·2321	1·117	1·143	1·248	·0101
56·50	21·39	29·30	97·6	2·23	·2549	1·120	1·226	1·253	·0101
63·25	48·80	52·50	91·6	2·21	·2955	1·230	1·253	1·506	·0104
35·70	21·38	24·40	91·6	2·21	·2366	1·117	1·143	1·229	·0101
52·30	37·60	41·10	88·4	2·19	·2718	1·187	1·209	1·393	·0102
56·50	21·39	29·40	85·3	2·178	·2586	1·128	1·226	1·254	·0100
35·70	21·40	24·50	79·3	2·15	·2438	1·118	1·143	1·229	·0102
63·25	48·80	52·60	79·3	2·15	·3050	1·230	1·253	1·507	·0104
56·50	21·39	29·60	73·1	2·12	·2660	1·130	1·226	1·255	·0101

TABLE IV.—Copper Pipe, No. III. 46 centims. long, 0·736 centim. diameter.
Values of $n = 1·83$.

T_0.	t_1.	t_2.	w.	w^{2-n}.	$\log \frac{T_0-t_1}{T_0-t_2}$.	$\overline{P^{n-2}}$.	$1+\alpha T_0$.	$1+\beta t_m$.	k.
39·30	14·06	20·72	393·7	2·76	·3060	1·090	1·157	1·174	·0102
31·80	14·06	18·58	393·7	2·76	·2935	1·084	1·125	1·163	·0100
26·25	14·06	17·13	393·7	2 76	·2915	1·081	1·105	1·156	·0102
20·45	14·07	15·64	393·7	2·76	·2820	1·077	1·082	1·149	·0101
17·10	14·07	14·82	393·7	2·76	·2840	1·075	1·068	1·144	·0103
39·30	13·90	20·78	296·0	2·63	·3160	1·089	1·157	1·173	·0100
51·65	37·50	41·80	296·0	2·63	·3620	1·184	1·207	1·397	·0100
42·55	28·33	32·40	296·0	2·63	·3370	1·146	1·170	1·303	·0099
28·20	14·06	17·80	296·0	2·63	·3070	1·082	1·113	1·159	·0100
39·42	13·90	21·00	244·6	2·55	·3255	1·090	1·158	1·174	·0100
26·25	14·04	17·30	244·6	2·55	·3100	1·080	1·105	1·156	·0100
20·55	14·05	15·75	244·6	2·55	·3035	1·077	1·082	1·148	·0100
38·00	14·20	21·80	90·0	2·15	·3840	1·092	1·152	1·180	·0099
56·50	14·20	28·55	90·0	2·15	·4140	1·107	1·227	1·214	·0099
38·00	14·20	22·20	60·0	2·005	·4095	1·093	1·152	1·182	·0099

TABLE V.—Transmission of Heat from Water to Metal. Copper Pipe No. II. 44·5 centims. long, 1·07 centims. diameter. Value of $n = 1{\cdot}825$.

T_0.	t_1.	t_2.	w.	w^{2-n}.	$\overline{P_m}^{-1}$.	$\log \frac{t_1 - T_0}{t_2 - T}$.	$\overline{P}^{n-2}$.	k.
30·85	52·25	46·60	88·4	2·19	2·7	·3065	1·226	·00743
36·00	52·20	47·80	88·4	2·19	2·85	·3165	1·228	·00726
42·15	52·20	49·20	88·4	2·19	3·03	·3540	1·231	·00778
44·90	52·30	50·05	88·4	2·19	3·12	·3620	1·232	·00763
31·70	53·60	47·60	88·4	2·19	2·75	·3200	1·231	·00765

TABLE VI.—Experiments on Jacket Water of No. I. Pipe. Area of Jacket 0·9 sq. centim. Value of $n = 1{\cdot}855$.

T_0.	t_1.	t_2.	w.	w^{2-n}.	$\log \frac{t_1 - T_0}{t_2 - T_0}$.	$\overline{P}^{n-2}$.	$\overline{P}^{-1}$.	Values of $\frac{\overline{P} \log \frac{t_1 - T_0}{t_2 - T_0} w^{2-n}}{P^{2-n}}$.
47·90	66·67	60·75	207·0	2·17	·3780	1·225	3·56	·282
39·50	54·21	50·00	207·0	2·17	·3370	1·192	3·00	·291
32·62	43·18	40·40	207·0	2·17	·3050	1·160	2·55	·301
47·85	66·83	60·75	164·0	2·09	·3857	1·225	3·56	·277
39·55	54·13	49·75	164·0	2·09	·3560	1·191	3·00	·295
32·58	42·87	40·00	164·0	2·09	·3270	1·160	2·55	·311
47·77	67·32	60·85	116·0	1·99	·4014	1·226	3·56	·275
39·60	53·95	49·35	116·0	1·99	·3860	1·192	2·99	·306
32·55	43·00	40·00	116·0	1·99	·3380	1·16	2·55	·305
48·00	67·87	60·75	73·0	1·86	·4425	1·227	3·57	·282
39·75	54·91	50·00	73·0	1·86	·3910	1·192	3·01	·288

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LEATHEM, J. G.—The Theory of the Magneto-Optic Phenomena of Iron, Nickel, and Cobalt.
Phil. Trans., A. 1897, vol. 190, pp. 89–127.

Kerr Phenomena, Mathematical Discussion of.
Leathem, J. G. Phil. Trans., A, 1897, vol. 190, pp. 89–127.

Magnetic Reflection, Experiments of Sissingh, Zeeman, and Wind, compared with Theory.
Leathem, J. G. Phil. Trans., A, 1897, vol. 190, pp. 89–127.

Hall Effect, for Rapidly Alternating Currents.
Leathem, J. G. Phil. Trans., A, 1897, vol. 190, pp. 89–127.

Metallic Films, Transmission of Light through.
Leathem, J. G. Phil. Trans., A, 1897, vol. 190, pp. 89–127.

V. *On the Theory of the Magneto-Optic Phenomena of Iron, Nickel, and Cobalt.*

By J. G. LEATHEM, *B.A., Fellow of St. John's College, and Isaac Newton Student in the University of Cambridge.*

Communicated by Sir ROBERT S. BALL, *F.R.S.*

Received May 11,—Read June 17, 1897.

Introduction.

1. IN his 'British Association Report' (1893), on the "Action of Magnetism on Light," Mr. LARMOR points out that there are two possible ways in which the magnetic field may be regarded as affecting the phenomena of light propagation. "The imposed magnetisation is an independent kinetic system of a vortical character, which is linked on to the vibrational system which transmits the light waves," and from the first point of view "the kinetic reaction between the two systems will add on new terms to the electric force," and so there would be a "magneto-optic term" in the expression for the kinetic energy. This type of theory includes MAXWELL's hypothesis of molecular vortices, and has been analytically treated by FITZGERALD and BASSET; it also includes the theory developed by DRUDE in his paper "Magneto-optische Erscheinungen," in 'Wiedemann's Annalen,' vol. 46. The great difficulty arises when one comes to consider the boundary conditions, as a discontinuity of electric force cannot be avoided; apparently the only satisfactory way of meeting this difficulty is to be found in LARMOR's suggested modification of FITZGERALD's analysis, involving the supposition that in the case of reflection of light at the surface of a magnetised metal the constraint introduces an irrotational or compressional wave of the ether set up at the reflecting surface and travelling with very great or infinite velocity through the space occupied by the metal; a satisfactory system of equations of propagation and boundary conditions is thus obtained by applying the principle of Least Action. I have worked out the mathematics of this theory and obtained the general solution of the problem of reflection from a magnet; on comparing this with the experimental results of several German and Dutch physicists, it appears that the agreement of the theory with experiment is at best very doubtful, even when allowance is made for the possibility of large errors of observation. There is moreover one phenomenon, recently discovered, which the theory quite fails to account for, viz., an effect of the component of the magnetic field perpendicular to the plane of incidence.

2. The second type of theory supposes that the imposed magnetisation "slightly

alters the structure of the medium which conveys the light vibrations, but does not exert a direct dynamical effect on these vibrations"; the isotropy of the medium is, as it were, destroyed, and rotational terms appear in the fundamental elastic relations between displacements and the corresponding forces. This theory, in its valid form as regards boundary conditions, has quite lately been formulated for transparent media by BASSET ('American Journal of Mathematics,' vol. 19, 1897, No. 1), and the same principles underlie the very different analysis of GOLDHAMMER in his memoir of 1892 ('Wied. Ann.,' vol. 46). LARMOR also independently formulated this theory in his 'British Association Report,' 1893, and more explicitly in 'Proc. Lond. Math. Soc.,' April, 1893. In his exposition it was shown that the rotational terms in the equations connecting electric displacement and electric force are not open to the objection that they would imply perpetual motions, as they involve only the rate of change of the force. The boundary conditions in this theory are of the standard form, namely continuity of the tangential components of electric and magnetic force, and of the normal components of magnetic induction and total current. It has been shown by BASSET how the whole scheme may be formulated from a single energy function by the principle of Least Action.

3. In the present paper it is proposed to take the fundamental equations of this type of theory in a general form on the lines of Mr. LARMOR's recent papers on Electrodynamics, and to develop them so as to obtain the solutions of the problems of the reflection of light at the surface of a magnet, and of the transmission of light through normally magnetised metallic films. The formulæ so obtained will be compared with the available experimental results, with a view to ascertaining to what extent the theory is in agreement with the facts. The theory involves a single magneto-optic constant which in metals may be assumed complex; we shall try whether it is possible, by giving suitable numerical values to the modulus and vector angle of this constant, to make the theory account for all the observed phenomena; and if so, we shall ascertain what these numerical values are. If successful we shall thus have a formulation of the phenomena in a mathematical scheme, which ought to serve as a guide in the elaboration of physical theory.

In carrying out this programme, I am aware that I shall be going over ground which has already been covered to some extent by GOLDHAMMER, and also by DRUDE, but my method will be entirely different from theirs, and I shall be able to use important experimental results which had not been published at the time their papers were written.

Notation.

4. The notation is nearly the same as MAXWELL's: (P, Q, R) is electromotive force, (u, v, w) the total current, (a, b, c) magnetic induction, σ specific conductivity, K specific inductive capacity taken as a pure ratio, c the velocity of radiation; (f'', g'', h'') corresponds to MAXWELL's total electric displacement; its components

(f, g, h) and (f', g', h') are the vectors $\mathfrak{D}$, $\mathfrak{D}'$ of LARMOR's theory ('Phil. Trans.,' 1895), namely (f, g, h) is the displacement involved in the æther strain, and $(f', g' h')$ that involved in the polarisation of the matter.

Fundamental Equations.

5. It being as usual assumed that for oscillations so rapid as those of light the effective magnetic permeability is unity, the fundamental equations of the theory are as follows :—

(i.) The two circuital relations

$$\frac{dc}{dy} - \frac{db}{dz} = 4\pi u, \qquad \frac{da}{dz} - \frac{dc}{dx} = 4\pi v, \qquad \frac{db}{dx} - \frac{da}{dy} = 4\pi w \quad . \quad . \quad (1),$$

$$\frac{dR}{dy} - \frac{dQ}{dz} = -\frac{da}{dt}, \qquad \frac{dP}{dz} - \frac{dR}{dx} = -\frac{db}{dt}, \qquad \frac{dQ}{dx} - \frac{dP}{dy} = -\frac{dc}{dt} \quad . \quad . \quad (2).$$

(ii.) The equations of the current

$$\left.\begin{aligned} u &= \sigma P + g_3 Q - g_2 R + \frac{df''}{dt} \\ v &= \sigma Q + g_1 R - g_3 P + \frac{dg''}{dt} \\ w &= \sigma R + g_2 P - g_1 Q + \frac{dh''}{dt} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

where the vector (g_1, g_2, g_3) represents the Hall effect.

(iii.) The displacement relations, and the elastic relations between electromotive force and the corresponding polarisation, viz.,

$$f'' = f + f', \quad g'' = g + g', \quad h'' = h + h'. \quad . \quad . \quad . \quad . \quad (4),$$

$$f = \frac{1}{4\pi C^2} P, \quad g = \frac{1}{4\pi C^2} Q, \quad h = \frac{1}{4\pi C^2} R. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5),$$

and

$$\left.\begin{aligned} f' &= \frac{K-1}{4\pi C^2} P + b_3 \frac{dQ}{dt} - b_2 \frac{dR}{dt} \\ g' &= \frac{K-1}{4\pi C^2} Q + b_1 \frac{dR}{dt} - b_3 \frac{dP}{dt} \\ h' &= \frac{K-1}{4\pi C^2} R + b_2 \frac{dP}{dt} - b_1 \frac{dQ}{dt} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

the vector (b_1, b_2, b_3) representing, in transparent matter, the whole magneto-optic effect.

The restriction of the relation to this form is justified as follows :—

When there is no matter present the polarisation (f', g', h') is null, and there is no rotatory effect. When there is matter present the action of an electric field (P, Q, R) on the polarisation induced by it gives rise to a pondero-motive force which does mechanical work in a small displacement of the matter, equal, for an element of volume $\delta\tau$, to

$$(\mathrm{P}\,\delta f' + \mathrm{Q}\,\delta g' + \mathrm{R}\,\delta h')\,\delta\tau.$$

When there is no conduction and therefore no dissipation, this quantity must be an exact differential, otherwise mechanical work could be gained in a complete cycle of displacement of the material medium, which would imply the possibility of perpetual motions. This restriction must also hold universally, because the nature of the molecular polarisation is independent of whether conduction is present or not. It requires that $\int(\mathrm{P}\,\delta f' + \mathrm{Q}\,\delta g' + \mathrm{R}\,\delta h')\,\delta\tau$ and therefore $\int(f'\,\delta\mathrm{P} + g'\,\delta\mathrm{Q} + h'\,\delta\mathrm{R})\,d\tau$ shall be the variation of $\int \mathrm{F}\,d\tau$, where F is some function of (P, Q, R) and its differential coefficients, terms at the time limits being left out of account. The expression for (f', g', h') in terms of (P, Q, R) can therefore involve no rotatory terms in (P, Q, R) itself, as Lord KELVIN first shewed, but it may have rotatory terms in d/dt (P, Q, R), which have the characteristics of the magneto-optic property. Rotatory terms of a certain type in the spacial fluxions of (P, Q, R) are also admissible; these lead to optical rotation of the structural kind; they are foreign to the present problem because they are isotropic, instead of being related to an imposed vector, the intensity of magnetisation. In either case higher differentiations of odd order might also come into the expressions: these would affect the relations of the phenomena to optical dispersion, but not the questions here treated.

Equations of Propagation.

6 From the fundamental equations we readily obtain

$$\left.\begin{aligned} u &= \left(\sigma + \frac{\mathrm{K}}{4\pi\mathrm{C}^2}\frac{d}{dt}\right)\mathrm{P} + \left(b_3\frac{d^2}{dt^2} + g_3\right)\mathrm{Q} - \left(b_2\frac{d^2}{dt^2} + g_2\right)\mathrm{R} \\ v &= \left(\sigma + \frac{\mathrm{K}}{4\pi\mathrm{C}^2}\frac{d}{dt}\right)\mathrm{Q} + \left(b_1\frac{d^2}{dt^2} + g_1\right)\mathrm{R} - \left(b_3\frac{d^2}{dt^2} + g_3\right)\mathrm{P} \\ w &= \left(\sigma + \frac{\mathrm{K}}{4\pi\mathrm{C}^2}\frac{d}{dt}\right)\mathrm{R} + \left(b_2\frac{d^2}{dt^2} + g_2\right)\mathrm{P} - \left(b_1\frac{d^2}{dt^2} + g_1\right)\mathrm{Q} \end{aligned}\right\} \quad . \quad . \quad . \quad (7).$$

For brevity we may put

$$\left\{\left(b_1\frac{d^2}{dt^2} + g_1\right), \left(b_2\frac{d^2}{dt^2} + g_2\right), \left(b_3\frac{d^2}{dt^2} + g_3\right)\right\} \equiv \left\{\eta_1, \eta_2, \eta_3\right\} \quad . \quad . \quad . \quad (8)$$

and

$$\sigma + \frac{K}{4\pi C^2}\frac{d}{dt} \equiv \frac{1}{H} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9),$$

so that equations (7) become

$$\left.\begin{aligned} u &= \frac{1}{H}P + \eta_3 Q - \eta_2 R \\ v &= \frac{1}{H}Q + \eta_1 R - \eta_3 P \\ w &= \frac{1}{H}R + \eta_2 P - \eta_1 Q \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10).$$

Now (g_1, g_2, g_3) and (b_1, b_2, b_3) are supposed to be exceedingly small quantities, so that (η_1, η_2, η_3) are also extremely small. If we neglect squares and products of (η_1, η_2, η_3) and solve equations (10) for P, Q, R, we get

$$\left.\begin{aligned} P &= H\,(u - H\eta_3 v + H\eta_2 w) \\ Q &= H\,(v - H\eta_1 w + H\eta_3 u) \\ R &= H\,(w - H\eta_2 u + H\eta_1 v) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

To get the equations of propagation differentiate with respect to the time the first of equations (1)

$$\begin{aligned} 4\pi\frac{du}{dt} &= \frac{d}{dz}\left(-\frac{db}{dt}\right) - \frac{d}{dy}\left(-\frac{dc}{dt}\right) \\ &= \frac{d}{dz}\left(\frac{dP}{dz} - \frac{dR}{dx}\right) - \frac{d}{dy}\left(\frac{dQ}{dx} - \frac{dP}{dy}\right) \text{ by (2)} \\ &= \nabla^2 P - \frac{d}{dx}\left(\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz}\right) \\ &= H\nabla^2(u - H\eta_3 v + H\eta_2 w) \\ &\quad - H^2\frac{d}{dx}\left\{\eta_1\left(\frac{dv}{dz} - \frac{dw}{dy}\right) + \eta_2\left(\frac{dw}{dx} - \frac{du}{dz}\right) + \eta_3\left(\frac{du}{dy} - \frac{dv}{dx}\right)\right\}, \end{aligned}$$

and hence, if for brevity we put

$$\Omega' \equiv \eta_1\left(\frac{dw}{dy} - \frac{dv}{dz}\right) + \eta_2\left(\frac{du}{dz} - \frac{dw}{dx}\right) + \eta_3\left(\frac{dv}{dx} - \frac{du}{dy}\right) \quad . \quad . \quad . \quad . \quad (12),$$

our equations of propagation become

$$\left.\begin{aligned} 4\pi\frac{du}{dt} &= H\nabla^2(u - H\eta_3 v + H\eta_2 w) + H^2\frac{d\Omega'}{dx} \\ 4\pi\frac{dv}{dt} &= H\nabla^2(v - H\eta_1 w + H\eta_3 u) + H^2\frac{d\Omega'}{dy} \\ 4\pi\frac{dw}{dt} &= H\nabla^2(w - H\eta_2 u + H\eta_1 v) + H^2\frac{d\Omega'}{dz} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (13).$$

Plane Waves in a Metallic Medium.

7. In the case of plane waves in a metallic medium, let us assume

$$(u, v, w) = (\mathrm{A}, \mathrm{B}, \mathrm{C})\, e^{\iota(lx+mz+pt)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where ι represents $\sqrt{-1}$, and write

$$l^2 + m^2 \equiv \omega^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15).$$

Substituting these values in the equations of propagation, we get

$$\left.\begin{aligned}(\mathrm{H}\omega^2 + 4\pi\iota p)\,\mathrm{A} &= \mathrm{H}^2\omega^2(\eta_3\mathrm{B} - \eta_2\mathrm{C}) \\ &\quad - \mathrm{H}^2 l\left\{\eta_1(-m\mathrm{B}) + \eta_2(m\mathrm{A} - l\mathrm{C}) + \eta_3(l\mathrm{B})\right\} \\ (\mathrm{H}\omega^2 + 4\pi\iota p)\,\mathrm{B} &= \mathrm{H}^2\omega^2(\eta_1\mathrm{C} - \eta_3\mathrm{A}) \\ (\mathrm{H}\omega^2 + 4\pi\iota p)\,\mathrm{C} &= \mathrm{H}^2\omega^2(\eta_2\mathrm{A} - \eta_1\mathrm{B}) \\ &\quad - \mathrm{H}^2 m\left\{\eta_1(-m\mathrm{B}) + \eta_2(m\mathrm{A} - l\mathrm{C}) + \eta_3(l\mathrm{B})\right\}\end{aligned}\right\} \quad . \quad (16).$$

Addition of l times the first of these to m times the last gives, as was to be expected,

$$l\mathrm{A} + m\mathrm{C} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17),$$

and hence if we eliminate A, B, and C, we get

$$\begin{vmatrix} l & o & m \\ \mathrm{H}^2\omega^2\eta_3 & \mathrm{H}\omega^2 + 4\pi\iota p & -\mathrm{H}^2\omega^2\eta_1 \\ \mathrm{H}\omega^2 + 4\pi\iota p + \mathrm{H}^2 lm\eta_2 & -\mathrm{H}^2 lm\eta_1 - \mathrm{H}^2 m^2\eta_3 & \mathrm{H}^2 m^2\eta_2 \end{vmatrix} = 0,$$

which reduces to

$$(\mathrm{H}\omega^2 + 4\pi\iota p)^2 + \mathrm{H}^4\omega^2(l\eta_1 + m\eta_3)^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

This equation gives the possible values of m corresponding to given values of l and p. It is a quartic and therefore has four roots, of which two have their imaginary parts negative and their real parts positive; let us denote these roots by m_1 and m_2, and the corresponding values of ω by ω_1 and ω_2 respectively, so that

$$\left.\begin{aligned}\mathrm{H}\omega_1^2 + 4\pi\iota p &= +\,\iota\,.\,\mathrm{H}^2\omega_1(l\eta_1 + m_1\eta_3) \\ \mathrm{H}\omega_2^2 + 4\pi\iota p &= -\,\iota\,.\,\mathrm{H}^2\omega_2^2(l\eta_1 + m_2\eta_3)\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (19).$$

In the particular case of η_1, η_3, both zero, the equation to determine m would be

$$(\mathrm{H}\omega^2 + 4\pi\iota p)^2 = 0,$$

so that if M be the value of m given by this equation, and Ω the corresponding value of ω

$$\left.\begin{aligned} \mathrm{H}\Omega^2 + 4\pi\iota p &= 0, \\ \text{and therefore} \quad \Omega^2 &= -\frac{4\pi\iota p}{\mathrm{H}} \\ \text{and} \quad \mathrm{M}^2 &= -\frac{4\pi\iota p}{\mathrm{H}} - l^2 \end{aligned}\right\} \quad . \quad . \quad . \quad (20)$$

the values coinciding in pairs.

The sign of M is ambiguous; we determine it by requiring that M shall have its imaginary part negative, in which case we shall find that its real part is positive. As we neglect second and higher powers of η_1 and η_3, the equations (19) of the general case may now be written, introducing this quantity M,

$$\left.\begin{aligned} \omega_1^2 - \Omega^2 &= + \iota \,.\, \mathrm{H}\Omega\,(l\eta_1 + \mathrm{M}\eta_3) \\ \omega_2^2 - \Omega^2 &= - \iota \,.\, \mathrm{H}\Omega\,(l\eta_1 + \mathrm{M}\eta_3) \end{aligned}\right\} \quad . \quad . \quad . \quad (21),$$

so that

$$\left.\begin{aligned} \omega_1^2 &= \Omega^2 \left\{1 + \iota \,.\, \frac{\mathrm{H}}{\Omega}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \\ \omega_2^2 &= \Omega^2 \left\{1 - \iota \,.\, \frac{\mathrm{H}}{\Omega}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \end{aligned}\right\} \quad . \quad . \quad . \quad (22),$$

$$\left.\begin{aligned} \omega_1 &= \Omega \left\{1 + \iota \,.\, \frac{\mathrm{H}}{2\Omega}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \\ \omega_2 &= \Omega \left\{1 - \iota \,.\, \frac{\mathrm{H}}{2\Omega}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \end{aligned}\right\} \quad . \quad . \quad . \quad (23),$$

$$\left.\begin{aligned} m_1^2 &= \mathrm{M}^2 \left\{1 + \iota \,.\, \frac{\mathrm{H}\Omega}{\mathrm{M}^2}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \\ m_2^2 &= \mathrm{M}^2 \left\{1 - \iota \,.\, \frac{\mathrm{H}\Omega}{\mathrm{M}^2}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \end{aligned}\right\} \quad . \quad . \quad . \quad (24),$$

$$\left.\begin{aligned} m_1 &= \mathrm{M} \left\{1 + \iota \,.\, \frac{\mathrm{H}\Omega}{2\mathrm{M}^2}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \\ m_2 &= \mathrm{M} \left\{1 - \iota \,.\, \frac{\mathrm{H}\Omega}{2\mathrm{M}^2}\,(l\eta_1 + \mathrm{M}\eta_3)\right\} \end{aligned}\right\} \quad . \quad . \quad . \quad (25),$$

results which will be of use later on.

Corresponding to the two values of m there are two sets of constants (A, B, C); these we distinguish by the suffixes ($_1$) and ($_2$).

Equation (17) shews that

$$C_1 = -\frac{l}{m_1} A_1 \quad \text{and} \quad C_2 = -\frac{l}{m_2} A_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (26).$$

This taken in conjunction with the second of equations (16) gives

$$(H\omega^2 + 4\pi\iota p) B = -\frac{H^2\omega^2}{m}(l\eta_1 + m\eta_3) A$$

whether the suffix be (1) or (2).

Hence in virtue of equations (19)

$$\iota . H^2\omega_1 B_1 = -\frac{H^2\omega_1^2}{m_1} A_1, \qquad -\iota . H^2\omega_2 B_2 = -\frac{H^2\omega_2^2}{m_2} A_2$$

or

$$B_1 = +\iota . \frac{\omega_1}{m_1} A_1, \qquad B_2 = -\iota . \frac{\omega_2}{m_2} A_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (27).$$

8. In the case of air (or any medium in which there is no magneto-optic rotation) η_1, η_2, η_3 are zero. For air also $\sigma = 0$, $K = 1$, $H = 4\pi c^2/\iota p = -\iota . 4\pi c^2/p$. The substitution of the exponential forms of u, v, w in the equations of propagation gives

$$-\iota . 4\pi c^2/p . \omega^2 + 4\pi\iota p = 0,$$

or $c^2\omega^2 = p^2$, as was to be expected.

Problem of Reflection.

9. We are now in a position to attack the problem of the reflection of light at the surface of a magnetised metal. Let the interface between the two media be the plane $z = 0$; the air occupying the space z positive, and the metal the space z negative. The plane of incidence is taken as the plane $y = 0$.

We assume that, *in the air*,

$$\left.\begin{aligned} u &= A_0 e^{\iota(lx + mz + pt)} + A e^{\iota(lx - mz + pt)} \\ v &= B_0 e^{\iota(lx + mz + pt)} + B e^{\iota(lx - mz + pt)} \\ w &= -\frac{l}{m} A_0 e^{\iota(lx + mz + pt)} + \frac{l}{m} A e^{\iota(lx - mz + pt)} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (28),$$

where A_0, B_0 represent the incident wave, and A, B the reflected wave.

In the metal,

$$\left.\begin{aligned} u &= A_1 e^{\iota(lx + m_1 z + pt)} + A_2 e^{\iota(lx + m_2 z + pt)} \\ v &= +\iota . \frac{\omega_1}{m_1} A_1 e^{\iota(lx + m_1 z + pt)} - \iota \frac{\omega_2}{m_2} A_2 e^{\iota(lx + m_2 z + pt)} \\ w &= -\frac{l}{m_1} A_1 e^{\iota(lx + m_1 z + pt)} - \frac{l}{m_2} A_2 e^{\iota(lx + m_2 z + pt)} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (29).$$

In this assumption we take account of only two of the four possible waves in the metallic medium; the other two are omitted because they are waves which travel in a direction that makes an acute angle with the axis of z; and as in our present problem all waves in the metallic medium are originated at the plane $z = 0$, only those can actually occur whose direction of propagation makes an obtuse angle with the axis of z.

10. The surface conditions which have to be satisfied are the continuity of

$$\begin{array}{ccc} P, & Q, & w \\ a, & b, & c \end{array}$$

across the interface; and, as usual, the continuity of Q involves that of c, while the continuity of b involves that of w.

Thus the conditions are four, namely continuity of

$$\left.\begin{aligned} &(1)\ w \\ &(2)\ P, \text{ which} = H\,(u - H\eta_3 v + H\eta_2 w) \\ &(3)\ Q, \text{ which} = H\,(v - H\eta_1 w + H\eta_3 u) \\ &(4)\ a, \text{ which leads to the continuity of } dQ/dz \text{ or } d/dz . H\,(v - H\eta_1 w + H\eta_3 u) \end{aligned}\right\} (30).$$

We shall denote the H of the metallic medium by H′ to distinguish it from that of air. In the air $H = -\iota . 4\pi C^2/p$. If τ be the periodic time, and λ the wave-length in air of the light considered,

$$p = 2\pi/\tau = 2\pi C/\lambda,$$

so that

$$H = -2\iota C\lambda.$$

Also since τ, and therefore p, is the same for the metal as for the air, we see by (20) that

$$H'\Omega^2 = -4\pi\iota p = H\omega^2$$

and therefore

$$\left.\begin{aligned} &H'\Omega^2 = -4\pi\iota p = H\omega^2 \\ &H/H' = \Omega^2/\omega^2 = R^2 e^{2\iota\alpha} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31),$$

where $Re^{\iota\alpha}$ is the quasi-refractive index of the metal (J. J. THOMSON, 'Recent Researches,' p. 419).

11. Let us now substitute the assumed exponential expressions for (u, v, w) in the surface conditions; we thus get

$$\left.\begin{aligned}
-\frac{l}{m}A_0 + \frac{l}{m}A &= -\frac{l}{m_1}A_1 - \frac{l}{m_2}A_2 \\
H(A_0 + A) &= H'(A_1 + A_2) \\
&\quad - H'^2\left\{\eta_3\left(\iota\frac{\omega_1}{m_1}A_1 - \iota\frac{\omega_2}{m_2}A_2\right) - \eta_2\left(-\frac{l}{m_1}A_1 - \frac{l}{m_2}A_2\right)\right\} \\
H(B_0 + B) &= H'\left(\iota\frac{\omega_1}{m_1}A_1 - \iota\frac{\omega_2}{m_2}A_2\right) \\
&\quad - H'^2\left\{\eta_1\left(-\frac{l}{m_1}A_1 - \frac{l}{m_2}A_2\right) - \eta_3(A_1 + A_2)\right\} \\
H\iota(mB_0 - mB) &= H'(-\omega_1 A_1 + \omega_2 A_2) \\
&\quad - H'^2\left\{\eta_1(-\iota l A_1 - \iota l A_2) - \eta_3(\iota m_1 A_1 + \iota m_2 A_2)\right\}
\end{aligned}\right\} \quad (32).$$

We may make the form of the first of these equations analogous with that of the others by multiplying it across by $H\omega^2$ or $H'\Omega^2$. The four equations may then be written as follows:

$$\left.\begin{aligned}
H(A_0 + A) &= H'(A_1 + A_2) - H'^2\iota.\eta_3\left(\frac{\omega_1}{m_1}A_1 - \frac{\omega_2}{m_2}A_2\right) - H'^2\eta_2 l\left(\frac{A_1}{m_1} + \frac{A_2}{m_2}\right) \\
H\frac{\omega^2}{m}(A_0 - A) &= H'\Omega^2\left(\frac{A_1}{m_1} + \frac{A_2}{m_2}\right) \\
-H\iota.(B_0 + B) &= H'\left(\frac{\omega_1}{m_1}A_1 - \frac{\omega_2}{m_2}A_2\right) - H'^2\iota\eta_3(A_1 + A_2) - H'^2\iota\eta_1 l\left(\frac{A_1}{m_1} + \frac{A_2}{m_2}\right) \\
-Hm\iota(B_0 - B) &= H'(\omega_1 A_1 - \omega_2 A_2) - H'^2\iota\eta_3(m_1 A_1 + m_2 A_2) - H'^2\iota\eta_1 l(A_1 + A_2)
\end{aligned}\right\} \quad (33).$$

When we substitute in these the values of ω_1, m_1, ω_2, m_2 found above, and neglect small quantities of the second and higher orders, we get

$$\left.\begin{aligned}
H(A_0 + A) &= H'\left\{1 - H'\eta_2\frac{l}{M}\right\}(A_1 + A_2) - H'^2\iota\eta_3\frac{\Omega}{M}(A_1 - A_2) \\
H\frac{\omega^2}{m}(A_0 - A) &= H'\frac{\Omega^2}{M}(A_1 + A_2) - \tfrac{1}{2}H'^2\iota.\frac{\Omega^3}{M^3}(l\eta_1 + M\eta_3)(A_1 - A_2) \\
-H\iota(B_0 + B) &= H'\frac{\Omega}{M}(A_1 - A_2) - \tfrac{1}{2}H'^2\iota\frac{(\Omega^2 + M^2)}{M^3}(l\eta_1 + M\eta_3)(A_1 + A_2) \\
-Hm\iota(B_0 - B) &= H'\Omega(A_1 - A_2) - \tfrac{1}{2}H'^2\iota.(l\eta_1 + M\eta_3)(A_1 + A_2)
\end{aligned}\right\} \quad (34).$$

From the second and third of these equations we readily get

$$H'\frac{\Omega}{M}(A_1 - A_2) = - H\iota(B_0 + B) + \tfrac{1}{2}H'\iota\frac{(\Omega^2 + M^2)}{M^2\Omega^2}(l\eta_1 + M\eta_3)\, H\frac{\omega^2}{m}(A_0 - A),$$

and

$$H'\frac{\Omega^2}{M}(A_1 + A_2) = H\frac{\omega^2}{m}(A_0 - A) + \tfrac{1}{2}H'\iota\frac{\Omega^2}{M^2}(l\eta_1 + M\eta_3)(-H\iota)(B_0 + B).$$

Substituting from these for $(A_1 - A_2)$ and $(A_1 + A_2)$ in the first and fourth of (34), remembering (31), and for brevity denoting $R^2e^{2\iota\alpha}$ by μ, we get

$$\mu(A_0 + A) = \left(1 - H'\frac{l}{M}\eta_2\right)\frac{M}{m}(A_0 - A) + \tfrac{1}{2}\frac{\mu}{M}H'(l\eta_1 - M\eta_3)(B_0 + B),$$

$$m(B_0 - B) = M(B_0 + B) - \tfrac{1}{2}H'\frac{\omega^2}{Mm}(l\eta_1 + M\eta_3)(A_0 - A),$$

which may be written

$$\left.\begin{aligned} &\left\{\mu + \frac{M}{m}\left(1 - H'\frac{l}{M}\eta_2\right)\right\}A - \tfrac{1}{2}\frac{\mu H'}{M}(l\eta_1 - M\eta_3)B \\ &\qquad + \left\{\mu - \frac{M}{m}\left(1 - H'\frac{l}{M}\eta_2\right)\right\}A_0 - \tfrac{1}{2}\frac{\mu H'}{M}(l\eta_1 - M\eta_3)B_0 = 0 \\ &\tfrac{1}{2}H'\frac{\omega^2}{Mm}(l\eta_1 + M\eta_3)A + (m + M)B - \tfrac{1}{2}H'\frac{\omega^2}{Mm}(l\eta_1 + M\eta_3)A_0 - (m - M)B_0 = 0 \end{aligned}\right\} (35),$$

and solving these for A and B, we get

$$\left.\begin{aligned} &\frac{A}{-\left\{R^2e^{2\iota\alpha} - \frac{M}{m}\left(1 + \iota\frac{2c\lambda}{R^2e^{2\iota\alpha}}\frac{l}{M}\eta_2\right)\right\}(m + M)A_0 - 2\iota c\lambda\frac{m}{M}(l\eta_1 - M\eta_3)B_0} \\ &= \frac{B}{-2\iota c\lambda\frac{\omega^2}{Mm}(l\eta_1 + M\eta_3)A_0 + \left\{R^2e^{2\iota\alpha} + \frac{M}{m}\left(1 + \iota\frac{2c\lambda}{R^2e^{2\iota\alpha}}\frac{l}{M}\eta_2\right)\right\}(m - M)B_0} \\ &= \frac{1}{\left\{R^2e^{2\iota\alpha} + \frac{M}{m}\left(1 + \iota\frac{2c\lambda}{R^2e^{2\iota\alpha}}\frac{l}{M}\eta_2\right)\right\}(m + M)} \end{aligned}\right\} (36),$$

which is the complete mathematical solution of the problem of reflection.

Relation of η_1, η_2, η_3 to the Magnetic Field.

12. Before going further, we must consider how (η_1, η_2, η_3) depend on the imposed magnetic field; and first it should be noticed that, though (η_1, η_2, η_3) appear as

operators, in the case of light vibrations they are algebraical quantities, because $d^2/dt^2 = -p^2$.

In theories of this nature it is usual to assume that (b_1, b_2, b_3) are proportional to the components, parallel to the axes, of the imposed magnetic force; the constants (g_1, g_2, g_3) of the HALL effect are also usually assumed to vary as the magnetic force; and therefore in this case (η_1, η_2, η_3) would do so also. But in experiments on the transmission of light through magnetised metallic films it is found that the rotation of the plane of polarisation certainly does not vary as the magnetic force, but very probably varies as the intensity of magnetisation, a quantity very difficult to determine. We shall see later that in the mathematical solution of the problem of transmission the rotation varies as η_3, and hence we are driven to the assumption that $(\eta_1\ \eta_2\ \eta_3)$ vary as the components of magnetisation. I shall also suppose that (b_1, b_2, b_3) vary as the components of magnetisation, which necessitates the assumption that (g_1, g_2, g_3) do so likewise; the question whether the HALL effect varies as the magnetic force or as the magnetisation has not, I think, been put to an experimental test; the latter supposition seems more probable.

These assumptions can be readily justified from physical considerations. For *in vacuo* there is no magneto-optic rotation, though there is magnetic force; it is therefore not the magnetic force, but matter, or some property of matter when under the influence of magnetic force, that causes the rotation; and the property of matter under the influence of magnetic force is not force, but magnetisation.

Denoting the components of the imposed magnetisation by $(\alpha_0, \beta_0, \gamma_0)$, we assume

$$(\eta_1, \eta_2, \eta_3) = C_0 e^{\iota x} (\alpha_0, \beta_0, \gamma_0) \quad \ldots \ldots \quad (37),$$

where $C_0 e^{\iota x}$ is the complex magneto-optic constant of the theory. For any particular metal the values of C_0 and x may be determined by experiment; and if we find that the numerical values of these constants, as determined by all the different sorts of experiments, are the same, we shall conclude that the theory can account for all the observed facts, and therefore constitutes a complete mathematical explanation of the phenomena.

The Optic Constants of Metals.

13. The constants R and α are different for different metals, and also for light of different colours. Their values have not been directly tabulated, but they are easily obtained from the tabulated values of DRUDE's optic constants; these latter are denoted by n and k, and are connected with R and α by the relations

$$R \cos 2\alpha = n^2 (1 - k^2), \qquad R \sin 2\alpha = -2n^2 k,$$

so that

$$R^2 = n^2 (1 + k^2), \qquad \tan \alpha = -k.$$

The values of n and k for iron, steel, and nickel will be found in a paper of DRUDE'S ('Wied. Ann.,' vol. 39, p. 481), quoted in THOMSON'S 'Recent Researches,' p. 421. The constants for cobalt are given by DRUDE in 'Wied. Ann.,' vol. 46, p. 407. These values are shewn in the following table :—

	Red light.			Sodium light.		
	nk.	n.	k.	nk.	n.	k.
Iron	..	..	..	3·20	2·36	1·36
Steel	3·47	2·62	1·32	3·40	2·41	1·38
Nickel	3·56	1·89	1·88	3·32	1·79	1·86
Cobalt	4·19	2·22	1·89	4·03	2·12	1·90

wherein, for red light, $\lambda = 630 \times 10^{-7}$ centim., and for sodium light we may take $\lambda = 589{\cdot}6 \times 10^{-7}$ centim.

Hence we find the corresponding values of R and α.

	Red light.		Sodium light.	
	R^2.	$-\alpha$.	R^2.	$-\alpha$.
Iron	..	..	15·86	53° 40′
Steel	18·82	52° 51′	16·87	54° 4′
Nickel	16·20	62° 0′	14·29	61° 44′
Cobalt	22·48	62° 7′	20·72	62° 14′

The KERR *Experiments.*

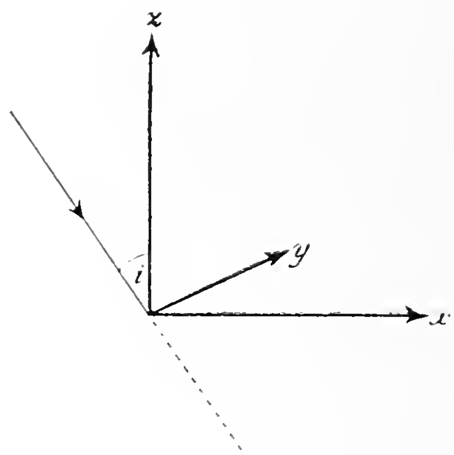
14. We shall first compare our theory with the results of the KERR experiments, which are so well known that they need not be here described.

In Dr. KERR'S second experiment the magnetisation is parallel to the reflecting surface, and to the plane of incidence; and the incident light is polarised perpendicularly to the plane of incidence. Thus, in our notation, $\beta_0 = 0$, $\gamma_0 = 0$, and $B_0 = 0$; and the reflected light is specified by A and B, whose values as given by formula (36) are

$$\left.\begin{aligned} A &= \frac{-(R^2e^{2\iota\alpha} - M/m)(M+m)A_0}{(R^2e^{2\iota\alpha} + M/m)(M+m)} \\ B &= \frac{-2\iota c\lambda\,(\omega^2/Mm)\,l\eta_1 A_0}{(R^2e^{2\iota\alpha} + M/m)(M+m)} \end{aligned}\right\} \quad \quad (38).$$

If the incident ray be as represented in the figure, and i be the angle of incidence, and p and ω be positive, then

$$l = -\omega \sin i$$
$$m = +\omega \cos i.$$



The incident ray being plane polarised, A_0 is real. But A and B are both complex, and have not necessarily the same vector angle; hence the reflected light is elliptically polarised. If θ be the angle through which the major axis of the ellipse of polarisation is rotated round the reflected ray (in the direction from the axis of x towards the axis of y) from the plane $x = 0$, since the modulus of B is very small compared with the modulus of A, θ is given by

$$\theta = \text{real part of } (\mathrm{B} \cos i/\mathrm{A})$$

in circular measure.

In the case of iron, KERR found that when α_0 is negative, θ is negative if i be less than about 75°; while if i be greater than 75°, θ is positive. The angle of incidence for which θ changes sign (and therefore vanishes) has been observed by different experimenters, whose results differ considerably. They are as follows:—

KERR 75°.
KUNDT 80° to 82°.
RIGHI 78° 54′.
SISSINGH 80°.
DRUDE 79°.

Now from (38)

$$\frac{\mathrm{B}}{\mathrm{A}} = \frac{2\iota c\lambda \dfrac{\omega^2}{\mathrm{M}m} l \mathrm{C}_0 e^{\iota x} \alpha_0}{(\mathrm{R}^2 e^{2\iota\alpha} - \mathrm{M}/m)(\mathrm{M} + m)}$$

so that

$$\frac{\mathrm{B}\cos i}{\mathrm{A}} = \frac{2c\lambda \sin i \cos i\, \mathrm{C}_0 \alpha_0 . \iota . e^{\iota x}}{\mathrm{R}^2 e^{2\iota\alpha} . \mathfrak{M} . (\mathfrak{M} + \cos i)(\mathfrak{M}\mathrm{R}^{-2} e^{-2\iota\alpha} - \cos i)} \quad . \quad . \quad . \quad . \quad (38^*),$$

where

$$\mathfrak{M} = \mathrm{M}/\omega,$$

and, therefore, since

$$\omega^2 \mathrm{R}^2 e^{2\iota\alpha} = \Omega^2 = \mathrm{M}^2 + \omega^2 \sin^2 i$$

$$\mathfrak{M}^2 = \mathrm{R}^2 e^{2\iota\alpha} - \sin^2 i \quad . \quad . \quad . \quad . \quad (39),$$

θ changes sign for that value of i which makes the vector angle of B cos i/A equal to an odd number of right angles. Let us assume that x must lie between 0° and 180°, leaving the sign of C_0 to be determined afterwards. To obtain the vector angle for any given angle of incidence we must calculate the vector angles of the various complex factors which occur in numerator and denominator of the fraction in equation (38*). This involves troublesome arithmetical work ; but it is preferable to the approximation on the supposition that R is large, used by J. J. THOMSON ('Recent Researches,' p. 498) in a similar investigation, as that method introduces an error of quite a large number of degrees.

Using the constants for yellow light, I get the following values :—

Angle of incidence.	Vector angle of $\mathfrak{M}$.	Vector angle of $\mathfrak{M}$ + cos i.	Vector angle of $\mathfrak{M}R^{-2}e^{-2\iota\alpha}$ − cos i
75°	−55° 15′	−52° 20′	117° 17′
78° 54′	−55° 18′	−53° 6′	100° 23′
80°	−55° 19′	−53° 19′	95° 2′

whence are derived the following :—

	Angle of incidence.	Vector angle of (B cos i/A).
KERR	75°	x + 187° 38′
RIGHI	78° 54′	x + 205° 21′
SISSINGH	80°	x + 210° 56′

So that if θ changes sign when i = 75°,

$$x = 82° \ 22'.$$

If when i = 78° 54′, then

$$x = 64° \ 39'.$$

If when i = 80°, then

$$x = 59° \ 4'.$$

And probably if θ changed sign when i = 78°, the corresponding value of x would be about 69°.

The uncertainty as to the exact value of the angle of incidence for which θ vanishes, and the large difference, caused by a small error of observation, in the resulting value of x, render this experiment unsuitable as a means of arriving at the exact value of x. It will, however, be useful in testing a value of x determined in some other way.

The experiment will also tell us the sign of C_0; for, in accordance with KERR's observations, when the incidence is very nearly normal, θ is of the same sign as α_0; and when the angle of incidence is nearly 90°, θ is of the opposite sign to α_0. Now the table of values given above indicates that as i passes through that value (be it 75° or 80°) for which θ vanishes, from a less to a greater value, the cosine of the vector angle from being negative becomes positive, so that for very great angles of incidence θ is of the same sign as $C_0\alpha_0$. Hence C_0 is negative.

15. In KERR's first experiment the magnetisation is parallel to the reflecting surface and the incident light is polarised in the plane of incidence. If θ be the rotation of the major axis of the ellipse of polarisation in the same sense as before, KERR found that θ has the same sign for all angles of incidence, and that this sign is opposite to that of α_0.

In this case $\eta_2 = 0$, $\eta_3 = 0$, $A_0 = 0$, and $\theta =$ real part of $(- A/B \cos i)$.

From result (36) we readily deduce that

$$\frac{A}{B \cos i} = \frac{-2c\lambda \sin i \cos i \, C_0\alpha_0\iota \, . \, e^{\iota x}}{R^2 e^{2\iota\alpha} \mathfrak{M} (\mathfrak{M} - \cos i) (\mathfrak{M} R^{-2} e^{-2\iota\alpha} + \cos i)},$$

of which the vector angle (including the minus sign) is $x - 90° - 2\alpha -$ sum of vector angles of $\mathfrak{M}$, $(\mathfrak{M} - \cos i)$, $(\mathfrak{M}R^{-2}e^{-2\iota\alpha} + \cos i)$.

If $i = 0$, vector angle of $A/B \cos i$ is

$$x + 128° \, 3'.$$

If $i = 61° \, 30'$ vector angle is

$$x + 115° \, 42'.$$

If $i = 90°$, vector angle is

$$x + 76° \, 4'.$$

And evidently for all angles of incidence the vector angle lies between $x + 76°$ and $x + 128°$. So that if x have any value between 14° and 142°, the cosine of the vector angle of $A/B \cos i$ is negative for all angles of incidence. Thus θ has always the same sign as $C_0\alpha_0$, that is, the opposite sign to α_0.

Hence any value of x lying between 14° and 142° satisfies all the conditions of KERR's first experiment.

16. In another of KERR's experiments the magnetisation is normal to the reflecting surface, and the incident light is polarised in the plane of incidence.

Here

$$\eta_1 = 0, \qquad \eta_2 = 0, \qquad A_0 = 0;$$

θ, having the same meaning as before, is found by KERR to be of opposite sign to γ_0 for all angles of incidence. Now θ is the real part of $- A/B \cos i$, and we readily deduce from formula (36) that

$$\frac{\mathrm{A}}{\mathrm{B}\cos i} = \frac{2c\lambda \cos i\, \mathrm{C}_0\gamma_0 (-\iota)\, e^{\iota x}}{\mathrm{R}^2 e^{2\iota\alpha} (\mathfrak{M} - \cos i)(\mathfrak{M}\mathrm{R}^{-2} e^{-2\iota\alpha} + \cos i)},$$

of which complex the vector angle is found in the usual way to lie, for all angles of incidence, between the values $x + 20° \, 43'$, corresponding to $i = 90°$, and $x + 74° \, 23'$, corresponding to $i = 0$.

If x have any value between 69° 17′ and 195° 37′, the cosine of this vector angle is always negative; and so θ has the same sign as $\mathrm{C}_0\gamma_0$, or the opposite sign to γ_0. Hence this experiment of KERR'S is satisfied if x have any value between 69° 17′ and 195° 37′.

17. In KERR'S fourth experiment the magnetisation is normal to the reflecting surface, and the incident light is polarised perpendicularly to the plane of incidence. Here $\eta_1 = 0$, $\eta_2 = 0$, $\mathrm{B}_0 = 0$, and θ is the real part of $\mathrm{B}\cos i/\mathrm{A}$ where, from (36),

$$\frac{\mathrm{B}\cos i}{\mathrm{A}} = \frac{2c\lambda \cos i\, \mathrm{C}_0\gamma_0 (-\iota)\, e^{\iota x}}{\mathrm{R}^2 e^{2\iota\alpha} (\mathfrak{M} + \cos i)(\mathfrak{M}\mathrm{R}^{-2} e^{-2\iota\alpha} - \cos i)},$$

of which complex the vector angle lies between the values $x - 105° \, 37'$, corresponding to $i = 0$, and $x + 20° \, 43'$, corresponding to $i = 90°$.

Now KERR found that θ is, for all angles of incidence, of opposite sign to γ_0, that is, of the same sign as $\mathrm{C}_0\gamma_0$. Hence the cosine of the vector angle of $\mathrm{B}\cos i/\mathrm{A}$ is always positive. This is in accordance with our theory, provided the value of x lie between 15° 37′ and 69° 17′.

Obviously this conclusion is at variance with that derived from the preceding experiment, unless x happen to have exactly the value 69° 17′.

But this experiment of KERR'S was repeated by KUNDT, who found that θ has not the same sign for all angles of incidence, but that it vanishes and changes sign for an angle of incidence which he estimated at about 82°.

I have calculated the values of the vector angle of $\mathrm{B}\cos i/\mathrm{A}$ for several angles of incidence in the neighbourhood of 82°; they are as follows:—

Angle of incidence.	Vector angle of $\mathrm{B}\cos i/\mathrm{A}$.	Angle of incidence.	Vector angle of $\mathrm{B}\cos i/\mathrm{A}$.
75°	$x - 47° \, 37'$	85°	$x + 0° \, 43'$
78° 54′	$x - 29° \, 57'$	86°	$x + 5° \, 20'$
80°	$x - 24° \, 23'$	86° 30′	$x + 7° \, 27'$
82° 30′	$x - 11° \, 32'$	88°	$x + 13° \, 31'$

So that if we denote by i_0 that angle of incidence for which θ changes sign, the values of x corresponding to various hypothetical values of i_0 are as follows:—

i_0	75°	78° 54′	80°	82° 30′	85°	86°	86° 30′	88°
x	137° 37′	119° 57′	114° 23′	101° 32′	89° 17′	84° 40′	82° 33′	76° 29′

And if KUNDT's observation be accurate, the value of x is about 103°.

18. From the preceding paragraphs it appears that any value of x lying between 69° 17′ and 82° 22′ will account very well for the four KERR experiments (in the case of iron), except that the agreement with KUNDT's result in the fourth experiment would be imperfect to the extent of four or five degrees.

19. The KERR experiments were also tried on mirrors of nickel and of cobalt, but the observations made were so indefinite that they are of little use as a test of the present theory. In the case of polar reflection from nickel, when the incident light is polarised perpendicularly to the plane of incidence, KUNDT found that the rotation changes sign for an angle of incidence somewhere between 50° and 60°. I have calculated (for yellow light) the values of the vector angle of $B \cos i/A$ for these angles of incidence, and find them to be

$$x - 67^\circ\ 12' \quad \text{for} \quad i = 50^\circ,$$
$$x - 57^\circ\ 12' \quad \text{for} \quad i = 60^\circ,$$

so that any value of x lying between 147° 12′ and 157° 12′ will give a satisfactory explanation of this experiment. Also C_0 would be negative.

When the reflection is polar and the incident light is polarised in the plane of incidence, KUNDT finds that θ has the opposite sign to γ_0 for all angles of incidence. The vector angle of $A/B \cos i$ is found to be

$$x + 36^\circ\ 42' \quad \text{when} \quad i = 90^\circ,$$

and

$$x + 98^\circ\ 26' \quad \text{when} \quad i = 0^\circ.$$

The cosine of this vector angle will be negative for all angles of incidence, provided the value of x lie between 53° 18′ and 171° 34′.

The two experiments indicate that for nickel x has a value intermediate between 147° and 157°.

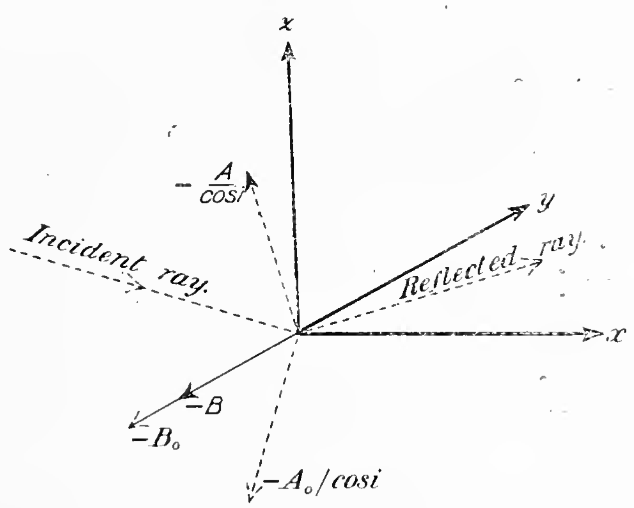
The Experiments of SISSINGH *and* ZEEMAN.

20. I now pass to a much more precise and accurate test of the present theory, the materials for which are to be found in the elaborate series of experiments made during the last few years at Leyden, by SISSINGH and by ZEEMAN.

The first of these series is described by Sissingh in a paper in the 'Archives Néerlandaises,' vol. 27. The experiments were made with an iron mirror, magnetised parallel to the reflecting surface; the amplitude μ and phase m of what Sissingh calls "the magneto-optic component of the reflected light" were measured for various angles of incidence.

One result of the observations is the conclusion that $\mu_i = \mu_p$, and $m_i = m_p$; that is, that for any given angle of incidence the magneto-optic component has the same amplitude and phase, whether the incident light be polarised in or perpendicularly to the plane of incidence. It is to be noticed that the phase of this component is defined as its retardation of phase calculated relatively to that component of ordinary metallic reflection which is polarised in the plane of incidence. The amplitude is reckoned on the supposition that the amplitude of the incident ray is unity.

The accompanying figure shews the relation between the axes of coordinates and the principal directions for the incident and reflected rays, as defined by Sissingh. The standard ray for phase is $-$ B. The standard ray for amplitude is $-\mathrm{A}_0/\cos i$, or $-\mathrm{B}_0$, according as the incident light is polarised perpendicularly to or in the plane of incidence.



It will be convenient to denote by ϑ the *acceleration* of phase of the magneto-optic component of the reflected ray calculated relatively to that component of ordinary metallic reflection which is polarised in the plane of incidence. If ϑ be calculated from theory, and m from experiment, the theory and experiment will agree if $\vartheta + m = 0°$ or $360°$

21. When the incident light is polarised in the plane of incidence, $\mathrm{A}_0 = 0$, and in formula (36) the incident ray is represented by $-\mathrm{B}_0$, the magneto-optic component of the reflected ray by $-\mathrm{A}/\cos i$, and the component relatively to which phase is to be measured, by $-$ B. Hence

$$\vartheta_i = \text{vector angle of } \{\mathrm{A}/\mathrm{B}\cos i\}_{\mathrm{A}_0=0}$$

$$= \text{vector angle of } \left\{\frac{2.\iota.c\lambda\,(m/\mathrm{M})\,\mathrm{C}_0 e^{\iota x}\,(l\alpha_0 - \mathrm{M}\gamma_0)}{\cos i\,(\mathrm{R}^2 e^{2\iota\alpha} + \mathrm{M}/m)\,(\mathrm{M} - m)}\right\} \quad . \quad . \quad . \quad . \quad (40).$$

When the incident light is polarised perpendicularly to the plane of incidence, $B_0 = 0$, and the incident ray is represented by $-A_0/\cos i$; the magneto-optic component of the reflected ray by $-B$, that is to say that term in $-B$ which contains the factor A_0. The ray relatively to which phase is measured is represented by that term in $-B$ which contains the (vanishing) factor B_0. If B_0 be supposed to be only just not zero, then, since the incident ray is supposed to be plane polarised, B_0/A_0 is a real quantity. Hence we have

$$\vartheta_p = \text{vector angle of } \left.\frac{2\iota c\lambda\,(\omega^2/Mm)\,C_0e^{\iota x}\,(l\alpha_0 + M\gamma_0)}{(R^2e^{2\iota a} + M/m)\,(M - m)}\right\} \quad . \quad . \quad . \quad . \quad (41).$$

From (40) and (41) we see at once that when the reflection is equatorial, that is, when $\gamma_0 = 0$,

$$\vartheta_i = \vartheta_p = \vartheta \text{ (say)},$$

and this agrees with SISSINGH's observations.

We also see that when the reflection is polar, that is when $\alpha_0 = 0$,

$$\vartheta_i = \vartheta_p \pm 180^\circ.$$

Now ZEEMAN, as a result of experiments on polar reflection described by him in the 'Archives Néerlandaises,' vol. 27, came to the conclusion that $m_i = m_p$. It is very possible that this discrepancy is due to his using a definition of m_i and m_p slightly different from SISSINGH's.

22. When the reflection is equatorial, we see from (40) that

$$\vartheta = \text{vector angle of } \frac{-\,2c\lambda\,\sin i\,C_0\alpha_0\,\iota\,.\,e^{\iota x}\cos i}{\mathfrak{M}R^2e^{2\iota a}\,(\mathfrak{M} - \cos i)\,(\mathfrak{M}R^{-2}e^{-2\iota a} + \cos i)}\,.$$

In determining ϑ from this expression there is an ambiguity to the extent of 180°; for in defining m (or $360^\circ - \vartheta$) SISSINGH requires that it shall not be altered when α_0 changes sign. Examining his paper, we see that in equatorial reflection the standard case is when α_0 is negative. Hence, remembering that C_0 is negative, we find that

$$\vartheta = x - 90^\circ - 2a - \text{the sum of the vector angles of}$$
$$\left[\mathfrak{M},\ (\mathfrak{M} - \cos i),\ \text{and}\ (\mathfrak{M}R^{-2}e^{-2\iota a} + \cos i)\right] . \quad . \quad (42);$$

and to get ϑ accurately for any particular angle of incidence, these three vector angles must be calculated.

The following table shews the results of SISSINGH's observations on the phase for various angles of incidence, and the theoretical values of the phase for the same angles of incidence, calculated from the present theory.

It is to be observed that the calculation involves only the ordinary optic constants of the metal, and that it is from the comparison with experiment that we derive information as to the value of x in the magneto-optic constant $C_0 e^{\iota x}$. There is thus no question of being able to adjust two coefficients, C_0 and x, so as to satisfy the observations, as might be supposed; only one coefficient x is involved, and the test is accordingly a severe one.

EQUATORIAL Reflection from Iron. Yellow Light. $\alpha_0 = -1400$ C.G.S.

Angle of incidence.	Calculated value of $\vartheta - x + 180°$.	SISSINGH'S observed value of $m - 180°$.	$\vartheta + m - x$.
86° 0′	267° 25′	29° 26′	296° 51′
82° 30′	274° 41′	24° 22′	299° 3′
76° 30′	283° 29°	14° 49′	298° 18′
71° 25′	288° 47′	10° 3′	298° 50′
61° 30′	295° 42′	1° 49′	297° 31′
51° 22′	300° 12′	− 1° 0′	299° 12′
36° 10′	304° 33′	− 5° 51′	298° 42′
24° 16′	..	doubtful	
12° 0′	..	doubtful	
6° 0′	..	doubtful	

The constancy of the angles in the last column is remarkably good; and the theory accounts for the phenomena with great accuracy if the value assigned to x be the mean of the amounts by which these angles respectively fall short of 360°, namely

$$x = 61° \, 39'.$$

23. When the reflection is polar, we see from (41) that

$$\vartheta_p = \text{vector angle of } \frac{2C\lambda C_0 \gamma_0 \iota \, . \, e^{\iota x}}{R^2 e^{2\iota\alpha} (\mathfrak{M} - \cos i)(\mathfrak{M} R^{-2} e^{-2\iota\alpha} + \cos i)} .$$

Taking γ_0 positive as the standard case, and remembering that C_0 is negative, we find that

$$\vartheta_p = x - 90° - 2\alpha - \text{the sum of the vector angles of } \left[(\mathfrak{M} - \cos i) \text{ and } (\mathfrak{M} R^{-2} e^{-2\iota\alpha} + \cos i) \right] \quad . \quad . \quad . \quad . \quad (43).$$

Experiments as to the amplitude and phase of the magneto-optic component of light reflected from an iron mirror, magnetised normally to the reflecting surface, have been made by ZEEMAN. He gives an account of these in the 'Archives Néerlandaises,' vol. 27; he confines himself to one angle of incidence, viz., $i = 51° \, 22'$.

His result as regards phase compares with theory as follows :—

POLAR Reflection from Iron. Yellow Light. $\gamma_0 = + 850$ C.G.S.

Angle of incidence.	Calculated value of $\vartheta_p - x + 180°$.	ZEEMAN'S observed value of $m - 180°$.	$\vartheta_p + m - x$.
51° 22′	245° 30′	49° 55′	295° 25′

The theory will agree accurately with the experiment if the value of x be

$$x = 64° \ 35'.$$

The values of m given by ZEEMAN for yellow light are
(1.) Derived from "rotations to zero"

$$m_i = 48° \ 58' + 180° \qquad m_p = 50° \ 53' + 180°.$$

(2.) Derived from "minimum rotations"

$$m_i = 45° + 180° \qquad m_p = 44° \ 53' + 180°.$$

In determining the phase the method of "rotations to zero" is preferable to that of "minimum rotations," and so in the above table I have used the mean of the values got by the former method.

24. Another test of the present theory is afforded by observations of the amplitude of the "magneto-optic component." This is denoted by μ_i or μ_p, according as the incident light is polarised in or perpendicularly to the plane of incidence. In the former case the magneto-optic component is represented by $-\mathrm{A}/\cos i$, and the incident ray by $-\mathrm{B}_0$; in the latter case the incident ray is represented by $-\mathrm{A}_0/\cos i$, and the magneto-optic component by $-\mathrm{B}_0$. Hence

$$\mu_i = \operatorname{mod}\left(\frac{\mathrm{A}}{\mathrm{B}_0 \cos i}\right)_{\mathrm{A}_0 = 0} \qquad \mu_p = \operatorname{mod}\left(\frac{\mathrm{B} \cos i}{\mathrm{A}_0}\right)_{\mathrm{B}_0 = 0} \quad . \quad . \quad . \quad . \quad (44).$$

Thus, for *equatorial* reflection, we readily derive from (36)

$$\mu_i = \operatorname{mod} \frac{2c\lambda \sin i \cos i \, \mathrm{C}_0 \alpha_0 e^{\iota x}}{\mathfrak{M}\mathrm{R}^2 e^{2\iota a} (\mathfrak{M} + \cos i)(\mathfrak{M}\mathrm{R}^{-2} e^{-2\iota a} + \cos i)},$$

$$\mu_p = \text{the same},$$

and therefore

$$\mu_i = \mu_p = \mu \text{ (say)},$$

which agrees with SISSINGH's result.

If for brevity we put $2c\lambda C_0 \alpha_0 / R^2 \equiv L$, we have

$$\mu = L \,.\, \text{mod} \frac{\cos i \sin i}{\mathfrak{M}(\mathfrak{M} + \cos i)(\mathfrak{M} R^{-2} e^{-2\iota\alpha} + \cos i)},$$

and the latter factor may be calculated for any angle of incidence.

In the following table the theoretical values of μ for various angles of incidence are compared with the values observed by SISSINGH. Here again the theoretical value of μ involves the magneto-optic constant $C_0 e^{\iota x}$ only by being proportional to C_0, and x is not involved: thus we have not available any adjustment of x to improve the agreement, and the test is very severe.

In fact one set of experiments involves C_0 only, and the other set x only: so that a complex magneto-optic constant really gives no more opportunity for adjustment than would a real one.

EQUATORIAL Reflection from Iron. Yellow Light. $\alpha_0 = -1400$ C.G.S.

Angle of incidence.	Calculated value of $\log_{10} \mu - \log_{10} L$.	SISSINGH's observed value of $10^3 \times \mu$.	$\left(\frac{\text{Calculated value of } \mu/L}{\text{Observed value of } \mu}\right)$.
86° 0′	$\bar{2}$·1506	·284	49·81
82° 30′	$\bar{2}$·3513	·530	42·37
76° 30′	$\bar{2}$·4916	·715	43·38
71° 25	$\bar{2}$·5397	·815	42·51
61° 30′	$\bar{2}$·5634	·820	44·63
51° 22′	$\bar{2}$·5373	·760	45·34
36° 10′	$\bar{2}$·4305	·630	42·78
24° 16′	$\bar{2}$·3577	·430	52·99
12° 0′	$\bar{3}$·9834	·260	37·02
6° 0′	$\bar{3}$·6849	·125	38·73

Exact agreement of theory with experiment would be indicated by the numbers' in the last column being all equal. Though this is not the case, their approximation to equality is, considering the probability of errors in the observations, remarkably good. The mean of these numbers is about 44; and if we assume C_0 to have such a value that $L = \frac{1}{44}$, the ratios of the calculated to the observed amplitudes for the above angles of incidence taken in order are 1·13, 0·96, 0·99, 0·97, 1·01, 1·03, 0·97, 1·20, 0·84, and 0·88 respectively.

The corresponding value of $-C_0$ is

$$-C_0 = \frac{R^2}{44 \times 2c\lambda \times 1400}$$

where

$$\lambda = 589{\cdot}6 \,.\, 10^{-7}, \qquad R^2 = 15{\cdot}86, \qquad c = 3 \,.\, 10^{10},$$

the units being electromagnetic and C.G.S. And hence

$$\log_{10}(-C_0) = \overline{11}\cdot 8623,$$
$$-C_0 = 7\cdot 283 \times 10^{-11}.$$

25. For *polar* reflection, we derive from (44) and (36)

$$\mu_p = \text{mod.}\frac{2c\lambda\cos i\,(-C_0)\,\gamma_0\iota\,.\,e^{\iota\omega}}{R^2 e^{2\iota a}(\mathfrak{M}+\cos i)(\mathfrak{M}R^{-2}e^{-2\iota a}+\cos i)};$$
$$= -\mu_i, \qquad = \mu\ (\text{say}).$$

Comparing this with the amplitude in equatorial reflection, we find

$$\frac{\mu\,(\text{equatorial})}{\mu\,(\text{polar})} = \text{mod.}\frac{(-\alpha_0)\sin i}{\mathfrak{M}\gamma_0}.$$

If $-\alpha_0 = 1400$, $\gamma_0 = 850$, $i = 51^\circ\ 22'$, the value of this ratio, as calculated from theory, is ·321.

But the values ascribed to α_0, γ_0, and i correspond to the experiments of SISSINGH and ZEEMAN; and the latter found experimentally

$$\frac{\mu\,(\text{SISSINGH})}{\mu\,(\text{ZEEMAN})} = \cdot 294.$$

So that here again we have a very fair agreement of the theory with experiment.

Nickel.

26. In the paper already quoted ZEEMAN gives a few measurements made by himself on polar reflection from nickel. He also quotes experimental results of KUNDT ('Wied. Ann.,' vol. 23), and DRUDE ('Wied. Ann.,' vol. 46), which he expresses in a form similar to his own. These I have used to form the following tables, wherein the theoretical values of the phase and amplitude are in all cases calculated for yellow light.

EQUATORIAL Reflection from Nickel. (Probably) White Light. $\alpha_0 =$

Angle of incidence.	Calculated value of $\vartheta - x + 180°$.	KUNDT's observed value of $m_i - 180°$.	$\vartheta + m_i - x$.
30° 6′	337° 17′	− 3° 50′	333° 27′
40°	334° 48′	−64° 18′	270° 30′
50°	331° 15′	−64° 46′	266° 29′
61° 30′	325° 11′	−52° 21′	272° 50′
65° 18′	322° 27′	−53° 18′	269° 9′
75°	312° 43′	−49° 54′	262° 49′

Fairly good agreement is here indicated (except in case of first angle of incidence) if the value of x be about

$$x = 91° \ 30'.$$

EQUATORIAL Reflection from Nickel. White Light. $\alpha_0 =$

Angle of incidence.	Calculated value of $\vartheta - x + 180°$.	DRUDE's observed value of $m - 180°$.	$\vartheta + m - x$.
60°	326° 9′	−48° 22′	277° 47′
65°	322° 40′	−46° 3′	276° 37′
75°	312° 43′	+11° 41′	324° 24′
80°	305° 11′	− 8° 42′	296° 29′

If $x = 76° \ 30'$ or thereabouts a fairly good agreement is indicated, except in the case of $i = 75°$. For this case DRUDE's observation differs widely from KUNDT's, and is perhaps wrong.

EQUATORIAL Reflection from Nickel.

Angle of incidence.	Calculated value of $\log_{10} \mu - \log_{10} L$.	KUNDT's observed value of $10^3 \times \mu$.	$\left(\frac{\text{Calculated value of } \mu/L}{\text{Observed value of } \mu}\right)$.
30° 6′	$\bar{2}$·4193	·21	125·1
40°	$\bar{2}$·5205	·77	43·05
50°	$\bar{2}$·5842	1·39	27·62
61° 30′	$\bar{2}$·6139	·90	45·68
65° 18′	$\bar{2}$·6111	·84	48·62
75°	$\bar{2}$·5635	·23	159·1

The agreement of theory with experiment is here more defective. As the intensity of magnetisation is not stated, this series of experiments gives no information as to the value of C_0.

POLAR Reflection from Nickel. Yellow Light. $\gamma_0 =$

Angle of incidence.	Calculated value of $\vartheta_p + 180° - x$.	ZEEMAN's observed value of $m - 180°$.	$\vartheta_p + m - x$.
50°	268° 33′	11° 40′	280° 13′

shewing agreement if $x = 79° 47'$.

The experiments quoted in the two following tables are from a paper of ZEEMAN's ('Communications from the Leiden Laboratory of Physics,' No. 10) :—

POLAR Reflection from Nickel. White Light. $\gamma_0 = 2190$ C.G.S.

Angle of incidence.	Calculated value of $\vartheta_i - x$.	Observed value of m.	$\vartheta_i + m - x$.
25°	276° 13′	5° 9′	281° 22′
39° 4′	272° 43′	9° 17′	282°

shewing agreement if $x = 78° 19'$.

POLAR Reflection from Nickel. White Light. $\gamma_0 = 2190$ C.G.S.

Angle of incidence.	Calculated value of $\log_{10}(-\mu_i) - \log_{10} L'$.	ZEEMAN's observed value of $10^3 \times \mu$.	$\left(\frac{\text{Calculated value of } \mu/L'}{\text{Observed value of } \mu}\right)$.
39° 4′	$\bar{1}$·2940	− ·975	201·8
25°	$\bar{1}$·3000	− 1·00	199·5

wherein $L' \equiv 2c\lambda\,(-C_0)\,\gamma_0/R^2$.

The agreement indicated is excellent, provided C_0 have such a value that

$$L' = \tfrac{1}{200},$$

namely,

$$\log(-C_0) = \overline{12}\cdot 9649$$
$$-C_0 = 9\cdot 225 \times 10^{-12}$$

The experimental results used in the following table are taken from a paper by Dr. C. H. WIND ('Communications from the Leiden Laboratory of Physics,' No. 9) :—

POLAR Reflection from Nickel. Yellow Light.

Angle of incidence.	Strength of magnetic field in C.G.S. units.	Calculated value of $\vartheta_i - x$.	Observed value of m_i.	$\vartheta_i + m_i - x$.
39° 4′	2190	272° 43′	14° 32′	287° 15′
55°	9560	266° 7′	17° 47′	283° 54′
75°	12470	249° 28′	32° 25′	281° 53′

For incidence of 39° 4′ ZEEMAN'S result is to be preferred to that of WIND, as he took more precautions to eliminate causes of error. For the other two angles of incidence agreement is indicated if x is about 77°.

In estimating the consistency of the above results it is to be remembered that the optic constants (R and α) for different specimens of nickel are often sensibly different. (I have used the same set of values all through.) Indeed in the case of iron ZEEMAN found that his observations of the optic constants of a particular mirror, made respectively before and after an observation of the KERR phenomena, differed considerably.

Cobalt.

27. In his paper in the 'Archives Néerlandaises,' vol. 27, ZEEMAN describes experiments made by himself on mirrors of cobalt, and also quotes the results of experiments made by DRUDE. The comparison of these with the present theory is shewn in the following tables :—

POLAR Reflection from Cobalt. White Light.

Angle of incidence.	Calculated value of $\vartheta_p - x + 180°$.	ZEEMAN'S observed value of $m - 180°$.	$\vartheta_p + m - x - 360°$.
45°	272° 11′	20° 34′	−67° 15′
60°	265° 34′	27° 40′	−66° 46′
73°	255° 6′	37° 55′	−66° 59′

Good agreement is indicated if $x = 67°$.

POLAR Reflection from Cobalt. Green Light.

Angle of incidence.	Calculated value of $\vartheta_p - x + 180°$.	ZEEMAN's observed value of $m - 180°$.	$\vartheta_p + m - x - 360°$.
50°	270° 23′	25° 9′	−64° 28′
60°	265° 34′	32° 30′	−61° 56′
72°	256° 13′	45° 51′	−57° 56′

This shews fairly good agreement if x is about 61° 30′. In the above ϑ is calculated from the constants for yellow light.

EQUATORIAL Reflection from Cobalt. White Light.

Angle of incidence.	Calculated value of $\vartheta - x + 180°$.	DRUDE's observed value of $m - 180°$.	$\vartheta + m - x - 360°$.
35°	337° 39′	−77° 24′	−99° 45′
60°	328° 40′	−25° 27′	−56° 47′
75°	315° 59′	−12° 56′	−56° 57′
83°	302° 9′	−12° 57′	−70° 48′

Here the agreement is not so good; the last three angles of incidence would indicate that x is about 61° 30′. DRUDE's method is that of minimum rotations, wherein errors of observation influence the phase to a much greater extent than in the method of null-rotations.

The following experiments are from a paper of ZEEMAN's ('Communications from the Leiden Laboratory of Physics,' No. 5):—

POLAR Reflection from Cobalt. White Light. $\gamma_0 = 430$ C.G.S.

Angle of incidence.	Calculated value of $\log_{10}(\mu_p) - \log_{10} L'$.	ZEEMAN's observed value of $10^3 \times \mu$.	$\left(\dfrac{\text{Calculated value of } \mu/L'}{\text{Observed value of } \mu}\right)$.
45°	$\bar{1}$·2333	1·58	108·3
60°	$\bar{1}$·2092	1·50	107·9
73°	$\bar{1}$·1413	1·17	118·3

wherein $L' = 2c\lambda(-C_0)\gamma_0/R^2$.

Here the agreement is very good, the indicated value of L′ being about $\frac{1}{111}$; if this be so, we have for cobalt

$$\log(-C_0) = \overline{10}\cdot0889,$$

i.e.,

$$-C_0 = 1\cdot227 \times 10^{-10}.$$

The Hall Effect.

28. Before leaving this part of the subject it is worth while to investigate whether the ordinary HALL effect is large enough to contribute, to any appreciable extent, to the phenomena we have been considering.

If $\mathfrak{C}$ be HALL's constant, as usually defined, the equations into which it enters are of the type

$$P = P' + \mathfrak{C}(\beta_0' w - \gamma_0' v),$$

where $(\alpha_0', \beta_0', \gamma_0')$ is the magnetic force. Comparing this with the form that equations (11) would assume if (b_1, b_2, b_3) were zero, it appears that

$$\mathfrak{C}\beta_0' = H^2 g_2 = -\frac{4c^2\lambda^2}{R^4 e^{4\iota a}} g^2;$$

so that

$$g_2 = -\mathfrak{C}\frac{R^4 e^{4\iota a}}{4c^2\lambda^2}\beta_0'.$$

Now for iron,

$$\mathfrak{C} = 7850 \times 10^{-15},$$

and if we substitute the values of R, α, λ, corresponding to yellow light, we find

$$g_2 = -\beta_0' \,.\, Q \,.\, e^{\iota(145^\circ\, 20')}$$

where $\log_{10} Q = \overline{16}\cdot4466$.

But we see from § 24 that $\eta_2 = \beta_0 \,.\, C_0 e^{\iota x}$, where $\log_{10}(-C_0) = \overline{11}\cdot8623$. Also $\beta_0 = \mu\beta_0'$, where μ is the magnetic permeability of iron and is greater than unity.

Hence the modulus of the fraction g_2/η_2 has a logarithm less than $\overline{6}\cdot5843$, so that the modulus itself is less than $\frac{1}{260000}$. Thus it appears that the ordinary HALL effect is more than two hundred thousand times too small to account for the KERR phenomena.

But, in order that the coefficients (b_1, b_2, b_3) should be real, it is necessary that the imaginary parts of the complexes (η_1, η_2, η_3) should be supplied by (g_1, g_2, g_3), that is to say by the HALL effect. Hence it must be concluded that the coefficient of the HALL effect is very much greater for excessively rapidly alternating currents than for steady ones. There is nothing unnatural in this, for the incipient conductions which make optical opacity have no relation of continuity whatever with the steady conduction in an ordinary current; thus MAXWELL found that the ordinary coefficients of "conductivity" are very much smaller in the optical circumstances. And it may be noticed that, as $\mathfrak{E}$ is proportional to electromotive force divided by current, a greatly diminished conductivity will correspond to a greatly increased value of $\mathfrak{E}$.

The value which HALL's constant would, on this supposition, have for yellow light, is obtainable from the equation

$$\text{Imaginary part of } \eta_2 = \text{imaginary part of } -\mathfrak{E}\frac{R^4 e^4{}_{\iota\alpha}}{4c^2\lambda^2}\beta_0$$

wherein I now take the HALL effect to be proportional, not to the magnetic force, but to the intensity of magnetisation.

This gives

$$\mathfrak{E} = -\frac{4c^2\lambda^2}{R^4 \sin 4\alpha} C_0 \sin x$$
$$= +(5{\cdot}670) \text{ for iron, and yellow light.}$$

The real part of g_2 is then $C_0 \sin x \cot 4\alpha . \beta_0$, so that, if $(b_1, b_2, b_3) = E_0(\alpha_0, \beta_0, \gamma_0)$,

$$-p^2 E_0 = C_0 \cos x - C_0 \sin x \cot 4\alpha$$
$$= -C_0 \frac{\sin(x - 4\alpha)}{\sin 4\alpha}$$

and hence we find

$$\log_{10} E_0 = \overline{41}{\cdot}0939, \qquad E_0 = (1{\cdot}242) . 10^{-41}.$$

Effect of Magnetisation Perpendicular to the Plane of Incidence.

29. A very interesting inference from the presence of η_2 in the equations (36) is that, if the present theory be true, the component of magnetisation perpendicular to the plane of incidence will produce an effect not quite the same as the KERR phenomenon, but of the same order of magnitude.

On enquiring whether such an effect had ever been observed or measured, I found that a few months ago it was predicted from theoretical considerations by Dr. C. H. WIND, in a paper which has as yet appeared only in Dutch. Acting on this prediction ZEEMAN sought the phenomenon experimentally, found it, and succeeded in measuring it. His results are published in the 'Communications from the Leiden Laboratory of Physics,' No. 29.

Let us suppose that the magnetisation is entirely perpendicular to the plane of incidence; then $\eta_1 = 0$ and $\eta_3 = 0$, and the reflected light is specified by A and B, where, from equations (36),

$$A = \frac{-\left\{R^2 e^{2\iota a} - \frac{M}{m}\left(1 + \iota \frac{2c\lambda}{R^2 e^{2\iota a}} \frac{l}{M} \eta_2\right)\right\}}{\left\{R^2 e^{2\iota a} + \frac{M}{m}\left(1 + \iota \frac{2c\lambda}{R^2 e^{2\iota a}} \frac{l}{M} \eta_2\right)\right\}} A_0,$$

$$B = \frac{m - M}{m + M} B_0.$$

From these expressions we see that, if the incident ray is polarised in the plane of incidence, so that $A_0 = 0$, the expression for the reflected ray does not involve η_2; and so the magnetisation β_0 produces no effect. This is in agreement with the prediction of WIND.

But if the incident ray be polarised perpendicularly to the plane of incidence, so that $B_0 = 0$, the reflected ray is given by A, which does contain η_2. Instead of having

$$A = \frac{-(R^2 e^{2\iota a} - M/m)}{(R^2 e^{2\iota a} + M/m)}$$

as would be the case if there were no magnetisation, we have A equal to this value multiplied by the factor

$$\frac{1 - \{\iota . 2c\lambda R^{-2} e^{-2\iota a} (l/m) C_0 e^{\iota x} \beta_0\}/\{R^2 e^{2\iota a} - M/m\}}{1 + \{\iota . 2c\lambda R^{-2} e^{-2\iota a} (l/m) C_0 e^{\iota x} \beta_0\}/\{R^2 e^{2\iota a} + M/m\}},$$

which is the same as

$$1 + C_0 \beta_0 . 2c\lambda . \sin 2i . \frac{\iota . e^{\iota . x}}{R^4 e^{4\iota a} (\cos i - \mathfrak{M} R^{-2} e^{-2\iota a})(\cos i + \mathfrak{M} R^{-2} e^{-2\iota a})},$$

and the effect of this factor, which of course differs from unity by only a very small quantity, is to slightly alter both the amplitude and the phase of the still plane-polarised reflected ray.

Now the change of phase produced by a factor of the form $1 + Qe^{\iota q}$, where Q is very small, is $\tan^{-1}\{Q \sin q/(1 + Q \cos q)\}$, and therefore, in circular measure, is approximately $Q \sin q$.

Hence, if for brevity we put

$$\cos i - \mathfrak{M} R^{-2} e^{-2\iota a} \equiv Y e^{\iota y}, \qquad \cos i + \mathfrak{M} R^{-2} e^{-2\iota a} \equiv Y' e^{\iota y'},$$

the acceleration of phase produced in the reflected ray by the component β_0 of magnetisation is, in circular measure,

$$C_0 . \beta_0 . 2c\lambda . \frac{\sin 2i}{R^4 . Y . Y'} \sin (x + 90° - y - y' - 4\alpha).$$

In ZEEMAN'S experiment the angle of incidence was 75° and the intensity of magnetisation was a little over 1100 C.G.S., the mirror being of iron. Under these circumstances he found the acceleration of phase to be ·003 × 90° with a mean error of ·001 × 90°.

Calculating the theoretical value, we notice that

$$Y = \cdot 2249, \qquad y = -(62^\circ\ 43'),$$
$$Y' = \cdot 4601, \qquad y' = 25^\circ\ 44',$$

and if we assume $x = 63^\circ$ (which is about the mean of the values indicated by the experiments of SISSINGH and ZEEMAN), then

$$x + 90^\circ - y - y' - 4\alpha = 360^\circ + 44^\circ\ 39';$$

we may also assume

$$\log_{10}(-C_0) = \overline{11}\cdot 8623.$$

With these values we find that the change of phase indicated by the theory is, in circular measure,

$$\cdot 003818,$$

or, in degrees,

$$\cdot 00243 \times 90^\circ.$$

This agrees very well with ZEEMAN'S observations.

Transmission through Metal Films.

30. Another effect of the action of magnetism on light is the rotation of the plane of polarisation of normally incident light, on passing through very thin films of magnetised metal. The principal experiments in this subject have been made by KUNDT, DU BOIS, LOBACH, and DRUDE; they found that the rotation is always in the direction of the magnetising current, and measured it in special cases. It is desirable to compare these measurements with the mathematical solution of the problem worked out on the basis of the present theory.

Let the film be bounded by the planes

$$z = 0 \qquad \text{and} \qquad z = -h,$$

and let the incident light fall normally on the surface $z = 0$. The external magnetic field is supposed to be parallel to the axis of z, so that $\alpha_0 = 0$ and $\beta_0 = 0$. The plane of polarisation of the incident light is taken as the plane of yz.

Thus we may assume the following expressions to represent the light in the air on the two sides of the film, and in the film itself.

In the air ($z > 0$)

$$u = A_0 e^{\iota(mz+pt)} + A e^{\iota(-mz+pt)},$$
$$v = \qquad B e^{\iota(-mz+pt)},$$
$$w = 0.$$

In the metal ($0 > z > -h$)

$$u = A_1 e^{\iota(m_1 z+pt)} + A'_1 e^{\iota(-m_1 z+pt)} + A_2 e^{\iota(m_2 z+pt)} + A'_2 e^{\iota(-m_2 z+pt)},$$
$$v = \iota A_1 e^{\iota(m_1 z+pt)} + \iota A'_1 e^{\iota(-m_1 z+pt)} - \iota A_2 e^{\iota(m_2 z+pt)} - \iota A'_2 e^{\iota(-m_2 z+pt)},$$
$$w = 0.$$

In the air ($z + h < 0$)

$$u = E e^{\iota(mz+pt)}, \qquad v = F e^{\iota(mz+pt)}, \qquad w = 0,$$

wherein the incident ray is represented by A_0, the reflected ray by (A, B), and the transmitted ray by (E, F).

It should be noticed that in these assumptions multiple reflections are not neglected; all waves in the film are included in the complex constants A_1, A'_1, A_2, and A'_2.

For surface conditions we may establish the continuity of

$$H(u - H\eta_3 v), \quad H d/dz\,(u - H\eta_3 v), \quad H(v + H\eta_3 u), \quad \text{and} \quad H d/dz\,(v + H\eta_3 u)$$

respectively; the second of these is the expression of the continuity of the magnetic force b, and is used instead of the continuity of w to which it is equivalent, as it leads to more symmetrical analysis.

At the surface $z = 0$ these boundary conditions lead to the equations

$$H(A_0 + A) = H'(A_1 + A'_1 + A_2 + A'_2) - H'^2 \iota\eta_3 (A_1 + A'_1 - A_2 - A'_2),$$

$$Hm(A_0 - A) = H'(m_1 A_1 - m_1 A'_1 + m_2 A_2 - m_2 A'_2)$$
$$\qquad - H'^2 \iota\eta_3 (m_1 A_1 - m_1 A'_1 - m_2 A_2 + m_2 A'_2),$$

$$-H\iota B = H'(A_1 + A'_1 - A_2 - A'_2) - H'^2 \iota\eta_3 (A_1 + A'_1 + A_2 + A'_2),$$

$$-Hm\iota B = H'(-m_1 A_1 + m_1 A'_1 + m_2 A_2 - m_2 A'_2)$$
$$\qquad + H'^2 \iota\eta_3 (m_1 A_1 - m_1 A'_1 + m_2 A_2 - m_2 A'_2).$$

At the surface $z = -h$, (if for brevity we write $-\iota m_1 h = \theta$, and $-\iota m_2 h = \phi$), the boundary conditions lead to the equations

$$\mathrm{HE}e^{-\iota mh} = \mathrm{H}'(\mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} + \mathrm{A}_2e^{\phi} + \mathrm{A}_2'e^{-\phi})$$
$$- \mathrm{H}'^2\iota\eta_3(\mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} - \mathrm{A}_2e^{\phi} - \mathrm{A}_2'e^{-\phi}),$$

$$\mathrm{H}m\mathrm{E}e^{-\iota mh} = \mathrm{H}'(m_1\mathrm{A}_1e^{\theta} - m_1\mathrm{A}'_1e^{-\theta} + m_2\mathrm{A}_2e^{\phi} - m_2\mathrm{A}_2'e^{-\phi})$$
$$- \mathrm{H}'^2\iota\eta_3(m_1\mathrm{A}_1e^{\theta} - m_1\mathrm{A}_1'e^{-\theta} - m_2\mathrm{A}_2e^{\phi} + m_2\mathrm{A}_2'e^{-\phi}),$$

$$- \mathrm{H}\iota\mathrm{F}e^{-\iota mh} = \mathrm{H}'(\mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} - \mathrm{A}_2e^{\phi} - \mathrm{A}'_2e^{-\phi})$$
$$- \mathrm{H}'^2\iota\eta_3(\mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} + \mathrm{A}_2e^{\phi} + \mathrm{A}_2'e^{-\phi}),$$

$$\mathrm{H}m\iota\mathrm{F}e^{-\iota mh} = \mathrm{H}'(- m_1\mathrm{A}_1e^{\theta} + m_1\mathrm{A}_1'e^{-\theta} + m_2\mathrm{A}_2e^{\phi} - m_2\mathrm{A}_2'e^{-\phi})$$
$$+ \mathrm{H}'^2\iota\eta_3(m_1\mathrm{A}_1e^{\theta} - m_1\mathrm{A}_1'e^{-\theta} + m_2\mathrm{A}_2e^{\phi} - m_2\mathrm{A}_2'e^{-\phi}).$$

If, for brevity, we put $\mathrm{H}'\iota\eta_3 \equiv t$, and notice from equations (25) that in the present problem

$$m_1/\mathrm{M} = 1 + \tfrac{1}{2}t, \qquad m_2/\mathrm{M} = 1 - \tfrac{1}{2}t,$$

we readily reduce the boundary conditions to the following:—

$$\frac{\mathrm{H}}{\mathrm{H}'}(\mathrm{A}_0 + \mathrm{A}) = \mathrm{A}_1 + \mathrm{A}_1' + \mathrm{A}_2 + \mathrm{A}_2' - t(\mathrm{A}_1 + \mathrm{A}_1' - \mathrm{A}_2 - \mathrm{A}_2')$$

$$\frac{\mathrm{H}}{\mathrm{H}'}\frac{m}{\mathrm{M}}(\mathrm{A}_0 - \mathrm{A}) = \mathrm{A}_1 - \mathrm{A}_1' + \mathrm{A}_2 - \mathrm{A}_2' - \tfrac{1}{2}t(\mathrm{A}_1 - \mathrm{A}_1' - \mathrm{A}_2 + \mathrm{A}_2')$$

$$- \frac{\mathrm{H}}{\mathrm{H}'}\iota\mathrm{B} = \mathrm{A}_1 + \mathrm{A}_1' - \mathrm{A}_2 - \mathrm{A}_2' - t(\mathrm{A}_1 + \mathrm{A}_1' + \mathrm{A}_2 + \mathrm{A}_2')$$

$$\frac{\mathrm{H}}{\mathrm{H}'}\frac{m}{\mathrm{M}}\iota\mathrm{B} = \mathrm{A}_1 - \mathrm{A}_1' - \mathrm{A}_2 + \mathrm{A}_2' - \tfrac{1}{2}t(\mathrm{A}_1 - \mathrm{A}_1' + \mathrm{A}_2 - \mathrm{A}_2'),$$

$$\frac{\mathrm{H}}{\mathrm{H}'}\mathrm{E}e^{-\iota mh} = \mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} + \mathrm{A}_2e^{\phi} + \mathrm{A}_2'e^{-\phi} - t(\mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} - \mathrm{A}_2e^{\phi} - \mathrm{A}_2'e^{-\phi})$$

$$\frac{\mathrm{H}}{\mathrm{H}'}\frac{m}{\mathrm{M}}\mathrm{E}e^{-\iota mh} = \mathrm{A}_1e^{\theta} - \mathrm{A}_1'e^{-\theta} + \mathrm{A}_2e^{\phi} - \mathrm{A}_2'e^{-\phi} - \tfrac{1}{2}t(\mathrm{A}_1e^{\theta} - \mathrm{A}_1'e^{-\theta} - \mathrm{A}_2e^{\phi} + \mathrm{A}_2'e^{-\phi})$$

$$- \frac{\mathrm{H}}{\mathrm{H}'}\iota\mathrm{F}e^{-\iota mh} = \mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} - \mathrm{A}_2e^{\phi} - \mathrm{A}_2'e^{-\phi} - t(\mathrm{A}_1e^{\theta} + \mathrm{A}_1'e^{-\theta} + \mathrm{A}_2e^{\phi} + \mathrm{A}_2'e^{-\phi})$$

$$- \frac{\mathrm{H}}{\mathrm{H}'}\frac{m}{\mathrm{M}}\iota\mathrm{F}e^{-\iota mh} = \mathrm{A}_1e^{\theta} - \mathrm{A}_1'e^{-\theta} - \mathrm{A}_2e^{\phi} + \mathrm{A}_2'e^{-\phi} - \tfrac{1}{2}t(\mathrm{A}_1e^{\theta} - \mathrm{A}_1'e^{-\theta} + \mathrm{A}_2e^{\phi} - \mathrm{A}_2'e^{-\phi}).$$

Eliminating B, E, and F from the last six, and representing m/M by x, we get

$$\left\{1 + x - t\left(\tfrac{1}{2} + x\right)\right\} A_1 - \left\{1 - x - t\left(\tfrac{1}{2} - x\right)\right\} A_1'$$
$$- \left\{1 + x + t\left(\tfrac{1}{2} + x\right)\right\} A_2 + \left\{1 - x + t\left(\tfrac{1}{2} - x\right)\right\} A_2' = 0,$$

$$\left\{1 - x - t\left(\tfrac{1}{2} - x\right)\right\} A_1 e^{\theta} - \left\{1 + x - t\left(\tfrac{1}{2} + x\right)\right\} A_1' e^{-\theta}$$
$$+ \left\{1 - x + t\left(\tfrac{1}{2} - x\right)\right\} A_2 e^{\phi} - \left\{1 + x + t\left(\tfrac{1}{2} + x\right)\right\} A_2' e^{-\phi} = 0,$$

$$\left\{1 - x - t\left(\tfrac{1}{2} - x\right)\right\} A_1 e^{\theta} - \left\{1 + x - t\left(\tfrac{1}{2} + x\right)\right\} A_1' e^{-\theta}$$
$$- \left\{1 - x + t\left(\tfrac{1}{2} - x\right)\right\} A_2 e^{\phi} + \left\{1 + x + t\left(\tfrac{1}{2} + x\right)\right\} A_2' e^{-\phi} = 0.$$

The last two equations give

$$\frac{A_1 e^{\theta}}{(1 + x) - t\left(\tfrac{1}{2} + x\right)} = \frac{A_1' e^{-\theta}}{(1 - x) - t\left(\tfrac{1}{2} - x\right)} = \xi \text{ (say)}$$
$$\frac{A_2 e^{\phi}}{(1 + x) + t\left(\tfrac{1}{2} + x\right)} = \frac{A_2' e^{-\phi}}{(1 - x) + t\left(\tfrac{1}{2} - x\right)} = \zeta \text{ (say)}$$

and substitution from these in the first leads to

$$\xi\left[e^{-\theta}\left\{(1 + x) - t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\theta}\left\{(1 - x) - t\left(\tfrac{1}{2} - x\right)\right\}^2\right]$$
$$= \zeta\left[e^{-\phi}\left\{(1 + x) + t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\phi}\left\{(1 - x) + t\left(\tfrac{1}{2} - x\right)\right\}^2\right],$$

whence,

$$\frac{A_1 e^{\theta}}{\left\{1 + x - t\left(\tfrac{1}{2} + x\right)\right\}\left[e^{-\phi}\left\{1 + x + t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\phi}\left\{1 - x + t\left(\tfrac{1}{2} - x\right)\right\}^2\right]}$$
$$= \frac{A_1' e^{-\theta}}{\left\{1 - x - t\left(\tfrac{1}{2} - x\right)\right\}\left[e^{-\phi}\left\{1 + x + t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\phi}\left\{1 - x + t\left(\tfrac{1}{2} - x\right)\right\}^2\right]}$$
$$= \frac{A_2 e^{\phi}}{\left\{1 + x + t\left(\tfrac{1}{2} + x\right)\right\}\left[e^{-\theta}\left\{1 + x - t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\theta}\left\{1 - x - t\left(\tfrac{1}{2} - x\right)\right\}^2\right]}$$
$$= \frac{A_2' e^{-\phi}}{\left\{1 - x + t\left(\tfrac{1}{2} - x\right)\right\}\left[e^{-\theta}\left\{1 + x - t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\theta}\left\{1 - x - t\left(\tfrac{1}{2} - x\right)\right\}^2\right]}.$$

Substituting from these in the sixth and eighth of the surface conditions, we get

$$-\iota\frac{F}{E} = \frac{X - Z}{X + Z}$$

where

$$X \equiv (1 - t)(2 - t)\left[e^{-\phi}\left\{1 + x + t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\phi}\left\{1 - x + t\left(\tfrac{1}{2} - x\right)\right\}^2\right]$$

and

$$Z = (1 + t)(2 + t)\left[e^{-\theta}\left\{1 + x - t\left(\tfrac{1}{2} + x\right)\right\}^2 - e^{\theta}\left\{1 - x - t\left(\tfrac{1}{2} - x\right)\right\}^2\right].$$

Now

$$\theta = -\iota m_1 h = -\iota \mathrm{M}h\,(1 + \tfrac{1}{2}t)$$

$$\phi = -\iota m_2 h = -\iota \mathrm{M}h\,(1 - \tfrac{1}{2}t)$$

and therefore, if the modulus of $\iota \mathrm{M}h$ be not exceedingly great,

$$e^{\theta} = e^{-\iota\mathrm{M}h}\,(1 - \tfrac{1}{2}t\,.\,\iota\mathrm{M}h) \qquad e^{-\theta} = e^{\iota\mathrm{M}h}\,(1 + \tfrac{1}{2}t\,.\,\iota\mathrm{M}h)$$

$$e^{\phi} = e^{-\iota\mathrm{M}h}\,(1 + \tfrac{1}{2}t\,.\,\iota\mathrm{M}h) \qquad e^{-\phi} = e^{\iota\mathrm{M}h}\,(1 - \tfrac{1}{2}t\,.\,\iota\mathrm{M}h).$$

Substituting these expressions, we readily find that, to the first order in t,

$$\begin{aligned}\mathrm{X} = e^{\iota\mathrm{M}h}\,(1+x)\left\{2(1+x) - t(1-x) - t\,.\,\iota\mathrm{M}h\,(1+x)\right\}\\ - e^{-\iota\mathrm{M}h}\,(1-x)\left\{2(1-x) - t(1+x) + t\,.\,\iota\mathrm{M}h\,(1-x)\right\},\end{aligned}$$

$$\begin{aligned}\mathrm{Z} = e^{\iota\mathrm{M}h}\,(1+x)\left\{2(1+x) + t(1-x) + t\,.\,\iota\mathrm{M}h\,(1+x)\right\}\\ - e^{-\iota\mathrm{M}h}\,(1-x)\left\{2(1-x) + t(1+x) - t\,.\,\iota\mathrm{M}h\,(1-x)\right\},\end{aligned}$$

and so

$$\iota\frac{\mathrm{F}}{\mathrm{E}} = \frac{t\left[e^{\iota\mathrm{M}h}\,(1+x)\left\{1 - x + \iota\mathrm{M}h\,(1+x)\right\} - e^{-\iota\mathrm{M}h}\,(1-x)\left\{1 + x - \iota\mathrm{M}h\,(1-x)\right\}\right]}{2\left\{e^{\iota\mathrm{M}h}\,(1+x)^2 - e^{-\iota\mathrm{M}h}\,(1-x)^2\right\}}$$

whence

$$\frac{\mathrm{F}}{\mathrm{E}} = \tfrac{1}{2}\mathrm{H}'\eta_3\,\frac{\left[\left\{\dfrac{1-x}{1+x} + \iota\mathrm{M}h\right\} - \left\{\dfrac{1-x}{1+x} - \left(\dfrac{1-x}{1+x}\right)^2\iota\mathrm{M}h\right\}e^{-2\iota\mathrm{M}h}\right]}{\left[1 - \left(\dfrac{1-x}{1+x}\right)^2 e^{-2\iota\mathrm{M}h}\right]},$$

and if θ be the angle through which the major axis of the ellipse of polarisation of the transmitted light is rotated from the axis of x towards the axis of y, θ is the real part of F/E. For a given metal, and given values of h and λ, this angle can be calculated exactly from the above formula, but such calculation would be very tedious. It is well therefore to examine the relative magnitudes of the different terms in cases corresponding to the known experiments on this subject, in order to see whether there is any approximate formula of a simpler character. It will be sufficient to consider some of the experiments on transmission through films of *iron* described by LOBACH ('Wied. Ann.' vol. 39, p. 356) and by DRUDE ('Wied. Ann.' vol. 46, p. 416).

Now

$$\frac{1-x}{1+x} = \frac{\mathrm{R} - \cos\alpha + \iota\sin\alpha}{\mathrm{R} + \cos\alpha - \iota\sin\alpha}$$

$$= \left(\frac{\mathrm{R}^2 + 1 - 2\cos\alpha}{\mathrm{R}^2 + 1 + 2\cos\alpha}\right)^{\frac{1}{2}} e^{\iota\tan^{-1}\{2\mathrm{R}\sin\alpha/(\mathrm{R}^2-1)\}}$$

so that for iron

$$(1 - x)/(1 + x) = (\cdot 7500)\, e^{-\iota(23^\circ\, 21')}$$
$$= (\cdot 6887) - \iota(\cdot 2973).$$

Also

$$-\,2\iota \mathrm{M}h = -\,2\iota(\cos\alpha + \iota\sin\alpha)\,\mathrm{R}mh$$
$$= (\sin\alpha - \iota\cos\alpha)\,\mathrm{R}h\,(4\pi/\lambda)$$
$$= -\,4\pi\,\{(3\cdot 21) + \iota(2\cdot 36)\}\,h/\lambda.$$

In DRUDE's experiments the values of h/λ lie between ·065 and ·332.

In LOBACH's experiments the values of h/λ lie between ·042 and ·167.

Hence, in the two sets of experiments, the greatest value of the modulus of $e^{-2\iota \mathrm{M}h}$ is about ·1838, and therefore the greatest value of the modulus of $\left(\frac{1 - x}{1 + x}\right)^2 e^{-2\iota \mathrm{M}h}$ is about ·1035 ; this corresponds to the thinnest film, for the thicker films the modulus is very much smaller.

Hence if we neglect the fourth term in the numerator and the second term in the denominator, and put

$$\mathrm{F}/\mathrm{E} = \tfrac{1}{2}\mathrm{H}'\eta_3\Big[\{(1 - x)/(1 + x)\}\,\{1 - e^{-2\iota \mathrm{M}h}\} + \iota\,.\,\mathrm{M}h\Big]$$

we have an approximate formula whose error, for the very thinnest film considered, will not exceed about 10 per cent., and is very much smaller for the large majority of the experiments.

Putting in numerical values, this becomes (for iron, and sodium light),

$$\mathrm{F}/\mathrm{E} = -\,\iota\,.\,c\lambda \mathrm{C}_0\gamma_0 e^{\iota\omega}\mathrm{R}^{-2}e^{-2\iota a}\Big[\Big\{(3\cdot 21) + \iota(2\cdot 36)\Big\}2\pi h/\lambda$$
$$+ (\cdot 7500)\, e^{-\iota(23^\circ\, 21')}\Big\{1 - e^{-\{(3\cdot 21) + \iota(2\cdot 36)\}4\pi h/\lambda}\Big\}\Big].$$

In the experiments the film is generally magnetised as strongly as possible, but there is no direct way of ascertaining the intensity of magnetisation attained. Thus γ_0 is to a certain extent indeterminate. According to EWING the maximum intensity of magnetisation for some specimens of iron is about 1730 C.G.S. units. I shall therefore assume $\gamma_0 = 1730$; also I take C_0 as determined by $\log_{10}(-\,\mathrm{C}_0) = \overline{11}\cdot 8623$.

In one of LOBACH's experiments the thickness of the film is given by

$$h = 82 \times 10^{-7} \text{ centim.}$$

The light used is sodium light, so that $\lambda = 5896 \times 10^{-8}$ centim. ; and the rotation (θ) in the direction of the magnetising current is observed to be 1·62 degrees.

To compare this with the rotation indicated by theory, we notice that

$$h/\lambda = \cdot 1391, \quad \text{and} \quad 2\pi h/\lambda = \cdot 8740.$$

So that

$$F/E = -\iota . c\lambda C_0 \gamma_0 e^{\iota x} R^{-2} e^{-2\iota a} \left[(2\cdot 805) + \iota (2\cdot 063) + (\cdot 7500)\, e^{-\iota (23^\circ\, 21')} \right]$$

(the other term being in this case so small that it may be neglected)

$$= -\iota . c\lambda C_0 \gamma_0 e^{\iota x} R^{-2} e^{-2\iota a} (3\cdot 934)\, e^{\iota (27^\circ\, 21')}$$

$$= c\lambda C_0 \gamma_0 \frac{3\cdot 934}{15\cdot 87}\, e^{\iota \{x - 2a - 90^\circ + 27^\circ\, 21'\}}$$

$$= c\lambda C_0 \gamma_0 \frac{3\cdot 934}{15\cdot 87}\, e^{\iota (107^\circ\, 41')}$$

x being taken as 63°.

The angle θ is the real part of this complex, and is therefore ·01677 in circular measure, being positive when γ_0 is positive, so that it is a rotation in the direction of the magnetising current. This theoretical value of θ in degrees is ·961. As we have seen, the observed value in degrees is 1·62; in comparing these results it should be noticed that one factor of the theoretical value of θ is the cosine of an angle which is just about 17° greater than a right angle; this angle contains x, whose value we have had to guess; a comparatively small error in the value assigned to x will therefore make a considerable error in the calculated value of θ. The values of C_0 and γ_0 being also uncertain, the agreement of the theory with experiment may be regarded as good.

In one of DRUDE's experiments $h/\lambda = \cdot 332$, and the light used is red; the observed rotation is 4·25 degrees. If we substitute this value of h/λ in the above-obtained approximate formula, we find

$$F/E = -\iota . c\lambda C_0 \gamma_0\, e^{\iota x} R^{-2} e^{-2\iota a} (8\cdot 715)\, e^{\iota (32^\circ\, 3')}$$

$$= c\lambda C_0 \gamma_0 \frac{8\cdot 715}{15\cdot 87}\, e^{\iota (112^\circ\, 23')}$$

whence $\theta = \cdot 05187$ in circular measure, or 2·972 degrees.

In this case, in addition to the possible causes of error referred to in connexion with the previous experiment, it is to be noticed that though the experiment was made with red light, it has been necessary in the calculation to use the values of C_0, x, R, and a for yellow light, for lack of information as to their values in the case of red light. When also it is borne in mind that the value of the magneto-optic constant derived from observation of reflection from mirrors has here been applied to test experiments on transmission through thin films, with results not only of the same order of magnitude but identical within the limits of uncertainty of the intensity of magnetisation, the agreement must be considered as a very satisfactory vindication both of the theory and of the experiments.

Conclusion.

31. The various results obtained in this paper do not, I think, require any detailed comment. They may be fairly claimed to shew a remarkably good agreement between theory and experiment, a better agreement, I believe, than is shewn in the papers of GOLDHAMMER and DRUDE. The only considerable discrepancy arises in connexion with the original KERR experiments; but here it is to be remembered that the experiments of KERR, and those of SISSINGH and ZEEMAN, are not measurements of different phenomena, but are different ways of measuring the same phenomenon. Hence any theory that agrees with one of these sorts of experiments ought to agree equally well with the other sort; and if this is found not to be the case it is probably not the fault of the theory, but must be attributed to inaccuracy in one of the sets of experimental results. Thus it would appear that the original experiments of KERR, who was the pioneer in this subject, have been quantitatively much improved on by later investigations.

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VI. *On the Occlusion of Oxygen and Hydrogen by Platinum Black.*—Part II.

By Ludwig Mond, *Ph.D., F.R.S.*, William Ramsay, *Ph.D., F.R.S.*, *and* John Shields, *D.Sc., Ph.D.*

Received July 21, 1897.

Contents.

I. *Introduction.*

In a former paper ('Phil. Trans.,' A, 1895, vol. 186, p. 657–693) we gave an account of some experiments on the occlusion of oxygen and hydrogen by platinum black, and pointed out that freshly-made platinum black invariably contains a considerable quantity of oxygen. Most of the specimens which we examined contained approximately 100 volumes, or 0·66 per cent. of oxygen, which could only be completely removed by heating *in vacuo* at a dull red heat, and although a large fraction of the total oxygen can be extracted at about 400° C., the substance which remains behind is no longer platinum black but platinum sponge which has a much lower absorptive power for gases.

When hydrogen is admitted to platinum black containing x volumes of oxygen, $2x$ volumes are converted into water, and the remainder of the gas absorbed, which, in general, is about 100 volumes, is really occluded. The heat evolved on the occlusion of hydrogen by platinum black has been measured by Berthelot ('Ann. de Chim. et de Phys.,' 1883, vol. 30, p. 519), and by Favre ('Comptes Rend.,' vol. 77, p. 649, and vol. 78, p. 1257), but we have already (*loc. cit.*, p. 693) expressed ourselves as dissatisfied with the results they obtained, since the heat due to the combination of the oxygen pre-existing in the platinum black with the hydrogen is also included in these measurements.

During our attempts to solve the problem whether the occlusion of gases by metals is a chemical or physical phenomenon, we have investigated the thermal changes which take place on the occlusion of hydrogen and oxygen by platinum black. Independent of the object for which the investigation was undertaken, the results we have obtained, which we now beg to lay before the Society, are of interest in connection with many electrical experiments where platinum or platinised electrodes are employed. The present communication also contains an account of the behaviour of several other gases towards platinum black, together with some speculations regarding the occlusion of oxygen.

II. *The Heat of Occlusion of Hydrogen by Platinum Black.*

In attempting to determine the heat evolved on the occlusion of hydrogen by platinum black several courses were open to us. The first of these, namely, the preparation of pure platinum black, and the subsequent treatment of this with hydrogen in the calorimeter, had to be abandoned, as we have up to the present been unable to obtain this substance free from occluded gases.

A second conceivable method would be to treat platinum black as we find it, that is to say, platinum black containing oxygen, small quantities of carbon dioxide, and usually traces of other substances,* with hydrogen, and to make a correction for the heat evolved due to the formation of water. This method, however, has two disadvantages; firstly, the correction for the heat evolved on the formation of water is much greater than the constant to be measured, and secondly, it is very difficult to estimate exactly the quantity of oxygen in the sample experimented upon. Any attempts to remove the oxygen by heating *in vacuo* resulted in the formation of platinum sponge, which occludes relatively only small quantities of hydrogen.

As will be seen in Section III., chemical methods for removing the oxygen without introducing other deleterious substances also proved unsuccessful, and hence we were reduced to the third alternative. This consisted in fully charging up the platinum black with hydrogen at atmospheric pressure, removing as much as possible of the hydrogen by means of the pump, at as high a temperature as the platinum black could safely stand (184° C.) without being converted into sponge, and finally charging up fully again with hydrogen in the calorimeter. In this way the hydrogen converts all the oxygen initially present in the platinum black into water (the bulk of which is subsequently removed), and also exerts its full influence on oxides of nitrogen or other impurities before the heat evolved on the true occlusion of hydrogen is measured in the calorimeter.

It seems to us that the only objection which can be raised to this method of

* The chief of these are oxides of nitrogen, derived from nitroso compounds formed on the solution of platinum in *aqua regia*, and which cannot be destroyed even on repeatedly evaporating the solution successively with water and hydrochloric acid.

determining the heat of occlusion is in connexion with a statement of BERTHELOT'S,* that two definite compounds of platinum and hydrogen exist, viz., $Pt_{30}H_2$ and $Pt_{30}H_3$, corresponding to the amount of hydrogen which can be extracted from platinum black *in vacuo* at two different temperatures and to two different heats of occlusion. FAVRE,† also, found on admitting hydrogen fractionally, in small portions at a time, that the heat evolved per gram of hydrogen occluded became less and less. As our results will show, the heat evolved per gram of hydrogen occluded for the fraction of hydrogen which can be removed at the ordinary temperature, by means of the pump, from platinum fully charged with hydrogen at atmospheric pressure, is the same as for the portion which can be extracted *in vacuo* at 184° C., and consequently we believe that the results obtained by BERTHELOT and by FAVRE were due to the fact that the samples of platinum black examined by them contained oxygen.

Having stated in general terms the method adopted by us for the determination of the heat of occlusion of hydrogen by platinum black, we will now describe the apparatus employed.

During the first part of this investigation we found that although the bulk of the hydrogen or oxygen occluded by platinum black was absorbed almost immediately, a slow absorption went on for hours. This being the case, we found it better to make use of a Bunsen's ice calorimeter instead of an ordinary water calorimeter, which gives the best results when the reaction is nearly instantaneous. We are of opinion that the ice calorimeter has been too often neglected on account of the difficulty of obtaining pure snow; but, by adopting the device suggested by Professor C. V. BOYS,‡ we have been able to obtain very satisfactory results. The calorimeter itself, L, which was made sufficiently large to accommodate the experimental tube D, was surrounded by an air-jacket, J, as shown in fig. 1, and the whole suspended inside three concentric cylinders. The cylinders themselves, with their drain-pipes, were sunk in a large cubical wooden box, the space between the outer cylinder and the box being packed with cotton wool. Pounded Norwegian ice was placed inside the cylinders and heaped up outside, so that the whole of the projecting part of the experimental tube was covered with melting ice. The function of the air-jacket is to diminish the too rapid transference of heat between the calorimeter, which should be exactly at 0° C., and the melting ice, which, since it is more or less impure, has always a lower melting-point than pure ice. The calorimeter was filled in the usual way with boiled-out distilled water, and, after some mercury had been introduced into the lower part, the thistle funnel, tap, and side capillary tube were sealed on. In order to produce the sheath of ice on the outside of the inner tube of the calorimeter, the whole was cooled down to 0° C., and some solid carbon dioxide introduced. This produced intense local undercooling, and consequently some ice crystals

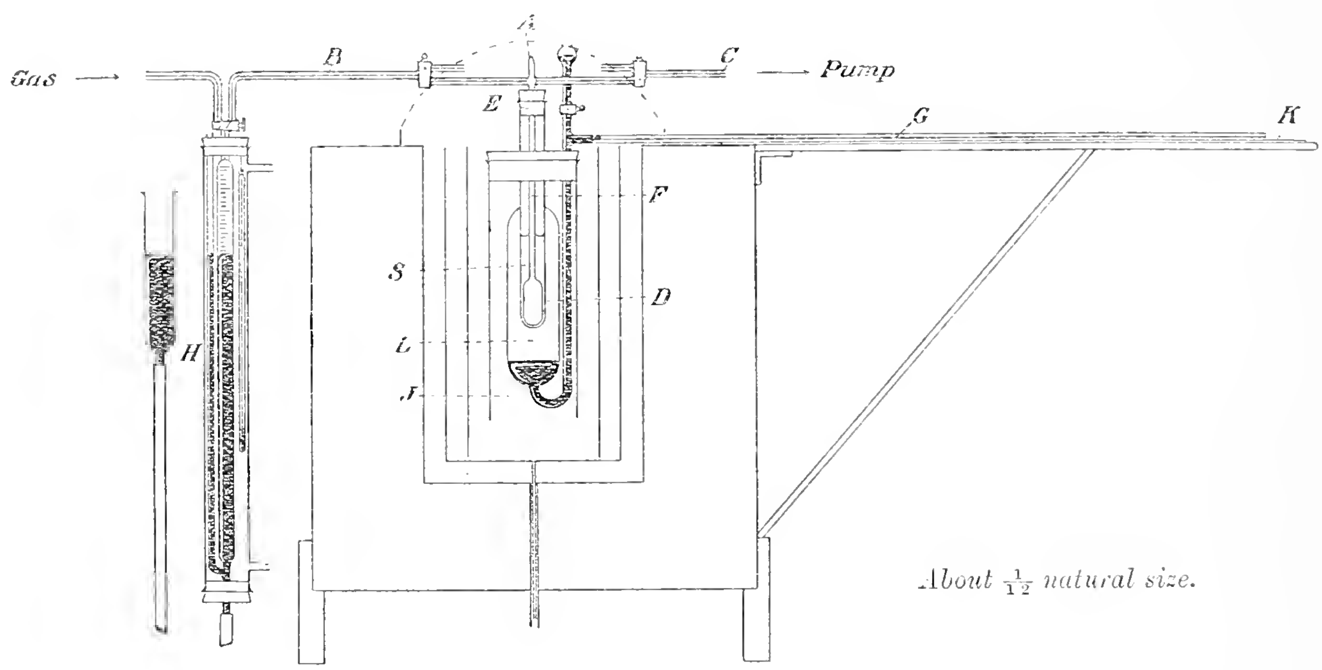
* BERTHELOT, *loc. cit.*

† FAVRE, *loc. cit.*

‡ C. V. BOYS, 'Phil. Mag.,' 1887, vol. 24, p. 214.

separated out.* The ice sheath was then made to grow to the required size by pouring ether into the inner tube and blowing a current of air through it. Lastly, sufficient salt solution, S, cooled down to zero, was introduced into the inner tube, so that when the experimental tube was inserted, the whole of the bulb and a considerable portion of the stem were covered with the solution; and care was also taken that the surface of the salt solution was 3 or 4 centims. below the top of the ice-sheath, so that most of the heat radiating from it would be caught in the sheath. The amount of mercury sucked into or expelled from the apparatus was preferably determined by the deflection of the meniscus in the capillary tube, rather than by weighing, since the progress of the reaction could easily be seen at any instant. No errors due to the "sticktion" of the mercury need be feared if the tube is tapped

Fig. 1.



occasionally. The capillary tube was one especially selected from a large number. For over a metre of its length its mean capacity was 0·0001196 cub. centim. per millim., and the deviations from the mean value at ten points along its length were respectively + 9, + 17, + 6, − 1, + 4, − 11, − 16, − 1, + 1, − 7, units in the last significant figure. In translating the deflection of the mercury meniscus in millims. into heat units, we have accepted DIETERICI's† value, that 1 gram calorie corresponds to the displacement of 0·01544 gram of mercury; and hence a deflection of 1 millim. in the tube we employed represents 0·1053 of a gram calorie, or 0·001053 of a hundred-gram calorie, denoted by K in this paper.

Before proceeding to use the calorimeter for our experiments, the specific heats of

* If this precaution is not taken, the water may be cooled several degrees below 0° C., and, when once ice begins to form, it does so so suddenly that the apparatus is liable to burst.

† DIETERICI, 'Wied. Ann.,' 1889, vol. 37, p. 499.

pure lead and zinc were determined, to see that everything was in good working order. The values we found were as follows :—

Specific heat of lead	*0·0299*	between	0°	and	38°0 C.	
Spring found	0·0305	,,	17°	,,	108°	,,
	0·03195	,,	13°	,,	197°	,,
	0·03437	,,	16°	,,	292°	,,

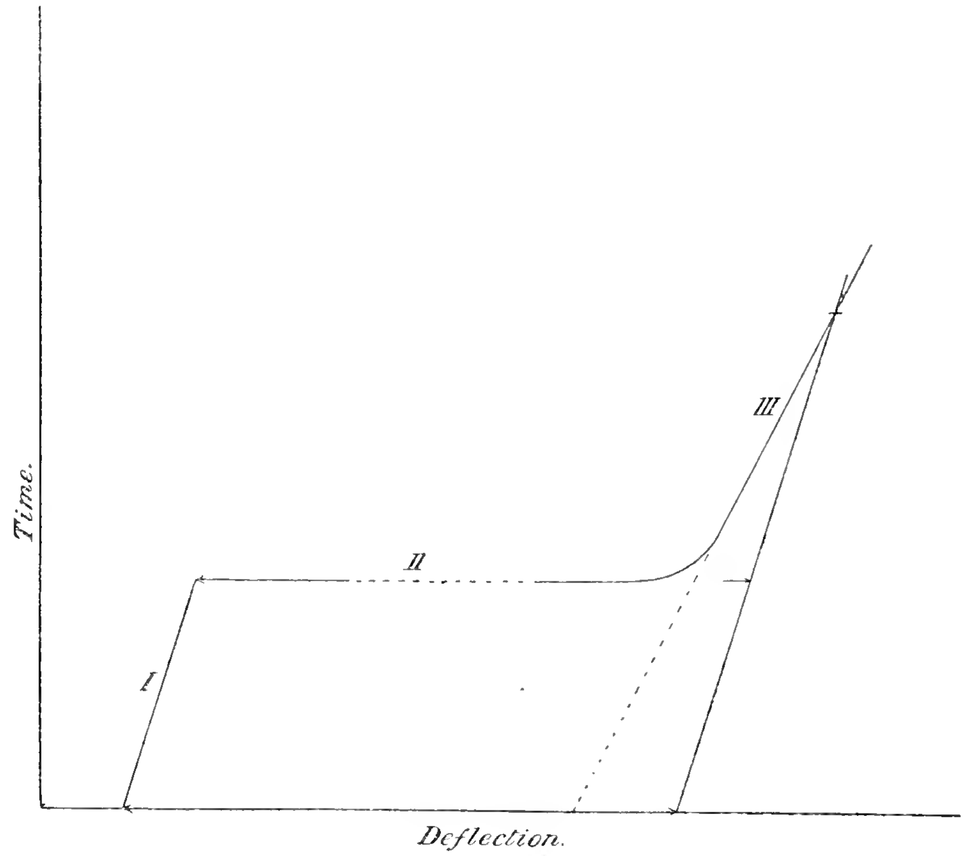
Specific heat of zinc	*0·09312*	between 0° and 15°·8 C.
Kopp found	0·0932	Temperature interval not stated.
Bunsen found	0·0935	
Schüller and Wartha found	0·0939	

The numbers we find are lower than those recorded by other observers, but this, at least in the case of lead, is as it should be, since the determination was made at a lower temperature.

Having satisfied ourselves that the instrument gave correct results, an experimental tube of the form shown in fig. 1 was made. It consisted of a bulb, D, to contain the platinum black, which along with a portion of the stem was immersed in the salt solution contained in the inner tube of the calorimeter. Communication between the bulb and the system of taps and capillary tubes which projected above the calorimeter was made by means of a tube F, just sufficiently wide to admit of the introduction of the platinum black. When the experimental tube was in position, the mouth of the inner tube of the calorimeter was closed by the india-rubber stopper E. After a known weight of platinum black had been introduced, and before the apparatus was finally fixed in position in the calorimeter, the tube F was sealed off at the point A. The tube B was now connected with a gas burette whilst C was placed in communication with the pump, and the capacity of the tube determined by filling with dry air at a known temperature and pressure, exhausting and remeasuring at the same temperature and pressure.

The platinum black was now fully charged up by admitting pure hydrogen under atmospheric pressure. After standing for a day, the tube was jacketed with aniline vapour at 184°, and the excess of hydrogen filling the apparatus, together with the hydrogen and water which can be removed *in vacuo* at the ordinary temperature, and that which is given off at 184° C., extracted by means of the pump. Both taps being now shut, the experimental tube in a vacuous state was cut off, cooled to 0°, and introduced into the calorimeter. In this position it was again connected up with the hydrogen apparatus and the pump, and when equilibrium had set in, hydrogen was admitted, from a gas burette, and the deflection of the mercury meniscus in the capillary side tube, G, noted from time to time until equilibrium was presumably again established. As a rule the position of the meniscus in the capillary tube never remains stationary, whether an experiment is in progress or not, in consequence of the very slow melting of ice or freezing of water inside the calorimeter. When the ice is first produced in the calorimeter the whole apparatus must stand for several days surrounded by ice before equilibrium between the ice and water inside is established. Owing to adventitious circumstances, atmospheric pressure, etc., the normal creepage of the meniscus sometimes amounted to several millims. an hour, but experiments were never conducted until this had diminished to one or two millims., whilst in most of the experiments the normal creepage was less than one millim. per hour.

Fig. 2.



After admitting hydrogen the normal creepage was never the same, as, even after three or four hours, hydrogen continues to be slowly absorbed. This difficulty was overcome in the following way:—After the last reading of the deflection was taken, the volume of hydrogen used was immediately noted, and by subtracting what was required to fill the experimental tube at 0° C. (the top of the tube and the taps being covered with melting ice) the quantity of hydrogen occluded up to the time of the last reading of the deflection was found. The final occlusion of hydrogen was of course very slow, and the real calorimeter deflection was obtained by plotting the readings in a co-ordinate system as shown in fig. 2.

The first part of the curve I, represents the normal creepage before the admission of hydrogen. The horizontal part II, a large portion of which is omitted, shows the chief deflection due to the main occlusion of the hydrogen during the first few minutes (10–15 min.). The curve then bends upwards, III, but never becomes quite parallel with the first part owing to the very slow absorption going on. Instead of drawing the tangent to part III. of the curve, which would obviously be wrong, a closer approximation to the true deflection can be obtained by drawing a parallel to part I. of the curve through the last point of observation, at which point the volume of occluded hydrogen was determined.

Blank experiments showed that admission of the relatively small quantity of gas from the burette to an empty experimental tube produced no appreciable deflection.

Several preliminary experiments were made, using small quantities of platinum black, but the results of these were unsatisfactory on account of the difficulty of measuring the deflection and the amount of hydrogen occluded accurately enough. To obviate this difficulty, experiments were made on a larger scale, and with as much platinum black as could conveniently be introduced into the experimental tube. The results of these experiments are given in the following Table I., the experimental tube being removed from the calorimeter, re-exhausted at 184° C., and replaced in the calorimeter again before admitting hydrogen the second time.

TABLE I.

Experiment.	Platinum black used.	Hydrogen occluded.		Heat evolved.		Heat evolved per gram of hydrogen occluded.
				Deflection.	Heat in calories. 1 K = 100 cal.	
	grams.	cub. centims.	grams.	millims.	K.	K.
I.	9·744	8·34	0·000751	49·6	0·05223	69·6
II.	9·744	7·38	0·000664	43·6	0·04591	69·1

TABLE II.

Experiment.	Platinum black used.	Hydrogen occluded.		Heat evolved.		Heat evolved per gram of hydrogen occluded.
				Deflection.	Heat in calories. 1 K = 100 cal.	
	grams.	cub. centims.	grams.	millims.	K.	K.
I*a*.	9·744	−2·51	−0·000226	−14·1	−0·01485	−65·7*
I*b*.	9·744	2·13	0·000192	11·6	0·01221	63·7
III.	1·9193	1·90	0·000171	11·1	0·01169	68·3

The mean of the two results is an evolution of 69·4 K per gram of hydrogen occluded. This value, however, may represent the sum of two other values, one of which relates to the heat evolved on the occlusion of the hydrogen, which can be

* Heat *absorbed* per gram of hydrogen *removed*.

extracted from fully charged platinum black *in vacuo* at the ordinary temperature, whilst the other corresponds to the occlusion of the hydrogen, which can be removed from the platinum black *in vacuo* by raising its temperature from the ordinary temperature up to 184° C. In order to test the question whether the occlusion of different fractions of hydrogen corresponds to different heat changes, as has been stated by FAVRE and BERTHELOT, some additional experiments were made, the results of which are given in tabular form in Table II.

When Experiment I. was over, the platinum black remained fully charged with hydrogen at 0° C. Before removing the experimental tube from the calorimeter for the purpose of re-exhausting it at 184°, an attempt was made to measure the heat *absorbed* on removing as much of the hydrogen as could be pumped off at 0° C., with the results given under Experiment I*a*. As will be seen from the table, 2·51 cub. centims. of hydrogen were removed. In Experiment I*b*. the platinum black was again fully charged up with hydrogen. The results of these two experiments, namely, 65·7 K *absorbed* per gram of hydrogen *removed*, and 63·7 K evolved per gram of hydrogen occluded, are sufficiently close to each other and to the mean value 69·4 K, considering the probable errors which will be discussed immediately, to show that there is no difference between the thermal changes which take place when the two different fractions of hydrogen are occluded. In Experiment III. a small quantity of platinum black was employed, and it was exhausted at 230° C. instead of at 184° C. before being placed in the calorimeter. Approximately, the same value was obtained for the heat evolved per gram of hydrogen occluded.

The probable accuracy of the results may be estimated from the following considerations. The hydrogen was measured in a calibrated burette, H, divided into tenths of a cubic centimetre, which was read to one-hundredths of a cubic centimetre. The volume of hydrogen occluded (always reduced to 0° C. and 760 millims.), given in the foregoing tables, represents the difference between the total hydrogen measured and that required to fill the experimental tube. The measurements are also liable to slight errors involved in reading temperature and pressure for the reduction of the gas volumes to standard conditions. The burette could be read with certainty to one-fiftieth of a cub. centim., but if we allow the error from all sources to be one-twentieth of a cub. centim., this would mean in Experiments I. and II. a possible error of about $\frac{3}{4}$ per cent.; whilst in I*a*., I*b*., and III. the error might amount to $2\frac{1}{2}$ per cent. The calorimeter deflection, which was observed on a scale etched on a plate-glass mirror, K, lying underneath the capillary tube, could be read accurately to 0·1 of a millim.; but, if we admit a total error of 0·5 of a millim. for plotting the curve and for any change of normal creepage during the experiment, then the error involved in the heat measurement may approach 1 per cent. in Experiments I. and II., and 5 per cent. in Experiments I*a*., I*b*., and III.

The total error in the most unfavourable circumstances in the first two experiments may thus amount to about one and a half per cent., whilst in the last three it might exceed seven per cent.

If we take the mean of Experiments I., II., and III., and also include another independent result, 68·2 K, which was obtained incidentally during the determination of the heat evolved on the occlusion of oxygen by platinum black,* we find as the general mean value for the heat evolved per gram of hydrogen occluded, 68·8 K, or 137·6 K per gram-molecule; and we think that, with the liberal allowances we have made for experimental error, this number may be taken as correct within one or two per cent.

According to BERTHELOT the hydrogen which cannot be pumped off at the ordinary temperature from platinum black charged with hydrogen forms the compound $Pt_{30}H_2$, whilst that which does come off is obtained by the dissociation of $Pt_{30}H_3$ into the first compound and hydrogen.

The heats of formation of these hypothetical compounds are + 339 K for the first, and + 426 K for the second; that is to say, + 170 K are evolved per gram of hydrogen occluded in the first instance, and 87 K per gram in the second. From the results which we have obtained it follows that the arguments put forward by BERTHELOT in favour of the existence of these compounds cannot be justified. The second value for the heat of occlusion of hydrogen, which is only half the first, is still much higher than the real value, 68·8 K, and we can only account for these different and high numbers found by BERTHELOT on the assumption that the platinum black employed by him contained oxygen. If reference is made to Table III.† it will be found that when the platinum black contained oxygen, the following numbers were found by us for the heat evolved per gram of hydrogen *absorbed*: 203, 195, 183, 173, and 163 K, which are of the same order of magnitude as that given by BERTHELOT for the supposed heat of occlusion of hydrogen in the first compound, viz., 170 K.

FAVRE has attempted to distinguish between the heats evolved on the occlusion of hydrogen by platinum and by palladium, inasmuch as when hydrogen is admitted in small portions at a time to palladium, the heat of occlusion remains constant; whilst in the case of platinum the heat evolved becomes gradually less and less. This difference, which however is only apparent, is also exemplified in Table III., and it is only necessary to point out that it is the result of adding hydrogen to platinum charged with oxygen, and therefore, in this respect, the supposed difference between the behaviour of platinum and palladium to hydrogen does not exist.

* Page 145, Table III, Operation 15.

† Page 145.

III. *On some Attempts to remove the Oxygen from Platinum Black without destroying its Occlusive Power* (*Occlusion* SO_2, CO, NH_3, *&c.*).

As far as we know the thermal change which takes place when oxygen is occluded by platinum black has never been measured. In 1883 the attempts which BERTHELOT made in this direction proved fruitless. We have already pointed out that platinum black, as usually prepared, invariably contains oxygen which cannot be removed by heating *in vacuo* without destroying the black and converting it into sponge, and consequently we tried a large number of experiments, having for their ultimate object the removal of the oxygen from platinum black, or, in other words, the preparation of a sample which *per se* at 0° C. would occlude oxygen directly.

All our attempts to prepare such a specimen have hitherto been unsuccessful, but some of them which are interesting in themselves may be briefly recorded here.

Sulphur dioxide.—One of the first of these was to treat platinum black with sulphur dioxide, in the hope that the sulphur trioxide formed, along with any occluded sulphur dioxide, might be completely extracted *in vacuo* at a temperature which would not impair the absorptive power of the platinum black for oxygen.

It was found, however, that although practically all the oxygen was removed as SO_3 in this way, the platinum black itself was charged with 84·2 volumes of sulphur dioxide. Of this occluded sulphur dioxide, which for the purpose we had in view was quite as objectionable as oxygen, only about one-fifth, or 15 volumes, could be removed *in vacuo* at the ordinary temperature; whilst for its complete removal ignition at a red heat, and consequently the conversion of the black into sponge, was necessary.

From the fact that the platinum black originally contained about 79·5 volumes of oxygen, and that 84·2 volumes of SO_2 were subsequently occluded, it may be inferred that platinum black occludes approximately the same number of volumes of both gases.

Carbon monoxide.—As we already know from former experiments, carbon dioxide may be readily removed from platinum black at temperatures which do not seriously affect its absorptive power. Attempts were therefore made to convert the oxygen contained in platinum black into carbon dioxide by the admission of carbon monoxide. Experiments showed that the bulk of the oxygen could be easily removed in this way, but on exhausting the tube it was found that carbon monoxide was itself occluded by the platinum black, and could only be removed by heating to redness in a vacuum.

In two experiments a sample of platinum black which contained initially about 90 volumes of oxygen was found to have occluded 95·7 and 93·5 volumes respectively of carbon monoxide.

Formic acid.—The method we have generally adopted for the preparation of platinum black has been by the reduction of sodium platinichloride by sodium formate, and it might be expected that the oxygen contained in platinum black could be removed by treatment with formic acid.

In the first of two experiments which were performed, the details of which need not be given, the platinum black was treated with the vapour of formic acid; whilst in the second, it was warmed with a dilute solution of formic acid in the experimental tube. In the latter case the excess of water and formic acid was evaporated off *in vacuo*, suitable absorbing agents having been introduced between the pump and experimental tube, and the residual substance dried at 100° C. *in vacuo* before exhausting at a higher temperature.

On gradually heating the platinum black obtained after treatment with (A), the vapour of formic acid, and (B), an aqueous solution of formic acid, gas was continuously given off *in vacuo* until a dull red heat had been maintained for some time. In both cases the gas pumped off was found to be a mixture of carbon monoxide and hydrogen, as shown in the following table.

	A. Platinum black treated with the vapour of formic acid.	B. Platinum black treated with dilute aqueous formic acid.
	vols.	vols.
CO	88·3	86·8
H_2	27·5	38·4
Total.	115·8	125·2

Hitherto we were inclined to ascribe the presence of oxygen in platinum black to the fact that it was washed and dried at 100° in the presence of air, since platinum black, when heated in an atmosphere of oxygen, absorbs this gas until the temperature reaches 360°–380° C. If freshly reduced platinum black contains either or both of these gases occluded in it, then it is easy to account for the presence of oxygen in the substance we actually obtain; for, on coming into contact with the air, both of these substances would be immediately burnt out and oxygen would take their place.

As a last resource for the preparation of pure platinum black, free from oxygen, we originally intended, if all else failed, to attempt to wash and dry it out of contact with the air; but these experiments show that even if we did succeed in keeping out oxygen, we might, it is true, obtain platinum black free from oxygen, but it would on the other hand be equally valueless for our purpose, since it would in all probability contain carbon monoxide and hydrogen.

Methyl alcohol, ammonia, &c.—Platinum black submitted to the action of the vapour of methyl alcohol, and then subjected to a preliminary exhaustion at 100° C., was found on heating to a red heat *in vacuo* to give off 101 volumes of a gas consisting of 11 volumes of carbon dioxide and 90 volumes not absorbed by alkaline pyrogallate. On transferring the latter to the eudiometer and exploding with oxygen, it was found to be composed chiefly of hydrogen, together with a small quantity of some hydrocarbon which was not further investigated.

On several occasions we have obtained specimens of platinum black of low absorptive power, which may readily be distinguished by the fact that they have a grey appearance instead of the usual dead black. (Platinum black of this description is apparently formed by the reduction of solutions which are acid instead of neutral or slightly alkaline.)

A quantity of this greyish platinum black from a preparation which on ignition in a vacuum gave off only 21 volumes of oxygen was treated with ammonia gas.*

After exhausting at the ordinary temperature, 24·8 volumes of gas were extracted on ignition and were found to consist of CO_2 4·2 volumes, O_2 0·0 volumes, H_2 15·6 volumes, and N_2 5·0 volumes.

The total gas extracted, viz., 24·8 volumes, is sufficiently close to 21 volumes, the amount of oxygen originally contained in another sample of the same preparation, to warrant the inference previously drawn with regard to sulphur dioxide, and confirmed in the case of carbon monoxide, hydrogen, methyl alcohol and formic acid, that, when these reducing substances act on platinum black the oxygen is removed but its place is taken by nearly the same number of volumes of the reducing substance or its products of decomposition. In former experiments, this number always approximated to 100 volumes, since the platinum black investigated contained nearly 100 volumes of oxygen. Furthermore, the removal of the reducing substance or its products from the platinum black seems to be just as difficult to accomplish as the removal of the oxygen itself.

An attempt to remove the oxygen by treating with hydrogen peroxide also proved unsuccessful.

When a platinum wire serves as the cathode in a vacuum tube through which an electric discharge is sent, the platinum volatilises, or gets thrown off, and forms a mirror on the walls of the tube. It was thought that by making a mass of platinum black the cathode in such a tube, the oxygen might be removed. On trying the experiment, however, only a very few bubbles of gas were extracted, and these were obviously due to the heating effect alone, and not to any mysterious action during the passage of the current.

In most of these experiments the reducing substance was employed in excess, but

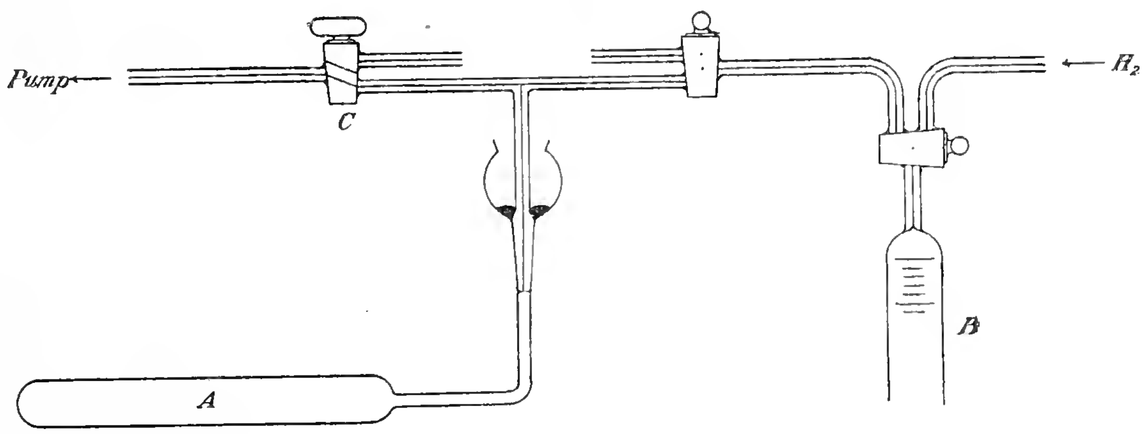
* The ammonia gas employed in this experiment was obtained by admitting strong aqueous ammonia at the bottom of a small flask, provided with a side tube dipping into mercury, nearly filled with chips of caustic potash, so that the gaseous ammonia given off might be dried as it ascended. When potash is dissolved in water, heat is evolved, and when ammonia is liberated from an aqueous solution, heat is absorbed, and the latter effect seems to be greater than the former, for, on admitting ammonia, the flask became very cold, and it was only necessary to warm it gently with the hand to obtain a steady stream of the gas.

in the following section the result of admitting the theoretical quantity of a reducing agent, viz., hydrogen, is described.

IV. *On the Existence of Platinum Oxygen and Platinum Hydrogen in presence of each other.*

All previous attempts to prepare a specimen of platinum black which would occlude oxygen directly having failed, the following experiments were made in the hope of obtaining the substance we were in search of. The amount of oxygen contained in a certain sample of platinum black was determined by a direct experiment; and it was found that 5·815 grams gave, on exhaustion *in vacuo* at a red heat, 25·56 cub. centims. = 92·6 volumes of oxygen. By taking another weighed quantity of the same sample and charging it with rather less than the amount of hydrogen theoretically necessary for converting all the oxygen into water, it was expected that the platinum black would then be in a position (after pumping off the water formed at the ordinary temperature) to absorb oxygen directly in the calorimeter. For this

Fig. 3.



purpose 5·051 grams of platinum black were placed in the experimental tube, A, (fig. 3) which was then exhausted. It was estimated that this quantity of platinum black contained 22·20 cub. centims. of oxygen, for the complete conversion of which into water 44·4 cub. centims. of hydrogen would be necessary.

As a matter of fact, only 39·05 cub. centims. of hydrogen were admitted from the gas burette, B. The apparatus was then allowed to stand for a few hours, when the tap, C, communicating with the pump was opened. Practically all the hydrogen admitted was found to be absorbed, for on putting the pump in action only about a quarter of a cub. centim. of gas was extracted. On surrounding the experimental tube, A, with a copper sheath and applying a Bunsen burner, 10·75 cub. centims. of gas were pumped off, which on examination was found to consist of:—

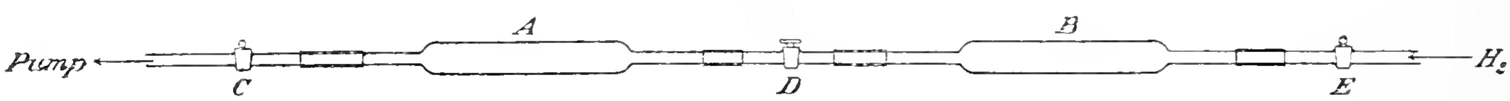
CO_2	2·53 cub. centims. = 10·5 vols.
O_2	3·04 ,, ,, = 12·7 ,,
H_2	5·00 ,, ,, = 20·8 ,,
Residue	0·18 ,, ,, = 0·7 ,,
Total	10·75 ,, ,, = 44·7 ,,

It would appear, therefore, from this experiment that the hydrogen, on being admitted to the platinum black, attacks the portion with which it first comes in contact, converting the oxygen into water, and then takes its place, instead of first converting as much as possible of the oxygen into water. It is also noteworthy that the oxygen and hydrogen pumped off did not combine on passing over the heated platinum black or sponge, but this, perhaps, may be due to the fact that they come off at different temperatures, and that the bulk of the gas diffused at once into the Töpler pump, which had a much larger volume than the experimental tube. If this explanation is correct, then, even on allowing the tube to stand for weeks or months one could scarcely expect all the admitted hydrogen to combine with the oxygen in the platinum black, since the hydrogen is practically not given off into the vacuum again, and consequently could not diffuse along to the other end of the tube containing the occluded oxygen, and *vice versâ*.

That the explanation we have given is substantially correct was proved by slightly modifying the conditions of experiment.

5·330 grams of platinum black was divided into two portions of 2·608 grams and 2·722 grams, which were introduced into the hard-glass tubes, A and B respectively, of fig. 4.

Fig. 4.



The two portions were thus separated by a narrow tube provided with a stop-cock, D. When the whole apparatus, C A D B E, was exhausted, the stop-cock, C, was shut. It was calculated that 46·86 cub. centims. of hydrogen were sufficient to convert all the oxygen in both portions of the platinum black into water. 48·00 cub. centims., or rather more than what was theoretically necessary, were admitted from a gas burette through the stop-cock, E. After standing for about two hours, the communication between the two tubes remaining open all the while, the tap, C, was opened, and it was found that the vacuum inside the apparatus was practically complete, or that all the hydrogen which had been admitted was absorbed.

The stop-cock, D, was now closed, and the tube, A, nearest the pump, heated to a dull red heat and exhausted. When the exhaustion was complete, D was opened, and, A still being kept hot, the tube B was similarly exhausted at a red heat, and the gas collected separately.

The composition of the gases obtained in this way is shown by the following analysis :—

	Gas from tube A, adjacent to pump.	Gas from tube B, adjacent to burette.
CO_2	1·91 cub. centims. = 15·5 vols.	1·14 cub. centims. = 8·8 vols.
O_2	2·94 „ „ = 23·7 „	0·00 „ „ = 0·0 „
H_2	0·40 „ „ = 3·2 „	7·36 „ „ = 56·6 „
Total . .	5·25 „ „ = 42·4 „	8·50 „ „ = 65·4 „

These results confirm our previous statement that when a limited quantity of hydrogen is admitted to platinum black containing oxygen, it simply removes the oxygen, as water, from that portion of the platinum black with which it first comes into contact, and then takes its place; for it will be seen from the above Table that the platinum black in the tube adjacent to the burette contained an excess of hydrogen associated with it, whilst the portion next the pump contained an excess of oxygen.

Precisely the same thing, but only in reversed order, takes place when oxygen is admitted to platinum black charged with hydrogen, as an experiment performed in the converse way clearly showed.

These results show that, in the case of the first experiment which led to their being performed, we may have a given quantity of platinum black containing both, what we may be permitted provisionally to call, *platinum oxygen* and *platinum hydrogen*, but they give us no trustworthy indication of how much of each is present; for, it is more than likely that on heating, some of the oxygen from the platinum oxygen combines with some of the hydrogen from the platinum hydrogen forming water; and that the free gases finally obtained are only the portions which from accidental circumstances escape the catalytic action of the platinum.

The quantities of platinum oxygen and platinum hydrogen existing in the same sample are thus probably greater than the above experiments seem to indicate.

V. *The Heat of Occlusion of Oxygen by Platinum Black.*

Having failed in the preparation of a sample of platinum black which would occlude oxygen directly at 0° C., and so enable us to determine directly the heat of occlusion of oxygen, our only alternative was to remove as much of the oxygen as possible by charging with hydrogen and then to make allowance for the heat due to the formation of water in subsequently charging with oxygen.

All the experiments made in the preceding section go to show that if a portion of the oxygen in platinum black be discharged by the addition of even considerably less than the theoretical quantity of hydrogen, the resulting substance still contains platinum oxygen and platinum hydrogen existing together, and that the quantity of hydrogen pumped off such a mixture at a red heat, *in vacuo*, even although relatively small, does not necessarily represent the actual amount of hydrogen existing in the mixture as platinum hydrogen, but only that fraction of it which escapes the catalytic action of the platinum or platinum oxygen.

In the experiments which follow an attempt was made to reduce the amount of this hydrogen to as small an extent as possible. The apparatus used was precisely similar to that employed for the determination of the heat evolved on the occlusion of hydrogen. The amount of platinum black used was 12·555 grams, and after its introduction, the capacity of the experimental tube was carefully determined.

According to calculation the above quantity of platinum black contained 55·18 cub. centims. of oxygen, and this requires for its complete conversion into water about 110 cub. centims. of hydrogen. Before placing the apparatus in the calorimeter it was sealed on, on one side to the pump, and on the other to the burette supplying pure hydrogen. After completely exhausting, at the ordinary temperature, 96·40 cub. centims. of hydrogen were admitted and allowed to remain overnight. Next day communication was made with the pump, and whilst the platinum black was heated in boiling aniline (temp. 184° C.), as much water and gas as possible were pumped off. In this way 1·52 cub. centims. of hydrogen were extracted. The experimental tube, in the vacuous state, was then removed from the pump and burette and its contents thoroughly mixed up by prolonged shaking. After having been shaken at intervals for a day it was cooled to 0° C. and inserted into the calorimeter and sealed on once more to the pump and to the burette, furnishing pure oxygen.

It was hoped that by this treatment all the platinum hydrogen might be destroyed and that the specimen would be able to occlude oxygen directly. Oxygen was, therefore, admitted in small portions of about 5 cub. centims. at a time, and the results are given in tabular form in Table III. For the sake of convenience, all the results have been tabulated together, and have been further sub-divided into a series of eight experiments, or twenty-seven operations.

TABLE III.—Calorimetric Experiments on the Occlusion of Oxygen by Platinum Black.

Experiment.	Operation.	Gas. Admitted.	Gas. Exhausted.	Gas absorbed.		Calorimeter deflection.	Heat evolved. K = 100 gram. cal.	Heat evolved per gram of gas absorbed.
		cub. centims.	cub. centims.	cub. centims.	grams.	millims.		K.
I.	1.	5·32 O_2	Nil	5·32 O_2	0·00761 O_2	135·8	0·1430	18·8
	2.	5·51 ,,	,,	5·51 ,,	0·00788 ,,	133·0	0·1401	17·8
	3.	4·37 ,,	,,	4·37 ,,	0·00625 ,,	108·6	0·1143	18·3
	4.	5·28 ,,	1·74	3·54 ,,	0·00506 ,,	76·1	0·0801	15·8
	5.	25·86 ,,	24·96	0·90 ,,	0·00129 ,,	12·2	0·0129	10·0
			Totals .	19·64 ,,	0·02809 ,,	..	0·4904	
II.	6.	47·59 H_2	Nil	47·59 H_2	0·00428 H_2	826·8	0·8698	203·1*
III.	7.	5·03 O_2	Nil.	5·03 O_2	0·00720 O_2	151·2	0·1592	22·1
	8.	5·49 ,,	,,	5·49 ,,	0·00785 ,,	159·3	0·1678	21·4
	9.	5·08 ,,	,,	5·08 ,,	0·00727 ,,	148·3	0·1561	21·5
	10.	32·49 ,,	24·85	7·64 ,,	0·01092 ,,	149·1	0·1570	14·4
			Totals .	23·24 ,,	0·03324 ,,	..	0·6410	
	11.	21·25 H_2	Nil	21·25 H_2	0·00191 H_2	353·5	0·3722	194·7*
	12.	45·69 ,,	0·50	42·19 ,,	0·00407 ,,	705·5	0·7429	182·7*
IV.	13.	19·27 ,,	0·30	18·97 ,,	0·00171 ,,	279·8	0·2947	172·6*
	14.	10·14 ,,	about. (8·0)	?	?	42·0	0·0442	?
	15.	33·75 ,,	[24·74] 30·89	9·01 ,,	0·000811,,	52·5	0·0553	68·2
V.	16.	5·30 O_2	Nil	5·30 O_2	0·00758 O_2	220·3	0·2320	30·6
	17.	6·08 ,,	,,	6·08 ,,	0·00869 ,,	190·3	0·2004	23·1
	18.	5·24 ,,	,,	5·24 ,,	0·00750 ,,	166·8	0·1756	23·4
	19.	5·96 ,,	,,	5·96 ,,	0·00853 ,,	174·2	0·1834	21·5
	20.	5·52 ,,	,,	5·52 ,,	0·00789 ,,	163·6	0·1723	21·8
	21.	5·32 ,,	,,	5·32 ,,	0·00761 ,,	160·7	0·1692	22·2
	22.	5·98 ,,	,,	5·98 ,,	0·00852 ,,	155·4	0·1637	19·1
	23.	6·00 ,,	3·60	2·40 ,,	0·00343 ,,	38·5	0·0405	11·8
			Totals .	41·80 ,,	..	..	1·3371	

TABLE III.—Calorimetric Experiments on the Occlusion of Oxygen by Platinum Black (continued.)

Experiment.	Operation.	Gas. Admitted.	Gas. Exhausted.	Gas absorbed.		Calorimeter deflection.	Heat evolved. K = 100 gram. cal.	Heat evolved per gram of gas absorbed.
		cub. centims.	cub. centims.	cub. centims.	grams.	millims.		K.
VI.	24.	115·05 H_2	[24·48] 32·43	90·57 H_2	0·00815 H_2	1260·0	1·3268	162·8
VII.	25.	4·79 O_2	Nil	4·79 O_2	0·00685 O_2	199·5	0·2100	30·6
	26.	60·76 ,,	[24·75]	36·01 ,,	0·05149 ,,	1012·8	1·0663	20·7
			Totals .	40·80 ,,	..	..	1·2763	
VIII.	27.	Oxygen occluded. Exhausted at a red heat		21·67 O_2	0·03098 O_2			

Let us consider first of all the first experiment of five operations. Starting with the platinum black prepared as above, successive quantities of oxygen were added until after Operation 5 it was fully charged up with oxygen. The absorption of the oxygen in the first three operations was very rapid, but became slower in the fourth and still slower in the fifth. The table sufficiently explains itself. In Operations 1, 2 and 3, the heat evolved per gram of oxygen absorbed remains pretty nearly the same. In Operation 4 there is a distinct diminution in the amount of heat evolved, whilst in 5 this has tailed off to 10 K. These results are remarkable. If all the numbers in the last column had remained constant, we might have concluded that we were dealing only with the direct occlusion of oxygen, or, even if the last two or three values had remained constant we might have accepted these as representing the heat of occlusion of oxygen. As it is, however, and knowing as we do that the platinum black, in spite of the treatment to which it was subjected, may have contained some platinum hydrogen, we must conclude that the first four numbers, at any rate, are composite numbers partially due to the heat of occlusion of oxygen and partially to the heat of formation of water. It might have been expected that the first charge of oxygen would have removed all the hydrogen existing in the platinum, and it is curious that for small concentrations of platinum hydrogen, the ratio of the quantity of oxygen which goes to form water to that which is occluded remains constant; for this is obviously what the approximate constancy of the first three numbers means. For greater concentrations of platinum hydrogen, as can be seen from Operation 16, this no longer holds good.

The net result of these five operations is to fix the upper limit of the heat of occlusion of oxygen at 10 K per gram. From the fact that in Operation 4 some oxygen was extracted from the tube, we may suppose that at this stage most, if not all, the hydrogen had been removed as water, and that the number 15·8 is obtained because the effect due to true occlusion preponderates over that due to the formation of water. From the smallness of the quantity of oxygen occluded in Operation 5, no great reliance can be placed on the value 10 K, taken singly, but in all probability we are dealing in this case with true occlusion.

In Operation 6, 47·59 cub. centims. of hydrogen were admitted to the experimental tube still in the calorimeter, the heat evolved per gram of hydrogen absorbed being 203·1 K. This number has an asterisk attached to it to signify that the results may be calculated in another way, namely, 47·59 cub. centims. = 0·00428 gram of hydrogen were absorbed, the heat evolved being 0·8698 K. Now, one gram of hydrogen on being burnt to water evolves 342 K, hence, if we assume that the whole of the 47·59 cub. centims. of hydrogen were oxidised to water, the heat evolved should be $342 \times 0·00428 = 1·4645$ K. If the difference, viz., $1·4645 - 0·8698 = 0·5947$ K, represents the heat absorbed on removing 23·8 cub. centims. of oxygen from the platinum black, then the heat absorbed on the removal of one gram of oxygen will be $0·5947 \div 23·8 \times 0·00143 = -16·8$ K. This number approaches those obtained in Operations 1–4, but, of course, has the negative sign. The assumption on which this number is calculated, viz., that the *whole* of the hydrogen added went to form water, cannot be justified, and hence this number cannot be regarded as the true amount of heat absorbed per gram of oxygen removed.

The 47·59 cub. centims. of hydrogen added in Operation 6 were less than what is theoretically required for the complete removal of the oxygen contained in the platinum black, and, consequently, before Operation 7 was started, the platinum black was in pretty much the same condition as before the first operation, except that in all probability it contained a larger proportion of platinum hydrogen, since it had not been shaken up or exhausted at 184° C. This possibly explains why the values of the heat evolved per gram of oxygen absorbed in Operations 7, 8, and 9 are higher than in the corresponding Operations 1, 2, 3, and 4.

In Operation 10 the platinum black was fully charged up. If this had been done in two stages, say, by adding 6 cub. centims. and then 1·64 cub. centims., the value for the last would probably have been much less than 14·4 K. The falling off in Experiment III. is otherwise very similar to that in Experiment I.

In the fourth experiment hydrogen was added in successive stages. During Operation 14, 10·14 cub. centims. of hydrogen were admitted, and on making connexion with the pump it was found that the vacuum was far from complete. Since the pressure within the apparatus was not known, it was impossible to estimate how much hydrogen was really absorbed, and how much was simply filling the experimental tube. About 8 cub. centims. altogether were pumped off. In Operation 15 the hydrogen was admitted at full atmospheric pressure. The volume of hydrogen required to fill the experimental tube is given in square brackets [24·74], whilst 30·89 cub. centims. were afterwards extracted, the difference between them having been removed from the platinum *in vacuo*.

Operation 15 is interesting, as the value 68·2 K represents the true heat of occlusion of one gram of hydrogen, which is in good agreement with the values formerly obtained, the mean of which was about 69 K.

Just as in the case of 6, Operations 11, 12, and 13 can be calculated in a similar way, and give the following values per gram of oxygen removed, if we assume that in each case the whole of the hydrogen is oxidised to water, and that none goes to form platinum hydrogen.

Operation 11	. . . .	− 18·4 K	per gram of oxygen removed.
,, 12	. . . .	− 20·1 K	,, ,, ,,
,, 13	. . . .	− 21·3 K	,, ,, ,,

Experiment V. is similar to I. and III., except that we start in this case with platinum black, which has been fully charged with hydrogen, and from which all the hydrogen which comes off *in vacuo* at 0° C. has been removed.

For the first addition of oxygen a greater amount of heat is evolved, viz., 30·6 K, probably due to the relatively large formation of water compared with the quantity of oxygen really occluded. The last charge of oxygen gives out 11·8 K per gram of oxygen absorbed, and this seems to be due for the most part to the true occlusion of oxygen.

At this stage we intended to stop the series of experiments and to find out, by exhausting at a red heat, how much oxygen had actually been occluded in the last set of operations; but, since there was some uncertainty whether the water which was formed was itself occluded or absorbed by the platinum, producing heat changes, or whether it simply condensed on the inner walls of the experimental tube and on the platinum black, it seemed that some light might be thrown on this point by a slight alteration in subsequent experiments.

The high value, 30·6 K, was obtained in Operation 16: but before this experiment was performed, the platinum black previously charged with hydrogen was kept in communication with a P_2O_5 tube *in vacuo* for two days, in order to remove as much as possible of the water along with the hydrogen. In Operation 24 the platinum black was fully charged with hydrogen. [24·48] cub. centims. were required to fill the experimental tube, whilst 32·43 cub. centims. were pumped off as expeditiously as possible, in order to leave most of the water formed, which diffuses very slowly through the capillary tubing, in the experimental tube. In the next operation, 25, a small quantity of oxygen was admitted, but the value for the heat evolved per gram of oxygen absorbed was 30·6 K, which is identical with the value obtained in Operation 16, although in the latter case as much water as possible had been previously removed from the platinum before starting. This experiment is, perhaps, not quite conclusive, but we think, from the general behaviour of platinum black, that it had already, from the very beginning, absorbed or occluded its quantum of water, and that this is not affected by placing the platinum in a vacuum, or by condensing more dew on its surface.

This assumption was confirmed by surrounding the bulb of a BECKMANN thermometer in an ordinary calorimeter with 5 grams of platinum black, dried at 100° C. When the temperature had become steady, the platinum black was moistened with water at the same temperature, injected from the outer bath of the calorimeter, but no definite rise or fall of temperature could be detected.

After charging up fully with oxygen, in Operation 26, the experimental tube was removed from the calorimeter, the platinum black transferred to a hard-glass tube, exhausted at the ordinary temperature, and then heated to redness *in vacuo*. 21·67 cub. centims. = 0·03098 gram of oxygen were pumped off at a red heat, and this represents the amount of oxygen which was actually occluded in Operations 25 and 26.

So far we have not been able to obtain any very definite direct value for the heat of occlusion of oxygen, except that in Operations 5, 10, and 23 we found approximate values, viz., 10·0, 14·4, and 11·8 K per gram of oxygen occluded.

With the data which we possess, however, and knowing that during the last process of charging with oxygen 21·67 cub. centims. of oxygen were occluded, we can calculate the heat of occlusion indirectly.

(1.) In Experiment VII., 40·80 cub. centims. of oxygen were used, and of this 21·67 were actually occluded, whilst the remainder, 19·13 cub. centims., must have been burnt to water. Now, on combustion, 19·13 cub. centims. = 0·02735 gram oxygen should produce $\frac{684 \times 0{\cdot}02735}{16} = 1{\cdot}1695$ K. But, during the combustion, $2 \times 19{\cdot}13 = 38{\cdot}26$ cub. centims. = 0·003443 gram of hydrogen must have been removed from the platinum black, and since 69 K are absorbed on the removal of 1 gram of hydrogen, the removal of 0·003443 gram should absorb $0{\cdot}003443 \times 69 = 0{\cdot}2376$ K.

If, therefore, the heat of formation of water minus the heat absorbed on the removal of the hydrogen, *i.e.*, $1{\cdot}1695 - 0{\cdot}2376 = 0{\cdot}9319$ K, represents all the heat changes which take place, with the exception of the heat evolved or absorbed on the occlusion of 0·03098 gram of oxygen, then the difference between the actual heat developed, viz., 1·2763 K and 0·9319 K, *i.e.*, + 0·3444 K, represents the heat evolved on the occlusion of 0·03098 gram of oxygen, or 11·1 K are evolved per gram of oxygen occluded.

This number is in good agreement with those indicated by the preceding more direct measurements.

(2.) The amount of heat absorbed on the removal of oxygen from platinum black can be calculated in a somewhat similar way.

In Experiment VII., as we have just seen, 38·26 cub. centims. of hydrogen must have been left over occluded from the preceding Experiment VI. Besides this, however, 7·95 cub. centims. of hydrogen were pumped off *in vacuo* in Operation 24, so that in Experiment VI., altogether, 38·26 + 7·95 = 46·21 cub. centims. of hydrogen must have been occluded. The total hydrogen absorbed was 90·57 cub. centims.; hence 90·57 — 46·21 = 44·36 cub. centims. must have combined with 21·18 cub. centims. of oxygen occluded in Experiment V. to form water.

The evolution of heat corresponding to this formation of water is

$$44{\cdot}36 \times 0{\cdot}00009 \times 342 = 1{\cdot}3652 \text{ K}.$$

Similarly, the heat evolved on the occlusion of 46·21 cub. centims. hydrogen is

$$46{\cdot}21 \times 0{\cdot}00009 \times 69 = 0{\cdot}2869 \text{ K}.$$

The sum of the two heats evolved, viz., 1·6521 K, is greater than the heat actually evolved, viz., 1·3268 K, by 0·3253 K.

Consequently, 0·3253 K must have been absorbed on removing 22·18 cub. centims. = 0·03172 grm. of oxygen, and hence — 10·3 K were *absorbed* per gram of oxygen *removed*.

(3.) Going back another step, we find that, from Experiment VI., 22·18 cub. centims. of oxygen must have been occluded in Experiment V. The total oxygen used in Experiment V. was, however, 41·80 cub. centims., and therefore 19·62 cub. centims., = 0·02805 gram of oxygen, must have formed water with the corresponding quantity of hydrogen pre-existing in the platinum black, giving out

$$\frac{684 \times 0{\cdot}02805}{16} = 1{\cdot}1995 \text{ K}.$$

19·62 cub. centims. of oxygen would remove 39·24 cub. centims. of hydrogen, with the absorption of

$$39{\cdot}24 \times 0{\cdot}00009 \times 69 = 0{\cdot}2438 \text{ K}.$$

The heat due to the formation of water and the removal of hydrogen is thus 1·1995 — 0·2438 = 0·9557 K, whilst the heat actually evolved was 1·3371 K. Consequently, 0·3814 K have been liberated on the occlusion of 0·03172 gram of oxygen; or 12·0 K were evolved for every gram of oxygen occluded.

All these calculations depend on the final measurement of the oxygen occluded, and consequently any error involved in this determination would be magnified at each step backwards.

We have thus obtained three values for the amount of heat evolved per gram of oxygen occluded, or, what in one case amounts to the same thing, viz., the heat *absorbed* per gram of oxygen *removed.*

From Experiment VII.	11·1 K per gram occluded
" " VI.	−10·3 K " removed
" " V.	12·0 K " occluded

The mean of the three is 11·1 K per gram of oxygen occluded, and the first of these, for reasons which have just been stated, is the most trustworthy. Deviations due to the magnification of any errors, are already apparent in the next two, although it is satisfactory to find that their mean is identical with the first. If it had been possible to obtain another pair of values, then from the method of calculation the fourth would probably have been less than 10·3, and the fifth greater than 12·0, although the mean might have approximated to 11·1 K.

Altogether, therefore, we have six determinations of the heat of occlusion of oxygen, three of which were obtained more or less directly, whilst the other three were indirect measurements, namely :—

From Operation	K.	From Experiment	K.
5	10·0	VII.	11·1
10	14·4	VI.	10·3
23	11·8	V.	12·0

Of these, 14·4 K is obviously too high, since the final amount of oxygen absorbed in Operation 10 is so large that it probably includes some heat due to the formation of water.

The mean of the other five determinations is + 11·0 K per gram of oxygen occluded, and we think, although from the nature of the experiments and the difficulties encountered in determining this constant it is scarcely so satisfactorily established as the corresponding number for hydrogen, this value may be accepted as a pretty fair approximation to the amount of heat evolved per gram of oxygen occluded, or referred to a gram-atom of oxygen + 176 K.

VI. *Speculations on the Nature of the Occlusion of Gases by Platinum Black.*

With regard to the occlusion of hydrogen and other gases by platinum black, we are not as yet in a position to form a definite opinion. The difficulties which lie in the way are considerable, and when we remember that the question, whether the much better defined product obtained by occluding hydrogen in palladium is to be regarded as containing the compound Pd_2H, or whether it is simply a solid solution of hydrogen in palladium, has not yet been definitely settled, it is not astonishing that the difficulties in deciding the corresponding question for the less well-defined product obtained by charging platinum black with hydrogen can not easily be surmounted. As far as we can see at present, the only solution to the problem is to be got by a more minute study of the physical properties of the substance.

It must be admitted that the inference which we have drawn, viz., that any given sample of platinum black occludes approximately the same volume of the different gases, seems to point to the conclusion that we are dealing with some phenomenon which is conditioned by the extent of the surface of the platinum black, as otherwise it is not quite clear why hydrogen, oxygen, carbon monoxide, and sulphur dioxide should all be absorbed in equal volumes. On the other hand, we have to deal with the equally significant fact that neither nitrogen nor carbon dioxide are absorbed except in comparatively small quantity. If, however, we confine our attention to the occlusion of oxygen by platinum black, the balance of the evidence which we can bring forward at the present time seems to indicate the formation of a definite compound or oxide.

If, in the first place, the occlusion of oxygen were merely the physical condensation of oxygen in the capillary pores of the platinum black, we should expect that raising the temperature would either have very little effect or would simply re-evaporate the condensed oxygen, whilst, as a matter of fact, rise of temperature is accompanied by increased absorption up to the temperature of about 360°–380° C., when the oxygen is again given off.

This behaviour seems to militate against the view that we are dealing with physical condensation or liquefaction in the pores. Solid solution however is not excluded; but if it be remembered that platinum black absorbs about 100 volumes of oxygen, whilst there is no authenticated case on record in which platinum in the concrete form has been known to absorb more than a few volumes at the outside, it would follow that, if we were really dealing with solid solution, platinum in *all* its forms should absorb approximately the same quantity of oxygen, although we would be quite prepared to find a different *rate* of absorption in the different forms.

If we admit the possibility that the different varieties of platinum may be allotropic modifications, then the above arguments would not necessarily hold good, as may readily be seen by a comparison of the properties of red and yellow phosphorus. Carbon bisulphide may be regarded as being soluble in the yellow variety, but insoluble in the red, since it is purely a conventional matter which we call the solvent and which the dissolved substance.

In connexion with the view that the absorption of oxygen by platinum black may simply be due to superficial oxidation, we made a few experiments with the object of comparing the general behaviour of platinum black charged with oxygen with the lowest oxide of platinum. Several samples of platinous hydrate $Pt(OH)_2$ were prepared by boiling a dilute solution of potassium platinochloride K_2PtCl_4 with the theoretical quantity of caustic potash according to the method recommended by

JULIUS THOMSEN. We were never able to obtain the platinous hydrate perfectly free from chlorine in this way, and we were surprised to find that after drying at 100° C. it always contained more than the theoretical amount of oxygen, and that the oxygen appeared to increase with the time taken in drying. On heating at higher temperatures in a current of dry air, still further quantities of oxygen were absorbed, but between 237° and 360° oxygen is again lost, reminding one of the behaviour of platinum black which begins to give off its oxygen at about 360° under ordinary atmospheric pressure.

Platinous hydrate appears to lose the bulk of its water at about 200°–250° C., and the oxide PtO so formed, begins to give off its oxygen very slowly at 380° *in vacuo.* At 444° a large fraction of the oxygen may be slowly pumped off, as the following table shows, but for its complete removal ignition at a red heat is necessary.

First three hours at 444° . .	8·50	cub. centims. extracted.
Second day ,, ,, . .	15·45	,, ,, ,,
Third ,, ,, ,, . .	2·50	,, ,, ,,
Fourth ,, ,, ,, . .	1·39	,, ,, ,,
Fifth ,, ,, ,, . .	1·02	,, ,, ,,
Sixth ,, ,, ,, . .	0·63	,, ,, ,,
Exhausted at 460° . .	4·56	,, ,, ,,
,, ,, a red heat . .	6·68	,, ,, ,,
	40·73	

A previous analysis of the same sample showed that 41·01 cub. centims. should have been obtained.

Platinum black and platinous oxide in general appear to behave in pretty much the same way, except that when heated *in vacuo* the oxygen comes off the oxide proper more slowly and at a slightly higher temperature.

Although some important evidence might lead us to suppose that the absorption of oxygen by platinum black is simply due to the superficial oxidation of the finely divided metal, the question cannot yet be regarded as definitely settled.

What is either a very curious coincidence or the best argument in favour of the view that the occlusion of oxygen is simply superficial oxidation is to be found in THOMSEN'S* determination of the heat of formation of platinous hydrate.

According to THOMSEN the reaction

$$\mathbf{Pt} + O + H_2O = \mathbf{Pt(OH)_2} + 179\ \mathrm{K}$$

takes place with the evolution of 179 K for 16 grams of oxygen, whilst we found that the occlusion of the same quantity of oxygen by platinum black was accompanied by the evolution of 176 K, the water which is a necessary factor in the above reaction being always present in platinum black.

* J. THOMSEN, 'Thermochemische Untersuchungen,' vol. 3, page 429.

INDEX SLIP.

VII. *The Sensitiveness of the Retina to Light and Colour.*

By Captain W. DE W. ABNEY, *C.B., (late) R.E., D.C.L., F.R.S.*

Received May 10,—Read June 3, 1897.

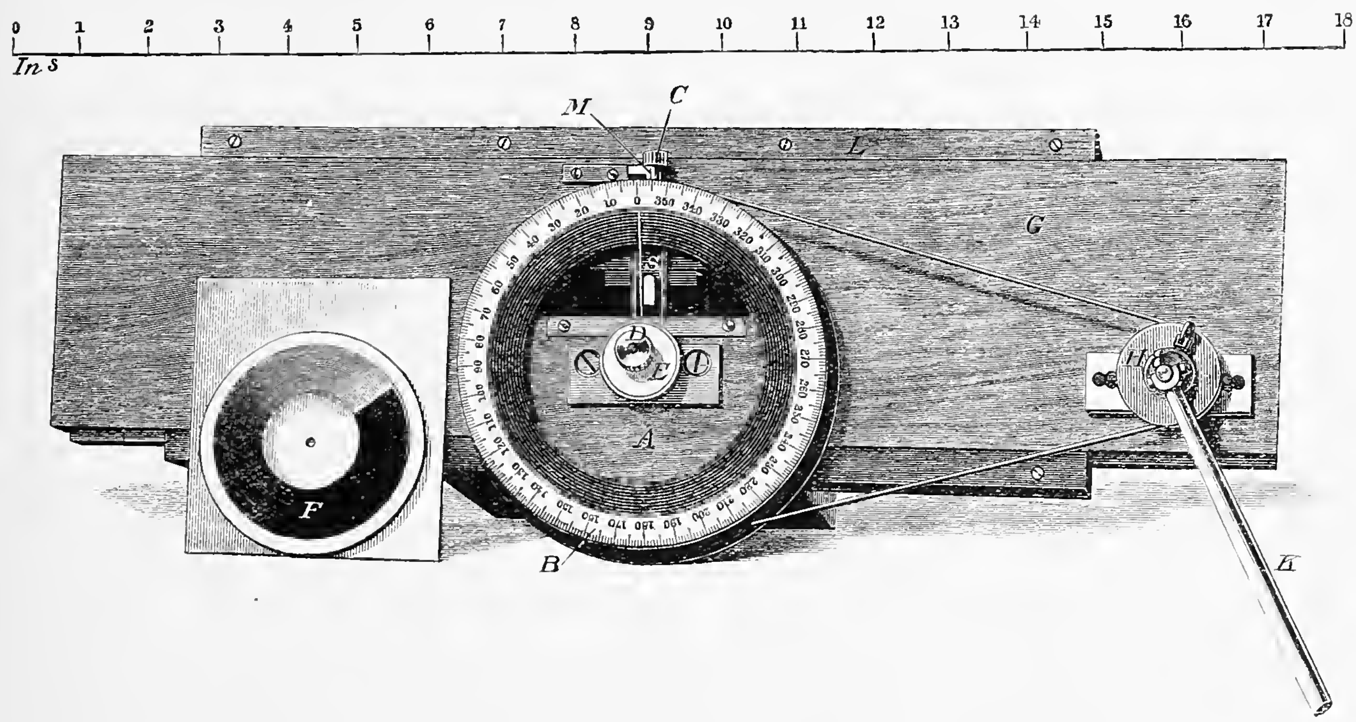
1. *New Form of Apparatus for Reducing the Intensity of Light.*

IN "Colour Photometry," Part III. ('Phil. Trans.,' A, 1892), a description was given of the apparatus employed for estimating the intensity of light of any colours which just failed to cause any sensation on the retina. In the following research a modified form of apparatus has been employed, and experience has shown it to be equally as accurate as and more convenient in many ways than that formerly used. When rotating sectors are employed with less than 4° of aperture, small errors in the reading of the graduated arc cause appreciable errors in the result. Hence, the more nearly the zero reading is approached, the less trustworthy are the determinations of the diminution of the total intensity, and this uncertainty affects all observations in which the light is reduced below $\frac{1}{40}$ of its initial amount. The sectors have also the disadvantage of not being noiseless, and of requiring an electro or other motor to work them. With the extinction box, described in Part III., the difficulty of being able to reduce the light to, say $\frac{1}{10000}$ part of its initial intensity, was overcome by using diaphragms of varying aperture in front of the ground glass, which was practically the source of illumination. It is obvious that any instrument which would allow a similar decrease in illumination without the intervention of a diaphragm would be advantageous, more particularly if it were noiseless. In the estimation of star magnitudes by extinction, a wedge has been used from time to time by various investigators, and in 1870 I myself used one for the purpose of measuring the intensities of the electric light. As a rule, these wedges are of dark green glass which have a fairly high exponential coefficient of absorption. The fact that it has a dominant colour at once indicates that for the comparison of spectrum colours by extinction such a wedge should be avoided. Thanks to the kindness of Dr. GROSSMANN, of Liverpool, I had put into my possession a pair of black glass wedges, and with these I hoped to avoid the difficulty caused by the colour of the green glass wedges, but after mounting them and preparing to use them, it was found that the material exhibited bands of absorption in several parts of the spectrum; after graduating them for each spectrum colour, the results were so unsatisfactory that their use had to be abandoned. Reduction of intensity by means of a polarizing apparatus had already been tried and found unsatisfactory, when the beam of light approached the point of absolute visual extinction.

2. *Gelatine Wedge in Annular Form.*

It occurred to me that a few years ago Mr. LEON WARNERKE had brought out a "sensitometer" (an instrument for measuring the sensitiveness of a photographic plate) in which the apparatus for reducing the intensity of light admitted to a sensitive surface consisted of an annulus of gelatine of gradually increasing thickness, coloured either by a dye or by a powder of any colour which might be desired. I applied to Mr. WARNERKE to prepare for me some of these sensitometer screens, and to use as a powder very finely divided lampblack. He kindly gave me several specimens of three varieties, one having but little pigment mixed with the gelatine, another rather more, and the third with strong absorption. This last reduced the light which penetrated the thinnest part to $\frac{1}{70000}$ part when it had to pass through the thickest portion, the second only reduced it to rather less than $\frac{1}{1000}$ part, and for my purpose I selected this last with which to work as a rule, though in some cases the more opaque annulus was used. I am informed that the mode of the preparation of the annulus is as follows:—In a perfectly flat and thick steel disc a groove of gradually increasing depth is cut by a proper machine into an annulus about 1 inch broad. When the depth of the groove has been tested and found to increase proportionally to the arc of the circle, replicas of the disc and groove are made in non-oxidizable metal. The finest pigment is then mixed with semi-liquid gelatine, and when thoroughly incorporated the viscous liquid is poured into the groove, the top surface of the disc being very carefully levelled. A sheet of worked glass is then laid on the surface of the disc, and any excess squeezed out except a very minute film which appears colourless, and for which there is no means of escape. When the gelatine has properly set, the glass plate is removed with the relief of the groove attached to it. The gelatine annulus is allowed to dry, and is then ready for use. It struck me that such an annulus might be substituted for glass wedges, and after selecting one which was suitable as to colour absorption and regularity of absorption coefficient (if one may so call it), I finally determined to adopt it. It may be said here that not every specimen gave equally good results. It is only when the glass plate is perfectly flat that regularity in the increase in thickness, and consequent uniformity of coefficient per unit angle can be secured. The diminution in light in its passage through the annulus of pigmented gelatine is evidently due to a different cause to that of coloured glass. In the latter case it is due to true absorption, in the former to the obstruction by fine opaque particles. If the opaque particles possessed colour some of the light passing through the gelatine would be tinged with the light reflected from their surfaces, and if they were semi-transparent there would be not only obstruction but also absorption. As the particles were black and very opaque the light penetrating, and which was reflected from the surfaces of the particles, would be colourless, or rather uncoloured. A photographic plate sensitive to all visible rays was exposed to the spectrum, and on the same plate, and just below it, a more prolonged exposure was given after its passage through a portion of the annulus, and on development the two images appeared identical from B to, at all events, near G, but beyond the image in the extreme violet was rather less opaque in the latter than in the former, indicating a slightly increased absorption for those rays. The use of this annulus was therefore possible and easy between B and G, and between these limits only has it been employed. It seems probable that this falling off in the extreme violet may be due to scattering by fine particles. The following is the method adopted to mount the annulus :—A hole was pierced in the glass exactly at the centre of the two circles and the glass was cut into a disc concentric with the circles.

Fig. 1.

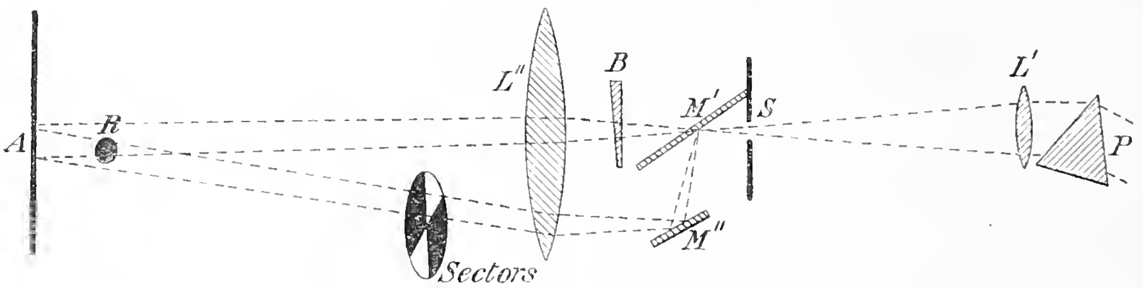


The disc of glass, A, is pierced in its centre with an aperture of just sufficient size to allow a pin with a screw-thread springing from a brass plate attached to the wooden slide, G, to penetrate. The disc of glass, F, with the annulus on it, being also pierced at its centre with an aperture, can be passed over the same pin. The two discs are clamped together with a milled-headed nut, D, a washer of paper, E, being placed between the two. The disc, A, is cemented into a circular ring, B, graduated into degrees. On A is ruled a line joining centre and the zero of the graduation. The junction of the most and least opaque parts of the annulus is caused to lie along that line. The disc, A, is mounted so that the marked line on it, when the zero point is at the index, M, passes across the centre of the slit, S, the width of which can be adjusted by a screw arrangement worked by the milled head, C. The brass ring is grooved on its outer edge, and round it and a brass pulley, H, passes a thread, and the ring, B, can be rotated by means of an universal joint and the handle, K. The pulley, H, by an obvious arrangement, can be moved towards or away from the annulus, thus allowing the thread to be slackened or tightened as may be desired. When F is in position the annulus covers the slit, S. By turning the handle, K, which is of such a length that it can be used 5 feet off from G, different parts of the annulus are caused to pass across S. When in use a small glow-lamp, carefully shaded, is attached to L, the frame of the camera, and after an extinction the readings are made by passing a current through it temporarily. It might be thought that an easier arrangement would be to place the mounted annulus in front of the eye, but the difficulty of reading without illuminating the retina prevented its adoption, and the uncertainty that would exist as to the precise part of the annulus which came between the spot of light and the eye, owing to the large eye-hole, also prevented its use in that position. Perfectly unfettered vision is essential.

3. *Graduation of Annulus.*

The annulus, when thus mounted, was ready for graduation, and as the method adopted is, it is believed, new, a description of it is given. The colour-patch apparatus described in Part II., "Colour Photometry" ('Phil. Trans.'), was utilized, but to the front of the slit in the spectrum a piece of plain worked glass, M′, was attached as shown (fig. 2). The beam of light coming from the prism, P, after

Fig. 2.



passage through the lens, L′, and the slit, S, was partly reflected from a plain mirror M′ on to M″, a silvered mirror, and partly transmitted through M′, the ratio of the amount of reflected to transmitted light varying according to the angle which M′ made with the direction of the beam. The direct beam after passing through M′ also passed through the annulus, B, and through the lens, L″, forming an image of the face of the prism, P, on A. The annulus, B, could be rotated so that the intensity of illumination of the patch of light on A could be increased or diminished at will. The reflected part of the beam also passed through the margin of the lens, L″, and formed a patch of light of the same colour and very closely of the same size on A, and by adjusting the mirror, M″, the two patches could be superposed. Rotating sectors with adjustable apertures were placed in the path of the reflected beam as shown. A rod, R, cast two shadows, which were arranged to touch one another. When the undiminished reflected beam fell on A the two shadows were made equally luminous by rotating B. The number of degrees from the zero point was then noted. The sectors then were set so that half the light of the reflected beam was cut off, and equality of illumination of the shadows again secured by rotating B. The number of degrees was again noted. When the sectors were set at 45° the same procedure was adopted. If the coefficient of absorption (obstruction) did not vary, the intervals between first and second readings and between the second and third should be the same. By again setting the angle of the sectors to give one-eighth part of the light, another reading could be obtained, and so on. By altering the angle of M′ a new set of readings could be obtained, till every part of the annulus had been tested. By altering the position of the slit, S, in the spectrum, the obstruction coefficient for any colour could be arrived at. It will be seen that by this method one beam of light alone is utilized in a very simple method, and is in fact a modification of the method adopted when graduating wedges in white light, as described in the monthly notices of the Royal Astronomical Society four years ago. From the extreme red to beyond G no difference was found in the "obstruction" coefficients above that which would arise from pure error in observation. The following is the adopted coefficient for the annulus mostly employed :—

TABLE I.—Scale of Wedge, the coefficient being ·0086 for each degree.

Degrees.	Log.	Value of the light penetrating.	Degrees.	Log.	Value of the light penetrating.
0	4·000	10,000	190	2·366	232
10	3·914	8,200	200	2·280	190
20	3·828	6,870	210	2·194	156
30	3·742	5,500	220	2·108	128
40	3·656	4,520	230	2·022	105
50	3·570	3,710	240	1·936	86·1
60	3·484	3.040	250	1·850	71·5
70	3·398	2,500	260	1·764	58·0
80	3·312	2,050	270	1·678	47·8
90	3·226	1,680	280	1·592	39·0
100	3·140	1,380	290	1·506	32·0
110	3·054	1,130	300	1·420	26·2
120	2·968	925	310	1·334	21·6
130	2·882	760	320	1·248	17·8
140	2·796	625	330	1·162	14·5
150	2·710	512	340	1·076	11·9
160	2·624	420	350	0·990	9·7
170	2·538	345	360	0·904	8·0
180	2·452	283			

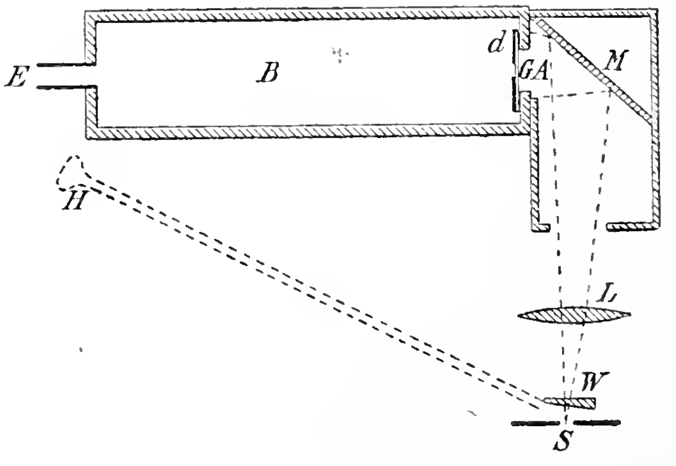
It may be noted that the annulus was graduated at the same distance from the screen as that at which it was employed in the subsequent experiments. This is, perhaps, an unnecessary refinement in the case of a medium presenting such fine particles, though it would not have been so had they been as coarse as the grains of silver in a photographic negative, for in the last case a certain amount of light is lost through scattering. The annulus having been graduated, it became necessary to ascertain what *breadth* of annulus might be used so that no appreciable error might be incurred by reading the centre of the slit as the mean reading of the light passing through it. If the slit occupied 10° of the annulus, the light passing through the margins would be as 2051 to 1683, and the mean of these two would be 1867. The light passing through the centre of the slit would be in reality 1858 on the same scale. There would be, therefore, a difference between the mean of the marginal rays and that of the mean absorption of ·9 per cent.

If the breadth of slit occupied 5° of the annulus, the difference between the mean of the marginal intensities and that of the actual central intensity would be ·2 per cent. The true mean of all the rays would be less than these figures. A slit of 5° width would, therefore, be admissible to use as giving an error much less than that found in observations—as a matter of fact, the slit was always considerably narrower than this, so that no appreciable error can be found on this account.

4. *New Extinction Box.*

In Part III. of "Colour Photometry" a diagram is given of an "extinction box," and also of the curves of extinction of the visible spectrum for various classes of colour vision. For the greater part of the experiments the box was slightly altered, so far as regards the method of admitting the light.

Fig. 3.



At the end of the box, B, an aperture was cut, which was closed by a piece of glass, G, finely ground on each side, or by an opal glass. Provision was made for the insertion of diaphragms, *d*, in front of the glass. The eye-piece, E, was at the opposite end of the box, as shown. Outside the box was a mirror, M, enclosed in a frame, as shown. The slit, S, in the spectrum and the collecting lens, L, together with the wedge, W, are shown in the figure. H is the handle used to move the annulus round its axis. This form of box only admits light through the end; there is no reflected light in the inside. The one thing necessary is to secure a good scattering of light by the ground glass, so that the direct light is inappreciable compared with that scattered, a desideratum which is obtained by using glass ground on each surface. The box in which M is fixed is blackened, or lined with black velvet, and M itself can be either a silvered or plain glass. In the old arrangement the light entered from the side and by reflection, and after passage through ground glass illuminated a white disc at the end of the box. When the disc was of fair size any reflections from the black interior were extinguished long before the light from the disc itself vanished, and hence no inconvenience was felt from the presence of the light from the black interior, which may be taken as about $\frac{1}{30}$ of that reflected from the disc. If, however, the area of the white disc is very much diminished, the illumination, as will be seen presently, may be as much as 100 times greater than on the larger disc and yet be invisible, and then the reflection from the interior of the box would be visible after the extinction of light from the small disc was completed. For this reason the new form of extinction box was designed.

5. *Sensitiveness of the Retina to the Spectrum Colours with Varying Sizes of Image.*

The first series of experiments to be described, though not first in order of making, was to ascertain the sensitiveness of the eye to different sizes of image at the point of extinction. It has long been known that equally illuminated images on the retina, but of varying size, produced different sensations of brightness, but I am unaware of any exact measures of the difference. These I propose to give, and trust that they will be of use in studying the physiology of the eye. It was, of course, impracticable to use many eyes in this research, and I have been content with the observations made by myself and by my assistant, Mr. BRADFIELD, more especially as they were made quite separately, at different times, and are quite confirmatory of each other. The size of the image on the retina is best expressed in the angular measure which the object subtends outside the eye, and this plan I have adopted throughout. In Part III., "Colour Photometry," the extinction curves showed the absolute values of the light of the various parts of the spectrum, when just failing to impress the retina, in terms of a candle, and a change in the scale of ordinates several times in the same figure was necessitated. When the results of my first set of experiments were plotted with the angles of the annulus at which extinction occurred as the ordinates, a much more convenient form of curve was obtained. As the angles are proportional to the logarithms of the extinction these latter were employed instead of the angles themselves, to enable the results obtained with any other wedge to be made comparable with those obtained with the new apparatus. For the sake of comparing the results of the extinction of the spectrum obtained in the former experiments, given in Part III., "Colour Photometry," the curve of the extinction for the normal eye was converted to a logarithmic curve, and the conversion will be found in Table II.

TABLE II.—Extinction by the Centre of the Eye, from Part III., "Colour Photometry."

Scale Number.	λ.	Reduction.	Log.	Scale Number.	λ.	Reduction.	Log.
64	7217	55,000	4·740	33	4963	10·2	1·009
63	7082	30,000	4·477	32	4924	11·6	1·064
62	6957	15,000	4·176	31	4885	13·6	1·130
61	6839	7,500	3·875	30	4848	16·3	1·212
60	6728	3,750	3·574	29	4812	20·5	1·312
59	6621	1,900	3·278	28	4776	26·0	1·415
58	6520	1,410	3·070	27	4742	31·0	1·491
57	6423	650	2·812	26	4707	38·5	1·585
56	6333	380	2·579	25	4674	46	1·663
55	6242	272	2·434	24	4639	56	1·748
54	6152	196	2·292	23	4608	67	1·826
53	6074	112	2·046	22	4578	80	1·903
52	5996	79	1·900	21	4548	95	1·978
51	5919	53	1·752	20	4517	107	2·029
50	5850	35	1·544	19	4488	124	2·093
49	5783	24	1·380	18	4459	140	2·146
48	5720	17	1·230	17	4437	160	2·204
47	5658	12·6	1·100	16	4404	180	2·255
46	5596	10·2	1·050	15	4377	200	2·301
45	5538	8·6	0·934	14	4349	220	2·342
44	5481	7·4	0·869	13	4323	240	2·380
43	5427	6·7	0·826	12	4296	270	2·431
42	5373	6·55	0·816	11	4271	300	2·477
41	5321	6·5	0·813	10	4245	335	2·525
40	5270	6·55	0·816	9	4221	375	2·574
39	5221	6·65	0·822	8	4197	430	2·633
38	5172	6·85	0·835	7	4174	490	2·690
37	5128	7·2	0·857	6	4151	560	2·748
36	5085	7·6	0 881	5	4131	640	2·806
35	5043	8·15	0·911	4	4106	750	2·875
34	5002	8·8	0·944				

When the ordinates of the curves are the logarithms of the actual values, the reduction in intensity of the same ray required to make it invisible under different conditions is readily seen. Thus, if two such curves are parallel to one another, we at once see that the intensities for all colours are proportionally reduced.

The first experiments were made with three illuminated circular discs, 2 inches, ½ inch, and ¼ inch in diameter, and reduction necessary to cause invisibility (which in future will be called, as it was in "Colour Photometry," Part III., the "extinction") for different parts of the spectrum was noted for both. The angular measure of these discs was 4° 11′, 1° 3′, and 31′ respectively.

The following were the readings obtained. The scale of the spectrum has been reduced to the standard scale used in Part III., "Colour Photometry."

TABLE III.

Standard scale numbers.	2-inch disc readings.		½-inch disc readings.		¼-inch disc readings.	
	Degrees.	Logs.	Degrees.	Logs.	Degrees.	Logs.
56·93	110	3·05	40	3·65		
55·82	125	2·83	71	3·38	20	3·83
53·60	198	2·30	139	2·81	80	3·31
51·38	235	1·93	180	2·45	128	2·90
49·16	277	1·62	220	2·11	160	2·69
46·94	310	1·34	250	1·85	187	2·36
44·72	331	1·15	275	1·63	215	2·15
42·50	345	1·05	290	1·51	230	2·02
40·28	345	1·05	285	1·55	225	2·07
38·06	340	1·07	281	1·57	221	2·09
35·84	335	1·12	275	1·62	218	2·12
33·62	320	1·24	255	1·81	195	2·22
31·40	300	1·42	240	1·93	180	2·45
29·18	280	1·59	230	2·02	165	2·58
26·96	265	1·74	207	2·22	150	2·71
24·74	240	1·94	180	2·45	130	2·88
22·52	215	2·15	154	2·68	110	3·05
20·30	205	2·24	145	2·75	92	3·24
18·08	193	2·30	137	2·81	80	3·31
15·86	180	2·45	115	3·00	60	3·48
13·64	162	2·61	96	3·17	45	3·61

All these readings were taken with the same intensity of light, and are, therefore, comparable with one another. These log readings were plotted, and freehand curves drawn through the points, and the following table constructed from them, the standard curve of extinction being added for comparison.

TABLE IV.—Obtained from the Freehand Curves drawn from Table III.

	λ.	Standard curve.	2-inch disc.	$\frac{1}{2}$-inch disc.	$\frac{1}{4}$-inch disc.		λ.	Standard curve.	2-inch disc.	$\frac{1}{2}$-inch disc.	$\frac{1}{4}$-inch disc.
60	6728	3·60				31	4885	1·15	1·43	1·90	2·42
59	6621	3·32				30	4848	1·22	1·55	1·97	2·50
58	6520	3·07				29	4812	1·31	1·58	2·06	2·57
57	6423	2·81	3·10	3·65		28	4776	1·41	1·62	2·12	2·62
56	6330	2·60	2·85	3·42	3·90	27	4742	1·50	1·75	2·18	2·72
55	6242	2·40	2·62	3·18	3·63	26	4707	1·58	1·82	2·35	2·82
54	6152	2·20	2·40	2·95	3·40	25	4675	1·66	1·90	2·43	2·92
53	6074	2 04	2·20	2·73	3·20	24	4634	1·75	2·02	2·52	2 97
52	5996	1·87	2·05	2·55	3·03	23	4608	1·83	2·07	2·60	3·02
51	5919	1·70	1·87	2·40	2·85	22	4578	1·91	2·15	2·65	3·07
50	5850	1·54	1·75	2·20	2·72	21	4548	2·00	2·20	2·70	3·15
49	5783	1·37	1·57	2·07	2·57	20	4517	2·05	2·28	2·77	3·20
48	5720	1·25	1·45	1·95	2·42	19	4488	2·09	2·30	2·82	3·27
47	5658	1·12	1·35	1·85	2·37	18	4459	2·14	2·35	2·90	3·32
46	5596	1·02	1·25	1·75	2·25	17	4437	2·20	2·40	2·95	3·40
45	5538	0·93	1·17	1·65	2·17	16	4404	2·25	2·50	3·00	3·47
44	5481	0·87	1·10	1·57	2·09	15	4377	2·30	2·54	3·06	3 53
43	5427	0·83	1·07	1·53	2·05	14	4349	2·34	2·57	3·15	3·58
42	5373	0·82	1·05	1·52	2·02	13	4323	2·38			
41	5321	0·815	1·045	1·52	2·02	12	4296	2·43			
40	5270	0·82	1·05	1·52	2·03	11	4271	2·48			
39	5221	0·825	1·05	1·53	2·05	10	4245	2·52			
38	5172	0·84	1·07	1·57	2·07	9	4221	2·57			
37	5120	0·86	1·09	1·58	2·08	8	4197	2·63			
36	5085	0·88	1·10	1·60	2·10	7	4174	2·69			
35	5043	0·91	1·15	1·65	2·14	6	4151	2·75			
34	5002	0·95	1·20	1·70	2·20	5	4131	2·80			
33	4963	1·00	1·25	1·77	2·27	4	4106	2·87			
32	4924	1·07	1·35	1·83	2·35						

These curves are shown on the diagram, fig. 4, and it will be evident that within the limits of error of observation they are parallel to one another. Had the same intensity of original light been used in these experiments as with the standard curve (viz., had the intensity of the light at D been equal to 1 amyl acetate lamp), Curve I. would have been superposed on Curve V., which is the curve of the standard extinction. The intensity of the D light actually employed was very closely $\frac{1}{17}$ of an amyl acetate lamp. The curves being very nearly parallel to one another show that the light has to be proportionally reduced throughout to cause extinction, and further that the smaller the angular aperture of the disc the less the reduction in the original intensity has to be. A reference to the results of further experiments, to be shortly given, will show that their distance apart has been correctly derived. As it might happen that the extinction of a point of light differed from one of very sensible size, a disc ·012 inch and subtending an angle of only 1′ 29″ was observed. The intensity of the spectrum had to be much increased, and therefore the results given in Table V. can only be regarded as showing that even in this case parallelism to the other curves is obtained. The curve (IV.) is also shown in fig. 4.

Fig. 4.

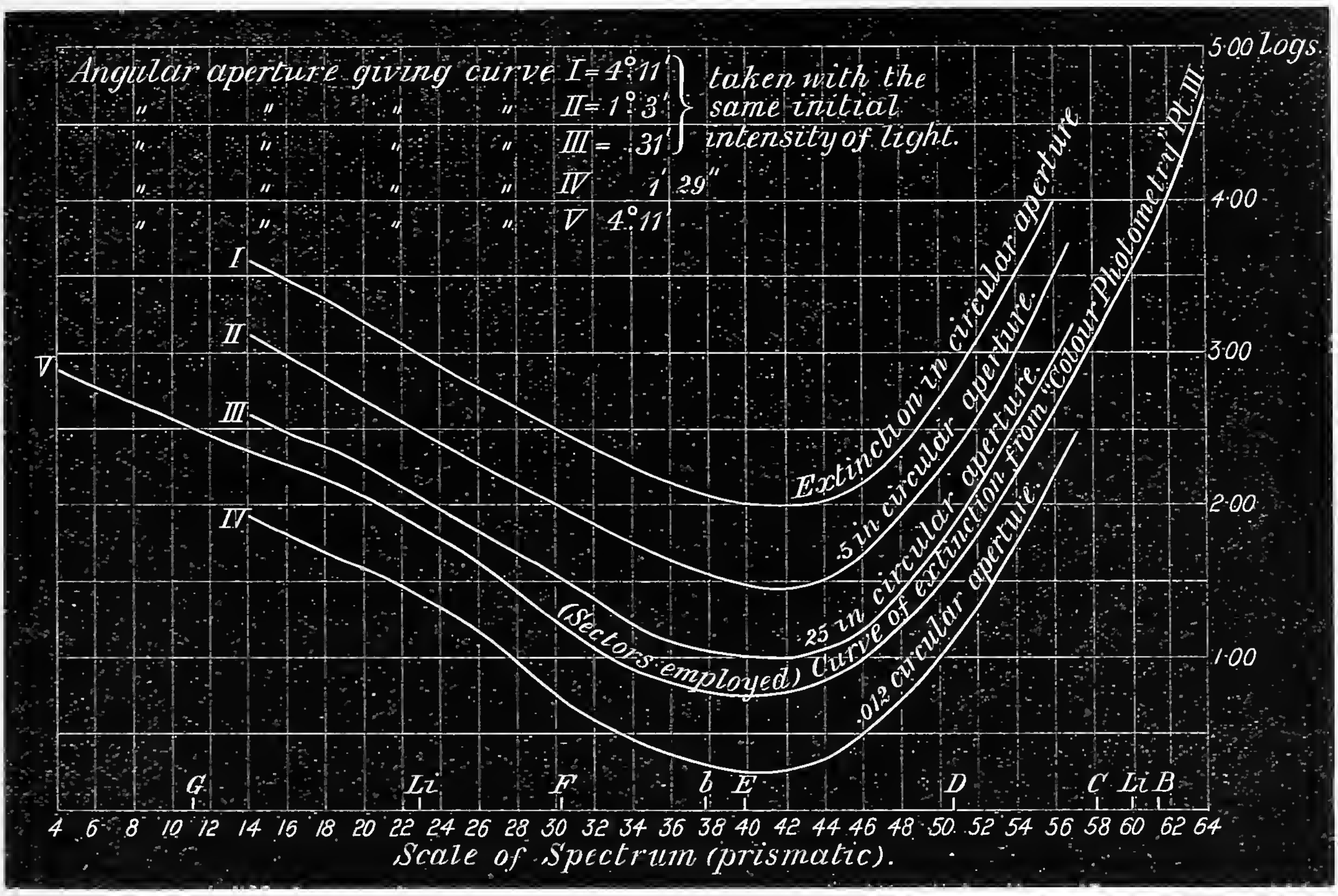


Table V.—Readings and logs with ·012 inch (1′ 30″) aperture.

Standard scale number.	·012-inch (1′ 30″) aperture readings.		Standard scale number.	·012-inch (1′ 30″) aperture readings.	
	Degrees.	Logs.		Degrees.	Logs.
58·57	60	3·50	34·15	295	1·46
56·35	90	3·23	31·93	280	1·59
54·13	140	2·80	29·71	260	1·76
51·91	190	2·37	27·49	240	1·94
49·69	250	1·85	25·27	210	2·19
47·47	275	1·64	23·05	185	2·41
45·25	298	1·40	20·83	175	2·50
43·03	310	1·33	18·61	155	2·68
40·81	320	1·25	16·39	135	2·84
38·59	320	1·25	14·17	127	2·94
36·37	314	1·30			

Some twenty-four series of observations with the spectrum were made with discs of different sizes, and all gave similar results to those tabulated, and hence we may fairly take it as proved that whatever the size of illuminated disc may be, all rays follow the standard curve of extinction, that is to say, that if with a disc of one size a green ray has to be reduced to $\frac{1}{1000000}$ of its original intensity to fail to stimulate the retina to cause a sensation of light, and with another smaller disc observed at the same distance to only $\frac{1}{500000}$, then every ray in the spectrum, when illuminating the same size of discs, will have to be reduced in the same proportion.

6. *The most Sensitive Condition of the Retina.*

In all observations of extinction, the greatest care must be taken to obtain the most sensitive condition of the eye, and I have found that it is useless to attempt any readings until the eye has been placed in darkness for at least twelve minutes. It is well to vary the plan of reading by first reducing the light till it disappears, and then to note the exact point when it reappears. The latter would be the best plan to use entirely, since the eye is in darkness till the very last part of the observation, were it not for the fact that, in noting the reappearance of the light, we are noting something less defined than when we are noting its disappearance. The combination of the two methods has been found to be the most satisfactory plan. A very great difficulty also often arises in keeping the axis of the eye employed in a line with the object observed, particularly when it is of very small angular aperture. It has been found advantageous to have a feeble light which can be flashed momentarily on the aperture to guide the eye just previous to the disappearance of the light. This is more particularly the case when it is desired to extinguish with the centre of the retina, as is the case in these observations. The intrinsic light in the eye is also sometimes difficult to deal with, but by judiciously recognizing the fact that it exists, and by giving proper intervals between each observation, one is enabled to surmount this difficulty. Again, a healthy state of mind and body is most essential when making observations, as the sensitiveness of the eye largely depends on it.

7. *Law Connecting the Angular Aperture with the Extinction.*

The next investigation carried out, was to ascertain if any law connected the angular aperture of the object observed with the diminution of the intensity of the light which was required to cause invisibility. For the purpose, a large number of diaphragms of very differing apertures were inserted in front of the ground glass (fig. 3). For the sake of plotting, in the first instance, and as they give the most rational scale, the diameters of the discs were expressed in powers of 2, thus $\frac{1}{2}$ inch, which is 2^{-1}, is used on the scale of abscissæ as -1 ; $\frac{1}{4}$ as -2, and so on—all diameters not being expressed in exact powers of 2 being calculated out in the ordinary way.

The following are the values, in inches, of the apertures used, and in powers of 2, and the angles they subtended at the distance from which they were observed :—

Diameter in inches.	Angles subtended.	Value in powers of 2.
2·0	4° 11′ 0″	+1·0
1·50	3 8 0	+0·6
0·94	1 57 0	−0·09
0·725	1 30 0	−0·48
0·525	1 5 0	−0·93
0·42	0 52 35	−1·25
0·35	0 43 43	−1·52
0·30	0 37 33	−1·74
0·17	0 21 17	−2·56
0·086	0 10 46	−3·56
0·036	0 4 30	−4·81
0·012	0 1 30	−6·40

These diaphragms were placed in front of the ground glass, and the light from the discs thus formed, extinguished. In the first set of experiments, the pure colours of the spectrum were employed; whilst in the others, ordinary lamp light and lamp light screened with different colour glasses or solutions were used, and identical results were found in all cases. The following tables are made from the mean readings, and the diagram shows them plotted :—

Fig. 5.

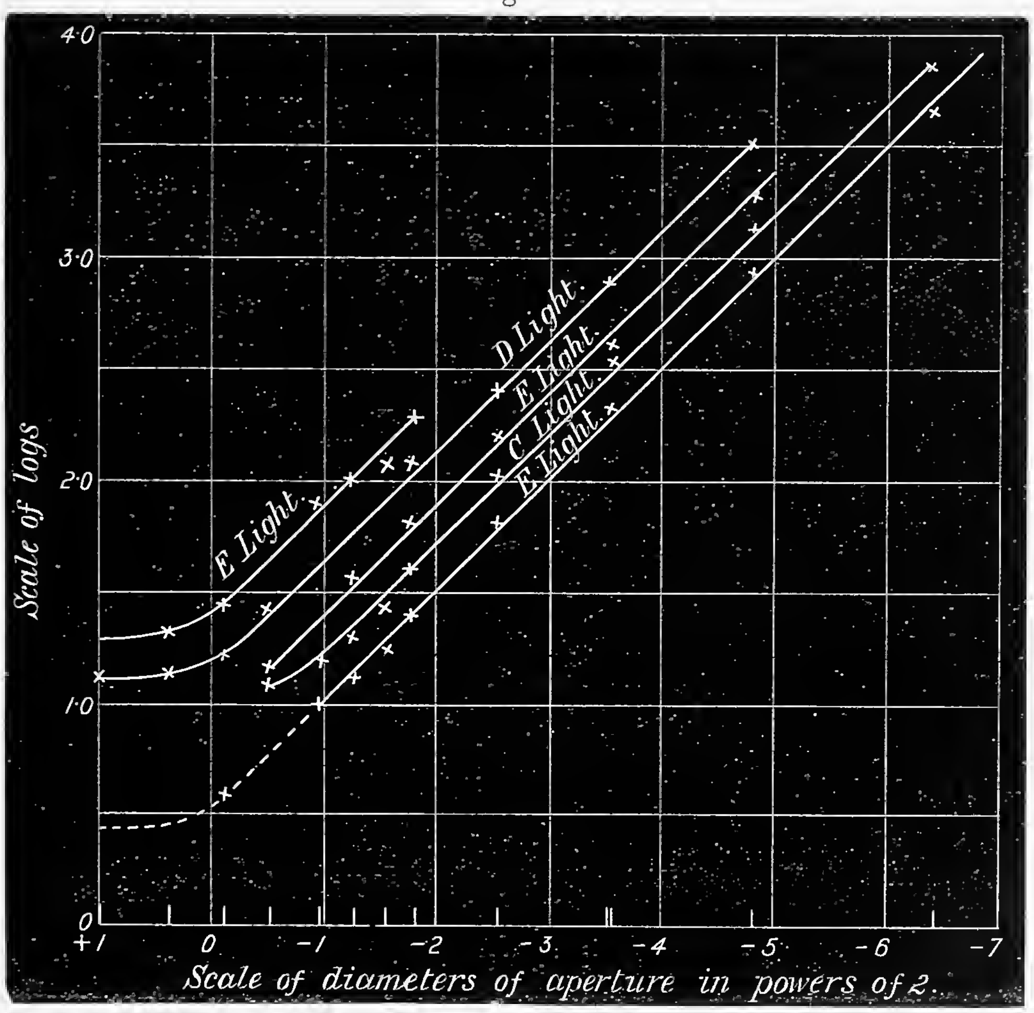


TABLE VI.—Extinctions with Apertures of Different Sizes.

E light.

Diameter.	Diameter in powers of 2.	Reading.	Log.
inches.			
0·725	−0·48	331	1·17
0·42	−1·25	285	1·55
0·30	−1·74	255	1·81
0·17	−2·56	209	2·20
0·086	−3·56	163	2·60
0·036	−4·81	85	3·27

C light.

Diameter.	Diameter in powers of 2.	Reading.	Log.
0·725	−0·48	340	1·076
0·525	−0·93	325	1·20
0·42	−1·25	312	1·30
0·35	−1·52	300	1·42
0·30	−1·74	275	1·63
0·170	−2·56	230	2·02
0·086	−3·56	178	2·57
0·036	−4·81	100	3·14
0·012	−6·40	15	3·85

E light.

Diameter.	Diameter in powers of 2.	Reading.	Log.
2·00	+1·0	315	1·29
1·50	+0·6	315	1·29
0·94	−0·09	303	1·39
0·525	−0·93	245	1·89
0·42	−1·25	234	1·98
0·35	−1·52	220	2·11
0·30	−1·74	200	2·28

E light.

Diameter.	Diameter in powers of 2.	Reading.	Log.
inches.			
0·94	−0·09	above 360	
0·725	−0·48	,,	
0·525	−0·93	352	1·00
0·42	−1·25	335	1·12
0·35	−1·52	323	1·22
0·30	−1·74	303	1·39
0·170	−2·56	255	1·81
0·086	−3·56	195	2·32
0·036	−4·81	123	2·94
0·012	−6·40	39	3·67

D light.

Diameter.	Diameter in powers of 2.	Reading.	Log.
2·00	+1·0	335	1·12
1·50	+0·6	332	1·13
0·94	−0·09	325	1·20
0·725	−0·48	300	1·42
0·30	−1·74	223	2·08
0·170	−2·56	185	2·40
0·086	−3·56	132	2·87
0·036	−4·81	55	3·51

The indications here given are that (fig. 5) the curves with apertures less than $1\frac{1}{2}$ inch diameter become straight lines, all of which are parallel, and it is somewhat remarkable that from that point the intensity of a light which will be just extinguished with a certain diameter of aperture may be increased 10 times, and yet be invisible when an aperture with $\frac{1}{4}$ of that diameter is employed; if the intensity of the light be increased 100 times, we have only to diminish the diameter of the aperture to $\frac{1}{16}$, and it will again disappear. (It may be noted that in the lowest curve the dotted portion is taken from the two top curves.) The equation connecting the two has the form of $I = x^m$, where m is determined by the slope of the line and the log 2.

We shall see further on that connection between the intensity of the light just necessary to fail to produce the sensation of colour takes the same form, the exponential coefficient being changed. The connection between star magnitudes and the intrinsic brightness of a measurable disc of the same brightness and colour will have the same form.

8. *The Extinction Dependent on the Least Diameter of the Aperture.*

There is a peculiarity in the result, in that it looks as if the diameter of the aperture and not its area, determined the intensity of the light required to be extinguished. A very ready way of ascertaining this was to use an adjustable slit, and to take the extinctions with varying apertures. In such a case the length of the slit would remain constant, whilst the width alone would vary. If it were the latter that determined the extinction there can be but little doubt that the area of the object played at all events a secondary part; for in the case of the circular apertures the areas would be as the squares of the diameters, whilst in the latter they would be as the width of the aperture. A slit from a spectroscope was employed which increased in aperture $\frac{1}{50}$ inch for each complete revolution of the screw. The head of the screw was divided into twenty-five divisions, so that five divisions on the screwhead opened the aperture ·004 inch, the smallest aperture employed was ·00155 inch and the largest ·06555. Intermediate apertures were also used, and extinction of white light and red light noted.

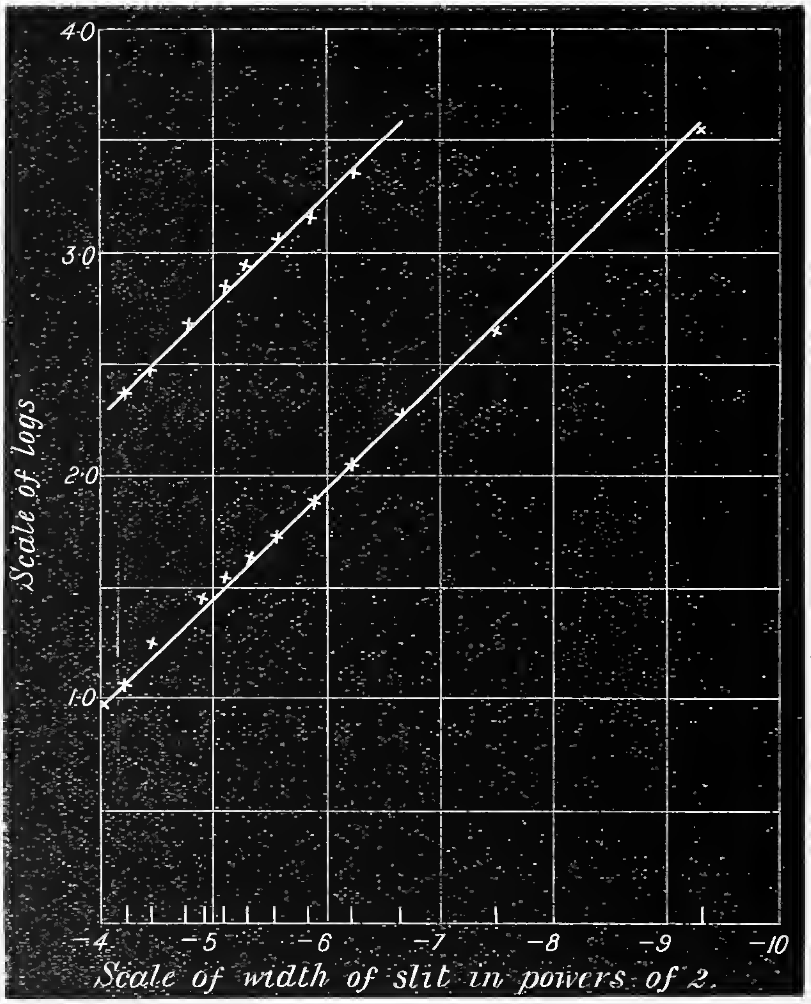
The following table gives the results of the measures, the apertures being put in powers of 2 :—

TABLE VII.

White light.				Red light.			
Absolute width in inches.	Width in powers of 2.	Readings.		Absolute width in inches.	Width in powers of 2.	Readings.	
		Degrees.	Logs.			Degrees.	Logs.
0·06155	−4·02	350	0·99	0·05355	−4·22	192	2·35
0·05355	−4·22	342	1·06	0·04555	−4·46	176	2·45
0·04555	−4·46	323	1·23	0·03755	−4·73	154	2·68
0·03355	−4·90	298	1·44	0·02955	−5·08	135	2·84
0·02955	−5·08	285	1·55	0·02555	−5·29	124	2·94
0·02555	−5·29	275	1·63	0·02155	−5·53	110	3·05
0·02155	−5·53	267	1·72	0·01755	−5·83	98	3·16
0·01755	−5·83	250	1·85	0·01355	−6·20	76	3·35
0·01355	−6·20	228	2·04	0·00955	−6·67	50	3·57
0·00955	−6·67	202	2·26				
0·00555	−7·49	156	2·67				
0·00155	−9·33	54	3·53				

The results are plotted diagrammatically in fig. 6, from which it will be gathered that again the curve is a straight line in each case, and that the ordinates bear the same relation to the abscissæ that they did in case of circular apertures. We may therefore presume, with a considerable degree of certainty, that it is the width of the aperture, horizontal or vertical (for the experiments were repeated in both directions), that governs the extinction. These results, it will at once be seen, have an important bearing on spectroscopic work, and the invisibility of lines when the light is feeble.

Fig. 6.



Taking into consideration the extinction curve of the spectrum, and these results, we can see how the green lines of a feeble spectrum will be the first to be seen (perhaps colourless), whilst others, though present, will fail to be seen except with a very wide slit.

A further experiment was made which confirmed the previous measures. The extinction of the light from a circular, a square, and a rectangular aperture of the same area was made. The circular aperture had a diameter ·94 inch, the sides of the square were ·84 inch, and of the oblong 1·68 × ·42 inch. In addition, an oblong aperture ·84 × ·42, exactly half the latter, was also used.

The following are the results of the extinction, and in the last column are given the results that would have been obtained from the curves already described:—

Table VIII.

Aperture.	Width of powers of 2.	Readings.		Logs from diagram.
		Degrees.	Logs.	
Circular disc, ·94 inch diam.	− ·09	234	1·98	1·98
Square, ·84 inch side	− ·25	216	2·14	2·15
Rectangle, 1·68 × ·42 inch	− 1·25	152	2·69	2·65
Rectangle, ·84 × ·42 inch	− 1·25	154	2·68	2·65

Remarks on this table seem unnecessary, as they so plainly indicate the guiding factor in the extinction.

This, perhaps, is one of the most curious results that have been obtained, for it is hard to conceive that the area of the retina impressed should not be a factor. The experiments clearly show that the estimate of small intensities of light by their effect on the light-perceiving apparatus is not a simple matter. The extinction of comparatively larger areas of light is most instructive. The light from a square, or a disc, or an oblong, just before extinction, is a fuzzy patch of grey, and appears finally to depart almost as a point. This can scarcely account for the smallest width of an illuminated surface determining the intensity of the light just not visible; but it tells us that the light is still exercising some kind of stimulus on the apparatus, even when all sensation of light is gone from the outer portions. The fact that the disappearance of the image takes place in the same manner, whether viewed centrally or excentrically, tells us that this has nothing to do with the yellow spot or fovea, but is probably due to a radiation of sensation (if it may be so called) in every direction on the retinal surface. Supposing some part of the stimulus impressed on one retinal element did radiate in all directions over the surface of the retina, the effect would be greatest in the immediate neighbourhood, and would be inappreciable at a small distance, but the influence exerted upon an adjacent element might depend not only on its distance, but also upon whether it was or was not itself exerted independently. Following the matter out further, we should eventually arrive at the centre of an area, being the part which was the recipient of the greatest amount of the radiated stimuli, and consequently that would be the last to disappear. With a slit aperture, the slit is visible till extinction is very nearly executed, but it finally merges into a fuzzy spot at the moment before it finally fails to make any impression of light.

9. *Extinction of Light Excentrically.*

A further investigation into the extinction of light at different angular distances from the centre of the eye was attempted. The experiments are of a very difficult nature, and it requires long practice to enable a satisfactory series to be made.

The method adopted was to place pins with heads painted with Balmain paint at every 5° from the central line joining the illuminated aperture and the position occupied by the eye. The paint was very feebly phosphorescent, and only just sufficient to fix the centre of the eye at the required angle from the object. The results of two experiments, red and white light (paraffin), at 10° are given. It appears from these that at this angular distance the extinction of all light from the red takes place when the light is about one-third brighter than is required for the centre of the eye. With the paraffin light it is somewhat less. With green light about E, and with blue at the lithium line, the necessary reduction of the light is greater than for the centre of the eye, a result already shown in "Colour Photometry," Part III.

TABLE IX.

Aperture.	Angle.			2^{-x}.	Red light.		White light.	
					Direct.	10° from axis.	Direct.	10° from axis.
	°	′	″					
0·940	1	57	0	0·09	275	255	305	290
0·724	1	30	0	0·48	252	230	270	265
0·525	1	5	0	0·93	225	204	265	240
0·420	0	52	35	1·25	217	195	252	230
0·350	0	43	43	1·52	195	174	235	220
0·300	0	37	33	1·74	185	162	215	200
0·170	0	21	17	2·56	152	125	174	157
0·086	0	10	46	3·56	93	75	118	105

There is a further falling-off of sensitiveness at greater angles than those shown in the tables, but the extinction is very difficult to make with certainty.

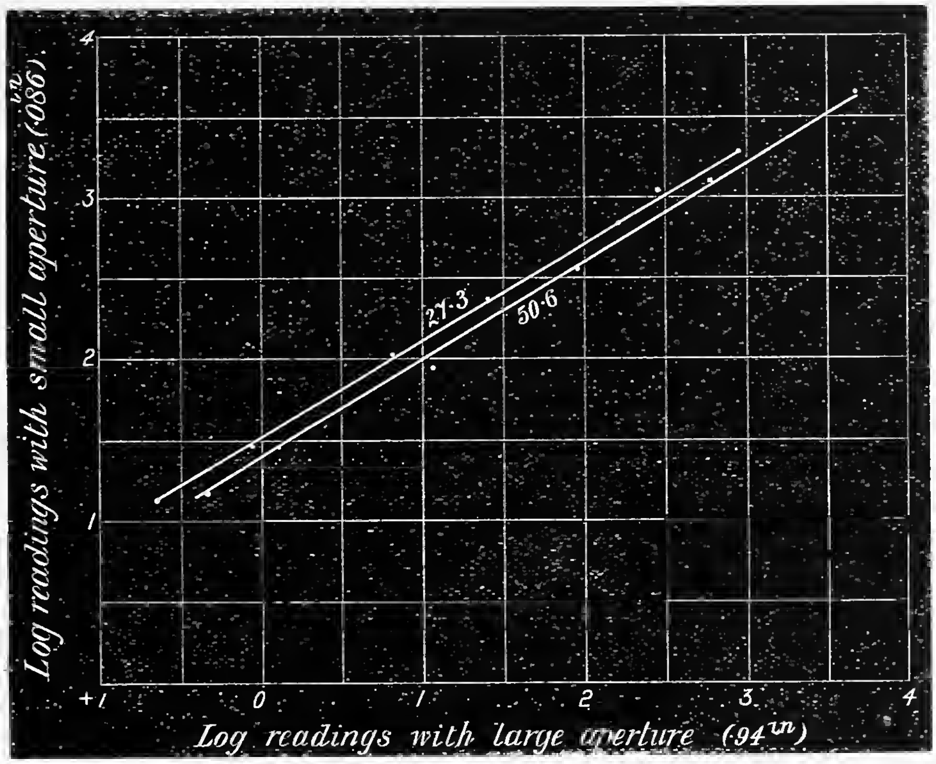
10. *Luminosity of the Light coming through different Apertures.*

The next point investigated, but without any great degree of detail, was the comparative luminosities of the same light coming through two apertures of different diameters. The method adopted was as follows:—The ground glass was illuminated uniformly with the light to be tested, and two apertures cut in a black mask were placed in contact with it, as shown. Sectors were placed close behind the larger aperture, and rotated with angular apertures of any desired amount. In front of the slit of the spectrum the annulus was placed so that a regular diminution of the light could be effected. The sectors having been set at 90°, the light coming through the bigger aperture was diminished to half. As the light coming through the small aperture is extinguished long before that coming through the larger one, there must be some intensity of light when the two apertures will appear equally bright to the eye in the extinction box. The light

coming through the slit is, therefore, diminished till the two appear equally bright. The diminution of light is noted, that coming through the larger aperture being diminished twice as much as that coming through the smaller. The sectors are again set at 45°, and the same procedure adopted as before.

Table X. gives the measurements thus made, and fig. 7 shows them diagrammatically. The ordinates are the intensities of light of the large aperture. These are derived from the value of diminution of the light falling on the ground glass, together with the reduction due to the sectors. The latter is converted into the degrees of the annulus and added to that by which the light has been diminished before falling on the plate. The abscissæ show the values of the reduction of the light on the smaller aperture. Both the annular values are shown as logarithms. Again, the resulting curve is for a large part of its length a straight line. Each aperture has its own

Fig. 7.



inclination and is determined by the extinction values of the two apertures. In making these determinations the eye has to judge the brightness of very dissimilar sizes of area, and it might be thought that this fact would present an almost insuperable difficulty in making very accurate measures. As a matter of fact, it was not so ; the greatest difficulty was encountered in those cases when the light of the large aperture was so diminished that it became colourless, whilst the other had very nearly its original tint. The red was perhaps the hardest to judge on that account; the other colours did not present any great difficulty. One of the curious phenomena encountered in these measures at times was a distinct scintillation of the light emitted by the small aperture. Sometimes this was perplexing, but never to the extent to render the comparisons uncertain.

TABLE X.

Sector in degrees.	Equivalent values of annulus.	Scale number, 27·3.				Scale number, 52·8.			
		Readings of apertures.				Readings of apertures.			
		S.	L.	Logs.		S.	L.	Logs.	
				S.	L.			S.	L.
180	0	..	..	..	..	..	..	..	..
90	35	90	125	3·23	2·92	100	135	3·14	2·84
45	70	140	210	2·80	2·19	185	255	3·42	1·80
22·5	105	200	305	2·28	1·38	270	380	1·68	·73
11·25	140	230	370	2·02	·82	305	445	1·38	·17
5·6	175	295	470	1·46	− ·04	310	520	1·33	− ·47
Extinction	200	325	525	1·21	− ·51	340	540	1·08	− ·64

Sector in degrees.	Equivalent values of annulus.	Scale number, 44.				Scale number, 50·6.			
		Readings of apertures.				Readings of apertures.			
		S.	L.	Logs.		S.	L.	Logs.	
				S.	L.			S.	L.
180	0	0	0	4	4	40	40	3·66	3·66
90	35	40	75	3·66	3·35	110	145	3·05	2·75
45	70	110	180	3·05	2·45	170	240	2·54	1·94
22·5	105	200	305	2·28	1·38	240	345	1·94	1·03
11·25	140	260	400	1·76	·56	300	440	1·42	·22
5·6	175	310	485	1·33	− ·17	330	505	1·16	− ·34
Extinction	200	350	550	·99	− ·73	340	540	1·08	− ·64

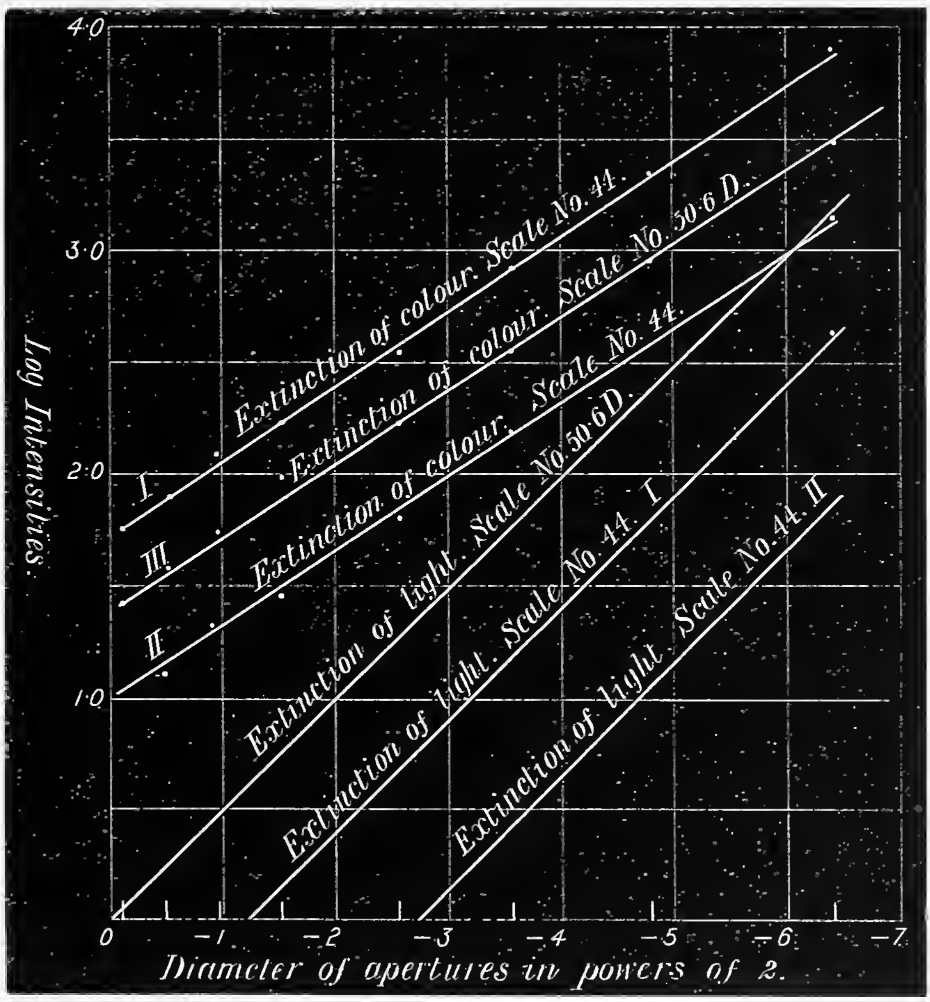
N.B.—S. and L. refer to the small and large aperture respectively. From fig. 5 it is found that the extinction value of the larger aperture, ·94 diameter, requires 200° more of the annulus to extinguish it than the smaller aperture ·086 diameter. This accounts for the last line in the table.

Fig. 7 shows the logarithm of the readings of S. N. 27·3 in the blue and S. N. 50·6 in the orange (D.). The other series of readings are so close to the above that they are omitted.

11. *Extinction of Colour from Spots of different Diameters.*

Following on the extinction of light came the investigation of the loss of colour from areas of varying angular aperture. In Part III. of "Colour Photometry," a method was described of estimating the point where all colour disappeared. In that paper no reference was made to the size of the area under examination, it being greater than the 4° diameter, and all apertures greater than this, as we have seen, behave alike. In this investigation a somewhat different method was adopted. Two apertures were placed side by side and a very feeble white light from the arc light

Fig. 8.



was caused to illuminate one aperture, while the colour under examination filled the other. The two were darkened together and the point of diminution where they perfectly matched in tint was taken as the point at which the colour of the latter vanished. It was very necessary to make the apertures of equal area and equally bright, as, if not, the measurements became more difficult. A large number of different rays were examined, with the centre of the retina, for colour persistency in this way, but the following will suffice to show that the colour extinction does not follow that of the light extinction when regard is had to the sizes of the apertures employed. In this case the intensity of the light to cause a loss of colour has to be increased tenfold, whilst the aperture is diminished to $\frac{1}{8}$ in diameter. In the extinction of light the same increase in intensity only requires a diminution to $\frac{1}{4}$ the diameter.

This seems to show that the stimulus required to produce colour is of a different order from that required to produce light. These experiments apply equally to all colours, and therefore to the sensations producing them, and if, as in the HERING theory, there is a black-and-white sensation co-ordinate with the red-green and yellow-blue sensations of light, the extinction of the white sensation ought to follow the same curve as that of the other sensations.

The following table gives examples of the extinction of colour. (See fig. 8.)

TABLE XI.

Diameter of aperture.	Diameter in powers of 2.	I.		II.		III.		Remarks.
		Reading.	Log.	Reading.	Log.	Reading.	Log.	
0·94	− ·09	260	1·76	350	0·99	300	1·42	Extinction of ·012 aperture in I., II., III. were in Logs. 2·62, 1·85, and 1·25 respectively; this would make the extinction of ·94 aperture $\frac{1}{200}$, that of the colour extinction ·2 for I., II., and for III., $\frac{1}{28}$. In Colour Photometry, Part III., they were $\frac{1}{130}$ and $\frac{1}{26}$ respectively.
0·724	− ·48	245	1·89	335	1·12	280	1·59	
0·525	− ·93	220	2·11	310	1·33	260	1·76	
0·35	−1·52	210	2·19	295	1·46	235	1·98	
0·17	−2·56	170	2·54	255	1·80	205	2·235	
0·086	−3·56	125	2·925	210	2·19	170	2·54	
0·036	−4·81	75	3·355	155	2·66	120	2·97	
0·012	−6·4	10	3·91	100	3·14	60	3·48	

Nos. I. and II. are the same ray (44 on the scale), but with different intensities. No. I. was measured by myself, and No. II. by Corporal ATTEWELL. No. III. was read by myself, and was D in the spectrum or scale No. 50·6. It may be remarked that with the small apertures the extinction of colour in the red was impracticable, as the extinction of light and colour took place together, as it should do according to other experiments.

12. *Colour Fields and Perimeters.*

An enquiry was next undertaken as to the variation in the extent of colour fields under different conditions of light. The question of colour fields is one regarding which much has been written, and experiments on the subject have been numerous, but with one or two exceptions it is believed that these latter have been carried out with pigment colours, which, from their nature, are impure. In the ordinary methods pursued the knowledge that is gained is slight compared with the trouble involved, and the colours selected have been based on the assumption that HERING's theory of colour vision is one that has been thoroughly established; experiments from which a great deal more may be learnt have been neglected or overlooked. I refer especially to the measurements of colour fields, where the colours used are pure spectrum colours. In these the colour may be isolated and viewed against a black background, as for instance, by throwing a spot of light of any desired colour on a white surface in a dark room. This is a very different condition to that which obtains in the ordinary procedure, when the retina receives not only the colour of the pigment but also is illuminated by extraneous light. In the experiments to be described, two kinds of perimeters were employed. One was the ordinary form but modified for use in a dark room. The arc, which subtended a semicircle, was internally coated with white, on which degrees were marked at the boundaries, and just below the eye was a small mirror on a ball and socket joint, which, by means of an arm, would cause a beam of light falling on it to be cast in any direction desired. Thus it could be caused to travel along the arc, which might be placed at any angle with the vertical. The light employed was that coming from the spectrum of an electric arc light, the crater of the positive pole being the source from which the spectrum was formed. The colour patch apparatus was employed to get a surface of monochromatic light, as described in Part II. of "Colour Photometry." A spot of light of any desired form or size was obtained by the plan described in my recent paper in the 'Proceedings of the Royal Society' "On the Formation of Monochromatic Images." The light issuing from the slit in the spectrum could be altered in intensity (1) by closing or opening that slit, (2) by placing the annulus already described in front of it, (3) by closing the slit of the collimator, (4) by using rotating sectors in front of either slit.

In the second form of perimeter a hollow white hemisphere made of "papier mâché" was employed. The centre of the surface was pierced with a circular aperture some $1\frac{1}{2}$ inches in diameter. This aperture was closed by a doubly-ground glass, and outside the shell apertures of any desired shape or dimensions could be placed in contact with the ground glass. The colour patch apparatus was caused to throw the patch of colour on to the ground glass, and when the last was removed the patch of white that the combining lens cast when the whole spectrum was uncovered fell upon the eye when placed at the centre of the hemisphere, thus insuring that every ray was equally received on the pupil when the ground glass was again interposed. It may be stated here, once for all, that when light falling on the ground glass was measured, by placing a white card in its place and balancing it with an amyl-acetate lamp, it was found that the brightness of the ground glass as seen from the centre of the hemisphere was within a very small fraction, twelve times that which was reflected from the white card.

The hemisphere was furnished with a chin and cheek rest, which would move round a vertical axis. It was divided internally into degrees. The eye was directed to any part of the surface by means of a small phosphorescent bead at the end of a stick; and a small electric lamp, which could be switched on by a simple movement of the hand, gave light sufficient to read the position occupied by the bead at any desired instant. The intensity of the light illuminating the ground glass was altered by any of the four methods mentioned above. The annulus was usually employed to effect the alteration, and it could be rotated at the will of the observer by a long handle attached to the rack and pinion motion of the rotating gear.

13. *Similarity of Fields for Different Colours.*

The order in which the experiments were made will not be followed, for, as a matter of fact, amongst those to be first described some were among the latest, and others among the earliest made. It was essential to know whether the fields for each colour were of the same form when the illumination was so adjusted that one point in a field of one colour coincided with one point in the field of a different colour. The following two sets of observations made by myself, and the succeeding ones made by one of my assistants (W. B.), will give the answer to the inquiry.

An aperture of ·525 inch subtending an angle of 2° 30′ was inserted behind the ground glass, and the light falling on the eye when D was the ray selected, was 4·5 amyl-acetate lamps at 1 foot. (In future this light will be designated as AL, and this particular illumination would be 4·5 AL.)

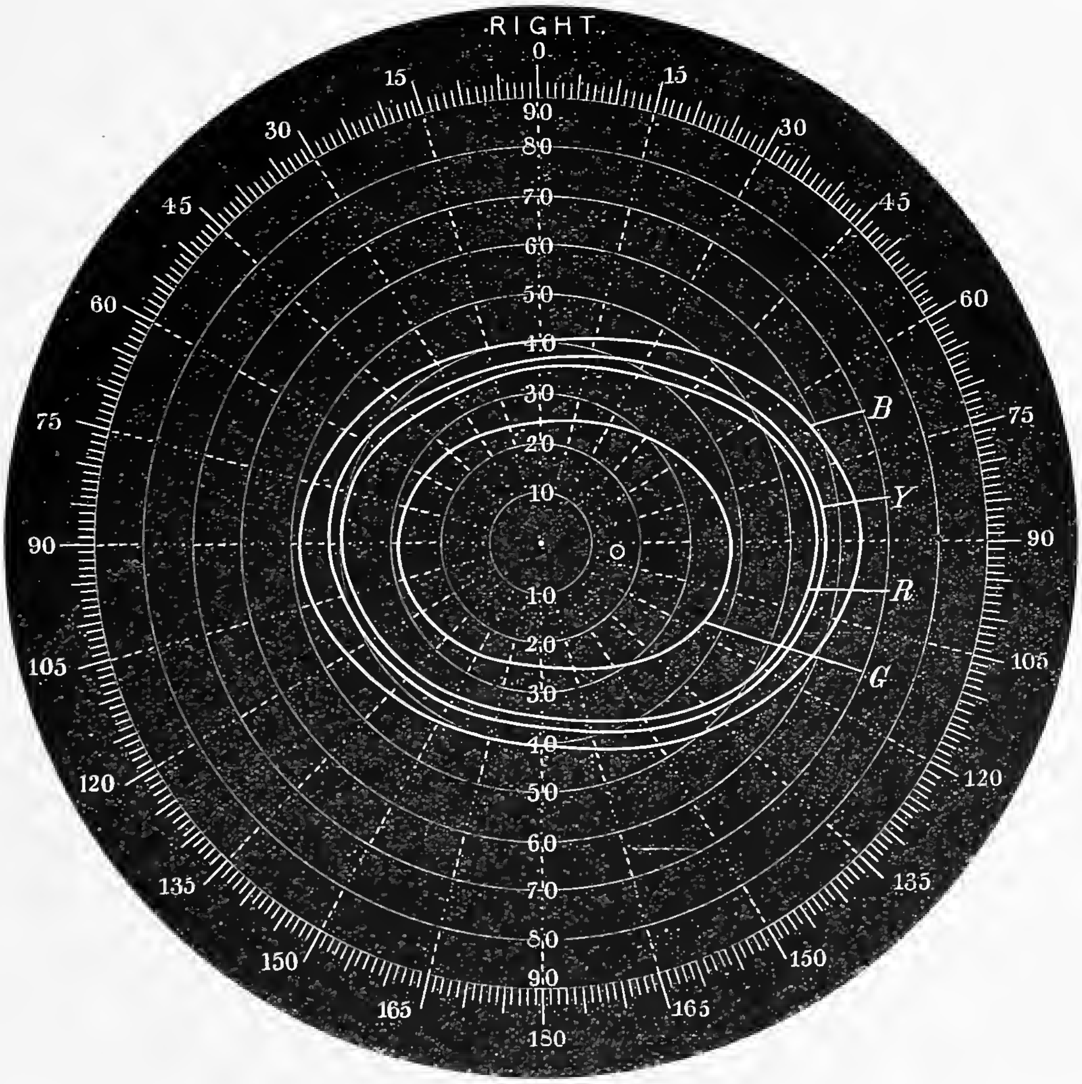
The following rays were used to illuminate the aperture: red lithium (λ 6705) D (λ 5892), a ray having the standard scale number ·36 (λ 5085), and the blue lithium ray (λ 4603). These had respectively the luminosities of ·3, 4·5, 2·1, and ·4 AL.

The measures were made with the right eye (see fig. 9).

TABLE XII.

Angle of field in degrees.	Extent of fields in degrees.			
	Red Li.	D.	SN 36.	Blue Li.
0	35	36	24	40
30 T.	37	40	27	47
60 T.	47	50	33	57
90 T.	55	57	38	65
120 T.	51	53	36	60
150 T.	41	43	29	50
180	34	36	25	40
150 N.	35	36	26	40
120 N.	37	38	27	45
90 N.	40	42	28	49
60 N.	38	40	27	45
30 N.	34	36	25	42

Fig. 9.



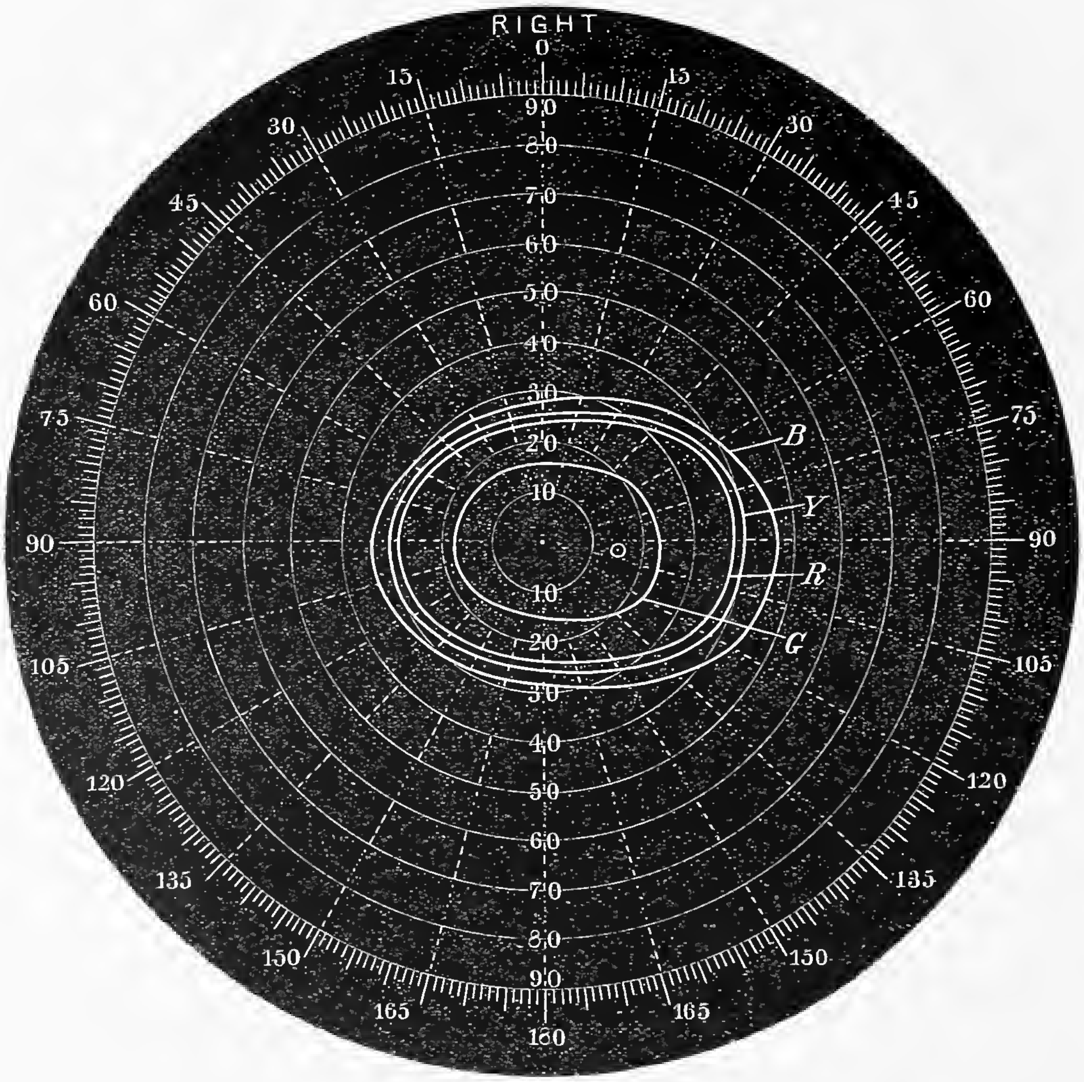
In the following observations the illumination by the D light was much reduced, being only ·23 AL, and for certain reasons, which will be apparent, the ray at scale number 41·7 was substituted for that at scale number 36. The other three rays were the same as before (fig. 10).

Table XIII.

Angle of field in degrees.		Extent of fields in degrees.			
		Red Li.	D.	SN 41·7	Blue Li.
0		23	25	15	28
30		28	27	16	32
60		35	37	21	40
90	T	38	40	23	47
120		35	37	22	42
150		27	30	18	35
180		23	25	16	28
150		25	26	16	29
120		28	30	18	32
90	N	29	30	18	34
60		26	27	17	30
30		23	25	16	28

Taking these sets of observations separately, the diagrams show that the fields for properly selected luminosities are evidently the same, the D and red lithium being very close to one another. If we compare the fields for the D and red lithium rays in the second table with that of the field for the green (S.N. 36) in the first table, we shall see that they are practically identical.

Fig. 10.



The next measurements were made by my assistant, and, since, as before stated, his colour fields differ considerably from my own, the confirmation obtained by his measurements appears very conclusive. They were made for illustrating a different part of the research, but they will be given here and referred to subsequently. Two places in the spectrum were selected, such that the two rays when combined would give white light, the white being that of the electric light, which is indistinguishable from the sensation produced by the coloured rays when falling on the peripheral portions of the retina. The first positions selected were in the red and green, at λ 6500 and λ 5002, corresponding to the scale of the spectrum with the numbers 57·8 and 34. The relative luminosities of the rays reaching the eye were 225 and 270 respectively.

Two other positions were chosen in the yellow-green at (λ 5614), and in the blue (λ 4603), corresponding to the scale numbers of the spectrum 46·3 and 22·8. The relative luminosities of the rays transmitted to the eye were 96·5 and 21·5 respectively.

The colour field for each of these four colours was taken with the left eye, and the following table shows the results (fig. 11):—

TABLE XIV.

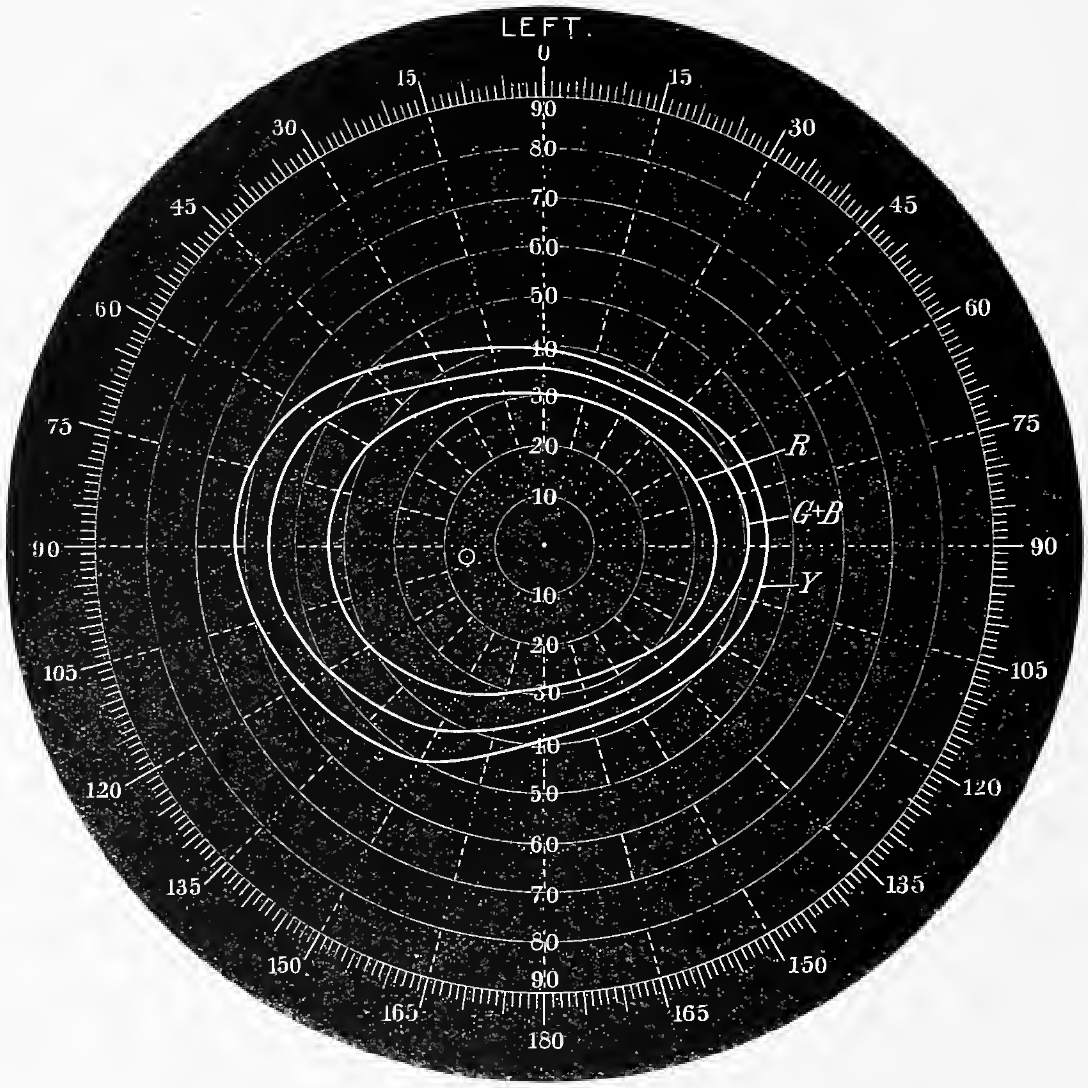
Angle of field in degrees.	Extent of fields in degrees.			
	Red.	Green.	Yellow-green.	Blue.
0	30	35	39	36
30 N	28	34	37	35
60 N	31	37	42	38
90 N	33	40	44	41
120 N	32	36	42	37
150 N	28	34	38	34
180	29	35	39	36
150 T	34	43	50	44
120 T	40	50	57	50
90 T	43	55	62	55
60 T	41	51	56	50
30 T	33	38	43	39

Here we have two fields, the green and the blue, which are practically identical, showing that the limits of the boundaries are not affected by the hue, though, of course, the illumination is very different in the two cases. Attention must here be drawn to the fact that, though, according to HERING'S theory, the fields of the dissimilation colours ought to be both external, or else both internal, to the fields of the assimilation colours, they differ in each pair, and the frequency of similar want of accordance has been very generally met with.

14. *Fields of Impure or Mixed Colours.*

When considering the question of the fields of mixed colours, such as those produced by pigments, it became apparent that a crucial test as to their efficiency might be made by mixing colours of the spectrum together to imitate some single spectrum colour, and, after making the mixture of the same luminosity, to compare the fields. With this in view, a red and green, near E, were mixed together to match the D light in hue and in intensity. The fields for each colour, including D, were taken, as also was that of the mixed colours.

Fig 11.



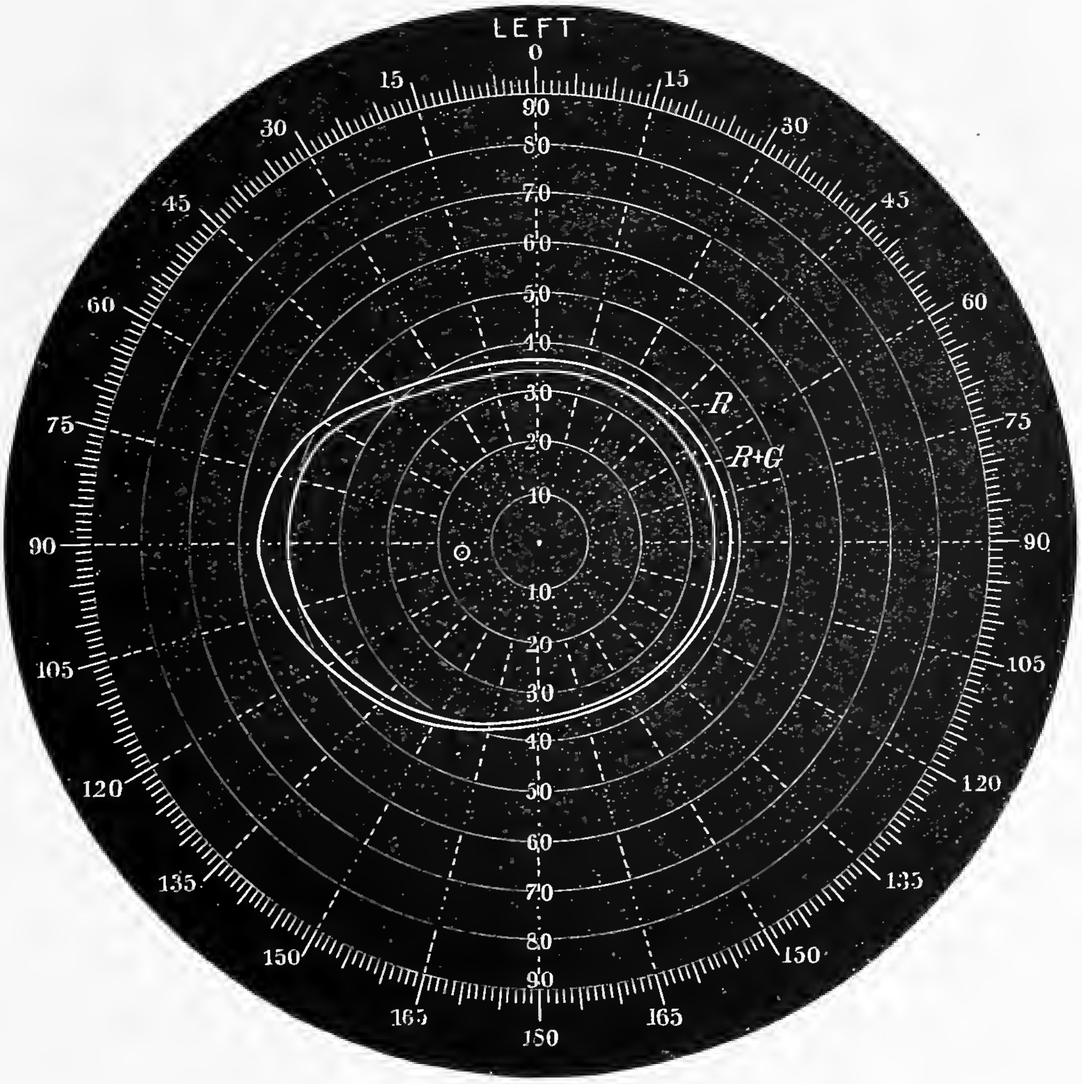
The following table gives the results :—

TABLE XV.

Angle of field in degrees.		Extent of field in degrees.			
		Red.	Green.	G + R.	D.
0	N	35	36	33	38
30	N	35	35	33	36
60	N	36	36	35	39
90	N	39	41	37	43
120	N	37	38	37	42
150	N	35	35	34	37
180		37	38	35	38
150	T	43	46	40	47
120	T	49	51	45	50
90	T	56	58	50	61
60	T	50	52	46	53
30	T	39	40	35	42

These colour fields all have the same shape (see fig. 12). They do not cut one another, and if we compare the fields of the red and the green with those of the green and the blue in the previous table, we shall see that they practically coincide. Thus the fields of a red, two greens and a blue, are the same when proper luminosities are taken for each. Before leaving this table, it is well to point out that the field for D

Fig. 12



is considerably more extended than that of the mixed colours, as are also the fields for green and red separately. We may conclude that the intrinsic white light in each colour, when added together, is greater than the intrinsic white light in the D ray. This points to the fact that the colours of pigments should not give the same fields as the spectrum colours with which they approximately match.

15. *Connection between Change of Intensity of Colour and Extent of Field.*

The difference in extent of field, caused by difference in illumination, was next determined in the horizontal directions. The four rays, red lithium, D, scale No. 41·7 in the green, and the blue lithium, were experimented with as fairly representative of the whole spectrum. The different rays were first allowed to pass through the annulus at 0°; and, subsequently, measures were made after passing through it, when its readings were 35, 70 . . . 280°, as every added 35° halved the previous intensity. The D light coming through the slit with the annulus at 0°, measured 4·5 AL. The following were the luminosities of the other rays coming through the same slit: red lithium, ·5 AL; SN 41·7, 3·2 AL; and blue lithium, ·3 AL (fig. 13).

Table XVI.

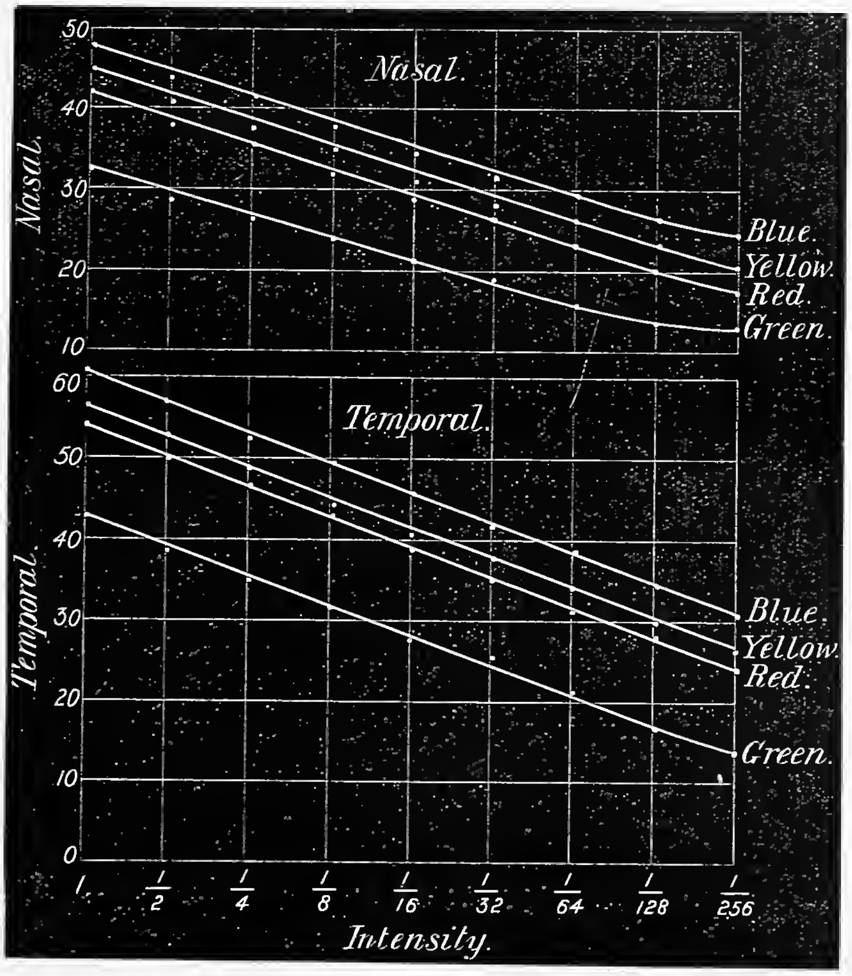
Degrees annulus.	Intensity of ray.	Reading of horizontal field in degrees.							
		Red Lithium.		D.		Scale No. 41·7.		Blue Lithium.	
		Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	Nasal.
0	1	54	42	57	45	43	33	61	48
35	$\frac{1}{2}$	50	38	53	41	39	29	57	44
70	$\frac{1}{4}$	47	36	49	37	35	27	53	42
105	$\frac{1}{8}$	43	32	45	34	32	24	50	38
140	$\frac{1}{16}$	39	29	41	31	28	22	46	34
175	$\frac{1}{32}$	35	26	37	28	25	19	42	31
210	$\frac{1}{64}$	32	24	33	26	21	16	39	29
245	$\frac{1}{128}$	28	20	30	23	17	14	35	26
280	$\frac{1}{256}$	24	18	26	20	14	13	31	25

We find from the above that the average diminution in field for each reduction of half intensity on the temporal side is 3·75°, and on the nasal side close upon 3°. Using these figures, the above table would be as follows:—

Table XVII.

Degrees annulus.	Intensity of ray.	Reading of horizontal field in degrees.							
		Red Lithium.		D.		Scale No. 41·7.		Blue Lithium.	
		Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	Nasal.
0	1	54	42	57	45	43	33	61	48
35	$\frac{1}{2}$	51	39	53	42	39	30	57	45
70	$\frac{1}{4}$	46·5	36	49·5	39	35·5	27	53·5	42
105	$\frac{1}{8}$	43·75	33	46	36	32	24	50	39
140	$\frac{1}{16}$	39	30	42	33	28	21	46	36
175	$\frac{1}{32}$	35	27	38	30	24	18	42	33
210	$\frac{1}{64}$	31·5	24	34·5	27	20·5	15	38·5	30
245	$\frac{1}{128}$	28	21	31	24	17	12	35	27
280	$\frac{1}{256}$	24	18	27·00	21	13	9	31	24

Fig. 13.



With my assistant (W. B.), these numbers appear to be 4 and 2·5 respectively, showing a consistent variation from my own measures. That there is a diminution in the angle of field in an arithmetical progression, as the intensity diminishes in geometrical progression, is somewhat strange, and appears to be unaccountable. It will be noticed that the region of the macula lutea has been avoided in these observations, as it seemed to be useless to attempt any observations on parts of the retina which were evidently unsuited for them.

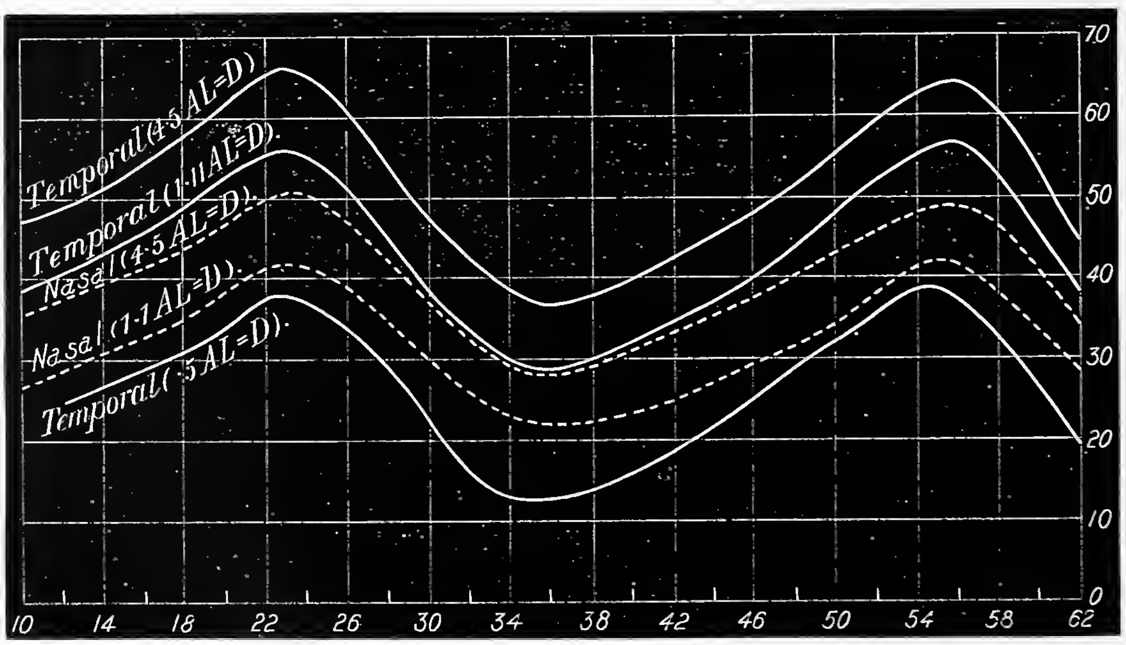
16. *Extent of Field for the Different Rays of the Spectrum.*

It now became of interest to ascertain the extent of the fields to my own eyes when a slit was passed unaltered through the spectrum, for it then became a matter of calculation to find the intensity (luminosity) of each colour required to give equal horizontal fields at any given angular distance from the centre of the retina, and as all fields are similar, when one has been measured for any colour, all the others may be constructed. The following is a table of three sets of observations. The two first were taken with an aperture of ·525 inch, with an angular value of 2° 30′. The third was taken with an aperture of ·086 inch, embracing an angle of 25′ only, the temporal extent being only observed with it.

TABLE XVIII.

Scale No.	λ.	No. 1.		No. 2.		No. 3.	Remarks.
		Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	
62	6957	44	34	37	28	18	The luminosity of the D light in No. 1 = 4·5 A L; an aperture of ·525 inch was used at 1 foot distance. The luminosity of the D light in No. 2 = 1·1 A L, with an aperture of ·525 inch. The luminosity of the D light in No. 3 was ·5 A L, an aperture of ·086 inch being used at 1 foot. The readings marked † were doubtful, as they fell on or close to the blind spot. They were obtained by reading at a small angle to the horizontal line.
60	6728	53	41	45	33	27	
58	6520	61	47	53	37	33	
56	6330	64	49	56	41	38	
54	6152	63	48	55	41	39	
52	5996	60	46	52	38	36	
50	5850	56	43	48	35	33	
48	5720	52	40	44	32	29	
46	5596	49	38	40	30	25	
44	5481	46	35	37	28	22	
42	5373	43	33	34	26	18†	
40	5270	40	31	32	24	16†	
38	5172	38	29	30	23	14†	
36	5085	37	28	29	22	13†	
34	5002	39	29	30	23	13†	
32	4929	42	32	33	25	16†	
30	4848	47	36	39	30	21	
28	4776	54	42	45	35	28	
26	4707	61	47	52	39	34	
24	4639	65	50	56	42	37	
22	4578	65	50	55	42	38	
20	4517	61	47	53	39	34	
18	4459	58	44	49	35	31	
16	4404	54	41	46	33	29	
14	4393	51	39	43	31	27	
12	4296	49	38	41	29	25	
10	4245	47	36	39	27		

Fig. 14.



If we plot the curves from the above table, and take the distance apart of the nasal from the temporal ordinates, we shall find that when the latter reads 40° the former reads 30°, no matter what the colour may be; and that, as the field increases about $7\frac{1}{2}$ degrees on the temporal side, the field on the nasal side increases nearly 6° —a variation which is in accordance with the table showing the field with variation of intensities of the beam (fig. 14).

17. *Luminosities of Colours for Equal Fields.*

From this curve we can calculate, within certain limits, the intensity (*i.e.*, luminosity) of any colour to give any required extension of field. Suppose, for instance, we required to know the luminosity of the whole of the colours of the spectrum at, say, 30° from the axis on the temporal side, which would give equal fields, we should proceed as follows:—Take the height of the ordinate of any colour above (or below) the ordinate of 30°, and divide it by 3·75; that would give a factor in powers of $\frac{1}{2}$ by which the intensity (luminosity) should be diminished, in order to cause the field of that particular ray to fall at 30°. Thus, at 48·4 of the spectrum scale, the height of the ordinate is 45° (that is, 15° above the ordinate of 30°). Hence, since $15/3·75 = 4$, the intensity of the ray would have to be diminished to $(\frac{1}{2})^4$ to cause its field on the temporal side to fall at 30°. The luminosity of this ray is at the maximum spectrum luminosity, or 100, and would thus have to be reduced to 100/16, or 6·25; whilst the luminosity at scale numbers 38 and 34 would remain the same, viz., 49 and 31 respectively. On this plan fig. 15 was calculated, which gives the comparative luminosities for equal fields, the maximum being made 100.

TABLE XIX.

Scale No.	Original Luminosity.	Luminosity for equal fields.	Scale No.	Original Luminosity.	Luminosity for equal fields.
60	3·5	·58	36	40	77·6
58	15	·56	34	32	53·5
56	34	1·00	32	25	29·0
54	55	1·6	30	19	9·5
52	70	2·5	28	15	2·5
50	94	8·1	26	13	·7
48	100	16·5	24	9·5	·24
46	96	32·6	22	7·5	·21
44	85	49·4	20	6	·20
42	74	86·2	18	5	·35
40	60	100·0	16	4	·42
38	50	98·0			

This calculation is made on the assumption that the comparative luminosities of the colours of the spectrum are the same at 30° on the temporal side as they are at 10°.

Fig. 15.

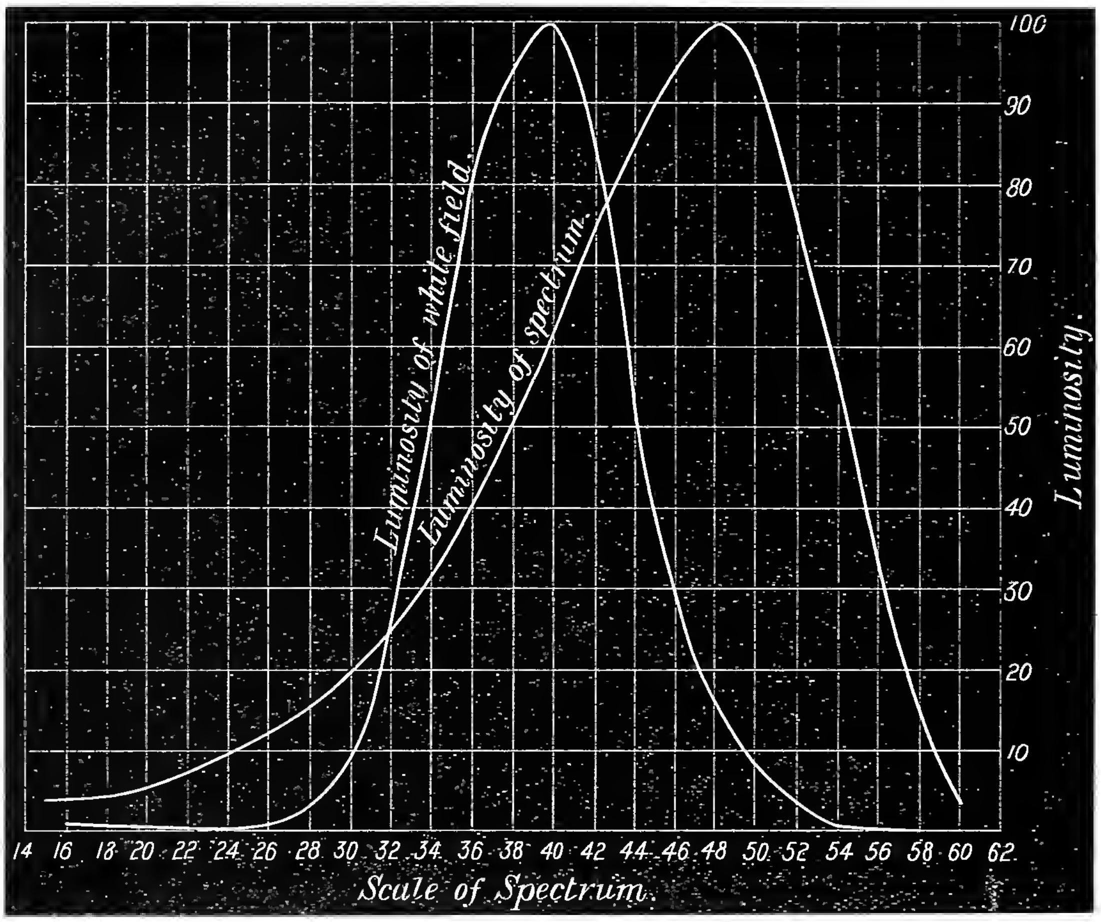
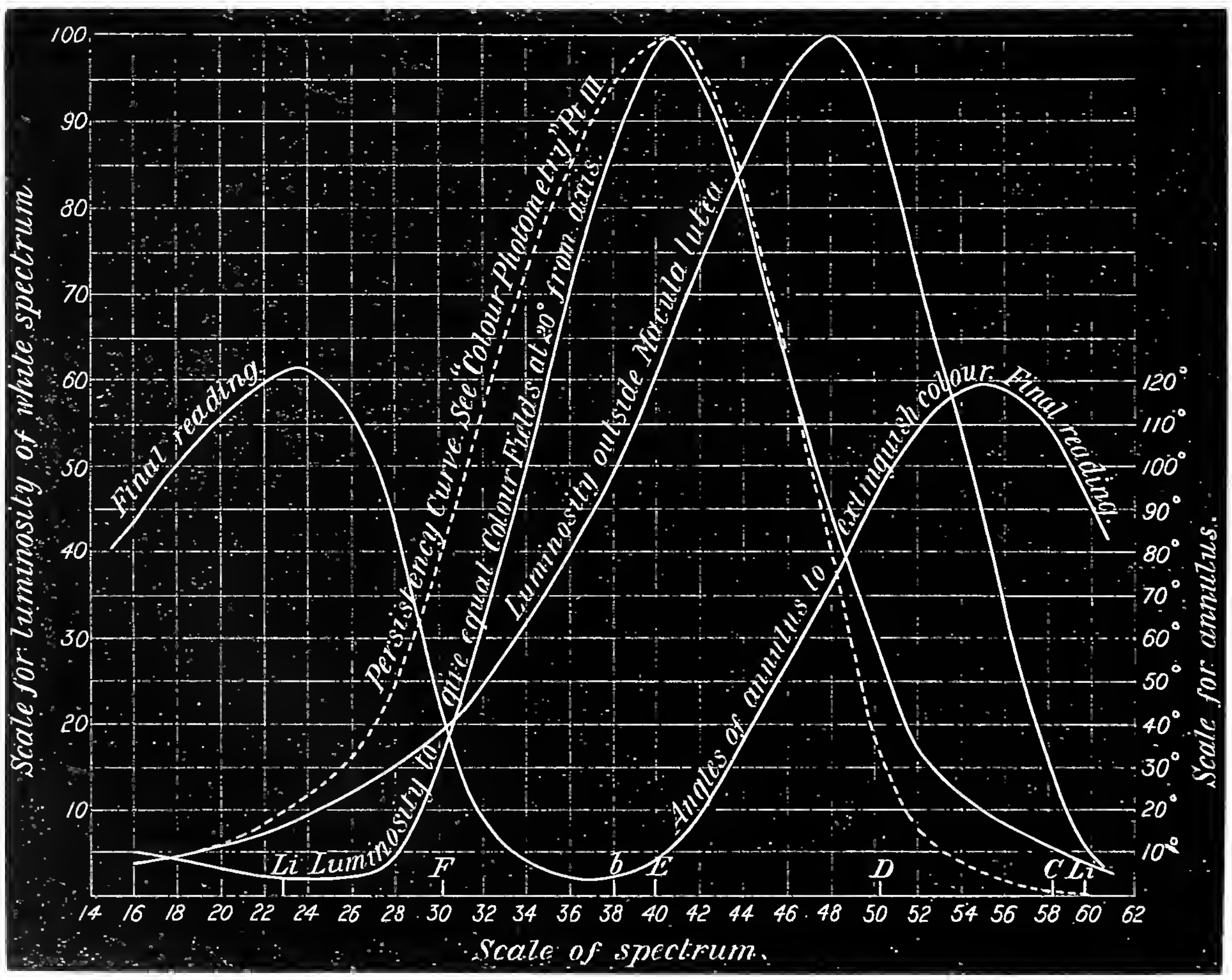

Luminosity of white field.
Luminosity of spectrum.
Luminosity.
100
90
80
70
60
50
40
30
20
10
14 16 18 20 22 24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62
Scale of Spectrum.

Fig. 16.


Scale for luminosity of white spectrum
100
90
80
70
60
50
40
30
20
10
Final reading.
Persistency Curve. See "Colour Photometry" Pt III.
give equal Colour Fields at 20° from axis.
Luminosity outside Macula lutea
Li Luminosity to
Angles of annulus to extinguish colour. Final reading.
F
b
E
D
C
Li
120°
110°
100°
90°
80°
70°
60°
50°
40°
30°
20°
10°
Scale for annulus.
14 16 18 20 22 24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62
Scale of spectrum.

In connection with this it may be well to quote another observation made direct for the purpose of ascertaining the luminosities of different colours required to give equal colour fields. The different parts of the spectrum were observed on the retina at 25° from the axis on the temporal side, and the luminosities reduced by the annulus till the colour disappeared. The readings were somewhat difficult to make, but the mean gives the table following. (See fig. 16.)

TABLE XX.

I. Scale No.	II. λ	III. Mean value of reading of annulus.	IV. Value of light when 0° = 100.	V. Luminosity of spectrum.	VI. Col. V. × Col. IV. ÷ 100.	VII. Col. VI. Max. = 100.
60	6728	90	16·2	3·5	·6	1·3
58	6520	112	10·9	12 5	1·4	3·1
56	6330	120	9·2	27·5	2·5	5·5
54	6152	118	9·0	43·0	3·8	8·4
52	5996	108	11·8	61·0	7·2	15·2
50	5850	93	16·0	79 0	12·6	28·5
48	5720	72	19·6	85·0	16·7	37·5
46	5596	55	33·7	81·0	27·3	62·0
44	5481	37	48	72·5	34·8	79·0
42	5373	19	70	62·5	43·7	98·0
40	5270	8	85	52·0	43·2	97·0
38	5172	5	90	41·5	37·3	84·5
36	5081	4	93	33·5	31·1	70·0
34	5002	8	85	26·5	22·5	51·0
32	4924	17	65	21	13·6	31·0
30	4848	43	43	16·5	7·1	15·8
28	4776	86	18·3	13	2·4	5·4
26	4707	114	10·5	10·5	1·3	2·3
24	4639	124	8·6	8·2	·71	1·6
22	4578	120	9·2	6·3	·58	1·27
20	4517	108	11·8	5·0	·50	1·30
18	4459	97	14·7	4·0	·58	1·27
16	4349	86	18·3	3·1	·57	1·25

At the time when these results were obtained an experiment was also made by the same eye to determine the variations in the fields for four different colours when the intensity was altered. The four colours chosen were the same as used in many of the experiments made with my own eyes. The fields were taken in the horizontal direction, and on the nasal and temporal sides. As the light was varied for each colour, in order that readings up to about 60° might be obtained, the fields for the rays are not comparable with each other. Each field must be considered by itself.

TABLE XXI.

Annulus reading.	Comparative intensity of light.	Red lithium.		D.		41·7 S.N.		Blue lithium.	
		Nasal.	Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	Nasal.	Temporal.
0	1	42	54	47	60	33	43	42	53
35	$\frac{1}{2}$	37	46	42	53	29	35	37	46
70	$\frac{1}{4}$	32	38	36	45	24	28	31	37
105	$\frac{1}{8}$	26	31	31	38	20	21	26	30
140	$\frac{1}{16}$	22	24	26	29	15	8	21	24

All we have to deal with here are the readings on the temporal side. We find that for each diminution of one-half intensity the field contracts 7° 5′, or twice that of the writer's. The figures in the table on the previous page are now explained, and they compare fairly with the results tabulated on p. 186. The variation in sensitiveness in different eyes is here well illustrated.

18. *Dependence of Field on the Size of the Coloured Spot.*

It has been shown that the loss of colour in the centre of the retina depends largely on the size of the spot of light viewed. Such being the case, it was to be presumed that the boundaries of a field would contract if the aperture used in the apparatus was diminished, and it seemed possible that some expression might be found which would connect the two together.

To make measurements of field with dimishing apertures the same kind of perimeter was employed as before, and the spot of light on the ground glass was diminished in size by placing circular apertures of diminishing diameter in contact with it. The fields were measured in a horizontal direction only at first, and the following table gives the mean of the actual measures. The intensity of the D light was 1·1 AL.

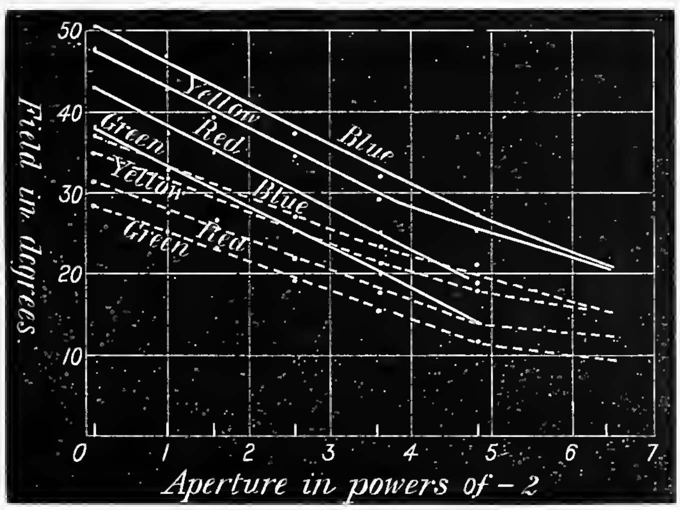
TABLE XXII.

Diameter of aperture in inches.	Angle subtended.	Diameter of aperture in powers of 2.	Red lithium.		D.		41·7.		Blue lithium.	
			Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	Nasal.	Temporal.	Nasal.
0·94	4° 18′ 0″	−0·09	42	32	48	35	38	28	50	37
0·525	2 30 0	−0·93	37	28	43	32	33	25	47	34
0·35	1 34 0	−1·52	35	26	39	29	31	23	42	31
0·17	0 49 0	−2·56	29	22	34	25	25	18·5	37	27
0·086	0 25 0	−3·56	25	17·5	29	21	20	15	32	23
0·036	0 10 0	−4·8	19	14	25	18	*b.s.*	12	27	21
0·012	0 3 30	−6·4	*b.s.*	12·5	20	15	*b.s.*	9	20	15

b.s. is blind spot where measures are impracticable.

This table when plotted gives a diagram, fig. 17, which shows that between apertures subtending 4° 28′ and 10′ (the power of $\frac{1}{2}$ being taken for the scale of abscissa), the fields decrease in extent and are practically straight lines. On the temporal side for each diminution in aperture to $\frac{1}{2}$ diameter the diminution *in field* is 5° and on the nasal side 4°. The diminution in field for a diminution of $\frac{1}{2}$ the intensity *of light*, it will be remembered, is 7·5° on the temporal side and 6° on the nasal side.

Fig. 17.



The diminutions in field thus bear the same ratio to one another, viz., 5 : 4. This might be expected, but the author was by no means prepared to find that it could be measured so closely as it has been. We thus arrive at the result that diminution in aperture is equivalent to diminution in intensity of light. When the apertures used were greater than the largest given in the table scarcely any alteration of the field was obtained. We may take it that any aperture subtending more than 5° will give the same field. With apertures between 5° and 3° the field will only slightly diminish. Referring to the table of extent of field for the whole spectrum, we shall find that the measured field for an aperture of ·086 inch agrees with the above determination very closely, taking into account the illumination.

19. *Relative Sensitiveness of the Different Parts of the Retina.*

One other determination of sensitiveness of the retina required to be made, viz., the general sensitiveness at all parts compared with that at the centre or close to the yellow spot. In "Colour Photometry," Part III., a comparison was made of the sensitiveness of the centre of the eye compared with that of a point 10° towards the periphery. Determinations of this kind are extremely difficult, and it is only by continued observation that an approach to correct measures can be made. In fact the eye requires training. Perhaps the easiest plan of explaining how the following determinations were made will be by describing a preliminary experiment. Procure a large sheet of black paper and lay it horizontally on a table near a window, so that it is equally illuminated. Cut out some small and equal discs of white paper or card and place two of them about 1 foot apart lying on the black paper. Place the eye about 12 inches above one of them, and receive its image on the centre of the retina. At the same time the image of the other will be received on the retina about 45° from the centre. This last white disc will appear to be very decidedly darker than the first.

Cut out a small disc in grey paper, and substitute it for the white disc, the image of which is viewed centrally. The other white disc may now be moved away from it till the two appear equally luminous. The distance from the grey disc to the white will give the field. By measuring the amount of white light reflected from the grey paper, the comparative luminosities of the discs are found, and from them the relative sensitiveness of the two portions of the retina are determined.

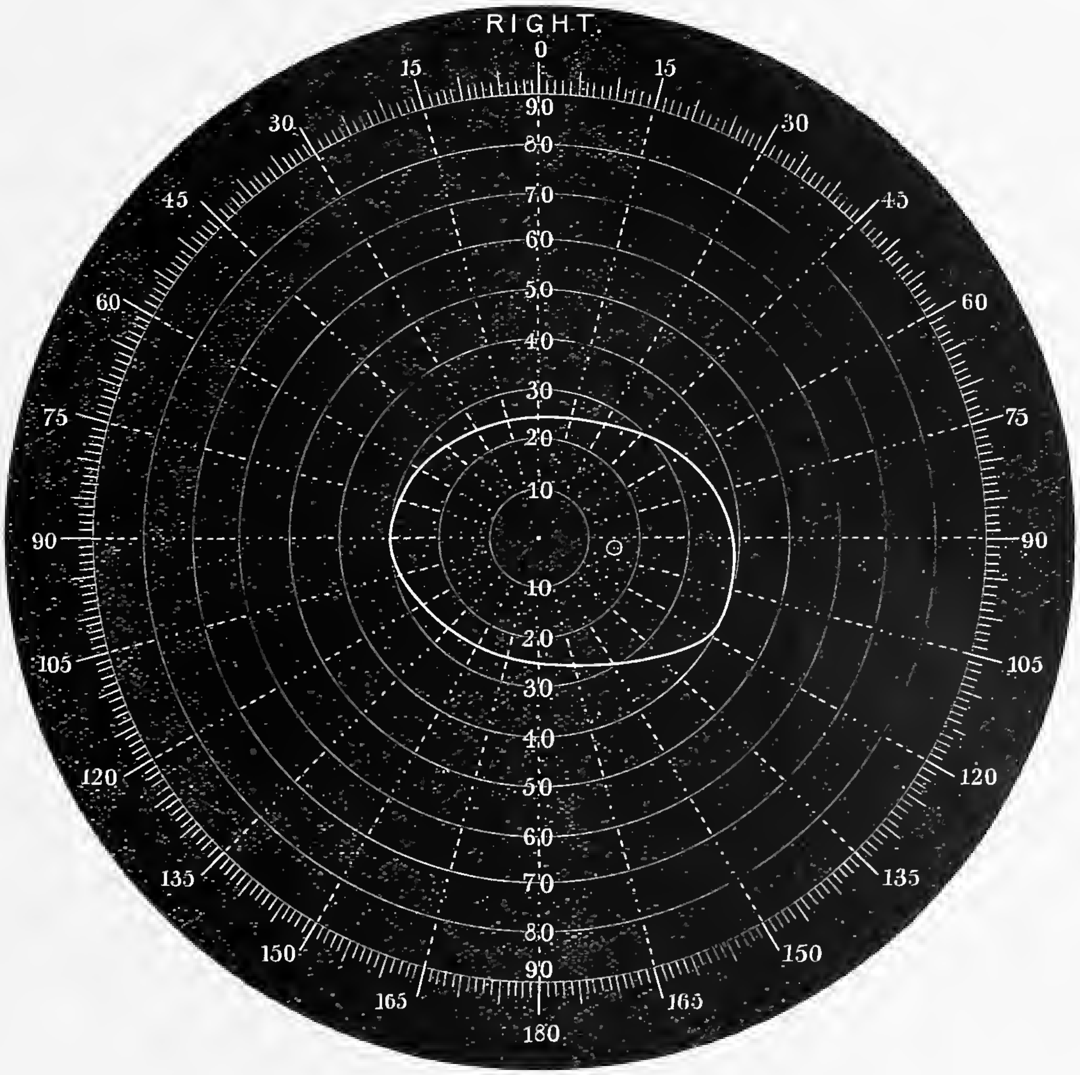
The same procedure can be carried out on an equally illuminated surface, and "iso-lumes" be made for any depth of grey. The following table gives one of the determinations, the grey in this case reflecting $\frac{1}{5 \cdot 4}$ of the white light reflected from the white disc. The diameters of the discs were half an inch, and were viewed with the right eye at a distance of two feet from a vertical screen (see fig. 18).

TABLE XXIII.

Angle with the vertical.	Field, in degrees.	Angle with the vertical.	Field, in degrees.
0	24	180	26
30 N.	27	150 T.	25
60 N.	33	120 T.	27
90 N.	39	90 T.	30
120 N.	40	60 T.	27
150 N.	30	30 T.	25

It will be seen that the "iso-lumes" are of the same character as the colour fields. Some small correction might have to be made for the projection of the white disc on the retina, since it would not be of the same angular dimensions as if viewed in a hemispherical perimeter.

Fig. 18.



An iso-lume.

Other modes of measurement were tried, and the results agreed very fairly *inter se.* The following iso-lumes were taken only in four directions, viz., two in the horizontal and two in the vertical (see fig. 19).

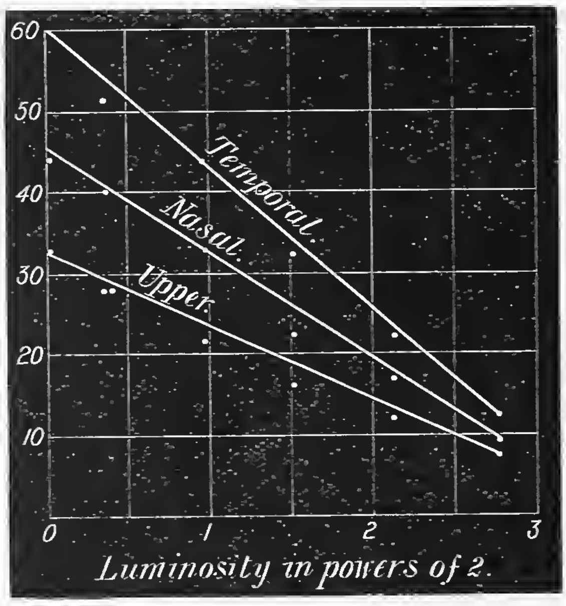
TABLE XXIV.

Light reflected from grey disc when white = 100.	Luminosity in powers of − 2.	Temporal reading.	Nasal reading.	Upper vertical reading.	Lower vertical reading.
60	2·74	12	9	8	9
38·5	2·10	22	17	12	13
25·5	1·51	32	23	16	18
16·8	0·90	43·5	33	22	24
13·5	0·31	51	40	28	28
11·5	0·00	60	44	33	34

Comparing these readings on the temporal and nasal sides, they agree, as well as could be expected from the nature of the observations, with the colour field curves.

They show that with an increase of double the luminosity the field is extended about 17° on the temporal side and about 13° on the nasal. As the increase of dimensions in these two directions of the colour fields for the same increase in luminosity is considerably less, it is evident that there is no exact connection between the luminosity of white light at the different parts of the retina and the luminosity of a colour-ray when colour is extinguished. The sensitiveness of the peripheral portions of the retina to light and colour is therefore different, as it was shown to be when the centre of the retina was under consideration.

Fig. 19.



A good many experiments have been carried out regarding the persistence of coloured images and the rate of perception, but these have indicated that the subject is one which should be treated of in a separate communication.

It may here be reiterated that the sensitiveness of the eye varies considerably at times, which may be due in all probability to the state of health of the observer. Much of the difficulty experienced in these observations has arisen from this variation. As the eye becomes practised to observation, however, the liability to variation very largely disappears, and at the present time readings made by my assistant and myself are very fairly comparable at all times. Whether, when the observations have ceased for some time and are then renewed, there will be a relapse, it is hard to say.

It will be seen that scarcely any reference has been made to the work of other observers. It has not been thought advisable to do so for various reasons, the principal one being that in the experiments described spectrum colours have been employed. The results obtained with these last cannot be comparable with those obtained with the use of impure colours.

20. *General Summary.*

The results of these investigations may be summarized as follows:

1. That where an image is received on the centre of the retina, the reduction in

intensity of the radiation which will just fail to produce the sensation of light depends (within limits) on the size of the image.

2. That the smallest diameter of the image and not its area determines the necessary reduction in intensity.
3. That the reduction in the intensity of the light of an image falling excentrically, which will just fail to produce the sensation of light, follows the same general law as if the image were received centrally.
4. That the visual brightnesses of illumination of a small and a large aperture when illuminated with the same light differ, and that such visual brightnesses are connected by a simple law.
5. That the reduction in the brightness of an image just sufficient to extinguish the sensation of colour, varies with the size of the image and follows a definite law, which, however, differs from that for the extinction of light.
6. That all fields for colours will have the same boundary when the intensity of the coloured ray is properly adjusted.
7. That there is a simple connection between the intensity of a colour and the extent of field.
8. That the colour fields depend on the size of the object viewed, and that the dependence appears to follow a simple empiric law.
9. That the retina is most sensitive to light at its central part and the sensitiveness diminishes towards the periphery.

These results as they stand do not seem to confirm either one of the two main theories of colour vision. The existence of a colour field at all is difficult to explain on the YOUNG theory, and the fact that a colour field for red can be obtained with bright illumination although the disappearance of this colour and light takes place almost together at the centre of the retina, is not easily accounted for on HERING'S theory. It appears as if light were the fundamental sensation caused by the main vibration generally, whilst colour is as it were an overtone to which the receiving nerves are less susceptible than to light, the further away they are situated from the centre, and may be due to the form of vibration.

In closing this paper I should be doing an injustice if I did not place on record the great assistance I have had during the whole of these investigations, which have extended over three years, from my assistant, Mr. WALTER BRADFIELD; with every new step I took he made himself thoroughly acquainted, and every series of measures I made myself he repeated with his own eyes. There is nothing stated as being fairly proved which has not been confirmed by him. Measurements of the kind recorded above are by no means as simple as they look on paper, but those given are the results of hundreds of observations, repetition being an absolute necessity to avoid false deductions.

INDEX SLIP.

VIII. *Total Eclipse of the Sun, 1896.—The Novaya-Zemlya Observations.*

By Sir George Baden-Powell, *K.C.M.G., M.P.*

Communicated by J. Norman Lockyer, *C.B., F.R.S.*

Received November 19,—Read November 19, 1896.

[Plates 1, 2.]

1. As the observations of the total eclipse of the sun in 1896, made in Novaya-Zemlya by the "Otaria" expedition, are the only British observations of that eclipse which secured successful results, it may be of some importance to detail the conditions under which those observations were made.

2. On learning that the Government funds available for the observation of the total eclipse of 1896 would not suffice for more than the parties detailed to Japan and to Norway, I willingly offered to take another party to Novaya-Zemlya in my yacht "Otaria."

3. It would thus be possible to increase the chances of a clear view of the eclipse; and the altitude of the sun at the time of the eclipse would be far greater in Novaya-Zemlya than on the Varanger Fjord.

Personally, I also formed high hopes as to results (especially photographic results) at a latitude far higher than any at which such powerful astronomical instruments had before been used.

4. I received and carefully stowed on the yacht instruments of various kinds; some selected by Mr. Norman Lockyer, with the approval of the Lords of Committee of Council on Education, from among those in the Solar Physics Observatory; and others by Mr. E. J. Stone, by special consent of the Trustees, from the instruments at the Radcliffe Observatory.

5. I had the good fortune to receive as my guests Mr. E. J. Stone, M.A., F.R.S., the Radcliffe Observer; Mr. W. Shackleton, F.R.A.S., one of the staff under the Solar Physics Committee; and Lieutenant Vernon Brooke Webb, R.N.

The late Prince Lobanoff, through our ambassador at St. Petersburgh, and at the request of the Foreign Office, was good enough to supply information for me, and to give special instructions to the officials of the Archangel Province to render every aid and assistance to our expedition.

11.12.97

6. We left Dundee on the 18th July, but were much delayed by persistent calms. Nevertheless we arrived safely off the coast of Novaya-Zemlya on the 2nd August.

7. At the small new Samoyede settlement of Karmakul we found two observing parties from Russia, constituted as follows :—

Imperial Academy of Science Party.

Prince BORIS GALITZINE (Leader)	Physics.
M. BACKLUND (Director, Pulkova)	Astronomy.
M. KOSTINSKEY (Pulkova)	,,
M. HANSKY ,, ...	,,
M. GOLDENING	Photography.
M. JACOBSON	Zoology.

Kasan University Party.

M. DUBJAGO	Astronomy.
M. GOLDHAMMER	Physics.
M. ZEIGEL	,,
M. KRASNOFF	Astronomy.
M. BELKBUTCH	Zoology.
M. BARANSVITCH	Photography.

8. The Russians had established their observation huts and instruments on rising ground, perhaps 60 feet above the sea-level, with barren undulating tundra, culminating two to three miles inland in a long range of hills, say 1500 feet to 2000 feet in altitude. We entered into friendly relations with them.

9. We decided on making our observation spot as far to the westward as possible, hoping thus to eliminate all chance of obstruction by clouds gathering on these inland hills.

We selected a site on an island, about 20 feet above sea-level, the strata being laminous slate, with an almost perpendicular upheaval. A bluff, some 30 feet high, protected the camp from seawards, but all was open to the eastward.

The precise position of the camp was—

Lat. 72° 22′ 40″ N.
Long. 52° 38′ 13″ E.

This position was verified by Lieutenant WEBB by measurement from one of the Russian triangulation points—a beacon—about a mile distant, the latitude and longitude of which were supplied by the Russian observers.

10. We erected shelter tents of spars and sails and set up the Willesden canvas huts for the 12″ Cooke siderostat, two large spectrum and one corona photograph telescopes.

11. Mr. STONE, with the aid of Lieutenant WEBB, used a small but powerful equatorial (presented to the Radcliffe Trustees by FRANCES ANNE, Dowager Duchess of Marlborough).

The spectroscope was one made, from Mr. STONE'S plans, specially for the eclipse by Mr. HILGER. The dispersion in the photographic camera consisted of a direct spectroscope, capable of separating the D lines, and a prism of crown glass of 60°. The light reflected from the surface of the prism was used for a second direct spectroscope, and the instrument, therefore, admitted of combined eye and photographic spectrum observations.

12. Mr. SHACKLETON worked with a prismatic camera of 3 inches aperture with two prisms of 60°, using the siderostat, and also a direct vision slitless spectroscope, by means of which he was enabled to signal with exactness the disappearance of the continuous spectrum.

13. I took charge of the coronagraph provided by Mr. LOCKYER. The telescope was one of $4\frac{5}{8}$ inches aperture and 6 feet 9 inches focal length. The object glass was not new, and decidedly green in tint, but it was the only one available for the expedition. The size of the image was 0·76 inch, and the ratio of aperture to focal length was $\frac{1}{17 \cdot 5}$.

The telescope was mounted in a N. and S. direction immediately above the prismatic camera, so that both could use the one siderostat. Mr. SHACKLETON, with commendable care and trouble, finally secured admirable adjustments of all these instruments.

14. Meteorological observations were made by means of a self-registering barometer and thermometer and a sympiesometer.

Lady BADEN-POWELL and the yacht's crew were detailed to make drawings of what they could see of the *outer corona*, on printed diagram plans designed by Mr. STONE, and to note the position of any "stars" seen, and otherwise observe general aspects.

15. Everything was ready well in time, and throughout the eclipse all proceeded most satisfactorily. In all, twenty-nine good photographs were secured.

16. The local *times* of commencement and end of totality were noted by the sailing-master of the "Otaria"—Captain G. WILLCOX—as being $7^h\ 34^m\ 54^s$ and $7^h\ 36^m\ 44^s$ respectively, but there was some doubt as to the signal for the end of totality. Captain WILLCOX also gave out the number of passing seconds, by chronometer, for the information of the observers.

17. With regard to the *spectroscopic* results obtained, both by Mr. STONE and Mr. SHACKLETON, I am informed that they promise to be of the highest scientific value; they are now being worked out by Mr. STONE and Mr. NORMAN LOCKYER respectively, but the final elucidation will naturally occupy some considerable time.

In all, four good photographs were secured by the slit and nineteen by the prismatic camera.

18. With regard to the results secured by the *coronagraph*, the following statements may be made :—

Assuming observations on the central line the following scheme of exposures had been prepared by Mr. LOCKYER :—

No.	Exposure.	Change of plates.
	seconds.	seconds.
1	5	
		5
2	15	
		5
3	60	
		5
4	2	
		5
5	18	

As the station we took up in Novaya Zemlya was about twenty miles from the central line, our period of totality was reduced to 1 minute 45·5 seconds, and Mr. SHACKLETON drew up the following revised scheme :—

No.	Exposure.	Change of plates.
	seconds.	seconds.
1	5	
		5
2	15	
		5
3	40	
		5
4	2	
		5
5	15	

With the aid of the mate of the "Otaria," Mr. KERLEY, I successfully made the exposures at the regulated times. The "Castle" plates, by MAWSON and SWAN, were used throughout. Five photographs of the corona were taken, with the following exposures :—

No.	Exposure.
	seconds.
1	5
2	15
3	40
4	2
5	15

The four plates exposed during totality were developed by Mr. SHACKLETON before leaving Novaya Zemlya, and the fifth at South Kensington about five weeks later.

Mr. LOCKYER has been good enough to have these enlarged at the Solar Physics Observatory, by Corporal HASLAM, R.E., and I have the pleasure of laying them before the Society (see Plate 1). It will be seen (by reference to 'Phil. Trans.,' 1889, vol. 180, pp. 291, &c.) that the expected agreement with the corona of 1886 is most marked.

The field of view of the telescope employed was circular, with an approximate diameter of 100 minutes of arc. In the longer exposed photographs the longest streamer is cut off by the boundary of the field at 1·06 of the moon's diameter. As seen visually the streamer was estimated to have a length of 1·76 of the diameter.

19. It should be noted that the other heavenly bodies seen and located by "Otaria" observers during totality were the planets *Mercury*, *Jupiter*, *Venus*, and *Mars*, and the star *Regulus*. The several independent sketches on plans, by eye, of the corona, coincided remarkably with the photographs.

I would add that during totality the darkness was not sufficient to interfere with the reading of the chronometer in the open, or of a BENSON's chronograph inside the telescope hut. The atmosphere may possibly have been lighted up by the large areas of snow-covered land in our neighbourhood.

20. The *Meteorological* observations made at the time were significant.

(1.) The self-registering (NEGRETTI and ZAMBRA) thermometer, placed in sunlight, indicated remarkable changes of temperature, ranging, during the period of the eclipse, from 47° Fahrenheit down to 36°, and rising, within three hours after totality, to the maximum for the day of no less than 62°.

(2.) The self-registering barometer (NEGRETTI and ZAMBRA), as will be seen by the accompanying photographic enlargement (not reproduced here), gave no indications except those of unevenness.

But readings, at necessarily irregular intervals, of the sympiesometer (NEGRETTI and ZAMBRA), before and after totality, gave the following results :—

Local time.		Thermometer.	Barometer.	
hrs.	mins.	°		
7	13	49	30	3·25
7	18	48	30	3·30
7	20	47·5	30	3 30
7	20	46·3	30	3·18
		Interval of totality.		
7	39	35·3	30	3·15
7	47	46	30	3·25

(3.) The wind was blowing moderately (BEAUFORT scale, 3 to 4) all the morning from about N. by E., some cirrus clouds gradually coming up from E.N.E. Just before totality there was a sudden change of wind to E.N.E. Several present, myself among the number, distinctly noticed that the wind died away during the totality.

21. All members of the "Otaria" party did everything in their power to make the observations a success; and thus full advantage was taken of the opportunities afforded by a happily clear view of the sun.

22. While it is cause for great regret that similar opportunities were not afforded to the skilled observers in Norway and Japan—or, indeed, at the other stations—it is highly satisfactory to know that the total eclipse of 1896 was most successfully observed at one British station, and that the results thus secured will be of high value to astronomical science.

ON THE PHOTOGRAPHS OF THE CORONA OBTAINED IN NOVAYA-ZEMLYA.

BY W. H. WESLEY.

The drawing, of which Plate 2 is a Woodburytype reproduction, was made from four negatives taken by Sir G. BADEN-POWELL and Mr. SHACKLETON. The moon's diameter is $\frac{8}{10}$ inch on the original negatives, and the drawing has been enlarged to a scale of $2\frac{1}{2}$ inches for the moon's diameter. The following are the particulars of the plates :—

Plate 1. Exposure, 5 seconds. Shows a faint fringe of corona on the W., and a greater extent on the E., where it reaches a height of 5′ or 6′. Plate very clean and definition good; the lower details on the E. limb extremely well seen; background of sky quite clear.

Plate 2. Exposure, 15 seconds. Very fine negative. Corona extends to about half a lunar diameter on the E., and more than half as far on the W., but the conspicuous ray to the N.W. is easily traced for a diameter from the limb, where it is cut

off by the boundary of the field. Near the limb the corona is very intense, but sufficiently transparent to allow the low details to be easily made out. Sky slightly fogged.

Plate 3. Exposure, 40 seconds. Very fine negative. Sky decidedly more fogged than in Plate 2, but corona very well defined, extending on the E. to about 21′ from the limb, and on the W. to 16′. The ray on the N.W. is cut off by the boundary of the field. Although the corona is very dense near the limb, the low details can be made out very well with suitable illumination.

Plate 4. Exposure, 2 seconds. Clear negative; shows prominences very well, but only a narrow, faint fringe of corona, more intense on the S.W. and almost invisible at the N. pole. Only a little of the lowest detail is shown.

The negatives appear well-focussed and the grain of the plates is fine, so that I have been able to make out much more of the lower details than in most recent eclipse photographs.

The corona of 1896 is remarkably symmetrical about the sun's axis; still more so than that of 1886, which it closely resembles. The northern polar rift is extremely well-marked; it extends for about 40° along the limb, and is filled with fine rays, attaining a height of about 11′; straight and nearly radial in the centre, and becoming more curved and inclined from the axis towards either side of the rift. As in 1886, the southern rift extends for a greater distance along the limb, but is much less distinct than the north polar rift, the rays filling it being broader and more diffused, and its boundaries less clearly defined.

The conspicuous ray (or group of rays) in the N.W. quadrant, mentioned in the description of the plates, bounds the N. polar rift to the W. with a decided curve of double curvature, similar to that shown in 1885. It shows indications of synclinal structure, but not so clearly as in 1886. On the small scale negative, taken by Dr. Hansky, of the Russian Expedition, this ray is shown tapering to a point and then slightly widening again, attaining a height of more than two diameters. At the base of this ray is a prominence, as in the corresponding ray of 1886. Immediately to the S. of this great ray is a well-marked opening in the corona, filled with three narrow, nearly radial rays. A sharp, narrow rift separates these from the somewhat inconspicuous equatorial group, within which two rays seem abruptly bent aside in an equatorial direction, and at the base are indications of small rays completely bending over. South of the equatorial group are two conical masses, showing distinct traces of synclinal structure. The southern of these masses is the larger, and forms the western boundary of the great southern rift. With the exception of the great N.W. ray, which extends further than any part of the corona, the western side is less conspicuous and extensive than the eastern, and shows less detail near the limb.

On the eastern side the north polar rift is bounded by a large mass, composed of broad rays showing some tendency to synclinal curves. The edge which bounds the rift is much inclined from the radial in an equatorial direction, and is more sharply defined than any other coronal feature. A large prominence (about latitude 45°) is at the base of the group; there is a decided thinning of the corona around it, and the lower rays, to a height of about 3′, appear to bend over the prominence.

A broad V-shaped opening, partly filled up with rays, separates the N.E. from the E. equatorial group. This is very large, reaching a greater height than any other portion of the corona (except the N.W. ray), and extending along the limb to about latitude 45° S., after which the group breaks up into a succession of broad rays curving away from the S. pole, and forming the eastern portion of the S. polar rift. The detail on the eastern side of the corona is extremely complex and interesting, but a few points only can be noted. A small hook-shaped ray, about $2\frac{1}{2}'$ high, springs from a small prominence (about 18° N. latitude), and is distinctly bounded by a narrow dark space or outline. A large double-headed prominence (latitude about 5° N.) is similarly outlined—the outline exactly following its contours. Apparently standing upon this prominence is a singular, dark, elliptical ring, about $2\frac{1}{2}'$ by 2′, its longer axis nearly radially directed. From the top of the ring rises a thin, tapering ray, curved towards the south. South of the bright prominence are small rays which appear cut across by dark veins at heights of 2′ and 3′ from the limb. Further south the great mass is broken up in a manner entirely unusual. The solar corona usually appears composed of overlapping rays emanating from the sun, but here it appears to be also broken up by dark channels into flocculent-looking masses, giving to it somewhat of the *curdled* appearance of some parts of the nebula in *Orion*. The great mass is roughly divided into a northern and southern portion by an irregular gap or dark stream, commencing at the top of a mass of rays about 6′ from the limb; this gap turns towards the north, then curves east, and is lost at a height of about 17′. The base of the equatorial mass is filled by rays having much contorted forms. It is impossible to resist the impression that this portion of the corona is torn by violent storms or perturbations.

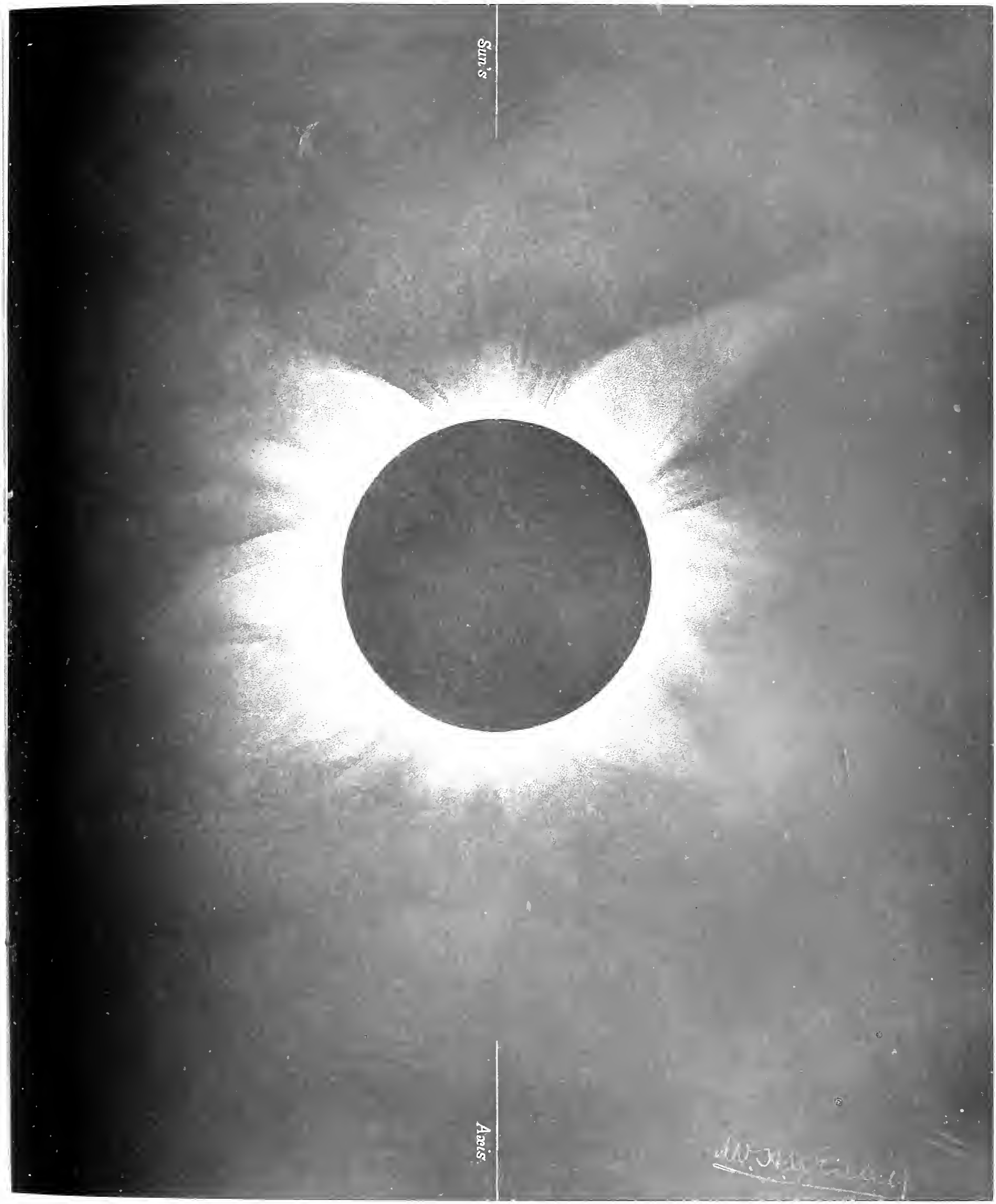
Conclusions.

1. The remarkable resemblance of the corona of 1896 to those of 1885 and 1886 confirms the now recognized theory of periodic changes in the corona in accordance with variations in the solar activity, as shown by sun spots.

2. The corona of 1896, as will be seen by the foregoing description, shows decided evidence of a connection between corona and prominences. This was indicated in 1893, but is still more striking in 1896.

3. The corona of 1896 shows the hitherto unperceived features of dark streams or veins, which it seems impossible to regard as merely spaces between bright rays. The only features I have hitherto seen, which to any degree resemble them, are the comet-like markings on the E. side of the corona of 1871. The dark bordering or outline to some of the prominences I have not observed on any other eclipse photographs.

N.

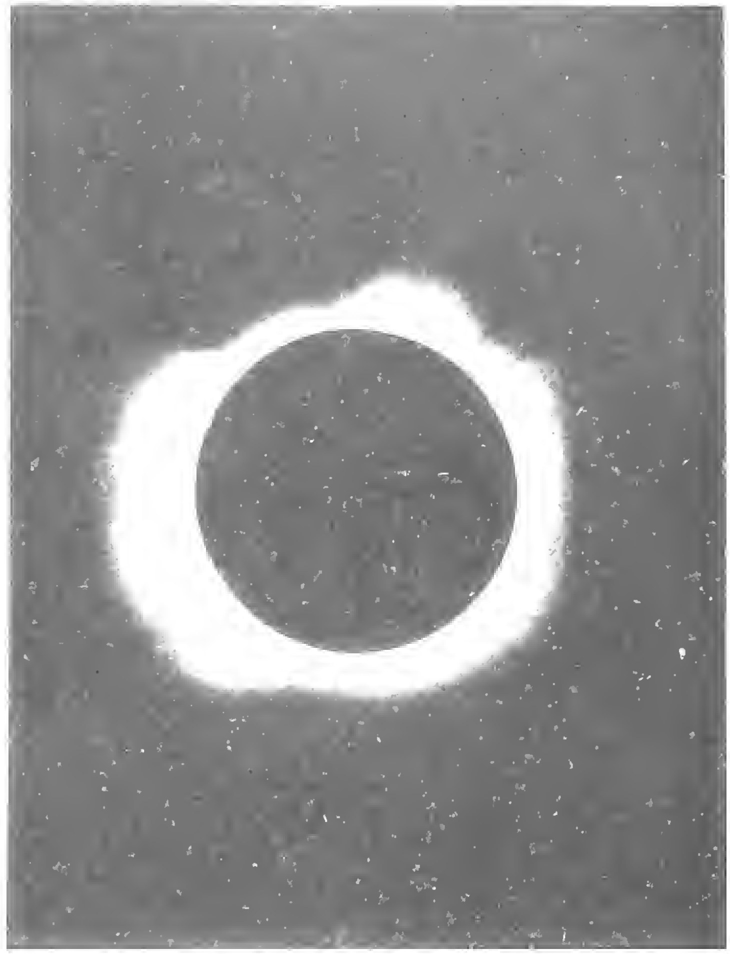


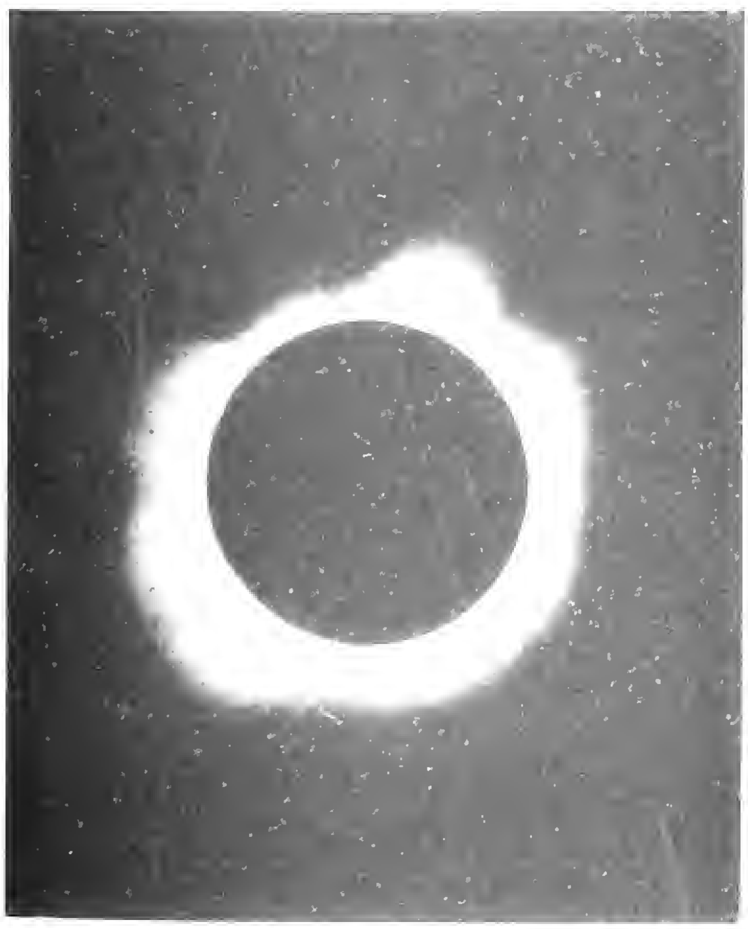
W.

S.

TOTAL ECLIPSE OF SUN, AUGUST 9, 1896.

1 

 2

3 

 4

1 EXPOSURE 5 SECS
BEGINNING OF TOTALITY

2 EXPOSURE 15 SECS
10 SECS IN TOTALITY

3 EXPOSURE 40 SECS
30 SECS IN TOTALITY

4 EXPOSURE 2 SECS
1 MIN 15 SECS IN TOTALITY

INDEX SLIP.

IX. *A Dynamical Theory of the Electric and Luminiferous Medium.—* Part III. *Relations with Material Media.*

By JOSEPH LARMOR, *F.R.S., Fellow of St. John's College, Cambridge.*

Received April 21, 1897,—Read May 13, 1897.

CONTENTS.

(The numbers refer to sections.)

9.2.98

1. In two previous memoirs* it has been explained, that the various hypotheses involved in the theory of electric and optical phenomena, which has been developed by FARADAY and MAXWELL, can be systematized by assuming the æther to be a continuous, homogeneous, and incompressible medium, endowed with inertia and with elasticity purely rotational. In this medium unitary electric charges, or electrons, exist as point-singularities, or centres of intrinsic strain, which can move about under their mutual actions; while atoms of matter are in whole or in part aggregations of electrons in stable orbital motion. In particular, this scheme provides a consistent foundation for the electrodynamic laws, and agrees with the actual relations between radiation and moving matter.

An adequate theory of material phenomena is necessarily ultimately atomic. The older mathematical type of atomic theory which regards the atoms of matter as acting on each other from a distance by means of forces whose laws and relations are gradually evolved by observation and experiment, is in the present method expanded and elucidated by the introduction of a medium through whose intervention these actions between the material atoms take place. It is interesting to recall the circumstance that GAUSS in his electrodynamic speculations, which remained unpublished during his lifetime, arrives substantially at this point of view; after examining a law of attraction, of the WEBERIAN type, between the "electric particles," he finally discards it and expresses his conviction, in a most remarkable letter to WEBER,† that

* 'Phil. Trans.,' 1894, A, pp. 719–822; 1895, A, pp. 695–743; referred to subsequently as Part I. and Part II. [In the abstract of the present Memoir, 'Roy. Soc. Proc.,' 61, on p. 281, line 6, *read* $2\pi n'^2 + \int i'' d\mathrm{F}$ for $2\pi n'^2$; line 35 *read* $\frac{2}{3} \cdot \frac{4}{5}\pi i'^2$ for $\frac{4}{5}\pi i'^2$; and on p. 284, line 18, *read* $m/2c \cdot \mathrm{E}(1 - m^2)$ for $\mathrm{E}(1 - m^2)$.]

† GAUSS, Werke, V., p. 629, letter to WEBER of date 1845; quoted by MAXWELL, "Treatise" II., § 861. After the present memoir had been practically completed, my attention was again directed, through a reference by ZEEMAN, to H. A. LORENTZ'S Memoir "La Théorie Electromagnetique de MAXWELL et son application aux corps mouvants," Archives Néerlandaises 1892, in which (pp. 70 *seqq.*) ideas similar to the above are developed. The electrodynamic scheme at which he arrives is formulated differently from that given in § 13 *infra*, the chief difference being that in the expression for the electric force (P, Q, R) the term $- d/dt$ (F, G, H) is eliminated by introducing the æthereal displacement (f, g, h). This applies also to the later "Versuch einer Theorie . . . in bewegten Körpern,' 1895. The author

the key-stone of electrodynamics will be found in an action propagated in time from one "electric particle" to another. The abstract philosophical distinction between actions at a distance and contact actions, which dates for modern science from GILBERT'S adoption of the scholastic axiom* *Nulla actio fieri potest nisi per contactum*, can have on an atomic theory of matter no meaning other than in the present sense. The question is simply whether a wider and more consistent view of the actions between the molecules of matter is obtained when we picture them as transmitted by the elasticity and inertia of a medium by which the molecules are environed, or when we merely describe them as forces obeying definite laws. But this medium itself, as being entirely supersensual, we must refrain from attempting to analyse further. It would be possible (*cf.* § 6) even to ignore the existence of an æther altogether, and simply hold that actions are propagated in time and space from one molecule of matter to the surrounding ones in accordance with the system of mathematical equations which are usually associated with that medium; in strictness nothing could be urged against such a procedure, though, in the light of our familiarity with the transmission of stress and motion by elastic continuous material media such as the atmosphere, the idea of an æthereal medium supplies so overwhelmingly natural and powerful an analogy as for purposes of practical reason to demonstrate the existence of the æther. The aim of a theory of the æther is not the impossible one of setting down a system of properties in which everything that may hereafter be discovered in physics shall be virtually included, but rather the practical one of simplifying and grouping relations and of reconciling apparent discrepancies in existing knowledge.

2. It would be an unwarranted restriction to assume that the properties of the æther must be the same as belong to material media. The modes of transmission of

remarks on the indirect manner in which dynamical equations had to be obtained, mainly on account of the absence of any notion as to the nature of the connexion between the stagnant æther and the molecules that are moving through it. "Dans le chemin qui nous a conduit à ces équations nous avons rencontré plus d'une difficulté sérieuse, et on sera probablement peu satisfait d'une théorie qui, loin de dévoiler le mécanisme des phénomènes, nous laisse tout au plus l'espoir de le découvrir un jour" (§ 91). In the following year (1893) similar general ideas were introduced by VON HELMHOLTZ, in his now well-known memoir on the electrical theory of optical dispersion, in which currents of conduction are included: but his argument is very difficult, and the results are in discrepancy with those of LORENTZ and the present writer in various respects in which the latter agree; moreover they are not consistent with the optical properties of moving material media. Both these discussions, of LORENTZ and of VON HELMHOLTZ, are in the main confined to electromotive phenomena: the treatment of the mechanical forces acting on matter in bulk would require for basis a theory of the mechanical relations of molecular media such as is developed in this paper. The results in the paper by ZEEMAN, above referred to, "On the Influence of Magnetism on the Light emitted by a substance," Verslagen Akad. Amsterdam, Nov. 28, 1896, have an important bearing on the view of the dynamical constitution of a molecule that has been advanced in these papers, and illustrated by calculation in an ideal simple case in Part I., §§ 114–8; *cf.* 'Roy. Soc. Proc.,' 60, 1897, p. 514. [See 'Phil. Mag.,' Dec. 1897: where the loss of energy by radiation from the moving ions is also examined.]

* GILBERT, *de Magnete*, 1600.

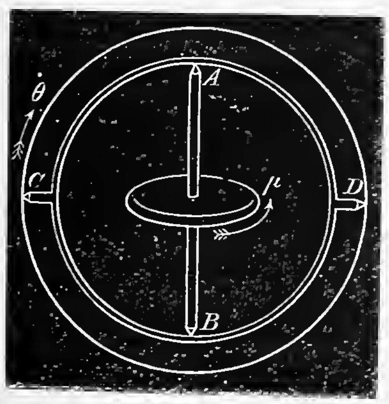
stress by media sensibly continuous were however originally formulated in connexion with the observed properties of elastic matter; and the growth of general theories of stress-action was throughout checked and vivified by comparison with those properties. It was thus natural in the first instance to examine whether a restriction to the material type of elastic medium forms an obstacle in framing a theory of the æther; but when that restriction has been found to offer insuperable difficulties it seems to be equally natural to discard it. Especially is this the case when the scheme of properties which specifies an available medium turns out to be intrinsically simpler than the one which specifies ordinary isotropic elastic matter treated as continuous.

A medium, in order to be available at all, must transmit actions across it in time; therefore there must be postulated for it the property of inertia,—of the same kind as ordinary matter possesses, for there can hardly be a more general kind,—and also the property of elasticity or statical resistance to change either of position or of form. In ordinary matter the elasticity has reference solely to deformation; while in the constitution here assumed for the æther there is perfect fluidity as regards form, but elastic resistance to rotational displacement.* This latter is in various ways formally the simpler scheme; elasticity depending on rotation is geometrically simpler and more absolute than elasticity depending on change of shape; and moreover no phenomenon has been discovered which would allow us to assume that the property of elasticity of volume, which necessarily exists in any molecularly constituted medium such as matter, is present in the æther at all. The objection that rotational elasticity postulates absolute directions in space need hardly have weight when it is considered that a definite space, or spacial framework fixed or moving, to which motion is referred, is a necessary part of any dynamical theory. The other fundamental query, whether such a scheme as the one here sketched could be consistent with itself, has perhaps been most convincingly removed by Lord KELVIN'S actual specification of a gyrostatic cellular structure constituted of ordinary matter, which has to a large extent these very properties; although the deduction of the whole scheme of relations from the single formula of Least Action, in its ordinary form in which the number of independent variables is not unnaturally increased, includes its ultimate logical justification in this respect.

* I find that the rotational æther of MACCULLAGH, which was advanced by him in the form of an abstract dynamical system (for reasons similar to those that prompted MAXWELL to finally place his mechanism of the electric field on an abstract basis) was adopted by RANKINE in 1850, and expounded with full and clear realization of the elastic peculiarities of a rotational medium: by him also the important advantage for physical explanation, which arises from its fluid character, was first emphasized. *Cf.* Miscellaneous Scientific Papers, pp. 63, 160. In RANKINE'S special and peculiar imagery, the æther was however a polar *medium* or *system* (as contrasted with a *body*) made up of polarized nuclei (*Cf.* Part I., §§ 37–8) whose vortical atmospheres, where such exist, constitute material atoms. The supposed necessity of having the vibration at right angles to the plane of polarization also misled him to the introduction of complications into the optical theory, such as æolotropic inertia, and to deviations from MACCULLAGH'S rigorous scheme.

On Material Models and Illustrations of the Æther and its associated Electrons.

3. Although the GAUSSIAN aspect of the subject, which would simply assert that the primary atoms of matter exert actions on each other which are transmitted in time across space in accordance with MAXWELL'S equations, is a formally sufficient basis on which to construct physical theory, yet the question whether we can form a valid conception of a medium which is the seat of this transmission is of fundamental philosophical interest, quite independently of the fact that in default of the analogy at any rate of such a medium this theory would be too difficult for development. With a view to further assisting a judgment on this question, it is here proposed to describe a process by which a dynamical model of this medium can be theoretically built up out of ordinary matter,—not indeed a permanent model, but one which can be made to continue to represent the æther for any assignable finite time, though it must ultimately decay. The æther is a perfect fluid endowed with rotational elasticity; so in the first place we have—and this is the most difficult part of our undertaking—to construct a material model of a perfect fluid, which is a type of medium nowhere existing in the material world. Its characteristics are continuity of motion and absence of viscosity: on the other hand in an ordinary fluid, continuity of motion is secured by diffusion of momentum by the moving molecules, which is itself viscosity, so that it is only in motions such as vibrations and slight undulations where the other finite effects of viscosity are negligible, that we can treat an ordinary fluid as a perfect one. If we imagine an aggregation of frictionless solid spheres, each studded over symmetrically with a small number of frictionless spikes (say four) of length considerably less than the radius,* so that there are a very large number of spheres in the differential element of volume, we shall have a possible though very crude means of representation of an ideal perfect fluid. There is next to be imparted to each of these spheres the elastic property of resisting absolute rotation; and in this we follow the lines of Lord KELVIN'S gyrostatic vibratory æther. Consider a gyrostat consisting of a flywheel spinning with angular momentum μ, with its axis AB pivoted as a diameter on a ring whose perpendicular diameter CD is itself pivoted on the sphere, which may for example be a hollow shell with the flywheel pivoted in its interior; and examine the effect of imparting a small rotational displacement to the sphere. The direction of the axis of the gyrostat will be displaced only by that component of the rotation



* The use of these studs is to maintain continuity of motion of the medium without the aid of viscosity; and also (§ 4) to compel each sphere to participate in the rotation of the element of volume of the medium, so that the latter shall be controlled by the gyrostatic torques of the spheres.

which is in the plane of the ring; an angular velocity $d\theta/dt$ in this plane will produce a torque measured by the rate of change of the angular momentum, and therefore by the parallelogram law equal to $\mu d\theta/dt$ turning the ring round the perpendicular axis CD, thus involving a rotation of the ring round that axis with angular acceleration $\mu/i \,.\, d\theta/dt$, that is with velocity $\mu/i \,.\, \theta$, where i is the aggregate moment of inertia of the ring and the flywheel about a diameter of the wheel. Thus when the sphere has turned through a small angle θ, the axis of the gyrostat will be turning out of the plane of θ with an angular velocity $\mu/i \,.\, \theta$, which will persist uniform so long as the displacement of the sphere is maintained. This angular velocity again involves, by the law of vector composition, a decrease of gyrostatic angular momentum round the axis of the ring at the rate $\mu^2/i \,.\, \theta$; accordingly the displacement θ imparted to the sphere originates a gyrostatic opposing torque, equal to $\mu^2/i \,.\, \theta$ so long as $\mu/i \,.\, \int\theta dt$ remains small, and therefore of purely elastic type. If then there are mounted on the sphere three such rings in mutually perpendicular planes, having equal free angular momenta associated with them, the sphere will resist absolute rotation in all directions with isotropic elasticity. But this result holds only so long as the total displacement of the axes of the flywheels is small: it suffices however to confer rotatory elasticity, as far as is required for the purpose of the transmission of vibrations of small displacement through a medium constituted of a flexible framework with such gyrostatic spheres attached to its links, which is Lord KELVIN's gyrostatic model* of the luminiferous working of the æther. For the present purpose we require this quality of perfect rotational elasticity to be permanently maintained, whether the disturbance is vibratory or continuous. Now observe that if the above associated free angular momentum μ is taken to be very great, it will require a proportionately long time for a given torque to produce an assigned small angular displacement, and this time we can thus suppose prolonged as much as we please: observe further that the motion of our rotational æther in the previous papers is irrotational except where electric force exists which produces rotation proportional to its intensity, and that we have been compelled to assume a high coefficient of inertia of the medium, and therefore an extremely high elasticity in order to conserve the ascertained velocity of radiation, so that the very strongest electric forces correspond to only very slight rotational displacements of the medium: and it follows that the arrangement here described, though it cannot serve as a model of a field of steady electric force lasting for ever, can yet theoretically represent such a field lasting without sensible decay for any length of time that may be assigned.

4. It remains to attempt a model (*cf.* Part I., § 116) of the constitution of an electron, that is of one of the point-singularities in the uniform æther which are taken to be the basis of matter, and at any rate are the basis of its electrical phenomena. Consider the medium composed of studded gyrostatic spheres as above: although the motions of the æther, as distinct from the matter which flits across it,

* Lord KELVIN, 'Comptes Rendus,' Sept. 1889: 'Math. and Phys. Papers,' III., p. 466.

are so excessively slow on account of its great inertia that viscosity might possibly in any case be neglected, yet it will not do to omit the studs and thus make the model like a model of a gas, for we require rotation of an individual sphere to be associated with rotation of the whole element of volume of the medium in which it occurs. Let then in the rotationally elastic medium a narrow tubular channel be formed, say for simplicity a straight channel AB of uniform section : suppose the walls of this channel to be grasped, and rotated round the axis of the tube, the rotation at each point being proportional for the straight tube to $AP^{-2} + PB^{-2}$: this rotation will be distributed through the medium, and as the result there will be lines of rotational displacement all starting from A and terminating at B : and so long as the walls of the channel are held in this position by extraneous force, A will be a positive electron in the medium, and B will be the complementary negative one. They will both disappear together when the walls of the channel are released. But now suppose that before this release the channel is filled up (except small vacuous nuclei at A and B which will assume the spherical form) with studded gyrostatic spheres so as to be continuous with the surrounding medium ; the effort of release in this surrounding medium will rotate these spheres slightly until they attain the state of equilibrium in which the rotational elasticity of the new part of the medium formed by their aggregate provides a balancing torque, and the conditions all round A or B will finally be symmetrical. We shall thus have created two permanent conjugate electrons A and B ; each of them can be moved about through the medium, but they will both persist until they are destroyed by an extraneous process the reverse of that by which they are formed. Such constraints as may be necessary to prevent division of their vacuous nuclei are outside our present scope ; and mutual destruction of two complementary electrons by direct impact is an occurrence of infinitely small probability. The model of an electron thus formed will persist for any finite assignable time if the distribution of gyrostatic momentum in the medium is sufficiently intense : but the constitution of our model of the medium itself of course prevents, in this respect also, absolute permanence. It is not by any means here suggested that this circumstance forms any basis for speculation as to whether matter is permanent, or will gradually fade away. The position that we are concerned in supporting is that the cosmical theory which is used in the present memoirs as a descriptive basis for ultimate physical discussions is a consistent and thinkable scheme ; one of the most convincing ways of testing the possibility of the existence of any hypothetical type of mechanism being the scrutiny of a specification for the actual construction of a model of it.

5. An idea of the nature and possibility of a self-locked intrinsic strain, such as that here described, may be facilitated by reference to the cognate example of a *material* wire welded into a ring after twist has been put into it. We can also have a closer parallel, as well as a contrast ; if breach of continuity is produced across an element of interface in the midst of an incompressible medium endowed with

ordinary material rigidity, for example by the creation of a lens-shaped cavity, and the material on one side of the breach is twisted round in its plane, and continuity is then restored by cementing the two sides together, a model of an electric doublet or polar molecule will be produced, the twist in the medium representing the electric displacement and being at a distance expressible as due to two conjugate poles in the ordinary manner. Such a doublet is permanent, as above; it can be displaced into a different position, at any distance, as a strain-form, without the medium moving along with it; such displacement is accompanied by an additional strain at each point in the medium, namely, that due to the doublet in its new position together with a negative doublet in the old one. A series of such doublets arranged transversely round a linear circuit will represent the integrated effect of an electric polarization-current in that circuit; they will imply irrotational linear displacement of the medium round the circuit after the manner of vortex motion, but this will now involve elastic stress on account of the rigidity. Thus with an ordinary elastic solid medium, the phenomena of dielectrics, including wave-propagation, may be kinematically illustrated; but we can thereby obtain no representation of a single isolated electric charge or of a current of conduction, and the laws of optical reflexion would be different from the actual ones. This material illustration will clearly extend to the dynamical laws of induction and electromagnetic attraction between alternating currents, but only in so far as they are derived from the kinetic energy; the law of static attraction between doublets of this kind would be different from the actual electric law.

6. According to the present scheme the ponderomotive forces acting on matter arise from the forces acting on the electrons which it involves; the application of the principle of virtual work to the expression for the strain-energy shows that, for each electron at rest, this force is equal to its charge multiplied by the intensity of the electric field where it is situated. It has been urged that a model of the æthereal electric field cannot be complete, and so must be rejected, unless it exhibits a direct mechanism by which the ponderomotive normal traction $F^2/8\pi$ is transmitted across the æther from the surface of one conducting region to that of another: but the position can be maintained that such a representation would transcend the limitations belonging to a mechanical model of a process which is in part mechanical and in part ultra-mechanical. Indeed if this force were transmitted in the ordinary elastic sense, the transmitting stress would have to be of the nature of a self-balancing FARADAY-MAXWELL stress involving the square of the æther-strain instead of its first power, and thus not directly related to elastic propagation. The model above described is so to speak made of æther, and ought to represent all the tractions that exist in æther, vanishing as they do over the surface of a conducting region: but the model does not in the ordinary sense represent matter at all, except in so far as the æthereal strain-form which constitutes the electron is associated with matter. It therefore cannot represent directly, after the manner of a stress across a medium, a force

acting on matter, for that would from this ultimate standpoint be a force acting on a strain-form spreading from its nucleus all through the medium, not a traction on a definite surface bounding the matter.

The fact is that transmission of force by a medium, or by contact action so-called, remains merely a vague phrase until the strain-properties of that medium are described; the scientific method of describing them is to assign the mathematical function which represents its energy of strain, and thence derive its relations of stress by the principle of virtual work; a real explanation of the transmission of a force by contact action must be taken to mean this process. Now in an elastic medium permeated by centres of permanent intrinsic strain, whether it be the rotational æther with its contained electrons, or an ordinary elastic solid permeated by polar strain-nuclei as described above, the specification of the strain-energy of the medium involves a mathematical function, not only of the displacement at each material point of the medium, but also of the positions of these intrinsic strain-centres which can move independently through it. To derive the play of internal force, this energy function must be varied with respect to all these independent quantities; the result is elastic tractional stress in the medium across every ideal interface, *together with* forcive tending to displace each strain-centre, which we can consider either as resisted by extraneous constraint preventing displacement of the strain-centre, or as compensated by the reaction of the inertia of the strain-form against acceleration.* Consider, for example, the analogy of the elastic solid medium, and suppose a portion of it to be slowly strained by extraneous force; two strains are thereby set up in it, namely that strain which would be thus originated if the solid were initially devoid of intrinsic strain, and that strain which has to be superposed in order to attain the new configuration of the intrinsic strain arising from the displacement of its nuclei. The latter part is conditioned by the displacement of these strain-centres, and in its production forces acting on them must be considered to assist, whose intensities may be determined as has been already done in the æthereal problem.

The attractions between material bodies are therefore not transmitted by the æther in the way that mechanical tractions are transmitted by an ordinary solid, for it is electric force that is so transmitted: but neither are they direct actions at a distance. The point of view has been enlarged: the ordinary notion of the transmission of force, as framed mathematically by LAGRANGE and GREEN for a simple elastic medium without singularities, is not wide enough to cover the phenomena of a medium containing intrinsic strain-centres which can move about independently of the substance of the medium. But the same mathematical principles lead to the necessary extension of the theory, when the energy function thus involves the positions of the

* Thus when the medium is in equilibrium, there is in it only the static intrinsic strain diverging from these centres, which gives rise to the forces between them; but when it is disturbed by radiation or otherwise, there is also the strain thence arising.

strain-centres as well as the elastic displacement in the medium; and the theory which in the simpler case answers fairly to the description of transmission by contact action has features in the wider case to which that name does not so suitably apply.* The strain-centres (that is, the matter) have, in the strict sense of the term, *energy of position*, or *potential energy*, due to their mutual configuration in the æther, which can come out as work done by mutual forces between them when that configuration is altered, which work may be used up either in accumulating other potential energy elsewhere, or in increasing the kinetic energy of the matter, which is itself, in whole or in part, energy in the æther arising from the movement of the strain-forms across it. Discussions as to transmission by contact are not the fundamental ones, as the above actual material illustration shows: the single comprehensive basis of dynamics into which all such partial modes of explanation and representation must fit and be coordinated is the formula of Stationary Action, including, as the particular case which covers all the domain of steady systems, the law that the mutual forces of such a system are derived from a single analytical function which is its available potential energy.

The circumstance that no mode of transmission of the mechanical forces, of the type of ordinary stress across the æther, can be put in evidence, thus does not derogate from the sufficiency of the present standpoint. The transmission of material traction by an ordinary solid, which is now often taken as the type to which all physical action must conform, is merely an undeveloped notion arising from experience, which must itself be analysed before it becomes of scientific value: the explanation thereof is the quantitative development of the notion from the energy function by the method of virtual work in the manner indicated in § 10 *infra*. This orderly development of the laws of action across a distance, from an analytical specification of a distribution of energy pervading the surrounding space, is the essence of the so-called principle of contact action. It is precisely what the present procedure carries out, with such generalization as the scope of the problem demands; besides attaining a correlation of the whole range of the phenomena, it avoids the antinomies of partial theories which accumulate on the æther contradictory and unrelated properties, and sometimes even save appearances by passing on to the simple fundamental medium those complex properties of viscous matter whose real origin is to be found in its molecular discreteness.

* An analogous principle applies in the vortex-theory illustration of matter. If we consider rigid cores round which the fluid circulates, they are moved about by the fluid pressure: but if we consider vortex-rings, say with vacuous cores, these are mere forms of motion that move across the fluid, and if we take them to represent atoms, the interactions between aggregations of atoms cannot be traced by means of fluid pressures, but can only be derived from the analytical character of the function which expresses the energy.

Æther contrasted with Matter.

7. The order of development here followed is thus avowedly based on the hypothesis that the æther is a very simple uniform medium, about which it may be possible to know all that concerns us; and the present state of the theories of optics and electricity does much to encourage that idea. This procedure is of course at variance with the extreme application of the inductive canon, which would not allow the introduction of any hypothesis not based on direct observation and experiment. But though that philosophy has abundantly vindicated itself as regards the secondary properties of matter, which are amenable to direct examination, its rigid application would debar us from any theory of the æther at all, as we can only learn about it from circumstantial evidence. We could then merely go on heaping up properties on the æther, on the analogy of what is known of matter, as circumstances necessitated; and this medium would be a sort of sink to dispose of relations that could not be otherwise explained. Whereas matter, with which we are familiar, is the really complicated thing on which all the maze of physical phenomena depends, so that it is doubtful whether much can ever be known definitely as to its ultimate dynamical constitution; our best chance is to try to approach it through the presumably simple and homogeneous æther in which it subsists.

For example, it is found that the transmission of electrostatic force is affected by the constitution of the material dielectric through which it passes, and this is explained by a perfectly valid theory of polarization of the molecules of the matter: to press the analogy and ascribe the possibility of transmission through a vacuum to polarization of the æther may be convenient for some purposes of description, but in the majority of cases the impression is left that the so-called polarization of the æther is thereby explained. Whereas the processes being, almost certainly, of totally different character in the two cases, it will conduce to accurate thought to altogether avoid using the same term in the two senses, and to speak of the *displacement* of the æther which transmits electric force across a vacuum as producing *polarization* in the molecules of a material dielectric which exists in its path, which latter in turn affects the transmission of the electric force by reaction. In trying to pass beyond this stage, we may accumulate descriptive schemes of equations, which express, it may be with continually increasing accuracy, the empirical relations between these two phenomena; but we can never reach very far below the surface without the aid of simple dynamical working hypotheses, more or less *a priori*, as to how this interaction between continuous æther and molecular matter takes place.

8. On the present view, physical theory divides itself into two regions, but with a wide borderland common to both: the theory of radiation or the kinetic relations of this ultimate medium; and the theory of the forces of matter which deals for the most part with molecular movements so slow that the surrounding æther is at each instant practically in an equilibrium condition, so that the material atoms practically

act on each other from a distance with forcives obeying definite laws, derivable from the formula for the energy. It is only in electromagnetic phenomena and molecular theory that non-vibrational movements of the æther are involved. The æther not being matter, it need not obey the laws of the dynamics of matter, provided it obey another scheme of dynamical laws consistent among themselves; these laws must however be such that we can construct in the æther an atomic system of matter which itself obeys the actual material laws. The sole spacial relations of the æther itself, on which its dynamics depend, those namely of incompressibility and rotational elasticity, are thus to be classed along with the existing EUCLIDEAN relations of measurements in space (which also might *a priori* be different from what they are) as part of the ultimate scheme of mental representation of the actual physical world. The elastic and other characteristics of ordinary matter, including its viscous relations, are on the other hand a direct consequence of its molecular constitution, in combination with the law of material energy which is itself a consequence of the fact that the energies of the atoms are wholly located in the surrounding simple continuous æther and are thus functions of their mutual configurations. In this way we come round again to an order of procedure similar to that by which CAUCHY and POISSON originally based the elastic relations of material bodies on the mutual actions of their constituent molecules.

Consider any two portions of matter which have a potential-energy function depending, as above explained, on their mutual configuration alone, the material movements being thus comparatively slow compared with the velocity of radiation; any displacement of them as a single rigid system, whether translational or rotational, can involve no expenditure of work; hence the resultant forcive exerted by the first system on the second must statically equilibrate that exerted by the second system on the first, these forcives must in fact be equal and opposite wrenches on a common axis; and the energy principle thus involves the principle of the balance of action and reaction, in its most general form. This stress, between two molecules, is usually sensible only at molecular range; hence the action of the surrounding parts on a portion of a solid body is practically made up of tractions exerted over the interface between them. Further, since rotation of the body without deformation cannot alter the potential energy of mutual configuration of the molecules, it follows that for a rectangular element of ordinary solid matter the tangential components of these tractions must be self-conjugate, as they are taken to be in the ordinary theory of elasticity. On the other hand, for a medium not molecularly constituted we can hardly treat at all of mutual configuration of parts, and the self-conjugate stress-relation will not be a necessary one.

A certain similarity may be traced with the view of FARADAY, who was disinclined to allow that ray-vibrations are transmitted by any medium of the molecular character of ordinary matter, but considered them rather as affections of the lines which represent electric force, the propagation being influenced by the material nuclei which in ponderable media disturb these lines. This propagation in time requires inertia and elasticity for its mathematical expression, and the problem of the free æther is to find what kind of each is requisite.

9. A theory which, like the present one, explains atoms of matter as made up of singularities of strain and motion in the æther, is bound to look for an explanation of gravitation by means of the properties of that medium; it cannot avail itself of Cotes's dogma that gravitation at a distance is itself as fundamental and intelligible as any explanation thereof could be. In further development of the illustrative possibilities of the pulsatory theory of gravitation, mentioned in the previous papers, we can (ideally) imagine the pulsation to have been applied initially over the outside boundary of the æthereal universe, and thence instantaneously communicated throughout the incompressible medium to the only places that can respond to it, the vacuous nuclei of the electrons; and we can even imagine the pulsations thus established as spontaneously keeping time and phase ever after, when the exciting cause which established this harmony has been discontinued.

It has been noticed in Part I, § 103, that gravitation cannot be transmitted by any action of the nature of statical stress; for then the approach of two atoms would increase the strain, and therefore also the stress, and therefore also in a higher ratio the energy of strain which depends on their product, and hence the mutual forces of the atoms would resist approach. As gravitation must belong to the ultimate constituents of matter, that is on this theory to the electrons, and must be isotropic all round each of them, it would appear that no mediate æthereal representation of it is possible except the one here considered. The radially vibrating field might be described formally as the magnetic field of the electron considered as a unipolar magnet, necessarily of very rapidly alternating type because otherwise a field of gravitation would be an ordinary magnetic field. The bare groundwork of this hypothesis may thus be formally expressed in Maxwell's language and developed along his lines, by postulating that the electron is not only a centre of steady intrinsic electric force, but also a centre of alternating intrinsic magnetic force, instantaneously transmitted because it would otherwise involve condensation, each force being necessarily radial.* The unsatisfactory feature is that this radial *quasi*-magnetic field is introduced for the sake of gravitation alone, which does not present itself as in any direct correlation with other physical agencies.

The following sections are occupied chiefly with an attempt to logically systematize, and in various respects extend, the electric aspect of molecular theory. The preceding paper dealt mainly with the molecular side of directly æthereal phenomena, such as electric and radiative fields; of the present one the earlier part follows up the same subject, and the remainder relates to the actions of the molecules of polarized

* Two steady magnetic poles of like sign would repel each other: but in the case of two poles pulsating in the same phases there is also an inertia term in the fluid æthereal pressure, and the result is as stated above. *Cf.* Hicks, 'Proc. Camb. Phil. Soc.,' 1880, p. 35.

material bodies on one another, and the material stresses and physical changes thereby produced. As in the preceding papers, the quantitative results are to a large extent independent of any special theory of the constitution of matter, such as is here employed to bind together and harmonize the separate groups of phenomena, and to form a mental picture of their mutual relations; so far as they are electric they may be based directly on MAXWELL's equations of the electric field in *free space*, which form a sufficient description of the free æther, and have been verified by experiment. In the FARADAY-MAXWELL theory, however, as usually expounded, an explanation of these equations is found, explicitly or tacitly, in an assumption that the æther is itself polarizable in the same manner as a material medium, and æther is in fact virtually considered to be matter; on the present theory the equations for free space are an analytical statement of the ultimate dynamical definition of the continuous æthereal medium, and the polarization of material bodies with the resulting forcive are deduced from the relation of their molecules to this medium in which they have their being.

10. In the modern treatment of material dynamics, as based on the principle of energy, the notion of configuration is, as above explained, fundamental. The potential energy, from which the forces are derived, is a function of the mutual configurations of the parts of the material system. In the case of forces of elasticity the internal energy is primarily a function of the mutual configurations of the individual molecules, from which a regular or organised part (§ 49 *infra*) is separated which is expressible in terms of the change of configuration of the differential element of volume containing a great number of molecules, and from which alone is derived the stress that is mechanically transmitted. In connexion with the discussion of contact action in § 6 above, the mode of this derivation and transmission becomes a subject of interest.* In the first place the primary notion of a *force* as acting from one point to another in a straight line, has to be generalized into a *forcive* in LAGRANGE's manner on the basis of the principle of virtual work: then the forcive arising from the internal strain-energy of the element of volume of the material is derived by variation of this organized energy, and appears primarily as made up of definite complex bodily forcives resisting the various types of strain that occur in the element: then these forcives are rearranged, by the process of integration by parts, into a uniform translatory force acting throughout the element of volume of the material (which must compensate the extraneous applied bodily forcive) together with tractions acting over its surface. When this is done also for adjacent elements of volume, other tractions arise which must compensate the

* It is here assumed that the direct action between the molecules is sensible only at molecular distances, which would not be the case if the material were electrically polarized. The statement also refers solely to transmitted mechanical stress of the ordinary kind: more complicated types, not expressible by surface tractions alone, are put aside, as well as molecular conceptions like the LAPLACIAN intrinsic pressure in fluids. *Cf.* §§ 44–6 *infra*.

previous ones over the part of the surface that is common to the two elements; and thus the uncompensated traction is passed on from element to element until finally the boundary of the material system is reached where it remains uncompensated and must be balanced extraneously. The outstanding irregular part of the aggregate mutual potential energy of the individual molecules, which cannot be included in a function of strain of the element of volume, cannot on that account take part in the transmission of mechanical forces, and is evidenced only in local changes of the physical properties and temperature of the material. *Cf.* § 48 *infra.*

The other main division of the energy is the kinetic part, which is specified in terms of the rate of change of configuration of the material system with respect to an extraneous spacial framework to which its position is referred. Whatever notions may commend themselves *a priori* as to the impossibility of absolute space and absolute time, the fact remains that it has not been found possible to construct a system of dynamics which has respect only to the relative positions of moving bodies; and the reason suggests itself, that there is an underlying part of the phenomena, which does not usually explicitly appear in abstract material dynamics, namely, the æthereal medium, and that the spacial framework in absolute rest, which was introduced by NEWTON and was probably a main source of the great advance in abstract dynamics originated by the *Principia*, is in fact the quiescent underlying æther. In this way the purely *a priori* standpoint is pushed away a stage, and we may find justification against the reproach that a philosophical formulation of dynamics should be concerned only with relative motions.

Relation to Gas-Theory: Internal Molecular Energy.

11. The kinetic theory of gases is considerably affected by the view here taken of the constitution of a molecule. In those simple and satisfactory features which are concerned only with the translatory motion of the molecules, it stands intact; but it is different with problems, like that of the ratio of the specific heats, which involve the internal energy. According to the usual hypothesis of the theory of gases, all the internal kinetic energy of the molecule is taken to be thermal and in statistical equilibrium, through encounters, with the translatory energy. But on the present view, the energy of the steady orbital motions in the molecule (including therein slow free precessions) makes up both the energy of chemical constitution and the internal thermal energy; while it is only when these steady motions are disturbed that the resulting vibration gives rise to radiation by which some of the internal energy is lost. The amount of internal energy can however never fall below the minimum that corresponds to the actual conserved rotational momenta of the molecule; this minimum is the energy of chemical combination of its ultimate constituents, while the excess above it actually existing is the internal thermal energy.* The present

* As a concrete illustration, we can imagine two ideal atoms, each consisting of a single gyrostat

view requires that the energy of chemical constitution shall be very great compared with the thermal energy; but for this very reason our means of chemical decomposition are limited, so that only a part of that energy is experimentally realizable.* This being the case, the alteration produced by external disturbance in the state of steady internal motions of the molecule consists in the superposition on it of very slow free precessional motions, which have practically no influence on its higher free periods:† and this explains why change of temperature has no influence on the positions of the lines in a spectrum. As a gas at high temperature must contain molecules with all amounts of internal thermal energy from nothing upwards, we should on the other hand, on the ordinary gas-theory, expect both a shift of the brightest part of a spectral line when the temperature is raised, and also a widening of its diffuse margin.

The ordinary encounters between the molecules will influence this thermal energy or energy of slow precessional oscillation, without disturbing the state of steady constitutive motion on which it is superposed, therefore without exciting radiation, which depends on more violent disturbances involving dissociative action.

On this view the postulates of the MAXWELL-BOLTZMANN theorem on the distribution of the internal energy in gases would not obtain, for the thermal energy of the molecule would not be expressible as a sum of squares. The ratio of the specific heats in a gas must still lie between 1 and $\frac{5}{3}$; but the nature of the similarity of molecular constitution in the more permanent gases, which makes the ratio of the total thermal energy to the translatory energy either $\frac{5}{3}$ or unity for most of them, would remain to be discovered. In those gases for which the latter value obtains, the energy of precessional motion in the molecule would be negligibly small, involving small resultant angular momentum and *possibly* small paramagnetic moment.

The necessity of a distinction such as that here drawn between the internal thermal energy and the energy of the vibratory disturbances of internal structure which maintain radiation, is well illustrated by the recent recognition (foreshadowed by DULONG and PETIT's researches on the law of cooling) and application by DEWAR of the remarkable insulating power of a vacuum jacket as regards heat. If this distinction did not exist, both conduction and convection must ultimately depend on

enclosed in a suitable massless case, coming into mutual encounter. We may imagine that neither of them has any internal heat; so that the internal energy of each is the minimum that corresponds to its steady gyrostatic momentum, and the axis of each gyrostat therefore keeps a fixed direction in space. The result of the encounter will be that the axis of each gyrostat acquires steady wobbling or free precessional motion, so that its internal energy is increased at the expense of the energy of translation of the atoms; but in this the simplest case there will be no unsteady vibration, such as could be radiated away. If however there are also other types of momenta associated with the atom, for example if the case of the gyrostat is not massless, the encounter will leave vibrations about the new state of steady motion, which if of high enough period will lead to loss of energy by radiation.

* Ideas somewhat similar to the above are advanced by WATERSTON in his classical memoir of 1845 on gas-theory, recently edited by Lord RAYLEIGH; 'Phil. Trans.' (A), 1892, p. 51.

† *Cf.* THOMSON and TAIT, 'Nat. Phil.' § 345 xxiv.

transfer by ordinary radiation at small distances, as FOURIER imagined; and it would not appear why convection by a gas, even when highly rarefied, is so much more efficient in the transfer of heat than radiation.

12. The result obtained by RAMSAY and YOUNG, and others, that all over the gas-liquid range the characteristic equations of the substances on which they experimented proved to be very approximately of the form $p = a\mathrm{T} + b$, where a and b are functions of the density alone, also supplies corroboration to this view. Expressing the increment of energy per unit mass $d\mathrm{E} = \mathrm{M}dv + \kappa d\mathrm{T}$, we have for the increment of heat supplied $d\mathrm{H} = d\mathrm{E} + pdv$; and the fact that $d\mathrm{E}$ and $d\mathrm{H}/\mathrm{T}$ are perfect differentials shows immediately that M is equal to $-b$ and κ is independent of v, so that the total (non-constitutive) energy per unit mass consists of two independent parts, an energy of expansion and an energy of heating.* The latter part is the thermal energy of the individual molecules; it is a function of their mean states and velocities alone, and constitutes almost all the energy in the gaseous state. The former part is the energy of mutual actions between the molecules; it is negative and bears a considerable ratio to the whole thermal energy in the liquid state, in the case of substances with high latent heats of evaporation; for all gases except hydrogen, inasmuch as they are cooled by transpiration through a porous plug, b is negative at ordinary densities. *Cf.* § 62, *infra.*

There would be no warrant for a view that the forces of chemical affinity fall off and finally vanish as the ultimate zero of temperature is approached. The translatory motions of the molecules would diminish without limit, and therefore also the opportunities for reaction between them, so that many chemical changes would cease to take place for the same reason that a fire ceases to burn when the supply of air is insufficient, or coal gas ceases to explode when too much diluted with air: but the energies of affinity exist all the time in probably undiminished strength, while the forces of cohesion are modified by the fall of temperature but not necessarily in the direction of extinction.

The Equations of the Æthereal Field, with Moving Matter: various applications: influence of Motion through the Æther on the Dimensions of Bodies.

13. Let (u, v, w) represent the total circuital current, and (u', v', w') the conducted part of it, which will be taken to include the current (u_0, v_0, w_0) of migration of the free electric charge as this is in all cases very small in comparison; let (f', g', h') denote the electric polarization of the material, and (f, g, h) the æthereal elastic displacement, so that the total circuital displacement of MAXWELL's theory is their sum (f'', g'', h''); let the space of reference be fixed with respect to the stagnant æther, and (p, q, r) be the velocity with which the matter situated at the point (x, y, z) is moving, and let δ/dt represent $d/dt + pd/dx + qd/dy + rd/dz$; let

* *Cf.* G. F. FITZGERALD, 'Roy. Soc. Proc.' 42, 1887: *cf.* also CLAUSIUS' early ideas on 'disgregation.'

(P, Q, R) denote the electric force, namely that which acts on the electrons, and (P′, Q′, R′) the æthereal force, that which produces the æthereal electric displacement (f, g, h); let ρ denote density of free electric charge. Then the electromotive equations are*

$$P = qc - rb - \frac{dF}{dt} - \frac{d\Psi}{dx}, \quad P' = -\frac{dF}{dt} - \frac{d\Psi}{dx}, \quad f = \frac{1}{4\pi C^2} P',$$

where

$$F = \int \frac{u}{r} d\tau + \int \left(B \frac{d}{dz} - C \frac{d}{dy} \right) \frac{1}{r} d\tau, \qquad a = \frac{dH}{dy} - \frac{dG}{dz};$$

and

$$u = u' + \frac{\delta f'}{dt} + \frac{df}{dt} + p\rho,\dagger$$

where

$$\rho = \frac{d(f' + f)}{dx} + \frac{d(g' + g)}{dy} + \frac{d(h' + h)}{dz}.$$

From the formula for (P, Q, R) FARADAY's law follows that the line integral of electric force round a circuit *in uniform motion with the matter* is equal to the time-rate of diminution of the magnetic flux through its aperture. The line-integral of the

* This scheme forms an improved summary of that worked out in Part II. §§ 15–19; the expressions there assigned for ρ and Ψ have here been corrected, and (u_0, v_0, w_0) is merged.

† [*Added Sept.* 14.—The term $\delta f'/dt$ in u arises as follows. In addition to the change of the polarization in the element of volume, df/dt, there is the electrodynamic effect of the motion of the positive and negative electrons of the polar molecule. Now the movement of two connected positive and negative electrons is equivalent to that of a single positive electron round the circuit formed by joining together the ends of their paths: and a similar statement holds when there are more than two electrons in the molecule. Hence the motion of a polarized medium with velocity (p, q, r), which need not be constant from point to point, produces the electrodynamic effect of a magnetization $(rg' - qh', ph' - rf', qf' - pg')$ distributed throughout the volume: *cf.* Part I, § 125. And it has been shown in Part II, § 31 that any distribution of magnetism (A, B, C) may be represented as a volume distribution of electric current equal to curl (A, B, C), which is necessarily circuital, together with a surface current sheet equal to $(Bn - Cm, Cl - An, Am - Bl)$. Thus, *when* (p, q, r) *is uniform and* (f', g', h') *is circuital*, the above magnetic distribution is equivalent to a current system $(pd/dx + qd/dy + rd/dz)(f', g', h')$ together with current sheets on interfaces of discontinuity: this system is to be added on to $d/dt\,(f', g', h')$ in order to give the full electrodynamic effect. Thus in these special circumstances the formulation in the test is correct in so far as it leads to the correct *differential* equations for the element of the medium: the *integral* expression there given for F is however only correct either when it is reduced to the differential form $-\nabla^2 F/4\pi = u + dC/dy - dB/dz$, which is derivable on integration of its second term by parts, or else when, the velocity of the matter still being uniform, discontinuous interfaces are replaced in the analysis by gradual though rapid transitions. These conditions are satisfied in all the applications that follow: but they would not be satisfied for example in the problem of the reflexion of radiation from the surface of moving matter.

But a formulation which is preferable to the above, in that it is *absolutely general*, is simply to implicitly include the above virtual magnetization directly in (A, B, C) and consequently change from $\delta f'/dt$ to df'/dt in the expression for u: this will also involve that the relation $A = \kappa\alpha$ which occurs lower down shall be replaced by $A = \kappa\alpha + rg' - qh'$, but there will be no further alteration in the argument of the text.. The boundary conditions of the text are unaltered.]

æthereal force (P′, Q′, R′) round a circuit *fixed in the æther* has the same value. Again if (F′, G′, H′) be defined so that $\mathrm{F}' = \int u/r \,.\, d\tau$, we have

$$a = \frac{d\mathrm{H}'}{dy} - \frac{d\mathrm{G}'}{dz} + 4\pi\mathrm{A} - \frac{d}{dx}\int\left(\mathrm{A}\frac{d}{dx} + \mathrm{B}\frac{d}{dy} + \mathrm{C}\frac{d}{dz}\right)\frac{1}{r}d\tau,$$

so that

$$\alpha + \frac{d\mathrm{V}'}{dx} = \frac{d\mathrm{H}'}{dy} - \frac{d\mathrm{G}'}{dz},$$

where (α, β, γ) is magnetic force and V′ is the potential of the magnetism: hence Ampère's law follows that the line integral of the magnetic force round *any* circuit is equal to 4π times the total current that flows through its aperture. These two circuital relations are coextensive with the previous equations involving the vector potential, and can thus replace them, when the difference between (P, Q, R) and (P′, Q′, R′) is inessential, that is (i) when the displacement currents are negligible, or (ii) when the matter is at rest; the quantity Ψ then enters as an arbitrary function in the integration of the equations.

The mechanical force acting on the matter, or ponderomotive force, is (X, Y, Z) per unit volume, where (§ 38 *infra*)

$$\mathrm{X} = \left(v - \frac{dg}{dt}\right)\gamma - \left(w - \frac{dh}{dt}\right)\beta + \mathrm{A}\frac{d\alpha}{dx} + \mathrm{B}\frac{d\alpha}{dy} + \mathrm{C}\frac{d\alpha}{dz} + f'\frac{d\mathrm{P}}{dx} + g'\frac{d\mathrm{P}}{dy} + h'\frac{d\mathrm{P}}{dz} + \rho\mathrm{P}.$$

The mechanical traction on an interface will be considered later (§ 39). In a magnetic medium the magnetic force (α, β, γ) differs from the magnetic flux (a, b, c) simply by not including the influence of the local Amperean currents; thus $\alpha = a - 4\pi\mathrm{A}$.

When there is no conductivity, the free charge must move along with the matter, so that

$$\frac{d\rho}{dt} + \frac{d\rho p}{dx} + \frac{d\rho q}{dy} + \frac{d\rho r}{dz} = 0;$$

therefore, from the circuitality of the total current, we must have, identically,

$$\frac{d\rho}{dt} = \frac{d}{dx}\left(\frac{\delta f'}{dt} + \frac{df}{dt}\right) + \frac{d}{dy}\left(\frac{\delta g'}{dt} + \frac{dg}{dt}\right) + \frac{d}{dz}\left(\frac{\delta h'}{dt} + \frac{dh}{dt}\right).$$

The latter is the same as the convergence of $(\delta/dt - d/dt)\,(f', g', h')$, which asserts (for the case of uniform motion that is contemplated) that mere convection of the polarized medium does not produce separation of free electricity. The relation between (f', g', h') and (P, Q, R) must be such as to strictly satisfy this equation. The quantity Ψ occurs in the equations of the field as an undetermined potential which is sufficient in order to conserve the condition of bodily circuitality $du/dx + dv/dy + dw/dz = 0$.

In order to express the conditions that must hold at an interface of transition, we notice that by definition F, G, H are continuous everywhere; but it is only when the media are non-magnetic that their rates of change along the normal (and therefore all their first differential coefficients) are also completely continuous. Across an interface the traction in the æther must be continuous, so that the tangential component of the æthereal force (P′, Q′, R′) must be continuous, which is satisfied by continuity of Ψ. The continuity of the total electric current secures itself without further condition by a compensating distribution of electric charge on the interface, that is by a discontinuity in $d\Psi/dn$. The tangential continuity of the elastic æther requires that the tangential component of the magnetic force (α, β, γ) must be continuous; the normal continuity of the magnetic flux is assured by the continuity of (F, G, H). It might be argued that if the electric force (P, Q, R) were not continuous tangentially, a perpetual motion could arise by moving an electron along one side of the interface and back again along the other side. But this reasoning requires that (p, q, r) shall be continuous across the interface, as otherwise the circuit returning on the other side could not be complete; and it also requires that there shall be no magnetization, as otherwise the mechanical force on the electrons in an element of volume, which is what we are really concerned with in the perpetual motion axiom, is different from the sum of the electric forces on the individual electrons, by involving (α, β, γ) instead of (a, b, c). We can thus assert continuity of the tangential electric force only in the cases in which it is already involved in that of the tangential æthereal force; and consistency is verified. The aggregate of all these interfacial electromotive conditions is thus continuity of the vector potential (F, G, H), and of Ψ, and of the tangential components of the magnetic force; they *formally* involve continuity of the tangential components of the æthereal force (P′, Q′, R′), and of the electric and magnetic fluxes. But further, in the equations from which AMPÈRE's circuital relation is derived above, it is only the normal space-variation of V′ that is discontinuous; hence continuity of the tangential magnetic force is involved in that of F, G, H, Ψ by virtue of the mode of expression of (F, G, H) in terms of the currents and the magnetism. Thus there are in all cases only four independent interfacial conditions to be satisfied.

The scheme is thus far absolute, in the sense that the relations between the variables are independent of the special molecular constitution of the matter that is present. The system of equations must now be completed for material media by joining to it the relations which connect the conduction current in the matter with the electric force, and the electric polarization of the matter with the electric force, and the magnetic polarization of the matter with the magnetic force, in the cases in which these relations are definite and can be experimentally ascertained. In the simplest case of isotropic matter, polarizable according to a linear law, they are of types

$$u' = \sigma P, \quad f' = (K - 1)/4\pi c^2\, P, \quad A = \kappa\alpha.$$

The expression for ρ leads in homogeneous isotropic media to

$$K\nabla^2\Psi = -\,4\pi c^2\rho + (K - 1)\,\{d/dx\,(cq - br) + d/dy\,(ar - cp) + d/dz\,(bp - aq)\}$$

so that Ψ is only in part an electrostatic potential. Inside a uniform isotropic conductor *at rest*, the condition of circuitality becomes $\sigma\nabla^2\Psi = d\rho/dt$; substituting this, we have $d\rho/dt + 4\pi C^2\sigma K^{-1}\rho = 0$, so that $\rho = \rho_0 \exp(-4\pi C^2\sigma K^{-1}t)$, showing that an initial volume density of free electricity would in that case be instantly driven to the boundary owing to the dielectric action. This proposition may be extended to æolotropic media.

14. The nature of the foregoing electric scheme may be elucidated by aid of some simple applications.

(i.) When a conducting system is in steady motion so that there is no *conduction* current flowing into it, the electric force (P, Q, R) must be null throughout its substance. Thus for the case of a solid conductor rotating round an axis of symmetry in a uniform magnetic field parallel to that axis, with steady angular velocity ω, the electric force in it, namely $(\omega cx - d\Psi_1/dx,\ \omega cy - d\Psi_1/dy,\ -d\Psi_1/dz)$, must be null, so that $\Psi_1 = \frac{1}{2}\omega c(x^2 + y^2) + A$; the polarization in it is therefore null, but there is in it an æthereal displacement $-(4\pi C^2)^{-1}(d/dx,\ d/dy,\ d/dz)\Psi_1$. In outside space, the electric force and æthereal force are each $-(d/dx,\ d/dy,\ d/dz)\Psi_2$, where Ψ_2 is that free electrostatic potential which is continuous with the surface value $\frac{1}{2}\omega c(x^2 + y^2) + A$ at the conductor. Inside the conductor this purely æthereal displacement involves an electrification of volume density $\rho = -\omega c/2\pi C^2$, which will be a density of free electrons or ions as all true electrifications are; while there is a compensating surface density σ equal to the difference of the total normal electric displacements on the two sides, that is to $(4\pi C^2)^{-1}(d\Psi_2/dn_2 + d\Psi_1/dn_1)$, where dn_2, dn_1 are both measured towards the surface, the outside medium being air for which K is unity. The value of the constant A is determined by the circumstance that the aggregate of this volume and surface charge shall be null when the conductor is insulated and unelectrified, or equal to the given total charge when it is insulated and charged: when it is uninsulated, the constant is determined by the position of the point on it that is connected to Earth, and therefore at zero potential. The procedure of Part II., § 25 is thus justified, because there is in fact no dielectric polarization in the conductor, but only æthereal displacement.

It remains to consider whether the parts of this volume density ρ and surface density σ of electrification are carried round with the conductor in its motion, or slip back through its volume and over its surface so as to maintain fixed positions in space. It is clear (as in Part II., § 27) that the same cause, namely, viscous diffusion of momentum among moving ions and molecules, which produces Ohmic resistance to a steady current, will lead to the electrons constituting electric densities being wholly carried on by the matter whenever a steady state is attained. This necessary consequence of the theory is in keeping with Rowland's classical experiments on convection currents. The excessively minute magnetic field due to these convection currents themselves has been neglected in the above analysis, which has enabled us to specify the slight redistribution of free charge on the rotating conductor when under the influence of a powerful extraneous magnetic field: when the magnetic field is due solely to its own motion the redistribution is of course absolutely negligible.

(ii.) In the case of a dielectric (as also in the above) the restriction to a steady state and permanent configuration may be dispensed with; for the magnetic field arising from induced displacement currents can always be neglected in comparison with the inducing field. Thus, (a, b, c) being the extraneous inducing field, the electric forces inside and outside a rotating mass are

$$(\omega cx - d\Psi_1/dx,\ \omega cy - d\Psi_1/dy,\ -\omega ax - \omega by - d\Psi_1/dz) \text{ and } -(d/dx,\ d/dy,\ d/dz)\,\Psi_2.$$

As there can be no free electrification,

$$\nabla^2\Psi_1 = (1 - \mathrm{K}^{-1})\,\omega\,\{2c + x\,(dc/dx - da/dz) + y\,(dc/dy - db/dz)\} \text{ and } \nabla^2\Psi_2 = 0\,;$$

while at the surface

$$\Psi_1 = \Psi_2, \text{ and } \mathrm{K}\,d\Psi_1/dn - (\mathrm{K} - 1)\,\omega\,\{cxl + cym - (ax + by)\,n\} = d\,\Psi_2/dn,$$

the outside medium being air. If the dielectric body is a sphere rotating in a uniform field $(0, 0, c)$ parallel to the axis, this gives by the usual harmonic analysis $\Psi_1 = \frac{1}{3}(1 - \mathrm{K}^{-1})\,\omega c r^2 + \mathrm{A}r\cos\theta + \mathrm{A}'$ and $\Psi_2 = \mathrm{B}r^{-2}\cos\theta + \mathrm{B}'r^{-1}$, where, r_1 being the radius, $\mathrm{A} = \mathrm{B}/r_1^{\,3} = -3\mathrm{K}/(2\mathrm{K}+1)\,r_1\,.\ \mathrm{A}' = -\frac{3}{2}/(\mathrm{K}+2)\,r_1^{\,2}\,.\ \mathrm{B}' = (\mathrm{K}-1)/(\mathrm{K}+2)\,.\,\omega c r_1$; thus determining the electric potential Ψ_2 in the space surrounding the rotating sphere.

15. More generally, let us consider steady distributions of electric charges on a system of conductors and dielectric bodies in motion through the æther. That there may be a steady state, without conduction currents, it is necessary that the configuration of the matter shall be permanent, and that its motion shall be the same at all times relative to this configuration and to the æther, and also to the extraneous magnetic field if there is one: this confines it to uniform spiral motion on a definite axis fixed in the æther. Referring to axes fixed in the material system, the vector potential has in the steady motion no time-variation: hence

$$(\mathrm{P}, \mathrm{Q}, \mathrm{R}) = -(d/dx, d/dy, d/dz)\mathrm{V},\ (\mathrm{P}', \mathrm{Q}', \mathrm{R}') = (\mathrm{P} - qc + rb, \mathrm{Q} - ra + pc, \mathrm{R} - pb + qa).$$

The magnetic induction through any circuit moving with the matter being constant, (P, Q, R) is derived (§12) from an electric potential function V. Inside a conductor the electric force must vanish, otherwise electric separation would be going on, therefore V must there be constant.

When the surrounding dielectric is free space, the total current in it, referred to these axes moving with the matter, is $-(pd/dx + qd/dy + rd/dz)\,(f, g, h)$. When the velocity (p, q, r) of the matter is uniform, it then follows from AMPÈRE's circuital relation that $(a, b, c) = 4\pi\,(qh - rg, rf - ph, pg - qf)$. Hence (f, g, h), given by $4\pi \mathrm{C}^2 f = \mathrm{P} - qc + rb$, is expressed in terms of (P, Q, R) by equations of type $(\mathrm{C}^2 - p^2 - q^2 - r^2)\,f = \mathrm{P}/4\pi - p/4\pi\mathrm{C}^2\,(p\mathrm{P} + q\mathrm{Q} + r\mathrm{R})$. The circuital quality of (f, g, h) thus gives the characteristic equation of the single independent variable

V of the problem, in the form $\nabla^2 V = c^{-2}(p\,d/dx + q\,d/dy + r\,d/dz)^2 V$, the boundary condition being that V is constant over each conductor.

Thus in the case of a system of conductors moving steadily through space with uniform velocity v in the direction of the axis of x, ϵ denoting $(1 - v^2/c^2)^{-1}$ we have $(f, g, h) = (4\pi c^2)^{-1}(P, \epsilon Q, \epsilon R)$, and therefore $(d^2/dx^2 + \epsilon\, d^2/dy^2 + \epsilon\, d^2dz^2)\,V = 0$. The distribution of electric force is therefore precisely the same as if the system were at rest, and the isotropic dielectric constant unity of the surrounding space changed into an æolotropic one $(1, \epsilon, \epsilon)$, *cf.* Part I. §115 ; and so would the surface density of true charge, which is the superficial discontinuity of total displacement, be the same, were it not that there is æthereal displacement *inside* the conductors which must be subtracted. The internal displacement current thence arising is $-(4\pi c^2)^{-1}\, v\,d/dx\,(0, -vc, vb)$; hence (a, b, c) is of the form $\{d/dx, (1 + v^2/c^2)^{-1}\, d/dy, (1 + v^2/c^2)^{-1}\, d/dz\}\,\phi$, by AMPÈRE's circuital relation : the circuitality of (a, b, c) then leads to a characteristic equation for ϕ, which must be solved so as to give at the surface of the conductor a value for the normal component of (a, b, c) continuous with the already known outside value, and the internal displacement is thereby determined. There is no bodily electrification inside the conductors, since this displacement is circuital.

We can restore the above characteristic equation of V, the potential of the electric force, to an isotropic form by a geometrical strain of the system and the surrounding space, represented by $(x', y', z') = (\epsilon^{\frac{1}{2}}x, y, z)$: the actual distribution of potential around the original system in motion corresponds then to that isotropic distribution of potential round the new system at rest which has the same values over the conductors. The æthereal displacements through related elements of area δS and $\delta S'$, of direction cosines (l, m, n) and (l', m', n') in the two spaces, multiplied by $4\pi c^2$, will be

$$-(l\,d/dx + \epsilon\, m\,d/dy + \epsilon\, n\,d/dz)\,V\delta S \quad \text{and} \quad -(l'd/dx' + m'd/dy' + n'd/dz')\,V'\delta S' ;$$

of these the second is always $\epsilon^{-\frac{1}{2}}$ times the first ; thus the elements of surface for which the total displacement is null correspond in the two systems, and therefore the lines and tubes of total displacement also correspond, the flux of displacement in these tubes being $\epsilon^{-\frac{1}{2}}$ times greater in the second system than in the first. But on account of the æthereal displacement in the interior, the outside tubes do not mark out the distribution of the charge on each conductor. If then a system of charged conductors has a velocity of uniform translation v through the æther : and an auxiliary system at rest is imagined consisting of the original system and its space each uniformly expanded in the ratio $\epsilon^{\frac{1}{2}}$ or $(1 - v^2/c^2)^{\frac{1}{2}}$ in the direction of the motion, and the charges on this new system are $\epsilon^{\frac{1}{2}}$ times those on the actual system : then the fields of æthereal displacement of the two systems agree in the surrounding spaces so as to be the same across corresponding areas, but the distributions of the charges on the conductors do not thus exactly correspond. [These results are obtained on the supposition that the structure of the matter is not affected by its motion. The conductors on which these charges are situated will, however, if the results of the more fundamental analysis of §16 are admitted, change their actual forms to a slight extent depending on $(v/\mathrm{C})^2$ when they are put in motion, and this change will react so that the distribution of charges and displacements will be the simple one there given.]

16. The circumstances of propagation of radiation in a material medium moving with uniform velocity v parallel to the axis of x will form another example. We may here (§13) employ the circuital relations, of types

$$4\pi u = \frac{d\gamma}{dy} - \frac{d\beta}{dz}, \qquad -\frac{\delta\alpha}{dt} = \frac{d\mathrm{R}}{dy} - \frac{d\mathrm{Q}}{dz}$$

where

$$u = \frac{df}{dt} + \frac{\delta f'}{dt}, \quad (f', g', h') = \frac{\mathrm{K}-1}{4\pi\mathrm{C}^2}(\mathrm{P}, \mathrm{Q}, \mathrm{R}), \quad (f, g, h) = \frac{1}{4\pi\mathrm{C}^2}(\mathrm{P}, \mathrm{Q} + vc, \mathrm{R} - vb).$$

There readily results, on eliminating the electric force (P, Q, R),

$$4\pi(u, v, w) = \operatorname{curl}(\alpha, \beta, \gamma), \quad \mathrm{D}^2/dt^2\,(a, b, c) = 4\pi\mathrm{C}^2 \operatorname{curl}(u, v, w),$$

where

$$\mathrm{D}^2/dt^2 = d^2/dt^2 + (\mathrm{K}-1)(d/dt + v\,d/dx)^2\,;$$

which agrees with the equation obtained in Part I. §124 and Part II. §13, leading to FRESNEL's law of alteration of the velocity of propagation.

Now let us consider the free æther for which K and μ are unity, containing a definite system of electrons which are grouped into the molecules of a material body moving across the æther with uniform velocity v parallel to the axis of x; and let us remove the restriction to steadiness of § 15. We refer the equations of free æther, in which these electrons are situated, to axes moving with the body: the alteration thus produced in the fundamental æthereal equations

$$4\pi d/dt\,.\,(f, g, h) = \operatorname{curl}(a, b, c), \quad -d/dt\,.\,(a, b, c) = 4\pi\mathrm{C}^2 \operatorname{curl}(f, g, h)$$

is change of d/dt into $d/dt - v\,d/dx$, leading to the forms

$$4\pi d/dt\,.\,(f, g, h) = \operatorname{curl}(a', b', c'), \quad -d/dt\,.\,(a, b, c) = 4\pi\mathrm{C}^2 \operatorname{curl}(f', g', h'),$$

where

$$(a', b', c') = (a, b + 4\pi vh, c - 4\pi vg), \quad (f', g', h'), = (f, g - v\,c/4\pi\mathrm{C}^2, h + v\,b/4\pi\mathrm{C}^2)\,;$$

from which eliminating the unaccented letters, *neglecting* $(v/\mathrm{C})^3$, and writing as before ϵ for $(1 - v^2/\mathrm{C}^2)^{-1}$, we derive the system

$$4\pi\frac{df'}{dt} = \frac{dc'}{dy} - \frac{db'}{dz} \qquad -(4\pi\mathrm{C}^2)^{-1}\frac{da'}{dt} = \frac{dh'}{dy} - \frac{dg'}{dz}$$

$$4\pi\epsilon\frac{dg'}{dt} = \frac{da'}{dz} - \left(\frac{d}{dx} + \frac{v}{\mathrm{C}^2}\frac{d}{dt}\right)c' \qquad -(4\pi\mathrm{C}^2)^{-1}\epsilon\frac{db'}{dt} = \frac{df'}{dz} - \left(\frac{d}{dx} + \frac{v}{\mathrm{C}^2}\frac{d}{dt}\right)h'$$

$$4\pi\epsilon\frac{dh'}{dt} = \left(\frac{d}{dx} + \frac{v}{\mathrm{C}^2}\frac{d}{dt}\right)b' - \frac{da'}{dy} \qquad -(4\pi\mathrm{C}^2)^{-1}\epsilon\frac{dc'}{dt} = \left(\frac{d}{dx} + \frac{v}{\mathrm{C}^2}\frac{d}{dt}\right)g' - \frac{df'}{dy}.$$

Now change the time variable from t to t', equal to $t - vx/c^2$, so that $d/dx + v/c^2 . d/dt$ becomes d/dx, and d/dt becomes d/dt', and these equations assume the form of an electric scheme for a crystalline medium at rest. Finally write x_1 for $x\epsilon^{\frac{1}{2}}$, a_1 for $a'\epsilon^{-\frac{1}{2}}$, f_1 for $f'\epsilon^{-\frac{1}{2}}$, dt_1 for $dt'\epsilon^{-\frac{1}{2}}$, keeping the other variables unchanged, and the system comes back to its original isotropic form for free æther. Thus the final variables (f_1, g_1, h_1) and (a_1, b_1, c_1) will represent the æthereal field for a correlative system of electrons forming the molecules of another material system at rest in the æther, of the form of the original one pulled out uniformly in the ratio $\epsilon^{\frac{1}{2}}$ along its direction of movement; the electric displacements through corresponding areas in the two systems are not equal, but their molecules are composed of equal electrons and are situated at corresponding points, and the individual electrons describe corresponding parts of their orbits in times shorter for the latter system in the ratio $\epsilon^{-\frac{1}{2}}$ or $(1 - \frac{1}{2}v^2/c^2)$, while those less advanced in the direction of v are also relatively very slightly further on in their orbits on account of the difference of time-reckoning. Thus we have here two correlative systems each governed by the circuital relations of the free æther: (i) a system in which the electric and magnetic displacements are (f, g, h) and (a, b, c), moving steadily with uniform velocity v parallel to the axis of x, (ii) the same system expanded in the direction of x in the ratio $\epsilon^{\frac{1}{2}}$ and at rest, the displacements at the corresponding points being $(\epsilon^{-\frac{1}{2}}f, g - vc/4\pi c^2, h + vb/4\pi c^2)$ and $(\epsilon^{-\frac{1}{2}}a, b - 4\pi vh, c + 4\pi vg)$, and the molecules being situated in the corresponding positions with due regard to the varying time-origin. Inasmuch as the circuital relations form a differential scheme of the first order which determines step by step the subsequent stages of a system when its initial state is given, it follows that if these two æthereal systems are set free at any instant in corresponding states, they will be in corresponding states at each subsequent instant, their electrons or singularities being at corresponding points. If then the latter collocation represent a fixed solid body, the former will represent the same body in uniform motion; one consequence of the motion thus being that the body is shrunk in the direction of its velocity v in the ratio $\epsilon^{-\frac{1}{2}}$ or $1 - \frac{1}{2}v^2/c^2$. It may be observed that there is here no question of verifying that the mechanical forces acting on the single electrons in the two cases are such as to maintain this correspondence; for in the present complete survey of the individual atoms there is no such entity as mechanical force, any more than there is on a free vortex ring in fluid; the notion of mechanical forces enters at a subsequent stage when we are treating of molecular aggregates considered as continuous bodies, and are examining the relations between the different groups into which our senses analyze their interactions (§ 48).

If this argument is valid, it will confirm the hypothesis of FitzGerald and Lorentz, to which they were led as the ultimate resource for the explanation of the negative result of Michelson's optical experiments; and conversely it will involve evidence that the constitution of a molecule is wholly electric, as here represented.

The reasoning given in Part II., § 13, was insufficient, because the correlation between the two systems was not there pushed to their individual molecules.

Consideration of a possible Limitation of the Rotational Scheme.

17. In the preceding sections the equations of the æthereal field have been expressed (as they were in Part II.) without reference to the dynamical hypothesis of a rotational æther which suggested their present form. Reverting now to that hypothesis, let us examine whether a limitation of the kind that was unavoidable in the material model of § 3, may not also be involved in the general scheme of a rotational æther. In the first place, it is natural to take the elastic rotation in the medium as very small, so that its translatory velocity which is connected therewith is also very small, though the velocities of the strain-centres which flit across it, and represent the matter, may have any values; this is in agreement with the conclusion derived from optical experiments that the æther is practically stagnant. But there is one conceivable class of cases in which the changes of position of the elements of the medium go on accumulating, that namely of a steady magnetic field kept up say by a current of electrons constrained to flow permanently round a circuit. On account of the smallness of the velocity of the æther, the corrections to the dynamical equations which arise from the velocity of convection of the elastic strain may always be left out of account, being utterly insignificant in ordinary electrodynamics, and actually beyond the limits of experiment in optics: yet in a magnetic field continuing steady *for an unlimited time* the elements of volume of the æther will ultimately have wandered far from their original positions, and a difficulty presents itself.* To cover such a case, the definition of the elastic rotation of the medium must be made more precise. For the motion of a perfect fluid, which is differentially irrotational at each instant, will yet result after a time in finite rotations of its elements of volume; for example it is known that if a rigid ellipsoidal shell be filled with perfect fluid, and be set rotating about a fixed axis,

* I am indebted to Lord RAYLEIGH for drawing my attention to this point, as one requiring further consideration.

[A steady magnetic field involves a cyclical motion of the æther; thus in a very great time even a very small velocity will produce large changes of position. It is true that any motion of electrons whatever will produce change of strain, and therefore movement in the æther, but that movement will be very slight, and will not be cumulative except in the one case of permanent cyclical motion which represents absolutely permanent magnets. If there were no other way out of the difficulty described in the text, it might be turned by simply asserting that absolutely permanent magnets do not exist.

The nature of the constraints which may be necessary to prevent the nucleus of an electron from ever becoming sub-divided is a different question, and wholly outside the scope of the present theory, which simply takes these nuclei to exist as it finds them without inquiring in detail into their structure.]

then after a certain interval of time the parts of the fluid will have returned to the original configuration with respect to the shell, so that the fluid will have been rotated bodily in space just like a solid, although its motion at each instant has been differentially irrotational. In fact when the change of position of the element of volume is finite we can no longer analyze it definitely into rotations and pure strains, in such wise that the order of their application shall be indifferent; thus we obtain no longer in that way an analytical specification of the æthereal elastic rotation, and a more precise formulation must be made. The rough material model of § 3 indicates the necessary modification: in that model a differential pure strain of the element of volume does not tend to rotate the sub-element which is elastically effective; thus the efficient elastic rotation is the vector sum of the series of differential rotations which the element of the æther has experienced in its previous history. This is therefore the more precise definition of the total rotation, proportional to (f, g, h), from which the electrostatic forcive is derived as in Part II., § 18: it makes the rotation equal to the curl of the linear displacement when these quantities are both small so that their squares and products can be neglected, but not after the long-continued cumulative effect of a permanent magnetic field has come in. In that case, however, the small irrotational velocity, say for the moment (p, q, r), which constitutes the magnetic field, will contribute to (f, g, h) only by shifting by convection the element of the medium along with its rotation, while the rotation so transferred will be continually re-adjusting itself by elastic action into the new equilibrium configuration: the relation between elastic rotation and magnetic force will then be of the type $df/dt = d\gamma/dy - d\beta/dz - (pd/dx + qd/dy + rd/dz)f$, where (p, q, r) is equal to (α, β, γ) multiplied by a very small scalar factor. Unless the velocity (p, q, r) is uniform, $d/dt\ (df/dx + dg/dy + dh/dz)$ will not be exactly null; so that the movement of the æther by the steady magnetic field will lead to a development of electric charge, extremely slow and gradual, throughout the volume concerned. On the other hand the combination of permanent electric and magnetic fields which is the origin of such a creation of electricity must be confined to a limited region, beyond which the æther is in equilibrium; therefore the electrification thus developed consists of compensating amounts of positive and negative signs. These diffuse charges, of the second order of small quantities, will subsequently by their mutual attractions drift together again and neutralize each other, by moving as strain-forms across the æther without sensibly interfering with the motion of the medium itself (§ 6). Thus a steady magnetic field of unlimited duration would not theoretically get interlocked with a concomitant electrostatic field, but would relieve itself by very slowly developing a very minute diffuse electrification which will simultaneously gradually fade away by its own natural actions, so that no sensible effect would ever be accumulated. The rotational æther scheme therefore would not break down in this limiting case, the consequence of long-continued cumulation being obviated by a process which is at each instant so insignificant as to be far below the reach of experience: electrons may, it is true, also conceivably obliterate each other in the same way as these diffuse electrifications, but that is a contingency of negligible probability with which we are familiar in all kinds of molecular theory.

Relations of Inductive Capacity and Optical Refraction to Density.

18. Let the medium be free æther containing n similar molecules per unit volume; and suppose each molecule to be polarized to moment μ by the field of electric force. This field is made up of the extraneous exciting field and that of the polarized molecules themselves; the latter again consists of a part arising from the polarized medium as a whole and a part involving only the immediate surroundings of the point considered. If h denote this local part, and H the remainder of the total electric field, we have relations of the types $\mu = k(\mathrm{H}+h)$, $i' = n\mu$, $h = \lambda . n\mu$, i' denoting the intensity of the polarization, k a constant independent of the density of the material medium, and λ a parameter which, as will appear, must be nearly independent of the density. These relations lead to $i' = kn(\mathrm{H} + \lambda i')$, that is, since by the definition of the inductive capacity K, $i' = (\mathrm{K} - 1)/4\pi . \mathrm{H}$ with electrostatic units, they lead to $3/4\pi . kn = (\mathrm{K} - 1)/(\mathrm{K} - 1 + 4\pi/\lambda)$; so that, ρ denoting density, $(\mathrm{K} - 1)/(\mathrm{K} - 1 + 4\pi/\lambda)\,\rho$ is constant for the same material medium. For fluid media at any rate, it will appear that λ must be very nearly equal to $\frac{4}{3}\pi$, so that for them LORENTZ's expression $(\mathrm{K} - 1)/(\mathrm{K} + 2)\,\rho$ should be approximately constant.

When the dielectric is a compound one consisting of n molecules of one kind and n' of another per unit volume, we have $i' = n\mu + n'\mu'$, $h = \lambda i'$; and $\mu = k(\mathrm{H} + h)$, $\mu' = k'(\mathrm{H} + h)$, so that $\mu/k = \mu'/k' = i'/(kn + k'n')$. Thus $i' = (kn + k'n')(\mathrm{H} + \lambda i')$, so that, with the above value $\frac{4}{3}\pi$ for λ, $3/4\pi . (kn + k'n') = (\mathrm{K} - 1)/(\mathrm{K} + 2)$. This formula gives an additive character to the refraction equivalent for a mixture; and also for a compound body, provided in the latter case the moment μ belongs to the individual atom, and is not sensibly affected by the molecular grouping of the atoms.

This investigation is of course not absolutely exact; but it is the first approximation in a statistical theory, and the question presents itself how far it is a sufficient approximation. On examination, it will appear that the coefficients k and k' are rightly taken to be numerical quantities independent of n and n', provided the distance between the effective poles of an atom or molecule is not a considerable fraction of the mean distance between adjacent molecules. The constancy of the value of λ, when the component densities are altered, appears from considerations of dimensions. For the force due to a polarized molecule varies as $\mu \times$ (distance)$^{-3}$: thus, as on change of density (distance)$^{-3}$ varies directly as density, the character of the arrangement of the molecules being supposed unaffected, the force due to the molecules surrounding the point is proportional to $\mu \times$ density, that is, it is equal to $\lambda n\mu$. For the case of a mixture λ is the same for both constituents; a result which may or may not hold good for a solution or a chemical compound.

19. The value of λ, namely $\frac{4}{3}\pi$, which has here been assumed, is not merely dictated by the form desired for the final result. That value has in fact already been specified as the first approximation in quite another connexion.* As this is the crucial point of the theory, it may be allowable to present the argument in detail. The total electric force acting on a single molecule is derived from the aggregate potential $V = \Sigma\,(\mu_x\, d/dx + \mu_y\, d/dy + \mu_z\, d/dz)\, r^{-1}$, where μ_x, μ_y, μ_z are the components of the moment μ of a polarized molecule. This potential, when the point considered is inside the polarized medium, involves the actual distribution of the surrounding molecules; and thus the force derived from it changes rapidly at any instant of time, in the interstices between the molecules. But when the point considered is outside the polarized medium, or inside a cavity formed in it whose dimensions are considerable compared with molecular distances, the summation in the expression for V may be replaced by continuous integration; so that, (f', g', h') being the intensity of polarization in the molecules of the dielectric,

$$V = \int (f'\, d/dx + g'\, d/dy + h'\, d/dz)\, r^{-1}\, d\tau\,;$$

and the force thence derived is perfectly regular and continuous. This expression may be integrated by parts, since, the origin being outside the region of the integral, no infinities of the function to be integrated occur in that region. Thus

$$V = \int (lf' + mg' + nh')\, r^{-1}\, dS - \int (df'/dx + dg'/dy + dh'/dz)\, r^{-1}\, d\tau\,;$$

that is, the potential at points in free æther is due to POISSON's ideal volume density ρ, equal to $-(df'/dx + dg'/dy + dh'/dz)$, and surface density σ, equal to $lf' + mg' + nh'$. When the point considered is in an interior cavity, this surface density is extended over the surface of the cavity as well as over the outer boundary. Now when it is borne in mind that, at any rate in a fluid, the polar molecules are in rapid movement, and not in fixed positions which would imply a sort of crystalline structure, it follows that the electric force on a molecule in the interior of the material medium, with which we are concerned, is an average force involving the average distribution of these polar molecules, and is therefore properly due to an ideal continuous density like POISSON's, even as regards elements of volume which are very close up to the point considered. To compute the average force which causes the polarization of a given molecule we have thus to consider that molecule as situated in the centre of a spherical cavity whose radius is of the order of molecular distances; and we have to take account of the effect of a POISSON distribution on the surface of this cavity, or more precisely of the result of an averaged continuous local polarization, surrounding the molecule, whose intensity increases from nothing at a certain distance from the centre up to the full amount i'

* "On the Theory of Electrodynamics," 'Proc. Roy. Soc.,' 52 (1892), p. 64.

at the limit of the molecular range, this intensity being uniform in direction and a function of the distance only. The force due to this is $\frac{4}{3}\pi i'$ along the direction of the polarization i'; which is therefore the local part to be added on to the electric force as ordinarily defined—namely, to that arising from the density ρ throughout the medium and the density σ on its external surface, and so everywhere derivable from a potential by the theory of gravitational mass-distributions. The value of the coefficient λ of the above analysis should thus be $\frac{4}{3}\pi$ for a fluid;* but it may deviate from this value somewhat in the case of a solid, especially of course if it be crystalline.

20. The mathematical principles, on which the above formula for the relation between inductive capacity and density is based, were first given by POISSON for the corresponding problem in magnetic polarization. The explicit application to electric polarization, on the lines of FARADAY's ideas, was made by Lord KELVIN and MOSSOTTI. The investigation of the formula which has been implicitly given by MAXWELL ("Treatise," § 313), expressed in terms of the cognate problem of conduction, is however valid only for the case in which the coefficient of polarization of the medium is small compared with unity, that is, only for gaseous media. The same formula, viewed as a relation between refractive index and density for transparent media, was obtained by LORENTZ† and was shown by him to be experimentally valid in an approximate way over the wide range of density including the liquid and gaseous states; though for the small changes of density induced in a liquid by alterations of pressure and temperature the effect of the change in the internal energy and mutual configuration of the molecules may considerably mask the direct effect of the slight change of density.‡ For gases, however, in which the molecules are more isolated and the changes of density greater, the refraction is found to be in accordance with the formula. The investigation of LORENTZ§ was probably the first effective attempt to introduce the molecular constitution of the medium into the electric theory of light, and so arrive at laws of refraction and dispersion. The *form* of the refraction constant was really settled by statical considerations akin to those here given; but the theory of electric propagation employed by him at that

* The fact that the values of the refractive index for liquids are slightly in excess of what LORENTZ's formula would give by computation from the values for their vapours, may be an indication that this averaged field of molecular action is slightly elongated instead of spherical.

† H. A. LORENTZ, "Ueber die Beziehung zwischen der Fortpflanzungsgeschwindigkeit des Lichtes und der Körperdichte." 'Wied. Ann.,' 9, 1879, p. 641.

‡ For these small changes, the LORENTZ refraction function $(m^2 - 1)/(m^2 + 2)$ is approximately proportional to that of GLADSTONE and DALE, their ratio $(m + 1)/(m^2 + 2)$ being nearly constant; but it does not appear why the latter function happens to be usually more nearly proportional to the density than the former. The results of RÖNTGEN and ZEHNDER, 'Wied Ann.,' 44, 1891, on the effects of pressure on various fluids, make the two formulæ in default in opposite directions by about equal amounts.

§ 'Verhandl. der Akad. Amsterdam,' 18; abstract in 'Wied Ann.,' 9, 1872, pp. 641–665.

time was the one developed in VON HELMHOLTZ's early memoirs on electrodynamics, and it would appear that discrepancies come in through treating the æther as polarized like a material dielectric; at any rate his final result (*loc. cit.* p. 654) seems to give the refractive index a value greater than unity for free æther, and one only infinitesimally different for a ponderable medium. A mathematical investigation has been given by Lord RAYLEIGH,* in which the range of density over which these statical computations are valid is tested by finding for certain cases the complete expressions in a statical theory, of which they form the first rough approximations. The result is rather unfavourable to LORENTZ's formula, so much so as perhaps to excite surprise at its close agreement with the facts when the range of density is so great as that between the liquid and gaseous states of the same substance. There is thus room for the statistical method under which the subject has here been approached,† in that it explains the wide range through which the formula proves to be valid as a first approximation; while it at the same time recognizes that when the change of density is itself small, but is accompanied by other kinds of physical change, the influence of the latter on the polar molecule may be sufficiently important to prevent its exact verification.‡ On the specific influence of temperature, *cf.* § 72 *infra.*

21. In thus basing a theory of refraction equivalents on the value of the inductive capacity, it has been tacitly assumed that the dispersion of the medium is small; hence the results apply certainly only in the cases in which there is approximate agreement between the inductive capacity and the square of the refractive index.§ When dispersion in *absolutely* non-conducting media is taken into account, as in the previous memoir, § 11, and *infra*, § 23, the formula however still holds, the constant κ, equal to f'/P of § 24, now involving the period of the light.

The fact that for gases, and a large class of denser bodies as well, the inductive capacity is approximately equal to the square of the refractive index, shows that in them the polarization of the molecules can completely follow the rapid alternations of electric force which belong to the light waves. Thus we can conclude that when the

* RAYLEIGH, "On the influence of obstacles arranged in rectangular order on the properties of a medium," 'Phil. Mag.,' 34, 1892 (2), p. 481.

† Since this was written, I have found that the analytical method here employed is essentially the same as that of CLAUSIUS ('Mechanische Wärmetheorie,' 2, 1879); the fundamental importance of the ideas involved, and the discussion here given of the value of λ, in the case of fluid media, may perhaps justify the retention of the above independent statement.

‡ A theory precisely similar to the above of course applies to determinations of molecular magnetization in solutions of iron or other salts; strictly it is not the coefficient of magnetization κ, but $\kappa/(1 + \frac{4}{3}\pi\kappa)$ that is proportional to the density of the magnetic molecules. The values of κ are however usually so small that this constant is practically equal to κ.

§ This accords with the conclusion drawn by LINDE from an experimental examination of the subject, 'Wied. Ann.,' 56, 1895, pp. 546–70 (see p. 566). [PHILIP, 'Zeitsch. Phys. Chem.,' 1897, finds that the CLAUSIUS formula is quite inapplicable to mixtures of substances with abnormally high values of K.]

polarization of a molecule is upset by an encounter with another molecule, it is instantly restored to its normal value, as soon as the violence of the encounter is over; so that, the relative times spent by the molecule in encounters being small in every case, they hardly affect the inductive capacity of the medium; or in other terms, the density by itself hardly affects the molecular refraction equivalent (except in so far as the restoration of the steady state may involve absorption, § 28 *infra*), and the constancy of the coefficient k is further justified.*

22. The molecular theory leads to the conclusion that the electric æolotropy of crystals in which the dielectric constant differs much from unity, may be in part due to the distribution of the molecules in space and in part to the orientation of the individual molecules; and that therefore the same applies to double refraction. The intrinsic polarity which is revealed by pyroelectricity and piezoelectricity also shows that orientation is a real cause. But magnetic æolotropy must practically be wholly due to orientation of the molecules, as the smallness of the susceptibility makes the effect of arrangement inappreciable. The double refraction induced in dielectrics in a strong electric field is possibly mainly due to molecular orientation, as also that arising from mechanical strain.

The difference of absorption in different directions in a crystal like tourmaline must be of an order of numerical magnitude not higher than the difference of the refractions: an easy computation shows that it is really of a considerably lower order. This crystalline absorption can only be due to molecular orientation: it is of course excessively smaller than the absorption in metals, which is comparable with the whole refractive index; it would not therefore sensibly affect the laws of reflexion.

General Theory of Optical Dispersion.

23. A formula for optical dispersion was obtained in § 11 of the second part of this memoir, on the simple hypothesis that the electric polarization of the molecules vibrated as a whole in unison with the electric field of the radiation. The kinetic molecule of § 11 *supra*, with its steady momenta, will however usually have various free periods, and as many absorption bands; to take account of them, and also for other reasons which will appear, it is desirable to have a more complete dynamical theory.

The problem of dispersion, in its general form, is thus that of the transmission

* The analysis of this section does not agree with a theoretical investigation of the inductive capacities of mixtures of non-conducting liquids which do not exhibit change of volume in mixing, given by SILBERSTEIN ('Wied. Ann.,' 1895); his result is that K, or what comes to the same under these conditions, $K-1$, is an additive physical constant, whereas the formula of CLAUSIUS and LORENTZ makes $(K-1)/(K+2)$ additive. The determinations made by SILBERSTEIN for mixtures of benzol and phenylethylacetate give results for the LORENTZ constant which are always in excess of the theoretical value, by amounts ranging up to 8 per cent.; the discrepancies for his own constant $K-1$ are rather smaller, and are in both directions.

of radiation across a medium permeated by molecules, each consisting of a system of electrons in steady orbital motion, and each capable of free oscillations about the steady state of motion with definite free periods analogous to those of the planetary inequalities of the Solar System; and its analysis will in fact resemble LAPLACE'S general investigation of the latter problem. If θ_1, θ_2, ... represent small deviations from the state of steady motion of a molecule, so that the coordinates of the system are $f_1(t) + \theta_1, f_2(t) + \theta_2, \ldots$, the kinetic and potential energy of the molecule when expanded in powers of these small quantities will assume the forms

$$\mathrm{T} = \text{const.} + [\theta_1, \theta_2, \ldots]_1 + [\dot\theta_1, \dot\theta_2, \ldots]_1 + [\theta_1, \theta_2, \ldots]_2 + [\dot\theta_1, \dot\theta_2, \ldots]_2 + [\{\theta_1, \theta_2, \ldots\}\{\dot\theta_1, \dot\theta_2, \ldots\}]$$

$$\mathrm{W} = \text{const.} + [\theta_1, \theta_2, \ldots]_1 + [\theta_1, \theta_2, \ldots]_2,$$

where the terms in T and W denote functions of the various degrees of these velocities and displacements, the last term in T being a lineo-linear function of them jointly. From these expressions the free vibrations are determined by the LAGRANGIAN method. As the undisturbed motion is steady, the type of a free vibration must be the same at whatever time it is excited, therefore the coefficients in T and W are all independent of the time; indeed if they were not constant the system could have no free periodic vibrations at all. The equations of the steady motion show that there can be no terms in T − W of the first degree in the displacements, when the coordinates are properly chosen.* At the present stage we may conveniently by transformation of coordinates express the LAGRANGIAN function, on which the motion in the molecule depends, in the form

$$\mathrm{T} - \mathrm{W} = [\dot\theta_1, \dot\theta_2, \ldots \dot\theta_n]_1 + \tfrac{1}{2}\{\mathrm{A}_1\dot\theta_1^2 + \mathrm{A}_2\dot\theta_2^2 + \ldots + \mathrm{A}_n\dot\theta_n^2\}$$
$$- \tfrac{1}{2}\{a_1\theta_1^2 + a_2\theta_2^2 + \ldots + a_n\theta_n^2\} + b_{11}\theta_1\dot\theta_1 + \ldots + b_{12}\theta_1\dot\theta_2 + b_{21}\theta_2\dot\theta_1 + \ldots,$$

from which, by a property which is an immediate corollary of the Action principle, we may subtract any perfect differential coefficient with respect to time, for example here

$$[\dot\theta_1, \dot\theta_2, \ldots \dot\theta_n]_1 + d/dt\,\{\tfrac{1}{2}b_{11}\theta_1^2 + \ldots + \tfrac{1}{2}(b_{12} + b_{21})\,\theta_1\theta_2 + \ldots\},$$

without affecting the course of the motion, leaving thus an *effective Lagrangian function*

$$\mathrm{L} = \tfrac{1}{2}\{\mathrm{A}_1\dot\theta_1^2 + \mathrm{A}_2\dot\theta_2^2 + \ldots + \mathrm{A}_n\dot\theta_n^2\} - \tfrac{1}{2}\{a_1\theta_1^2 + a_2\theta_2^2 + \ldots + a_n\theta_n^2\}$$
$$+ \tfrac{1}{2}\{e_{12}(\dot\theta_1\theta_2 - \dot\theta_2\theta_1) + \ldots\}.$$

24. We have now to determine the vibrations forced on this molecule by the uniform alternating field of electric force, say P parallel to the x axis, belonging to the radiation which is traversing the medium. Bearing in mind that the wave length covers about 10^3 molecules, it appears that if f' denote the total intensity

* This analysis, so far, is as given by ROUTH, "Advanced Rigid Dynamics," § 111.

per unit volume of polarization of the molecules, the electric force acting on a single molecule will, as in § 19 but now using electrodynamic units, be $P_1 = P + \lambda c^2 f'$; this force will maintain vibratory motion in the polar molecule, but will not cause any oscillation of its centre of mass. The interaction of the electric field with the internal coordinates of the molecule will thus introduce an extraneous potential energy function of the form

$$W' = F(t) - (c_1\theta_1 + c_2\theta_2 + \ldots . + c_n\theta_n) P_1,$$

higher powers of the small internal coordinates $\theta_1, \theta_2, \ldots . \theta_n$ being, as usual in problems of vibration, omitted; and here again the coefficients $c_1, c_2, \ldots . c_n$ must be independent of the time. There will also be terms in the kinetic energy involving the interaction of the magnetic intensity of the field with the component velocities of the molecular vibration: now in a train of waves of type exp. $q(t - K'^{\frac{1}{2}} c^{-1} z)$, the magnetic induction b, being derived from the electric force P, both in the plane xy of the wave-front, by the relation $-db/dt = dP/dz$, is equal to $K'^{\frac{1}{2}} c^{-1} P$: hence this part of the total kinetic energy will be of the form

$$T' = f(t) + (c'_1\dot{\theta}_1 + c'_2\dot{\theta}_2 + \ldots . c'_n\dot{\theta}_n) K'^{\frac{1}{2}} c^{-1} P,$$

where $c'_1, c'_2, \ldots . c'_n$ are coefficients independent of the time.

The form of W′ shows that $c_1\theta_1 + c_2\theta_2 + \ldots . + c_n\theta_n$ is equal to the electric polarization f_1 in the molecule on which the electric force P_1 acts. If unit volume of the medium contains n_1 molecules of one kind, n_2 of another and so on, and the polarizations in each molecule are respectively f_1, f_2 and so on, then

$$f' = n_1 f_1 + n_2 f_2 + \ldots .$$

25. To obtain the general equation of propagation in the æther, let $\mathfrak{F}$ denote the electric force, or the torque acting on the æther; and we have, as in Part II. § 11, the kinematic relation $(4\pi)^{-1}$ curl $\mathfrak{B} = d/dt(\mathfrak{D} + \mathfrak{D}') + \mathfrak{C}$, and also the dynamical equation $-d\mathfrak{B}/dt =$ curl $\mathfrak{F}$ where $4\pi c^2 \mathfrak{D} = \mathfrak{F}$. It is to be observed that this dynamical equation leaves out the purely local part of the electric force. The propagation of radiation of ordinary wave-length is in fact an action involving the medium in bulk, and not one of molecular type; thus in accordance with the YOUNG-POISSON principle (*infra* § 47) the local part of the electric force, arising from the surrounding molecules, is compensated intermolecularly by an influence on the physical properties of the material medium which thereby become functions of the density and strain, and this part therefore does not enter into the molar electric forcive maintaining the radiation. These equations lead to

$$c^2\nabla^2\mathfrak{D} = d^2/dt^2(\mathfrak{D} + \mathfrak{D}') + d\mathfrak{C}/dt.$$

Hence, when the current of conduction $\mathfrak{C}$ is non-existent, $K' = 1 + \mathfrak{D}'/\mathfrak{D}$; whilst here $\mathfrak{D}'$ is f', and $4\pi c^2\mathfrak{D}$, or P, is $P_1 - \lambda c^2 f'$; so that $(K' - 1)/(K' - 1 + 4\pi/\lambda) = \lambda c^2 . f'/P_1$, or taking λ equal to $\frac{4}{3}\pi$, we have $(K' - 1)/(K' + 2) = \frac{4}{3}\pi c^2 . f'/P_1$.

26. The value of f'/P_1 is to be obtained from the equations of forced vibration of

the molecules. By the LAGRANGIAN method, these equations expressed for a molecule of the first kind and for radiation of the above type e^{qt}, form a system, skew symmetric in so far as $e_{21} = -e_{12}$, of type

$$(A_1q^2 + a_1)\theta_1 + e_{12}q\theta_2 + \ldots + e_{1n}q\theta_n - c_1P_1 + c'_1K'^{\frac{1}{2}}C^{-1}qP = 0,$$

wherein

$$c_1\theta_1 + c_2\theta_2 + \ldots + c_n\theta_n - f_1 = 0.$$

They give the relation

$$f_1 = [\{A_1q^2 + a_1, e_{12}q, \ldots, e_{1n}q, -c_1\} P_1 + K'^{\frac{1}{2}}C^{-1}q \{A_1q^2 + a_1, e_{12}q, \ldots, e_{1n}q, c'_1\} P] \div \{A_1q^2 + a_1, e_{12}q, \ldots, e_{1n}q\},$$

in which the denominator represents a skew determinant, and each of the two coefficients in the numerator the same determinant bordered. The denominator involves when expanded only even powers of q, and when equated to zero it gives the periods of the free vibrations in the molecule; as these are all real the roots in q^2 must be all real and negative. The second term in the numerator has C^{-1} as a factor; we may therefore neglect it as has been done in the previous paper; this means that the elasticity of the æther is so high compared with its inertia that the pull exerted by it on the molecule will be important while the interaction of its kinetic energy will be negligible. The remaining determinant in the numerator, when expanded, contains only even powers of q and is of order lower by two than the denominator. Hence writing $-p^2$ for q^2, so that $2\pi/p$ is the period of the radiation, and expanding in partial fractions, we can express the equation in the form

$$4\pi C^2 \frac{f_1}{P_1} = \frac{g_1}{p_1^2 - p^2} + \frac{g_2}{p_2^2 - p^2} + \ldots + \frac{g_n}{p_n^2 - p^2},$$

in which $g_1, g_2, \ldots g_n$ are real quantities, positive or negative.

Now the index of refraction μ or $K'^{\frac{1}{2}}$ of the compound medium is given by the formula $(K' - 1)/(K' + 2) = n_1 \cdot \frac{4}{3}\pi C^2 f_1/P_1 + n_2 \cdot \frac{4}{3}\pi C^2 f_2/P_2 + \ldots$

The final result is thus

$$\frac{K' - 1}{K' + 2} = \Sigma nm, \text{ where } m = \frac{g_1}{p_1^2 - p^2} + \frac{g_2}{p_2^2 - p^2} + \ldots + \frac{g_n}{p_n^2 - p^2};$$

so that it is m and not μ^2 that has an infinity at each free period of the molecule. We here again arrive at LORENTZ's refraction-equivalent, and the theorem that it is an additive physical constant; but with the important addition that it is the law of dispersion of the molecular refraction-equivalent m, equal to $(\mu^2 - 1)/(\mu^2 + 2)\rho$, of each constituent of the medium, not that of the refractive index of the aggregate, which admits of simple theoretical expression. In physical investigations concerning laws of dispersion, it is thus essential to deal with simple substances; the dispersion in the molecular refraction-constant of a mixture, and no doubt also to some extent of a solution or chemical compound, is made up, according to this formula, of the aggregate of those of its constituents.*

27. Let us consider briefly the case of a perfectly transparent substance whose dispersion is dominated by a single free period, say $2\pi/p_1$: the equation is

$$\frac{\mu^2 - 1}{\mu^2 + 2} = \frac{ng_1}{p_1^2 - p^2}, \quad \text{that is,} \quad \mu^2 = 1 + 3\frac{ng_1}{p_1^2 - p^2} \bigg/ \left(1 - \frac{ng_1}{p_1^2 - p^2}\right).$$

It will be convenient to form a graph of the formula for μ^2; when p is small, μ^2 has a positive value, which should be the statical dielectric constant of the material; as p increases, μ^2 increases until it becomes infinite when $p^2 = p_1^2 - ng_1$; it then becomes negative, but again attains a positive value after $p^2 = p_1^2 + 2ng_1$ which corresponds to value zero. Thus there is a band of absorption, which is absolutely complete for some distances on both sides of the bright spectral line corresponding to the substance in the gaseous state, but which extends about twice as far on the upper side of that line as it does on the lower when ng_1 is positive, as will be the case when μ exceeds unity and the dispersion is in the normal direction. When, as in all ordinary media, the dispersion of the visible light is small, being for example of the order of one per cent. for glass, p_1 must be great compared with p, and the range of this single dominant ultra-violet band of absolutely complete absorption would be measured by an interval $(\delta p/p)$ equal to $\frac{1}{2}(\mu^2 - 1)/(\mu^2 + 2)$ below the free period, and one equal to $(\mu^2 - 1)/(\mu^2 + 2)$ above it, where μ is the index for luminous rays.

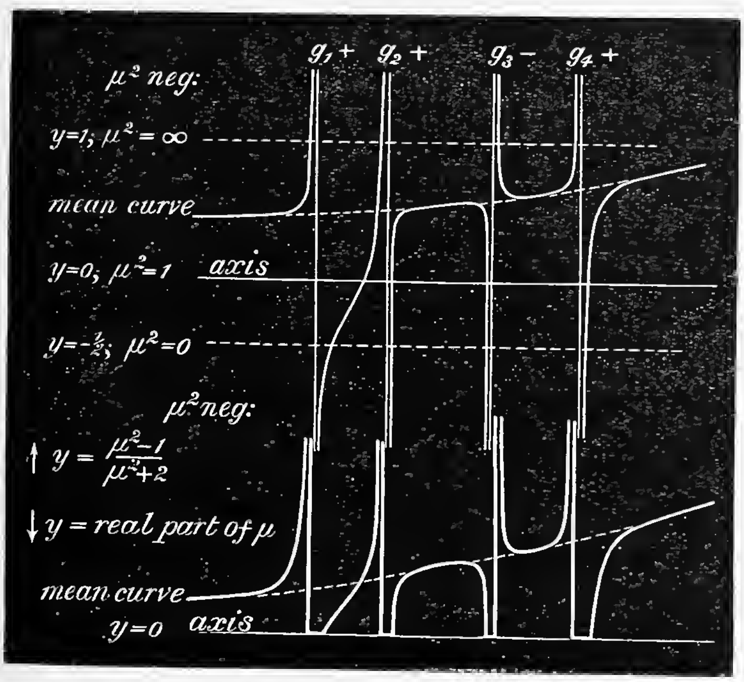
28. For a substance such as a gas, with numerous narrow bands of absorption, in the immediate neighbourhood of any one of them the value of μ^2 depends on that one alone; the breadth of the band of complete absorption thus corresponds to a total interval ($\delta p/p$ or $-\delta\lambda/\lambda$) equal to $3ng_1/2p_1^2$, which should thus be proportional to the density of the gas. The distance on each side of the band to which the anomalous dispersion extends, which may possibly be observed as has been done by KUNDT for sodium vapour, ought also to be of the order of magnitude of ng_1/p_1^2. The law of JANSSEN, that the amount of the absorption in a compressed gas is roughly proportional to the square of the density, seems to show that in dense media most of the actual specific absorption is outside these limits of complete blackness, and is conditioned by the molecular encounters deranging the states of steady directed synchronous vibration, say by rotation of the molecule, and so necessitating absorption of fresh energy from the radiation in order to re-establish them. It is to be observed that this process would be a true absorption of radiation which would go to heating the gas, as contrasted with mere refusal of a perfectly transparent gas to transmit radiation in a region in which μ^2 is negative.† The gradual change from an emission

* In cases however in which a formula of the CAUCHY type is sufficiently exact, so that $(\mu^2 - 1)/(\mu^2 + 2)\rho = A + B/\lambda^2 + C/\lambda^4 + \ldots$, not only is A an additive refraction-equivalent, but there will also be additive dispersion-equivalents B, C, ...

† The validity of the general formulæ is not vitiated by this circumstance that the molecules are in

spectrum of definite lines to a continuous spectrum, with increasing density, would thus be due, not to any want of definiteness of the free periods, but to changes in the orientation of the vibrating molecules arising from increased frequency of encounters, the corresponding rather abrupt changes in the radiation received at any point not being analyzable into the regular FOURIER periods.

29. The possible characteristics of the dispersion of an ideal perfectly transparent medium may be very simply represented by a graph of the general formula $(\mu^2 - 1)/(\mu^2 + 2) = \Sigma nm$. In a curve whose ordinate is $(\mu^2 - 1)/(\mu^2 + 2)$ and abscissa the frequency $p/2\pi$, all parts which lie outside the two horizontal dotted lines corresponding to ordinates $+ 1$ and $- \frac{1}{2}$ belong to regions of complete opacity; the points where the curve crosses the axis represent the free periods or bright lines. A mean continuous curve of dispersion may be sketched in, by a dotted line, which coincides with the actual curve in the parts where the dispersion is normal, and may be considered as gradually rising towards a band of intense absorption in the ultra-violet, which dominates the mean dispersion; near an absorption band the dispersion is anomalous, but if the band is narrow as in the case of gases, the anomaly is confined to very narrow range. The diagram here given represents a case of four free molecular periods, for the third of which g is negative while it is positive for the others. The refractive index that is determined by prismatic deviation is the real part of μ, taken positively (§ 34 *infra*). A graph of this quantity is represented by the thick broken line of the lower curve: thus near a free period $2\pi/p_1$ the ordinate rises to infinity when $p^2 = p_1^2 - ng_1$, then sinks instantly to zero, and remains zero until $p^2 = p_1^2 + 2ng_1$, when it becomes positive again. Slight general absorption would ease off the corners of this graph so



various orientations which change from time to time owing to encounters. The effect of this is that the coefficients which represent the interaction between the aggregate of the matter and the æther, in the element of volume, are now the steady aggregates of the coefficients $c_1, c_2, \ldots\ldots c_n$ which belong to the various simultaneous orientations of the molecules. Thus the analysis remains intact provided $c_1, c_2, \ldots\ldots c_n$ represent average values, and, where necessary, a coefficient of absorption is introduced to represent the abstraction of energy from the waves owing to the continual changes of molecular orientation. After each such change of orientation of a molecule, the energy of its previously accumulated synchronous vibration is radiated away or degraded into heat.

that it would not go up to infinity nor go down to zero: but there is nothing in its general aspect, at any rate for g positive, as for example given by VON HELMHOLTZ, and verified by PFLÜGER for anomalously dispersive solid dye stuffs, which specially favours any one theory of dispersion.

30. The medium has hitherto been taken as absolutely transparent, that is, no degradation of energy occurs in it, the absorption bands above so called being really produced by total and nearly total reflexion of the radiation, which thus is not absorbed by the medium, but simply cannot get into it. Suppose that there is present a conduction current, which may be considered to include all causes which put $\mathfrak{D}'$ out of phase with $\mathfrak{D}$ and so lead to regular absorption of the energy of the waves: it may be represented, as in Part II., § 11, by the formula

$$\mathfrak{C} = k(m'\, d/dt + \sigma')\,\mathfrak{D};$$

we now have

$$K' = 1 + \{\mathfrak{D}' + (d/dt)^{-1}\,\mathfrak{C}\}/\mathfrak{D},$$

thus simply adding to the formula for the square of the index of refraction the terms $-(km' + kp^{-1}\sigma'\iota)/(m'^2p^2 + \sigma'^2)$, which satisfactorily represent the general features of metallic propagation as was shown in Part II., § 11.*

It is noteworthy that as the period becomes very rapid the effective index of refraction always approximates to unity; so that very short waves will not suffer sensible reflexion, refraction, or diffraction even while their length may include many molecules of the material medium. In fact when the period of the radiation is sufficiently high, the free periods of the polar molecules are not quick enough to enable them to respond,† while the comparatively free ions are prevented by their

[* For example, it explains why the real part of μ^2 is negative for metals. In Part II. the generally received contention (based on a narrower theory) that μ^2 cannot be negative for purely elastic, that is dielectric, media was admitted without examination: but it is obviously inconsistent with the discussion above. As a concrete illustration, for a stretched thread weighted with equidistant particles, the square of the velocity of propagation of transverse waves of sufficiently short periods is negative: yet no inference follows as regards instability.]

† For a similar reason, the periods of luminous radiation are already too high to allow magnetic polarization to play any part in its propagation.

The statement in the text involves the currently received explanation of the Röntgen radiation. The different view has been recently advanced by Sir GEORGE STOKES that it may consist of sudden shocks transmitted through the æther from impacts by the molecules of the cathode streams. The molecules of matter lying in the track of the rays would not have time to be sensibly polarized by a sudden pulse which is over in a small fraction of their natural periods, and thus the pulse would pass across in the spaces between them, like sound through a grove of trees, without sensible refraction or diffraction: on the other hand the disruptive effect would resemble that of an explosive wave. Such pulses could hardly be other than the irregular beginnings of regular wave-trains sent out by the individual vibrating molecules; and as all radiation consists of such intermittent trains each with its irregular beginning, it would be assumed that the initial pulse is very much more intense in the electric bulb than in ordinary light, though still perhaps representing but a small portion of the total energy of the radiation. That the bombardment by the cathode streams is of a very disruptive, so to speak explosive, inertia from attaining any sensible velocity before the force is reversed; so that in neither way can the propagation of electric displacement across the medium be sensibly affected by the presence of the molecules. The formula shows, however, that the damping effect of conductivity usually persists to higher periods than the simple refracting effect of the excited vibrations.

The theory of dispersion would assume a simpler form if the molecules were systems vibrating about positions of equilibrium, instead of about states of steady motion. In that case the coefficients $e_{12}, e_{13}, \ldots . e_{1n}$ are null: the restoring forces, proportional to the velocities, to which these belong, are in fact introduced by the steady motions, and may be named, after Lord KELVIN, motional gyrostatic forces; they evidence themselves by causing slow precessional oscillations. The positional gyrostatic forces, or centrifugal forces proper, are merged in T − W along with the forces arising from the potential energy.* When these motional forces are absent, we have $\theta_1 = c_1 P_1/(a_1 - A_1 p^2)$ and similarly, so that $f_1/P_1 = \Sigma c_1^2/(a_1 - A_1 p^2)$; and as before, in a transparent medium, $(K' - 1)/(K' + 2) = \frac{4}{3}\pi c^2 P_1^{-1} \Sigma nf$. Thus the values of $g_1, g_2, \ldots . g_n$ are in this case all positive; so that, if this represented the facts, the fragments of a horizontal spectrum, with red on the left, would after further refraction by a prism of anomalous material with its edge horizontal and uppermost, all slope upwards from left to right. On the other hand, each change of sign from positive to negative among the successive values of $g_1, g_2, \ldots . g_n$ would give two fragments of the spectrum which would be curved back so as to be highest and lowest respectively near the middle, while a negative following a negative would imply slope upwards from right to left. According to KUNDT'S law the index is abnormally great on the lower side and abnormally small on the upper side of an absorption band; and if this generalization is universally valid, it will follow that $g_1, g_2, \ldots . g_n$ are actually all positive.

31. It is thus fundamentally desirable on various grounds to obtain information as to how the signs of these quantities depend on the gyrostatic coefficients; in particular because the present theory of gyrostatic molecules is a very wide one, and for example includes as a limiting case the hydrodynamical vortex atoms of Lord KELVIN, in which the constitution is purely gyrostatic but the number of degrees of freedom is infinite. It will be convenient for this purpose to change to new coordinates of which $c_1\theta_1 + c_2\theta_2 + \ldots . + c_n\theta_n$ is one, say the one with suffix unity: and to choose them semi-normal, so that the potential energy is represented by a

character, compared with ordinary molecular encounters, is in keeping with the rapid disintegration and evaporation of metallic plates under its influence. It is conceivable that the long-continued Becquerel radiation from fatigued phosphorescent substances arises in like manner from very sudden release of their molecules into new groupings, in the course of their gradual return to a natural or unfatigued configuration.

* The periods of small free vibrations, and the amplitudes of small forced ones, would not be affected by reversal of *all* the gyrostatic momenta in the system: in fact this reversal would just change the system into its optical image.

sum of squares, which would be all necessarily positive if there were no gyrostatic influence. It may then be shown in the manner of the preceding analysis that f_1/P_1 is equal to a fraction of which the denominator is the period determinant of the molecule and the numerator is the minor of its leading term ; the numerator is therefore the period determinant of the same molecule when its leading coordinate is prevented by constraint from varying. Thus we have the theorem

$$f_1/P_1 = A_1^{-1} (p^2 - \alpha'^2_2) (p^2 - \alpha'^2_3) \ldots . (p^2 - \alpha'^2_n)/(p^2 - \alpha_1^2) (p^2 - \alpha_2^2) \ldots . (p^2 - \alpha_n^2),$$

where $(\alpha_1, \alpha_2, \ldots . \alpha_n)/2\pi$ are the natural frequencies of the molecule, while $(\alpha'_2, \alpha'_3, \ldots . \alpha'_n)/2\pi$ are its frequencies when it is subjected to that particular constraint (namely on α_1) which would prevent it from vibrating under the influence of the incident radiation ; also A_1 is the coefficient of inertia of that particular vibration, so that its kinetic energy is $\frac{1}{2}A_1\dot{\theta}_1^2$.

This constraint may be represented analytically by making the elastic coefficient a_1 infinite; we may therefore attempt to trace, by the examination of graphs of the separate terms involved, the effects on the free periods of continuously varying this constant. The behaviour of a gyrostatic system may be very different from what experience teaches as to vibrations about configuration of rest, for the mere imposition of constraint to limit the vibrations of one coordinate may upset the stability of others: thus if x represent p^2, the present period equation is of type $\phi (x) + a_1\psi (x) = 0$, in which all the n roots of $\phi (x)$ are real and positive, while the same may not be true of the $n - 1$ roots of $\psi (x)$. It follows, easily, however, by application of the principle of energy,* that if the system be completely stable when all the gyrostatic motional forces are removed, then it will remain stable when these forces are restored ; and stability will therefore also be maintained when elastic connexions are strengthened or constraints are introduced.† In that case the roots of $\psi (x)$ will all be real and positive: and it is easy to deduce that they will separate those of $\phi (x)$, and that in consequence $g_1, g_2, \ldots . g_n$ will be all positive, so that KUNDT's law will hold good. The further conclusion is thus also somewhat probable that if the constitution of the gyrostatic molecule is thoroughly stable so that the imposition of mere constraint could not upset it, then KUNDT's law will hold.

32. The specific refraction $(\mu^2 - 1)/(\mu^2 + 2)$ always increases along with the index μ : if the dispersion were controlled solely by powerful absorption bands in the ultra-violet, with positive g, the trend of the index would always be in the same direction as the frequency increases. Hence in the large class of substances with normal dispersion of visible light for which K exceeds μ^2, there must also be strong

* *Cf.* THOMSON and TAIT's 'Nat. Phil.,' Ed. 2, Part I., p. 409 : the relations of the free periods of a gyrostatic system are there discussed at length in pp. 370–415.

† All the relations as to the march of the periods developed by RAYLEIGH ('Theory of Sound,' Ed. 2, §92*a*), from ROUTH's analysis will then hold good.

absorption in the ultra-red.* The specific refraction however always tends to the limit unity as μ^2 or K′ increases ; so that the large dielectric constants of water and alcohol (at ordinary temperatures) are not so abnormal in their optical as in their electrical aspect. These large values are an indication that the constituents of the molecules are distantly and loosely connected together, which may be related to the powerful action of these substances as solvents ;† it has been noticed that high inductive capacity is usually associated with conductivity.

33. It is of interest to contrast these results with the ones that flow from a purely mechanical theory of dispersion. If the molecule consist of a dynamical system, simple or gyrostatic, of dimensions small compared with the wave-length, joined on to the æther by mechanical connexions, the uniform oscillatory displacement of the æther will exert no differential statical force on the molecule, but the kinetic energy of the whole compound system will contain terms involving products of the

* In VON HELMHOLTZ's memoir on the electric theory of dispersion, he found satisfactory agreement between the formula with one ultra-violet absorption band and the observations for glycerine, and he suggested that agreement might also be established for carbon bisulphide by assuming slight dissipation such as would not sensibly modify the laws of reflexion: but he apparently omitted to notice that this verification is defective in not making the square of the index equal to the statical dielectric constant for the case of very long waves. To amend it, another region of absorption would have to be assumed in the ultra-red, far down so as not to sensibly affect the visible radiations, thus leading to the KETTELER type of formula, which is approximately $\mu = A + B\lambda^2 + C\lambda^{-2}$ for slightly dispersive substances: in the case of glycerine C would be small. When there is only one absorption band, the dispersion formula common to these discussions is in form the same as one derived by VON LOMMEL ('Wied. Ann.,' 13, p. 353) from a mechanical theory, and compared by him with observation for a considerable number of media. In all media whose dispersion is effectively controlled by one absorption band, that band must be far in the ultra-violet or else the dispersion of the visible radiation will be excessive, so that the formula must approximately coincide with CAUCHY's : thus it is only substances for which μ^2 is approximately equal to but slightly greater than K, which can have any chance of coming into that class.

It appears from the above that in this formula for dispersion in a medium dominated by one main band of absorption, as given in Part II., §11, we must make the distinction, that the value of $2\pi/p$ for which μ^2 is infinite is not the free period of a single molecule by itself, or that of the bright line in a gaseous spectrum, but is the period when it is vibrating in step with all the surrounding molecules under whose influence it lies.

For very slow periods there is no dispersion in a transparent medium and the refraction depends wholly on the statical character of the medium, including its density. For higher periods of the incident radiation, the free periods of the molecules introduce dispersion and also absorption bands ; but the position of these bands depends not merely on the free periods, but also slightly on the density of the medium, through the influence of the latter on its statical inductive capacity. It is not impossible that the free molecular periods, as well as the absorption bands, may be affected in this way. An influence of density of the medium on the position of the lines in the spectrum has been found and investigated by the Baltimore spectroscopists. [*Cf.* FITZGERALD, 'Astrophys. Journal,' 1896.]

† The reason here assigned is different from the one that has been given by various writers, that high inductive capacity of an intervening medium weakens the electric forces between the ions in the dissolved molecule. Here it is taken as an indication that the effective ions are far apart in the molecules of the solvent, so that a dissolved molecule can come under the influence of one of these ions alone, without much counteracting effect from the other ion.

displacement u of the æther and the coordinates $u, \theta_1, \theta_2, \ldots . \dot{\theta}_n$ of the small disturbance of the molecule, say terms $(c_0\dot{u} + c_1\dot{\theta}_1 + c_2\dot{\theta}_2 + \ldots . + c_n\dot{\theta}_n)\,\dot{u}$. In the equation of propagation, formed in the LAGRANGIAN manner, $\rho\, d^2u/dt^2$ will now be replaced by $d^2/dt^2\, (\rho u + c_0 u + c_1\theta_1 + c_2\theta_2 + \ldots . + c_n\theta_n)$; while in the equation of vibration of the molecule terms of type $c_1\, d^2u/dt^2$ will occur. The square of the index of refraction is thus given by $\mu^2 = 1 + (c_0 u + c_1\theta_1 + c_2\theta_2 + \ldots . + c_n\theta_n)/\rho u$; and this leads by analysis similar to the above to a dispersion formula $\mu_2 = A + \Sigma g_1/(p_1^2 - p^2)$.* It is to be noticed that, on a mechanical theory, the index does not finally tend to unity as the frequency $p/2\pi$ rises, for when the waves have ceased to excite internal vibrations in the molecules the æther is still loaded by their inertia; an exception occurs when the attachment of the molecule to the æther is such that, when owing to the high period it is not internally vibrating, the æther does not sensibly displace its centre of mass, in which case the constant A is unity and there is no effective load on the æther. If we suppose that each molecule has an attachment to a very large mass, so as to be practically anchored to it in space, this will require us to take one of the natural frequencies to be infinite in the above analysis, so that say p_1 is zero. When both these characteristics are present, we arrive at LORD KELVIN'S formula.† If on the other hand we take the medium to be like an elastic jelly, permeated by spherical portions of different inertia and elasticity, the problem is a quite different one, which forms in fact a rude mechanical analogue of the electric theory; and it was in this way that L. LORENZ independently arrived at the specific refraction formula above discussed.

* An equation equivalent to this with $g_1, g_2 \ldots .$ all positive, appears in SELLMEIER'S original paper ('Wied. Ann.,' 1872), based however on a much more special hypothesis.

† Baltimore Lectures, 1884: *cf.* also present memoir Part I., 'Phil. Trans.,' A, 1894, p. 820. In these lectures LORD KELVIN, with a view to explaining true absorption without introducing frictional forces into ultimate theory, contemplates the molecules as able to take up a vast amount of energy, near certain periods, before they attain to a steady state of synchronous vibration; as however that state must come after at any rate some millions of vibrations, and absorption would then cease, it is presumably part of the theory that the absorbed energy is constantly being degraded in the molecule by a process analogous to fluorescence, and so being got rid of by radiation at a lower period,—or it may be simply scattered owing to change of orientation of the steady state of vibration at which the molecule has arrived, due to encounters with other molecules, as indicated by JANSSEN'S law (*supra*) for gases. In the electric theory, metallic absorption is here taken to be chiefly due to the presence of free ions or electrons; but in weakly absorbing media it is probable that the former cause is the effective one. The only analytical way open for representing it is to introduce an absorption coefficient expressing the averaged rate at which the energy of the radiation is being exhausted.

The synchronously vibrating material molecules would not in any case give rise to further absorption by sending out energy in regular secondary waves: their uniformity in distribution and phase prevent this, just as they prevent the separate elements of the continuous æther from acting in the same way. The dust particles which give rise to the blue sky are irregularly distributed, and the individual secondary waves thence originating have irregular and independent phase-differences with reference to

Prismatic Deviation by Opaque Media.

34. The fact that light preserves its period shows (Part I., § 90) that the circumstances of its propagation across opaque media are determined simply by a complex index of refraction, of which the imaginary part represents the absorption. Measures of deviation by opaque prisms, such as those made by KUNDT, yield directly the value of this complex index, by simple consideration of the geometrical continuity of the traces of the waves along the interfaces, without the necessity of the intervention of any dynamical theory whatever and therefore free of all ambiguity of interpretation. The thickness of the portion of the prism that is traversed does not affect the deviation of the light; so it may be taken as null, and we have only to consider refraction into the prism, and then out of it at a plane inclined at an angle differing by α, the angle of the prism, without changing the point of incidence. Let the axis of x be in the first face of the prism, towards the edge, that of y normal to it and that of z parallel to the edge. Then for the incident, reflected, and refracted waves, of period $2\pi/p$, the vibration-vectors are proportional respectively to

$$\exp \iota(lx + my - pt),\ \mathrm{A} \exp \iota(lx - my - pt),\ \mathrm{A}' \exp \iota(lx + m'y - pt)$$

where

$$l^2 + m^2 = \mathrm{c}^{-2}p^2,\ l^2 + m'^2 = \mathrm{K}'\mathrm{c}^{-2}p^2,$$

c being the velocity in free space, and K′ the complex value of the square of the index of refraction. It will now be convenient to refer the second refraction to corresponding axes ξ, η, ζ related to the second face of the prism; thus

$$x = \xi \cos \alpha - \eta \sin \alpha,\ y = \xi \sin \alpha + \eta \cos \alpha,\ z = \zeta.$$

The vectors of the incident, reflected, and emergent beams are then proportional to

$$\mathrm{A}' \exp \iota(\lambda\xi + \mu\eta - pt),\ \mathrm{B} \exp \iota(\lambda\xi - \mu\eta - pt),\ \mathrm{B}' \exp \iota(\lambda\xi + \mu'\eta - pt),$$

where

$$\lambda = l \cos \alpha + m' \sin \alpha,\ \mu = -l \sin \alpha + m' \cos \alpha,\ \text{and } \lambda^2 + \mu'^2 = \mathrm{c}^{-2}p^2.$$

When the refracting angle α is small, this gives approximately $\mu'^2 = m^2 - 2lm'\alpha$,

the primary exciting wave. The analogous medium for sound, filled with fixed attuned resonators, is absorbent solely on account of the secondary radiation of the resonators: consequently if they were all alike and regularly distributed and they occupied a very large number of wave-lengths, there would be no absorption and the medium would be transparent to sound, unless it has the same period as the resonators when it could not penetrate into the medium at all. The correlative absorption of light would thus be a process special to it, arising from ionisation and molecular impact. Unless the absorption in iodine vapour is accomplished by ionisation so that it goes mainly into heat, there must be scattered light accompanying it and representing part of it.

or $\mu' = m - l/m \,.\, m'\alpha$, where $m' = \{(l^2 + m^2) \mathrm{K}' - l^2\}^{\frac{1}{2}} = j + \iota k$ say. Thus the emergent vibration-vector is represented by

$$\mathrm{B}' \exp \iota \{(l + am') \xi + (m - l/m \,.\, am') \eta - pt\},$$

that is

$$\mathrm{B}' \exp - ak (\xi - l/m \,.\, \eta) \exp \iota \{(l + aj) \xi + (m - l/m \,.\, aj) \eta - pt\}.$$

It therefore emerges at an angle of refraction ψ, away from the edge, given by

$$\cot \psi = (m - l/m \,.\, aj) / (l + aj) = m/l \,.\, \{1 - ajl (l^{-2} + m^{-2})\},$$

the angle of incidence being ϕ where $\cot \phi = m/l$. Thus $\psi = \phi + j/m \,.\, a$, and the deviation is $\psi - \phi - a$ that is $a (j' \sec \phi - 1)$, where j' is the real part of $(\mathrm{K}' - \sin^2 \phi)^{\frac{1}{2}}$. When the angle of incidence ϕ is small, the deviation is thus $(n - 1) a$, where n is the real part of the complex refractive index $\mathrm{K}'^{\frac{1}{2}}$. Thus the experiments of KUNDT on metallic prisms, and of PFLÜGER on anomalously refracting media,* determine the march of n. Although $\sin^2 \phi$ is not usually very considerable compared with K′, and thus oblique incidence on the prism does not very greatly affect the deviation,† yet it would seem desirable to have observations at oblique incidence, as they would give data for determining the imaginary part of the index also by this uniform method, and thus its complete value. If this were known for the neighbourhood of an absorption band, we should possess all the data requisite to guide and correct theory in the matter of optical dispersion; but a knowledge of n by itself is not of much service in this respect. The value of this method of prismatic deviation lies in the fact that the complex index is determined without the intervention of any considerations as to dynamical theory or the effect of surface contamination on polarization, which must enter into the interpretation of experiments on reflexion.

The Mechanical Tractions on Dielectric Interfaces: and the Mechanical Bodily Forcive.

35. When the local part of the forcive on the polarized molecules of the medium, arising from their interaction with the neighbouring polarised molecules, is left out of account, the remainder, which is the mechanical force on the element of volume, is derived from the energy function $-(f'\mathrm{P} + g'\mathrm{Q} + h'\mathrm{R})$; this would be also a potential function of the forces were it not that in it only the electric force (P, Q, R) is to be varied. When however the dielectric is homogeneous, the negation of perpetual motions requires that $f'd\mathrm{P} + g'd\mathrm{Q} + h'd\mathrm{R}$ shall be a complete differential; thus when the law of induced polarization is linear, the force will be derived from a potential function $-\frac{1}{2}(f'\mathrm{P} + g'\mathrm{Q} + h'\mathrm{R})$, and so will be balanced, as regards the interior of the medium and as regards the translatory part, by a hydrostatic pressure

* A. PFLÜGER, 'Wied. Ann.,' 56, 1895, p. 412.

† Cf. the measures of SHEA, 'Wied. Ann.,' 56.

$\frac{1}{2}(f'\mathrm{P} + g'\mathrm{Q} + h'\mathrm{R}) +$ const.; and when the medium extends continuously to a distance from the seat of the electric action, the constant in this expression must be null. When the medium is isotropic, the translatory force is all, there being no torque on the element of volume. In a fluid dielectric this compensating hydrostatic pressure actually exists, and has been measured; in a solid it is merely a compendious expression for the material reaction per unit volume against the electric forces transmitted by the æther from other matter at a distance. If however the fluid dielectric is heterogeneous there will not be a potential function, and it can only be in equilibrium when stratified in a certain manner; if gravity did not operate the surfaces of stratification would be the equipotentials of the field of force.

36. When there are in the electric field interfaces of transition between different dielectrics, there will also exist surface-tractions on them which may be evaluated by considering an actual, somewhat abrupt, interface to be the limit of a rapid continuous variation of the properties of the medium which takes place across a layer of finite though insensible thickness. Let then the total displacement (f'', g'', h''), with its circuital characteristic where there is no free charge, be made up of the dielectric material polarization (f', g', h'), and the displacement proper (f, g, h) which is the æthereal elastic rotation $(\mathrm{P}, \mathrm{Q}, \mathrm{R})/4\pi$. Thus if we neglect now the minute difference between the æthereal force $(\mathrm{P}', \mathrm{Q}', \mathrm{R}')$ and the electric force $(\mathrm{P}, \mathrm{Q}, \mathrm{R})$,

$$df'/dx + dg'/dy + dh'/dz = -\rho', \qquad df/dx + dg/dy + dh/dz = \rho + \rho',$$

where ρ' is the POISSON ideal volume-density corresponding to the polarization, and ρ is the volume-density of free electrons, surface distributions being now by hypothesis non-existent.* The mechanical forcive acting in the dielectric is, per unit volume, a force $(\mathrm{X}', \mathrm{Y}', \mathrm{Z}')$ and a torque $(\mathrm{L}', \mathrm{M}', \mathrm{N}')$, where

$$\mathrm{X}' = f'd\mathrm{P}/dx + g'd\mathrm{P}/dy + h'd\mathrm{P}/dz + \rho\mathrm{P}, \qquad \mathrm{L}' = g'\mathrm{R} - h'\mathrm{Q}.$$

The component parallel to x of the aggregate force acting on the whole transitional layer is the value of $\int \mathrm{X}'d\tau$ integrated throughout it. This integral is finite, although the volume of integration is small, on account of the large values of the differential coefficients which occur in the expression for X. To evaluate it, we endeavour

* The notation of Part II. is here maintained; thus (f'', g'', h'') represents the (f, g, h) of MAXWELL'S 'Treatise.' Electrostatic units are here employed. It may be well to recall the relations of these quantities. As the æthereal elemental rotation is from its nature circuital, the increment in its outward flux across any closed surface is equal to the amount of electrons that have crossed that surface into the enclosed region, arising partly from movement of free electrons, and partly from orientation of polar molecules over the surface so that one pole is inside and the other outside. Thus, (l, m, n) being the direction vector of the normal, and Δ representing a finite increment,

$$\Delta\int(lf + mg + nh)\,d\mathrm{S} = \Delta\int\rho\,d\tau - \Delta\int(lf' + mg' + nh')\,d\mathrm{S};$$

so that $\int(lf'' + mg'' + nh'')\,d\mathrm{S} = \int\rho\,d\tau$, which gives $df''/dx + dg''/dy + dh''/dz = \rho$.

by integration by parts to reduce the magnitude of the quantity that remains under the sign of volume integration, so that in the limit we may be able to neglect that part; thus we obtain $\int X'd\tau = \int (lf' + mg' + nh')\, P dS + \int (\rho' + \rho)\, P d\tau$. By the definition of electric force, (P, Q, R) is the force due to a volume distribution of density $\rho + \rho'$ and to extraneous causes; so that in the limit when the transitional layer is indefinitely thin, we have, by COULOMB's principle, $\int (\rho + \rho')\, P d\tau = \frac{1}{2} \int (\sigma' + \sigma)(P_2 + P_1)\, dS = (8\pi)^{-1} \int (N_2 - N_1)(P_2 + P_1)\, dS$, P_1, P_2 being the values of the x component P, and N_1, N_2 those of the normal component N, of the electric force (P, Q, R) on the two sides of the layer, all measured towards the side 2, while σ' and σ are the surface densities constituted in the limit by the aggregates of ρ' and ρ respectively taken throughout the layer. Hence in the limit $\int X'd\tau = \int (lf' + mg' + nh')\, P dS \Big|_1^2 + (8\pi)^{-1} \int (N_2 - N_1)(P_2 + P_1)\, dS$. Thus the electric traction on the interface of transition may be represented by a pull towards each side, along the direction of the resultant electric force F; this pull is on the side 2 of intensity $n'_2 F_2 - \frac{1}{2}(n'_2 - n'_1 - \sigma) F_2$, that is $\frac{1}{2}(\sigma + n'_2 + n'_1) F_2$ in the direction of F_2, where n' is the normal component of the polarization of the medium measured positive towards the side 2; on the face 1 the pull is $\frac{1}{2}(\sigma - n'_2 - n'_1) F_1$ now in the direction of F_1, n' being measured positive as before. As the tangential component of the electric force F is under all circumstances continuous across the interface, the total traction on both sides combined is along the normal, and equivalent to $\frac{1}{2}(n'_2 + n'_1)(N_2 - N_1)$ together with the tractions $\frac{1}{2}\sigma F_2$, $\frac{1}{2}\sigma F_1$ acting on the true charge σ, all the quantities being now measured positive in any the same direction. If n'' denote the normal component of the total displacement (f'', g'', h''), so that $n'' = N/4\pi + n'$, $n''_2 - n''_1 = \sigma$, the first part of this total traction is $\frac{1}{2}(n''_2 + n''_1 - N_2/4\pi - N_1/4\pi)(N_2 - N_1)$, which is simply $-2\pi n'^2_2 + 2\pi n'^2_1$ towards the side 2.* When the interface is between a dielectric 1 and a conductor 2, the traction is only towards the side 1 and is equal to $\frac{1}{2}(n'_1 + \sigma) F_1$, or $\frac{1}{2} n''_1 F_1$, per unit area, along the normal which is now the direction of the resultant force.

All this is quite independent of the law of the connexion between the polarization and the electric force in the material medium. Thus, under the most general circumstances as regards electric field, whether there is material equilibrium or not, the forcive on the material due to its electric excitation consists of the interfacial tractions thus specified, together with a force (X′, Y′, Z′) and a torque (L′, M′, N′) per unit volume, given by the formulæ $(X', Y', Z') = (d/dx, d/dy, d/dz)(f'P + g'Q + h'R)$ and $(L', M', N') = (g'R - h'Q,\ h'P - f'R,\ f'Q - g'P)$, in the former of which (f', g', h') is not to be differentiated.

The assumption underlying this analysis, that the transitions are gradual, will be

* It may be recalled that in the terminology here employed, the *true* electrification σ is the density of unpaired electrons; while the *true* electric current arises from the movements of all the electrons, free and paired, but does not include the change of æthereal strain which must be added in order to make up the *total* circuital current of MAXWELL.

sufficiently satisfied even if the intermediate layer is only one or two molecules in thickness; for as these molecules are arranged slightly in and out, and not in exact rows along the interface, their polarity can still be averaged into a continuous density as above. The aggregate tractions over a thin layer of transition thus do not depend sensibly on the nature of the transition, but only on the circumstances on the two sides of the layer.

37. In the case of a fluid medium, the bodily part of the forcive produces and is compensated by a fluid pressure $\int i'dF$, where i', being the polarization induced by the electric force F, is for a fluid in the same direction as F and a function of its magnitude. This pressure will be transmitted statically in the fluid to the interfaces;* combining it there with the surface traction proper, it appears that the material equilibrium of fluid media is secured as regards forces of electric origin if extraneous force is provided to compensate a total normal traction towards each side of each interface, of intensity $-2\pi n'^2 - \int i'dF$. In the case usually treated, in which a linear law of induction is assumed so that the relation between i' and F is $i' = (K-1)\,F/4\pi$, the mechanical result of the electric excitation of the fluid medium is easily shown to be the same† as if each interface were pulled towards each side by a FARADAY-MAXWELL stress, made up of a pull $KF^2/8\pi$ along the lines of force and an equal pressure in all directions at right angles to them. But this imposed geometrical self-equilibrating stress-system would not be an adequate representation of the mechanical forcive in a solid medium; for then the bodily forcive, instead of being wholly transmitted, is in part balanced on the spot by reactions depending on the solidity of the material. The forcive acting on isotropic material may however in every case, whether the induction follows a linear law or not, be expressed as an extraneous or imposed system, made up of bodily hydrostatic pressure $\int i'dF$ (which in the case of a fluid only relieves the ordinary fluid pressure and so diminishes the compression, § 79 *infra*) together with normal tractions on the interfaces between dielectric media, of intensity $-2\pi n'^2 - \int i'dF$ acting towards each side, and tractions $\frac{1}{2}n''F - \int i'dF$ on the surfaces of conductors acting towards the dielectric.

38. A similar analysis applies to the electromagnetic forcive acting on a magnetically polarized medium. Excluding as before the part arising wholly from the interaction of neighbouring molecules, which (§ 44 *infra*) is not transmitted by material stress, but is compensated on the spot by molecular action due to change of physical state induced by it, the electromagnetic forcive proper is made up of a bodily force (X, Y, Z) and torque (L, M, N), where, (u', v', w') representing the true current,

$$X = v'\gamma - w'\beta + A\,d\alpha/dx + B\,d\alpha/dy + C\,d\alpha/dz$$
$$= vc - wb + A\,d\alpha/dx + B\,d\beta/dx + C\,d\gamma/dx - \gamma dg/dt + \beta dh/dt,$$

* That is, a reacting pressure $\int i'dF$ exerted by the interface will keep the medium in internal equilibrium: no constant term is added because the pressure must vanish along with the polarization.

† It is a normal traction equal to $-(K-1)(KN^2 + T^2)/8\pi$ towards each medium, or in all a single traction $(K_2 - K_1)(2\pi n''^2/K_1K_2 - T^2/8\pi)$ towards the medium 1.

and $L = B\gamma - C\beta$. Under the usual circumstances, in which the æthereal displacement-current can be neglected, these expressions are identical with the ones given without valid demonstration in MAXWELL's 'Treatise.'* The remarkable property is there established (*loc. cit.*, § 643) that, independently of the form of the relation between magnetic induction and magnetic force in the medium and whether there is permanent magnetism or not, this bodily forcive (with the last terms neglected) can be formally represented in explicit terms as equivalent to an imposed stress: viz. $\mathfrak{H}$ denoting magnetic force and $\mathfrak{B}$ magnetic induction, the bodily forcive is the same as would arise from (i) a hydrostatic pressure $\mathfrak{H}^2/8\pi$, (ii) a tension along the bisector of the angle ϵ between $\mathfrak{H}$ and $\mathfrak{B}$, equal to $\mathfrak{H}\mathfrak{B}\cos^2\epsilon/4\pi$, (iii) a pressure along the bisector of the supplementary angle, equal to $\mathfrak{H}\mathfrak{B}\sin^2\epsilon/4\pi$, together with an outstanding bodily torque turning from $\mathfrak{B}$ towards $\mathfrak{H}$ and equal to $\mathfrak{H}\mathfrak{B}\sin 2\epsilon/4\pi$. When $\mathfrak{B}$ and $\mathfrak{H}$ are in the same direction, the torque vanishes, and a pure stress remains in the form of a tension $(\mathfrak{H}\mathfrak{B} - \frac{1}{2}\mathfrak{H}^2)/4\pi$ along the lines of force and a pressure $\mathfrak{H}^2/4\pi$ in all directions at right angles to them. There is no warrant for taking this stress to be other than a mere geometrical representation of the bodily forcive. It is however a convenient one for some purposes.† Thus the traction acting on the layer of transition between two media, in which (α, β, γ) changes very rapidly, which might be directly deduced in the same manner as the electric traction above (§ 35), may also be expressed directly as the resultant of these MAXWELLIAN tractions

* Vol. II., § 640. It will be observed that the force acting on the moving electrons which constitute the true current is here taken to be $(v'\gamma - w'\beta, \ldots, \ldots)$. In the investigation of Part II., § 15, which determines the motional force on a single electron, the expression for T represents the kinetic energy of the æther; it is transformed so as to be expressed in terms of the electric displacement of the æther and the electrons of the materials by introducing (F, G, H) whose curl gives the actual velocity of the æther near the electron; and finally, after the forces acting on the electrons and on the æthereal displacement have thus been separated out, (F, G, H) is eliminated by the same relation. Thus the force acting on the single moving electron comes out as $e(y\dot{\zeta} - \dot{z}\eta - \dot{F}, \ldots, \ldots)$, where $(\dot{\xi}, \dot{\eta}, \dot{\zeta})$ is the velocity of the medium; and the average force acting on the electrons in the element of volume, that is, the induced electric force causing electric separation in the element, is $e(\dot{y}c - \dot{z}b - \dot{F}, \ldots, \ldots)$, as there given. But in computing, as in Part II., § 23, the electromagnetic force on an element of volume carrying a current, it must be borne in mind that part of the above force on the single electron arises from the magnetism in this element of volume itself; and the principle of energy forbids that any part of the forcive on the mechanical element of volume of the material can arise from mutual actions inside the element, so that this part must be compensated by a reciprocal action of the moving electrons which constitute the current on those which constitute the magnetism, in a manner which might be expressed if necessary. Hence, when this local part is omitted in accordance with the general principle, the transmitted electromagnetic force is $(v'\gamma - w'\beta, \ldots, \ldots)$ as above, not $(v'c - w'b, \ldots, \ldots)$ as previously stated in closer accordance with the AMPÈRE-MAXWELL formula. *Cf.* § 44 *infra*. [Observe, however, that in quoting Part II., § 15 $(\dot{\xi}, \dot{\eta}, \dot{\zeta})$ must now represent the velocity of the æther multiplied by the square root of 4π times its very high coefficient of inertia: the unit of time was there tacitly chosen so that this factor should be unity.]

[† For example, the repulsion exerted by alternating currents on pieces of copper or other conducting masses may thus most conveniently be represented.]

towards the two sides of the interface. As there cannot be free magnetic surface-density or purely superficial current-sheets, the traction on the interface is represented, under the most general circumstances, whatever extraneous magnetic field may there exist, by purely normal pull of intensity $2\pi\nu^2$ towards each side, where ν is the normal component of the magnetization at that side. When the medium is non-magnetic, there is no such superficial traction, but only the bodily electromagnetic forcive on the *true* electric currents of the material medium, which is represented by the above stress system.

39. The form of the mechanical forcive is identical whether the polarization is electric or magnetic, provided there are no electric currents; in the first case it is the material reaction to the static strain in the æther, in the other case it is the reaction to the motional æthereal forcive arising from the revolving electrons in the molecule. Omitting for simplicity the slight effect of the convection current in cases where any exists, the forcive arising from the electric polarization of the medium consists of a bodily force (X′, Y′, Z′) and torque (L′, M′, N′), where

$$X' = f'\frac{dP}{dx} + g'\frac{dQ}{dx} + h'\frac{dR}{dx} + \rho P + g'\frac{dc}{dt} - h'\frac{db}{dt}, \quad L' = g'R - h'Q,$$

together with an interfacial traction between media 1 and 2 which is along the normal and equal to $-2\pi n'^2_2 + 2\pi n'^2_1$ towards the medium 2, n' representing the component of the polarity (f', g', h') along the normal; the motional forcive arising from the magnetic polarity and the electric currents, consists of a bodily force (X, Y, Z) and torque (L, M, N) where

$$X = A\frac{d\alpha}{dx} + B\frac{d\beta}{dx} + C\frac{d\gamma}{dx} + vc - wb - \gamma\frac{dg}{dt} + \beta\frac{dh}{dt}, \quad L = Bc - Cb,$$

together with a normal interfacial traction $-2\pi\nu_2^2 + 2\pi\nu_1^2$ towards the medium 2, ν representing the normal component of the magnetization (A, B, C). When the æthereal displacement current is neglected, the latter forcive is the same as would arise from MAXWELL'S magnetic stress specification. It may be shown that $X' = d/dx\,\{f''P - \frac{1}{2}(fP + gQ + hR)\} + d/dy\,(g''P) + d/dz\,(h''P) + hdb/dt - gdc/dt$, so that the former forcive is what would arise from an analogous electric stress specification in which (P, Q, R), (f', g', h'), $4\pi\,(f'', g'', h'')$, correspond to (α, β, γ), (A, B, C), (a, b, c) respectively, with the exception however in this case also of an outstanding bodily forcive $(hdb/dt - gdc/dt, \ldots., \ldots.)$ which is not included in the stress. A theory which assumes that there is but one medium in which everything is transmitted by contact action, not two interacting media matter and æther as here, is compelled to get rid of any outstanding forcive like this, which is not expressible explicitly in terms of stress: for this reason supporters of that view have found it necessary to introduce into the electric field a purely hypothetical mechanical forcive arising from the electric field acting on the so-called magnetic current $d/dt\ (a, b, c)$, in analogy with the AMPEREAN forcive arising from the magnetic field acting on the electric current. The addition of this forcive $(hdb/dt - gdc/dt, \ldots, \ldots)$ to (X', Y', Z') and the omission of $(-\gamma dg/dt + \beta dh/dt, \ldots, \ldots)$ from (X, Y, Z), permits both to be expressed *explicitly* in terms of stress.

MAXWELL'S *Theorem of a Representative Stress.*

40. The mechanical forcive acting in a polarized medium thus corresponds in the main to the system of bodily force and interfacial traction which is the result of MAXWELL'S magnetic stress ('Treatise,' § 640) considered as an extraneous system applied to the medium. The electric stress of MAXWELL ('Treatise,' § 105) is something wholly different, leading in the case of homogeneous media to interfacial tractions only, without bodily force; it could thus have valid application only to unpolarized media, as for example to the theory of gravitation which passes through material bodies just as through a vacuum. The proposition really established* is that the mechanical forcive due to attraction at a distance, obeying the law of inverse squares, between material bodies, may be represented by a connexion in the form of an imposed extraneous stress symmetrical with respect to the lines of force, acting across the intervening medium, *provided* that medium is not in any way polarized by the force. A stress restricted by this relation of symmetry involves only two variables, the principal tractions along and at right angles to the line of force; and the essence of MAXWELL'S theorem is that it is possible always to determine these two variables so as to satisfy the three equations of equilibrium of the element of volume of the medium. These principal tractions prove, as is well known, to be equal in magnitude but opposite in sign. The proposition is in itself so remarkable that it deserves to be formulated abstractly without reference to hypothetical applications. The representation of a given bodily forcive by a geometrical stress-system is in general a widely indeterminate problem, as the six stress components have to satisfy only three equations: but the condition of symmetry with respect to lines of force restricts the stress so much that such a representation would only in special circumstances be possible.

The regular local Molecular Forcive in an excited Dielectric: its Expression as a Stress-system: Examples of the Principle of the Mutual Compensation of local Molecular Forcives.

41. In the above estimate of the mechanical forces acting on an element of a polarized medium, the influence of the general mass of the medium on the molecules in the element has been alone included; it remains to consider the *rôle* of such terms as would arise from the special forcives of neighbouring molecules. The intensity of

* MAXWELL, "On physical lines of force," Part I., 'Phil. Mag.' 21, 1861, especially Prop. III.

the local part of the regular electric force acting at a molecule has already been assigned (§ 19) as $\frac{4}{3}\pi i'$, very approximately for the case of fluid media, possibly not so approximately for solids. The argument was that owing to the translational mobility of the surrounding molecules, their action on the one under consideration averages into that of the uncompensated distribution of poles which would exist on the surface of a small spherical cavity in a continuous uniformly polarized medium,—or, more precisely, into that of a spherical shell of poles, of thickness not indefinitely small but with this law of distribution around the centre. For the interior of a uniformly polarized medium the local part of the electric force is thus at each instant constant throughout this cavity and equal to $\frac{4}{3}\pi i'$; therefore the mechanical force exerted on the polar molecule (that is one involving equal numbers of positive and negative electrons) at the centre of the cavity is null, as it depends on the rate of variation of this electric force. But at a place where the polarization varies from point to point, the alteration in the law of surface-density over the cavity will supply a local part.

When the polarization i' changes only in magnitude and not in direction, this part will arise from a distribution of uncompensated poles over the surface of the cavity, of density $-(i'_0 + x\, di'_0/dx + y\, di'_0/dy + z\, di'_0/dz)\cos\theta$, where the subscript zero implies the value at the centre. If the axis of x is taken along the direction of i'_0, the electric potential U in the interior due to this distribution is equal to

$$-\tfrac{4}{3}\pi\left(i'_0 + \tfrac{1}{3}\frac{di'_0}{dx}\right)x - \tfrac{4}{5}\pi\left\{\tfrac{1}{3}\frac{di'_0}{dx}(2x^2 - y^2 - z^2) + \frac{di'_0}{dy}xy + \frac{di'_0}{dz}xz\right\}.$$

On a molecule of moment μ_x, at the centre, this gives a force

$$-\mu_x d/dx\,(d/dx,\ d/dy,\ d/dz)\,\mathrm{U},\ \text{that is } \tfrac{4}{5}\pi\mu_x\,(\tfrac{4}{3}d/dx,\ d/dy,\ d/dz)\,i'_0.$$

Thus there is a bodily force due to this cause, of intensity $\frac{2}{5}\pi\,(\frac{4}{3}d/dx,\ d/dy,\ d/dz)\,i'^2_0$; but there is not any bodily torque.

42. Now let us proceed to the general case, in which the direction of the polarization (f', g', h'), as well as its magnitude, varies from point to point; in the hypothetical case in which the effective distance between the poles of a molecule is small compared with the average distance between neighbouring molecules, we can express the molecular part of the forcive on an element of volume by simple summation for f', g', and h' separately, by aid of the expressions just found. Thus it consists of a bodily force $(\mathrm{X}_1, \mathrm{Y}_1, \mathrm{Z}_1)$ and torque $(\mathrm{L}_1, \mathrm{M}_1, \mathrm{N}_1)$, where $\mathrm{X}_1 = f' d\mathrm{P}_1/dx + g' d\mathrm{P}_1/dy + h' d\mathrm{P}_1/dx$, $\mathrm{L}_1 = g'\mathrm{R}_1 - h'\mathrm{Q}_1$, $(\mathrm{P}_1, \mathrm{Q}_1, \mathrm{R}_1)$ being the *local* part of the electric force in the spherical cavity, so that

$$\mathrm{P}_1 = \tfrac{4}{5}\pi\left\{\left(\tfrac{4}{3}x\frac{df'}{dx} + y\frac{df'}{dy} + z\frac{df'}{dz}\right) + \left(y\frac{dg'}{dx} - \tfrac{2}{3}x\frac{dg'}{dy}\right) + \left(z\frac{dh'}{dx} - \tfrac{2}{3}x\frac{dh'}{dz}\right)\right\}.$$

Hence

$$X_1 = \tfrac{4}{5}\pi\left\{\left(f'\frac{d}{dx} + g'\frac{d}{dy} + h'\frac{d}{dz}\right)f' + \tfrac{1}{2}\frac{d}{dx}(f'^2 + g'^2 + h'^2) - \tfrac{2}{3}f'\left(\frac{df'}{dx} + \frac{dg'}{dy} + \frac{dh'}{dz}\right)\right\}$$
$$= \tfrac{4}{5}\pi\left\{\frac{d}{dx}(f'^2 + g'^2 + h'^2) - \tfrac{2}{3}f'\left(\frac{df'}{dx} + \frac{dg'}{dy} + \frac{dh'}{dz}\right) - g'\left(\frac{dg'}{dx} - \frac{df'}{dy}\right) + h'\left(\frac{df'}{dz} - \frac{dh'}{dx}\right)\right\},$$

with similar expressions for Y_1 and Z_1; while the torque vanishes in the limit.

43. In these formulæ the aim has been simply to represent as they are the regular local forcives acting on the molecules, as a distribution of force throughout the volume and, if need be, of traction over the surfaces of the material, thus avoiding the use of any hypothetical stress-system which might be a geometrical equivalent. It will presently be shown that an extension of the ideas underlying the YOUNG-POISSON principle of the mutual compensation of molecular forcives, employed in the theory of capillary action, requires that this local forcive shall set up a purely local physical disturbance of the molecular configuration in the material, until it is thereby balanced; in the case of an isotropic medium in a steady state it must thus necessarily be expressible as an imposed stress symmetrical with respect to the direction of polarization.

Let us, therefore, with a view to the verification of this proposition, analyze the effects of an internal stress symmetrical with respect to the lines of some kind of polarization denoted generally by i or (f, g, h). Such a stress must be of the type of a tension $(p + q)i^2$ along these lines combined with a tension qi^2 in all directions at right angles to them; for the stresses we are examining clearly vary as the square of the polarization. Thus the stress must be made up of a hydrostatic pressure $-qi^2$ combined with a tension pi^2 along the lines of the polarization. The tractions exerted by the latter part on elements of interface parallel to the coordinate planes yz, zx, xy are, per unit area, (qf^2, qfg, qfh), (qgf, qg^2, qgh) and (qhf, qhg, qh^2). Hence the total force exerted by the stress on the element of volume $\delta x\, \delta y\, \delta z$ is, per unit volume, (X, Y, Z) where

$$X = \frac{d}{dx}(pi^2) + \frac{d}{dx}(qf^2) + \frac{d}{dy}(qgf) + \frac{d}{dz}(qhf)$$
$$= (p + \tfrac{1}{2}q)\frac{d}{dx}i^2 + qf\left(\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz}\right) + qg\left(\frac{df}{dy} - \frac{dg}{dx}\right) + qh\left(\frac{df}{dz} - \frac{dh}{dx}\right);$$

and, the stress being self-conjugate, there is no torque. On comparison of this force with the local molecular, or cohesive, force on the element of volume, of electric origin, expressed above, it appears that they are of the same type provided $f\,dx + g\,dy + h\,dz$ is an exact differential, which is the case with the equilibrium electric polarization i' or (f', g', h') induced in an isotropic medium, the electric force being always under conditions of equilibrium circuital. The material stress which represents the regular electrostatic part of the molecular forcive by which the molecules hang together, is therefore a tension $\tfrac{2}{3}.\tfrac{4}{5}\pi i'^2$ along the lines of the polarization i' combined with an equal pressure $\tfrac{2}{3}.\tfrac{4}{5}\pi i'^2$ in all directions at right angles to them.* If, however, the medium were crystalline, the stress would be of a more complex type than this, being related to the crystalline axes as well as the axis of polarization. When the interface is the surface of a conductor, the forcive on the charge of free electrons which pervades the layer of transition adds nothing to this effect beyond what has been already set down; for the electric force due to a volume-distribution of single poles or electrons has no finite part depending solely on the element of volume at which its value is expressed, that is, it involves no molecular term.

44. The analysis here given is not however numerically applicable to a case in which the effective distance between the poles of a molecule is comparable to the distance between neighbouring molecules. The system formed by a bundle of iron nails suspended from the pole of a magnet and hanging on to each other against gravity, which has been used as an illustration of the molecular part of the forcive in the previous papers, does not come under these formulæ. That system may however be employed with advantage as a real illustration of the general principles, especially if we imagine the magnetized iron nails to be connected by springs or imbedded in an elastic matrix. When no extraneous forces such as gravity act on this model of a molecular medium, it adjusts itself into a condition of internal equilibrium, in which attractions between the magnetic nails are locally balanced by repulsions exerted by the springs. The various local molecular forcives, typified here by these attractions between magnets and forces exerted by springs, precisely compensate each other in each portion of the medium. If an additional magnetic field is introduced, which alters the magnetic polarities of the nails, the parts of the medium will change their shapes and volumes until compensation again supervenes: there will thus occur an intrinsic deformation of the medium, and there may be also intrinsic changes of its physical properties, associated with the polarization and proportional in simple cases to its square. Suppose now that an extraneous force like gravity, or the magnetic field arising from the medium as a whole, begins to act, that is, a regular mechanical force on the medium in bulk so that it is in the aggregate proportional to the volume on which it acts; this will produce a further deformation, but one proportional to the first power of the exciting force. The local internal molecular forcive will again no longer be exactly balanced; but the unbalanced part will possess at each point the characteristics of an elastic stress system, because when the element of volume is small enough the tractions thus arising over its surface must equilibrate without any assistance from the then negligibly small extraneous bodily forcive. Even then however this elastic stress excited by an external field cannot be specified in terms of surface tractions unless the dimensions of the smallest element of volume which the circumstances require us to consider are large compared with the range of the intermolecular forces. Unless that is the case, the energy of elastic strain of the element of the medium, expressed in GREEN'S manner, will involve higher fluxions of

* This is an example of MAXWELL'S theorem, § 40 *supra*.

the displacement in addition to those of the first order, and the equilibrium between two contiguous portions will not depend on continuity of displacement and of surface traction alone: other quantities also would have to be continuous for which there is no interpretation in the ordinary analysis of elastic reactions: the elastic stress would in fact not then be expressible in terms of tractions on interfaces. In such a case the only procedure that seems open, as the science of mechanics is now constituted, would be to transfer the effects of that part of the elastic energy which involves higher differential coefficients to the class of intrinsic or non-mechanical deformations.

45. In this theory of electric polarization the division of the forcive per unit volume into a molar and a molecular part has been made by means of the ideal volume and surface densities of POISSON, which are the equivalent as regards outside points of the actual polarization of the material. This method consists essentially in computing the forcive by combining opposed poles of neighbouring elements, instead of taking the single polarized element as the unit; it shows that these adjacent poles nearly compensate each other except as regards a simple volume density whose attraction has no molecular part, and a surface density partly at the outer surface and partly at the surface of the cavity which contains the point under consideration. The effect of the latter surface density, depending as it does wholly on the immediate surroundings, is the molecular or cohesive part of the average forcive.

These principles may be enforced and illustrated by contrast with a procedure by separate molecules which would usually lead to a different result; it will suffice to consider the case in which the polarization is uniform in direction throughout the material. If the axis of x be taken in the direction in which the intensity of the electric polarization changes most rapidly in the neighbourhood of the point considered, it is easy to see that the bodily force on an element due to the surrounding polar molecules is parallel to x and equal to $-(\mathfrak{a}f'\,df'/dx+\mathfrak{b}g'\,dg'/dx+\mathfrak{b}h'\,dh'/dx)$, and thus derivable from a potential function $-\frac{1}{2}(\mathfrak{a}f'^2+\mathfrak{b}g'^2+\mathfrak{b}h'^2)$, where $\mathfrak{a}$, $\mathfrak{b}$ are constants. In the case of an interface of rapid transition from one uniformly polarized medium to another, there is thus a forcive only in the transition layer, and its integral throughout that layer is equivalent to a traction parallel to the axis of x, of intensity $-\frac{1}{2}(\mathfrak{a}f'^2+\mathfrak{b}g'^2+\mathfrak{b}h'^2)\sin^2\theta$ where θ is the angle between the interface and the axis of x, pulling at the interface into each medium. If the polarization (f', g', h'), or i', is normal to the interface, this traction is $-\frac{1}{2}\mathfrak{a}i'^2\sin^2\theta$, if tangential it is $-\frac{1}{2}\mathfrak{b}i'^2\sin^2\theta$. To estimate the values of $\mathfrak{a}$ and $\mathfrak{b}$, we may consider separately the forcives exerted by the molecules of the polarized medium on μ_x, μ_y, μ_z, the components of a molecular moment μ situated in the neighbourhood of the interface, in the case when the interface is normal to the axis of x. The outstanding terms in the aggregate forcive due to the surrounding molecules, which do not cancel each other by symmetry, are normal to the interface and make up $\mu_x\Sigma\mu'_x\,d^3r^{-1}/dx^3+\mu_y\Sigma\mu'_y\,d^3r^{-1}/dx\,dy^2+\mu_z\Sigma\mu'_z\,d^3r^{-1}/dx\,dz^2$; or, per unit volume, $-\frac{1}{2}(\mathfrak{a}f'^2+\mathfrak{b}g'^2+\mathfrak{b}h'^2)$ wherein $\mathfrak{a}=-2\mathfrak{b}$ because $\nabla^2r^{-1}=0$.

The point, however, to be noticed is that this expression for the *total* traction on an interface, due to both molar and molecular forcive, which makes the pull on an interface lying normal to the lines of polarization twice as great as the push on one tangential to those lines, holds only for the case in which the material is polarized in the same direction throughout the whole extent of its volume. We may estimate by itself the action of the surrounding portion only, extending to any distance we please; but the action of the remaining outside part of the medium will still involve that of an inner surface density of uncompensated poles which will remain of undiminished order of magnitude. This procedure by separate molecules is thus not suitable for discrimination between the forcive due to the medium as a whole, which is transmitted, and the molecular forcive which is compensated locally.

46. The justification of the theory here applied, which balances on the spot the molecular part of the forcive due to the electric polarization, by an intrinsic cohesive stress in the material which is independent of the material elastic constants and strains at the place, may be further enforced by consideration of the ideally simple case of a gas. If a system of bodily forces act on it from a distance, they can always be balanced by a simple increase of pressure when they are derived from a potential function; while if they were not so derived the medium could not be in equilibrium. The dual phenomenon of equilibrium of the element of volume maintained by a balance between two forcives, an extraneous and an internal one, is really a balance between a forcive on the element of volume acting from a distance by the mediation of the æther, and another forcive arising according to the explanations of the theory of gases from the impacts of the molecules surrounding the element and, in the case of dense media, in part also from cohesive molecular actions. It is in this case a balance between a static bodily forcive and a steady kinetic molecular one; if the force transmitted through the æther from a distance increases, and equilibrium is to be maintained, the molecular configuration must be adjusted so that the impacts and the local molecular attractions shall continue to preserve the balance. When to the forces acting from a distance are added coordinated electric attractions between the molecules of the polarized medium, a further adjustment of molecular configuration must ensue. Now when the gas is electrically polarized, the attractions between neighbouring molecules give a forcive, not isotropic like a fluid pressure, but depending on the direction of polarization; its action will thus alter the originally fortuitous arrangement of the velocities of the molecules of the gas so as to impart to their distribution a slightly axial character, and when this has resulted in a new steady state the pressure due to the impacts will be different according to the manner in which the element of interface that is pressed is related to the line of polarization. This molecular addition of an intrinsic local stress, which has not the character of the ordinary fluid pressure, will just balance the action of the local electric attractions when the state of the system has again become steady; and being thus itself completely compensated *locally*, there will remain nothing of the molecular part of the

electric mechanical forcive to be transmitted across the material medium. The argument applies with suitable modification to any isotropic medium, as well as to a gas; for an æolotropic solid the specification of the actual molecular stress of different origin which thus balances for each element the molecular electric forcive will be more complicated, involving the axes of æolotropy as well as the axis of polarization

47. For present purposes the important consequence is that, under circumstances of equilibrium, that part of the forcive on an element of a material body which arises from the excitation of neighbouring molecules and is expressed in terms of them alone, is not transmitted by material stress, but forms a balance on the spot with the cognate internal molecular forcives of other types.* The only circumstance that might apparently vitiate this conclusion would be that the transitions between different media may be too abrupt to be treated, from the point of view of individual molecules, as really gradual transitions, after the manner of the above analysis; but even if we could imagine such a case, the discrepancy must for fluids be made up, provided the interface is a permanent one, by capillary forces in the interfacial layer, the effect of an outstanding surface derangement of energy.

In a dielectric body situated in an electric field there is thus the mechanical strain due to the field; and there are also intrinsic change of volume and other dimensions and of physical properties, proportional to the square of the local polarization. If the dielectric is solid, those changes of dimensions may not fit in with the continuity of the material without the intervention of secondary strains; but in fluid media the case is simple and precise, as no strain other than mere compression can exist.

The Mutual Compensation of Local Molecular Agencies: Organized and Unorganized Energy: The Single Postulate of Thermodynamics, Available and Degraded Energy: Physical Basis of the Idea of Temperature.

48. The scope of these molecular considerations (§§ 43–47) is wider than the special problem of polarization by which they are here precisely illustrated. To an intelligence that could follow the play of interaction between the individual molecules of

* This principle of compensating molecular forcives was briefly enunciated for capillary action and applied by YOUNG in his Memoir "On the Cohesion of Fluids": 'Phil. Trans.,' 1805. It forms the basis of POISSON'S "Nouvelle Théorie de l'Action Capillaire," Paris, 1831, in which the attraction between the molecules of a fluid is balanced by a repulsion of much smaller range, supposed to be due to their caloric: *cf.* especially Ch. VII. *Cf.* also Lord RAYLEIGH "On the Theory of Surface Forces," 'Phil. Mag.,' 1883, 1890, 1892, especially 1892 (1) pp. 209–220: and VAN DER WAALS' "Essay on Continuity of the Liquid and Gaseous States." In these illustrative discussions, in which the intermolecular forces are restricted to a non-polar character, the compensating stress is usually found in the assumption of an intrinsic fluid pressure of range much shorter than that of the attractions between the molecules: the principle however in its general form only asserts that this compensation must exist, and there is no necessity to specify its character.

matter, mechanical forces, in the ordinary sense, would not exist. The actual interactions between the molecules are however necessarily presented to us divided into various statistical groups, which are the subjects of perception by different senses; and it is the business of physical theory to follow out the relations of these different groupings to each other, and to trace them all back into the ultimate unity. The total energy of the molecules of a material body, corresponding to any kind of excitation or polarization, is thus for us made up of various parts. There is a part involving the interaction, with any molecule under consideration, of other molecules at finite distances, which integrates into an energy function of applied mechanical forces of the system, such for example as gravitational or magnetic forces. Of the remainder of the energy, which arises from the mutual actions of neighbouring molecules, a regular or organized part can be separated out which represents the energy of elastic stress, and is a function of the deformation of the element of volume treated as a whole: this stress arising from the immediate surroundings in part compensates, for the element of mass under consideration, the applied mechanical forces aforesaid. The remaining, usually wholly irregular, parts of the local inter-molecular forces and motions compensate themselves mutually on the spot,—or at any rate can be considered as thus compensated by other such forces, of different origins, that are not at present under consideration.* The temperature depends in fact on this irregular *residuum* of forces, and so do the density and the other physical properties of the medium, which are thus affected when, owing to polarization or other excitation, this local part of the molecular forces and motions is altered. If we adhere to these principles, it will not be allowable, in deriving the applied bodily forces of a polarized material system from its organized energy of polarization, to vary such physical constants of the element of mass as occur in the expression for the energy; for we should thereby be trenching on that part of the energy whose variation is compensated molecularly without directly originating transmitted bodily forcive, *cf.* § 63.

49. It seems desirable to have names for the two parts into which the total energy of the molecules of a material medium is thus divided. If we agree to maintain the original precise meaning of the term mechanical (as above employed), viz. that a mechanical force is one which we can actually control for doing work for our purposes on matter in bulk, in contrast with a molecular force which we can reason about but not directly employ, we may call the regular part the *mechanical* energy, and the remaining wholly irregular part the *non-mechanical;* we may also use (as above) the

* The principle of D'ALEMBERT, which is the basis of the dynamics of finite material bodies, necessarily involves this order of ideas. That part of the aggregate forcive on the molecules in the element of volume which is spent in accelerating the motion of that element *as a whole*, is written off; and the regular part of the remainder must mechanically equilibrate. But the wholly irregular parts of the molecular motions and forces are left to take care of themselves; which they are known to do for the simple reason that the constitution of the material body is observed to remain permanent.

terms *organized* energy and *unorganized* energy with the same meaning, the reference being now to the material medium as a continuous organic whole, transmitting applied forces by stress, not as a numerical aggregate of separate molecules. But it is to be observed that the distinction which is thus intended to be made is not the same as the thermodynamic division into *free* and *bound* energy, employed by VON HELMHOLTZ, which is itself precisely equivalent to the earlier division into *available* and *dissipated* energy, formulated by Lord KELVIN and RANKINE. The energy which in its actual condition is as regards direct mechanical effect unorganized, may become in part organized by aid of a physical transformation involving sifting processes of molecular fineness, which are necessarily non-mechanical and have no place in the dynamics of finite bodies. Thus the unorganized energy of two masses of different gases, at the same temperature and pressure, may be in part converted into organized energy and so into mechanical work by allowing them to transpire into each other across a porous partition, the diameters of whose pores approach molecular dimensions; and the transformation in this case shows itself in a resulting fall of temperature, when the work has been done. In the same way mechanical work may be derived from the unorganized energy of liquids by utilizing osmotic pressure; and the stores of energy of chemical combination of electrolytic substances, which as it exists in them is unorganized, can be largely utilized by making use of the sifting agency of electric force on their dual constituents. All these unorganized energies are therefore in part thermodynamically available, and others not now available may become so by means of yet undiscovered processes. But the unavailable or bound energy of thermodynamics is the *residuum* which we cannot render mechanical by any sifting process in bulk, or by anything short of the application of constraint to the individual molecules. This *residuum* may not be absolutely irreducible, but as the knowledge of physical transformations increases, some parts of it may be raised into the domain of available energy: on the other hand the recognition of temperature coefficients in reversible processes will show that some energies previously considered as wholly available are really in part unavailable. Each such discovery in fact involves an amendment or improvement in the corresponding thermodynamic relations; a process which has happened, for example, with respect to Lord KELVIN's law of electromotive force of a voltaic cell.

50. Once the idea of temperature is acquired, the whole science of Thermodynamics is implicitly involved in the principle of dissipation, that the unavailable part of the energy of an isolated material system always tends to increase, never of its own accord to diminish. The inference follows directly from this principle, by the reasoning first employed by SADI CARNOT, that if the system pass from a state A to a state B such that it can retrace its path back to A, the unavailable part of the energy is not changed: thus there is a whole "plane" or *complexus* of states, with perfect continuity of transformation among them, so that any one state is freely convertible—whether the process has been actually discovered or not—with any other for which

the available energy is the same, by transition through any intermediate series of these states; and we can pass continuously from one such *complexus* to the others in which the whole series of possible states are included, by additions of available energy to the system. The available energy is thus an analytical function of the physical condition of the system, including its temperature; and the trend of spontaneous change in an isolated system is in the direction in which this function diminishes, the positions of stability, as regards mechanical and thermal and also constitutive disturbance, being those for which it is a minimum. The circumstances of all *steady* configurations of matter, whether static or kinetic, are determined by this law.* It is more direct to state the proposition in the form that the unavailable energy tends to a maximum, the presumption being that sensible energy is available until it is shown to be otherwise. This principle, that energy tends to become mechanically disorganized, or that it never spontaneously tends to organize itself, cannot from its nature be other than axiomatic: and the formation of the available energy function for the different states of matter is then the main business of Thermodynamics. The reversible processes which thermodynamic argument employs are ideal types of regular change, theoretically realizable by mechanical constraints which do not control the individual molecules—the limiting forms, it may be, of imperfectly reversible changes which we can actually produce; the states of matter thus derivable from each other are shown, from the equality of their available energies, to have definite mutual relations which are independent of the ideal process (or construction, to use a geometrical analogy) by which the transitions between these states have been imagined.

The really abstruse abstract problem of the subject is that of the nature of temperature; and the principle most in need of elucidation is that, when a body A is in thermal equilibrium with B, and also B with another C, then A would be in thermal equilibrium directly with C. The most definite thermal specification of a body is the quantity of energy it contains; two bodies are in thermal equilibrium when there is no tendency for energy to pass from one to the other, independently of change of molar configuration or molecular constitution; they are then said to be at the same temperature. The *rationale* of this transfer of energy has been made out for the case of gases, where the exchange takes place in encounters between the molecules, so that there is no tendency to transfer from one mass of gas to another in contact

* In so far as our constitutive knowledge of material systems relates merely to comparison of different steady states, it can be wholly based in WILLARD GIBBS' manner, like ordinary statics, on relations of available energy of a simply additive character: it is where our knowledge becomes more intimate, and we attempt to trace the courses and rates of kinetic phenomena, for instance in material kinetics, electrodynamics, optics and vibratory phenomena in general, that the simple relations of energetics become insufficient as a mathematical basis for general physics. The principle of available energy suffices for tracing the relations of matter in bulk through the various steady phases in which *ex post facto* it is found to exist: but the genesis of these phases is expressly excluded from its domain.

with it if the mean translatory energy of the molecules is the same for each. This principle of temperature-equilibrium shows that, for all states of matter, the equilibrium of energy between bodies in contact in a steady state involves that a definite molecular relation of the one body shall equilibrate a definite molecular relation of the other: and its universality requires that this relation, whatever it may prove to be, shall be a very fundamental one.

51. It would seem that we can make at any rate an advance towards a complete view by realizing that, even if our sensations of heat had not compelled us to assign a fundamental place to temperature in the physical scheme, the principle of negation of perpetual motions must have led to the formulation of that conception, just as it has in fact led to the conception of potentials. If thermal equilibrium between two homogeneous bodies A and B in contact were not conditioned merely by some physical property of A alone being equal to some property of B alone, then if we had A in contact with B, and B with C, each in a state of equilibrium, and, removing B by mechanical means, moved A into direct contact with C but with such ideal constraint applied to the matter in bulk that chemical action is prevented, the physical state of each of these latter bodies would become changed, involving the performance of mechanical work; and a self-acting cycle could be designed by which we might thus obtain an unlimited quantity of work, that is, so long as there remained any diffused molecular energy to be converted. Hence in equilibrium there must be a property, namely the temperature, of each individual body in the field that has the same value for all of them: although of course this does not prevent us from imagining a partition or constraint, nearly adiathermanous, across which such equilibrium would be established as slowly as we please. It follows also that equilibrium of temperature must be the same whether it is brought about by conduction or by radiation. Temperature, as thus introduced, has nothing to do directly with the field of force in which the body is situated: for the relations of bodies to fields of force, in which they are moved about, are treated independently in the consideration of energy relations, and must not be introduced twice over,—or, in other words, the perpetual motion principle can be directly applied.

The single fundamental principle, on which all thermodynamic and thermochemical theory rests, would thus be the axiom of the negation of perpetual motions: and this stands rather in the relation of a principle that could hardly be conceived to be otherwise on any feasible physical scheme, than of one of which we can expect to offer any formal demonstration. Various essays have been made to deduce CARNOT's principle and a dynamical specification of temperature from special hypotheses as to molecular action: it may be held that, in so far as these are useful it is by way of illustration. It is even possible to conceive, but only in a highly abstract sense, that thermodynamics might have been developed in CARNOT's manner out of the perpetual motion axiom alone, without the aid of JOULE's demonstration of the nature and measure of heat; there would then have been merely no knowledge of what had become of energy that had ceased to be mechanically available. It is thus the principle of the limited conservation of available energy, rather than the complete conservation of total energy, that reigns in general non-molecular physics.*

There is still however the complication that the available energy of a system is not a function of its state alone, but involves comparison with some standard state into which it is possible for the system to be transformed. To find the extent of this undetermined element, let us simplify the relations in the ordinary manner, by adopting the scale of temperature that is given by the expansion of an ideal perfect gas, and find out how much energy is dissipated or lost to available mechanical effect, when a quantity of heat H_1 is abstracted at the temperature T_1 and of it H is returned at the temperature T. If all possible mechanical effect were produced, only $H_1 . T/T_1$ would be thus returned instead of H: hence the dissipation is $H - H_1 . T/T_1$ or $T(H/T - H_1/T_1)$. Thus an operation of this kind which does not involve dissipation does not alter H/T: and by accumulation of such changes it follows that any two states of the system which are convertible without dissipation have $\Sigma H/T$ the same for both. The entropy function ϕ of CLAUSIUS thus necessarily enters into the analytical formulation of the principle of mechanical availability. Between a standard state at temperature T_0 and another state at T the dissipation is $T(\phi - \phi_0)$; thus the available energy A in the latter state is $E - T\phi + T\phi_0$, where E is the total energy which involves an undetermined constant part, and ϕ_0 is another undetermined constant which represents the entropy of the system in the standard state. The temperature of the standard state to which the system is referred could not of course be the ideal, practically infinitely remote, temperature which is called absolute zero; that would imply that the energy is all mechanically available as in ordinary statics.

The presence of this undetermined multiple of T does not really restrict the application of the theorem of minimum availability: it merely implies that when once mechanical and constitutional equilibrium has been determined at any assigned temperature by making A a minimun with respect to the other independent variables, still further degradation will occur if opportunity is allowed for fall of temperature by escape of energy from the system. All that it is necessary to ascertain in any problem is the equilibrium as regards physical state and chemical constitution *at each temperature*, and the capacity of the system for heat, which specifies the thermal change that occurs when the temperature is altered. There is no restriction involved in taking the temperature the same throughout the system, for that is a necessary condition of equilibrium: when it is convenient to imagine partitions impervious to heat, the parts of the system thus separated can be treated as independent systems. The available energy, here arrived at directly from the

* This seems to be substantially the position which RANKINE took up in 1853 ("Scientific Papers," p. 311): *cf.* also the weighty introduction to "Outlines of the Science of Energetics," 1858, *loc. cit.*, pp. 209-220. It is in fact the standpoint of CARNOT'S "Reflexions."

perpetual motion postulate, is the same as the free energy of VON HELMHOLTZ's exposition: he has explained ("Abhandlungen," II., p. 870) how its form can be experimentally ascertained for the different phases of matter, except as regards an undetermined part, as above, of form L + MT, where L and M are constants; that then the equilibrium state of a system of reacting bodies at any assigned temperature is the one that makes it minimum for that temperature, thereby formulating the general solution of the problem of physical and chemical equilibrium: while the other properties of the system, heat-changes and heat-capacities, as well as total energy and entropy, are obtained from it directly by processes of differentiation. The available energy is thus a single characteristic function which includes and determines completely the circumstances, mechanical, thermal, and constitutive, of the steady states of an inanimate material system.

Application to Fluids: LAPLACE'S *Intrinsic Pressure: Law of Osmotic Pressure: Laws of Chemical Equilibrium.*

52. In an incompressible fluid medium in equilibrium, no part of the bodily extraneous forcive is compensated by reaction arising from special strains produced around the element of volume itself; it is all transmitted by fluid pressure independently of the special physical constants of the medium. For equilibrium to subsist in a polarized fluid, the applied mechanical forcive must simply be derived from a potential. When the induced polarization follows a linear law, this potential must also be equal and opposite to the organized energy induced per unit volume in the medium on which this extraneous forcive operates; for the total organized energy that has been spent in the polarization of the element $\delta\tau$ is equal to $\delta\tau$ multiplied by the scalar product of the polarization and the polarizing force, and of this one half is mutual energy of the polarizations of the elements of volume and one half is mechanical work done in the process (*cf.* § 71). If therefore the organized energy of the internal excitation of the medium is expressible as a volume-density of energy represented by a continuous function, the fluid medium will be in internal mechanical equilibrium: but if that function is discontinuous so that in crossing some interface the density of induced energy abruptly changes its value (as for example may be the case when the interface separates two different substances) then in order to maintain equilibrium the applied forcive must include a traction applied to this interface along its normal, of intensity equal to the difference of the densities of energy on its two sides, and acting towards the side of smaller density of energy. At an external boundary there must similarly be applied an outward traction along the normal, equal in intensity to the density of organised energy induced in the part of the substance that is just inside.

To illustrate and elucidate this by the electric phenomena, consider the interface between two dielectric fluids to be maintained in position by an applied traction:

let an element δS of it sustain a displacement δn along the normal, of amount very slight compared with the linear dimensions of δS. If no other boundary within the range of the electric field is thereby affected,—for instance if each fluid is supposed to be continued in a narrow tube to a great distance beyond the field and simply advances or recedes in the end of this tube,—the change of organized electric energy is merely the substitution of a volume $\delta S \delta n$ of energy of the one intensity for the same volume of the intensity on the other side of the interface. The displacement of δS of course affects the state of the field all over, but by hypothesis the electric field was in internal equilibrium, so that the change of the organized energy of any volume-element of the mass arising from a slight derangement is of the second order of small quantities, and produces no sensible effect. The above change of energy is thus equal to the work done by the extraneous traction over δS; which confirms the result already obtained (§ 37) by detailed analysis of the polarization, that the traction is along the normal to the interface and equal in intensity to the difference of the densities of the organised electric energy on its two sides.

53. It is advantageous in connexion with this subject to form a definite conception of the transmission of ordinary mechanical pressure in a liquid. Let us imagine an ideal infinitely thin interface in the fluid: what concerns the equilibrium of the fluid on one side of it is not the pressure which that fluid exerts on the interface, but the forces that are exerted on that fluid itself both by the interface and by the molecular attraction of the fluid on the other side of it. As the range of molecular attraction is very small, these forces together make up a pressure on the fluid, equal in circumstances of molecular equilibrium to the resistance of the interface against the impacts of the molecules diminished by the attraction exerted on these molecules across the interface; and this is the pressure that is transmitted by the fluid. For imagine a canal or tube in the fluid, with infinitely thin sides, and of diameter large compared with the radius of molecular action, and consider the equilibrium of the mass of fluid contained in it between two cross-sections A and B. There will be this pressure acting on the fluid just inside A, and a similar pressure acting on the fluid just inside B; and unless these are equal, or balance each other with the aid of extraneous applied forces such as gravity, the mass of fluid cannot be in equilibrium. This is Pascal's principle, that the mechanical pressure is transmitted unchanged in amount, except in so far as it is compensated by extraneous mechanical forces. It is to be noticed that the argument does not assume that the fluid between A and B is homogeneous, all that is required is that it be in equilibrium; the cross-sections A and B may be in different fluids, with an interface between, and, provided the diameter of this ideal canal is large compared with the radius of molecular action, the interfacial forces will practically all be mutual ones between molecules inside the tube, and so will not affect the transmission of pressure. It is this transmitted pressure that is the subject of actual measurements: for example in Andrews' experiments on the compression of carbonic acid, it is the pressure

so transmitted through the mercury into the companion manometer tube containing perfect gas that is measured and is represented on his diagram of isothermal lines. The two terms of which it is the difference, namely the reaction of the interface against molecular impacts, and the molecular attraction across the interface, are separately represented in VAN DER WAALS' characteristic equation. When the virial equation of CLAUSIUS is applied to a mass of liquid with a free surface abutting on a gaseous atmosphere, there results the relation that the pressure of this atmosphere against an outer boundary, which is the same as the transmitted pressure in the liquid, is equal to two-thirds of the part of the mean density of kinetic energy in the liquid that is connected with encounters and mutual forces between the molecules, together with one-third of the mean virial per unit volume of these intermolecular forces, the latter part being negative and, if polar forces could be assumed absent, of CLAUSIUS' form $\frac{1}{3}\Sigma Rr$; and this without reference to the character of the transition between liquid and gas at the free surface. When on the other hand the virial equation is applied to a mass in the homogeneous interior of the liquid, bounded by an infinitely thin interface, the virial of each molecule vanishes because the attractions acting on it compensate each other on the average, and the result is that the kinetic pressure exerted by the fluid on this interface is simply two-thirds of the mean density of kinetic energy of the bodily motions of the molecules, their internal constitutive energies being excluded.* It follows that the mutual molecular attraction across the interface produces a pressure on the interface from each side equal to the mean virial per unit volume; as in fact would flow directly from the principle that two statically equivalent force-systems have the same virial.

54. Let us construct as above an ideal rigid tube, with infinitely thin walls which exert constraint on the molecules but no attraction, having one of its open ends A in the liquid and the other B outside it; but let us now suppose that the diameter of the tube is small compared with the radius of sensible molecular action, which implies that this radius extends over a considerable number of molecules. The molecular forces acting on each molecule in the tube, whether near the end of it or not, are now almost wholly due to molecules outside it, and are on the average self-balancing, except in the case of molecules at the free surface which are subject to the whole inward molecular attraction of the liquid. The equilibrium of the contents of the tube, which are liquid in one end and gaseous in the other, therefore requires that the kinetic pressure on the molecules in the liquid end A exceeds that on those in the gaseous end B by a constant amount, namely the pressure due to the inward attrac-

* Some consideration is required as to the omission of the virials of mutual forces acting inside the separate molecules: these must be taken as wholly compensated by kinetic energy of internal motions not thermal, which is legitimate in so far as molecular encounters do not sensibly excite radiation but only slow free precessional motions, and so do not sensibly disturb the configuration of the internal dynamical system of the molecule.

tion exerted on the surface molecules in the layer of transition. It follows that the pressure of molecular attraction across an internal interface, which is the virial per unit volume with changed sign, is equal to LAPLACE's intrinsic pressure K in the liquid arising from the inward attraction of the surface molecules. This equality is easily seen to involve the consequence that the layer of transition at the free surface is very thin compared with the radius of molecular attraction, an important conclusion of which the bases are here the statistical stability of the liquid state, the dynamical principle of the virial, and the hypothesis that the range of sensible molecular attraction extends over a considerable number of molecules in the liquid state. In the condensation of a vapour there is degradation of internal energy into sensible heat of amount equal to the latent heat of condensation diminished by the work of condensation of the vapour and increased by the volume of liquid thereby produced multiplied by the LAPLACIAN pressure K.

55. Consider two fluids, one the pure solvent and the other a solution, separated by a rigid porous partition, with extraneous pressure applied on the side of the solution to balance the osmotic pressure and so to produce equilibrium as regards transpiration through the partition. Now let a slight amount of transpiration occur by very slightly reducing this extraneous pressure: thereby work is done against that pressure, equal to its intensity multiplied by the change of volume owing to transpiration of the solvent into the solution. The operation takes place steadily under conditions of equilibrium, so that it can be reversed either by a known process or, as we might assume, by some process not yet discovered—in this case merely by reversing the pressure, or it may be cyclically by evaporation: thus the work is done at the expense of an equivalent of available energy, partly thermal, and partly of a molecular type which would otherwise run down into heat of mixing of the liquids. Hence the osmotic pressure between two fluids is equal to the whole amount of free or available (not total) energy that would be degraded when unit volume of the pure solvent is mixed with an indefinitely great volume of the solution into which it transpires, supposing that there is no sensible change of volume in that process; if there is change of volume this value must be altered in the ratio of the final to the original volume of the transpired material: so long as the dissolved molecules are out of each others' range of influence, the change of volume, if any, must be independent of concentration. This proposition will be exactly true if the pores in the partition are so narrow, that the cross-sections of the filaments of fluid contained in them each involve so few molecules that the mutual energy of the molecules of fluid in the pores is negligible compared with that of an equal mass of fluid in bulk. Inasmuch as to excite the osmotic pressure, pores or tubes of molecular fineness have to be employed, it follows that it is not an ordinary transmitted mechanical pressure; and the energy which is associated with it is not merely the organized energy from which the mechanical forcive is derived, but the whole amount of energy thermodynamically available. If the pores are wider, the mutual energy of the molecules in them ceases

to be negligible; the effective osmotic pressure then diminishes, being accompanied by diffusion in the pores which involves dissipation of energy that would otherwise produce osmotic effect. Thus the proposition that the osmotic pressure between two fluids is equal to the free or available energy of mixture per unit volume of transpiration, gives only the limiting value which applies to partitions with pores sufficiently narrow. In the equilibrium stage of transpiration through a colloid membrane, operating by absorption into one face of the membrane and evaporation from the other, the limiting pressure may however be reached, provided the action does not involve irreversible thermal processes in the membrane.

The osmotic pressure between a solution and the pure solvent is, from another point of view, the mean aggregate of the forces that have to be applied to the individual molecules of the dissolved substance in order to prevent them from travelling across the interface into the pure solvent, whether that forcive be applied by the resistance of a material partition, or as in the case of ions diffusing across the interface between two salt solutions in contact, by the pull of the electric field which the diffusion has produced—the unmodified molecules of the solvent being in each case free to move either way. Viewed in this light, there is nothing occult or merely analogical —unless it be the presence of ions—in the principles by which NERNST determines the constitution of the layer of transition which gives rise to the potential difference between two salt solutions, and so determines the voltaic and thermoelectric differences of potential at such transitions, by balancing a bodily force arising from osmotic pressure by another arising from the electric field due to the reacting double layer generated by the diffusion.

56. Suppose that the pressures on the two sides of a porous partition separating dielectric fluids are adjusted so that there is no flow across it. When an electric field is introduced this equilibrium is destroyed by the effective electric tractions on the interfaces of separation between the dielectric fluids in the individual pores. To re-establish equilibrium a difference of pressure at the two sides of the interface, equal to that of the electric tractions (§ 37), must be called into play: that is, an electric field influences the value of the osmotic pressure between dielectric fluids. This effect is of course directly connected, through a cyclic process, with an influence on vapour tension (§ 81, *infra*). Its amount is to a great extent independent of the size of the pores; though when the pores are of molecular dimensions it mainly arises from a bodily forcive on the contained filaments of fluid. This electric osmotic pressure will then even hold good with respect to liquids which readily mix; for the obliteration of the sharpness of the interface in the narrow tubes or pores of the partition will take place very slowly, while the formulæ of this memoir for electric tractions are precisely those which hold good for a gradual transition.

This action is different from the one discovered by QUINCKE and discussed by VON HELMHOLTZ,* forming in fact a further extension of the scope of the principle of

* VON HELMHOLTZ, "Studien über electrische Grenzschichten," 'Wied. Ann.,' 7, 1879.

electrolytic dissociation, in which a stream of conducting fluid forced through a porous non-conducting partition produces an electric current across it, and conversely an electric current forced across the partition carries the fluid with it. Over the surface of each pore there is, on the present view, the intrinsic static potential difference between the partition and the fluid, due to strong orientations of the polar molecules of these two media which lie along the interface, under their mutual influence which stands in place of von Helmholtz's attraction of matter for electricity as the exciting cause of voltaic phenomena;* and this difference will be in time diminished by the presence of free ions which become attached among the outward-pointing poles, thus constituting a reverse potential difference with which electro-capillarity deals. The flow of fluid through the pores carries on some of these ions along with it, which thus constitute the observed electric current. Sudden diminution in the extent of the surface would act similarly by crushing them out, as in the observed electrification near waterfalls: rapid extension of the surface, as in the formation of drops in air, should conversely eliminate the effect of the ions bound to the polarized air-film on the surface, by spreading them over a wider area, and so increase the potential difference towards the limiting statical value. On the other hand, when the media in contact are very dilute electrolytic solutions in the *same* solvent, the calculations of Nernst show that the potential difference is wholly an affair of ionic diffusion, as indeed it must be if the efficient polar molecules are all ionized; in that case the normal potential difference will require a sensible time to become established. When, in the case of a mercury electrode dropping into an electrolytic solution, sufficient time is not allowed, the part of the actual potential difference which arises from this cause and not from the intrinsic statical orientation of the molecules, will tend to a vanishing limit, except in so far as it is continually restored by a polarization current in the electrolyte.

57. In the case of very dilute solutions it is possible to obtain a definite expression for the limiting, or maximum, osmotic pressure. After a certain stage of dilution, each dissolved molecule is effectively out of touch with its fellows and is completely environed by a collocation of molecules of the solvent: further dilution therefore does not involve any sensible change in the mutual free energy of the solvent and the dissolved molecules; all that occurs is a wider separation of the dissolved molecules in space, with such energy changes as may be directly concerned in it. Suppose now that the dissolved substance is a gas, and that the solution is separated from the pure solvent by a partition which the latter can traverse while the gas cannot: whether such partitions are known to exist is inessential to the theoretical argument, the function of the partition being merely passive constraint exerted on the *aggregate* of

* [Helmholtz had to be content in his analysis with the crude conception that different kinds of matter attract electricity differently. On a scheme like the present the obvious explanation is that the polar molecules of the two substances act on each other across the interface, producing a certain regularity of orientation which forms the intrinsic double layer to which the potential difference is due.]

the dissolved molecules. The solvent will transpire across the partition into the solution, unless a definite osmotic pressure acts against it, when there will be equilibrium. Let us examine the change of free energy involved in the very slow transpiration of a certain volume; all essential that has happened has been an expansion of the molecules of the contained gas, each with its fluid environment, into a larger space. We may compare the two states of the gas, as it would exist free with these two different volumes, and then suppose that by an ideal process the fluid environment of the molecules is directly brought about in each case: that process will, as regards change of intimate molecular configuration, be essentially the same for both states of the gas, therefore the change of free energy due to the dilution of the solution is simply that which corresponds to the free gaseous expansion of the dissolved gas.* This conclusion carries with it, by the thermodynamic principle of free or available energy, a theoretical proof of VAN'T HOFF's generalization that the osmotic pressure of a very dilute solution is equal to the gaseous pressure of the dissolved molecules when they are supposed to occupy the same volume in the gaseous state. The extension of this proof to dissolved liquids and solids, which form the practically important case, is at first sight barred (unless it is formulated as in the footnote) by the fact that we cannot then actually have the molecules existing free at the same volume as they occupy in the dilute solution. But when the ANDREWS isothermal for the dissolved substance is made into a continuous curve by inserting a supersaturated wavy part, there will always be a real point on it corresponding to the volume occupied by the substance thus existing in a homogeneous condition, and also a corresponding pressure which at the small density under consideration would practically be that of the gaseous state: thus there would be no difficulty in the extension to dissolved solids and liquids, were it not that this point on the isothermal might be on the thoroughly unstable reach, along which rise of density corresponds to fall of pressure, so that any slight accidental inequality of density would be spontaneously increased. The successful use made of the ANDREWS diagram for numerical calculation of the properties of substances by VAN DER WAALS shows however that its physical reality is not destroyed by this instability: and when it is remembered that instability can be theoretically removed by slight constraint which does not sensibly affect the material transformations and does not affect the energy relations at all, it will appear that there is good reason for generalizing the law of osmotic pressure above demonstrated for gases. As before stated, what is most desirable to

* The circumstance which makes this purely imaginary process legitimate is that the available energy is a function of the constitution of the matter *in bulk*, not depending on the accidental characteristics of state or motion of the individual molecules: now the only change that has occurred as regards the constitution of the substance in bulk, that can affect either the available or the total energy, is the change of volume of the solution by transpiration of the pure solvent across the partition, which by the above affects it in a manner absolutely independent of the nature of the homogeneous solvent, and therefore of the existence of the solvent at all, because the relation of each molecule to the portion of the solvent within its sphere of influence is not changed.

supplement on mechanical principles an explanation like the present one is not so much any accession of logical rigour on ordinary thermodynamic premisses, as some precise notion of what is involved, as regards detailed molecular dynamics, in equality of temperature. In the present differential procedure no assumption has been made on that head; and no inference that osmotic pressure is, like gaseous pressure, due to simple molecular bombardment is warranted. When a theoretical basis is thus found for VAN'T HOFF'S principle, the laws of the molecular influence of dissolved substances on the freezing point and vapour tension of very dilute solutions of course go along with it.

58. It may be objected that the application of the principle to ionized solutions would compel us to admit the possible theoretical existence of a gas consisting of ions: but that is not really so, because the argument only compares one state of dilution with another. Yet on the other hand there is the hypothesis, supported by BRÜHL'S work on optical equivalents, that under certain circumstances oxygen is a tetrad element, so that the molecule H_2O can take up sufficient ions to form another saturated molecule of type $H_2 = O = X$, and that therein lies the cause of the regular ionization current produced by solution in water (the ions X being free only when passing from one such combination to another), as contrasted with the irregular ionization of free gases. Changes of valency in an element remain unexplained, but their occurrence is now usually accepted as matter of fact.* The function of the osmotic diaphragm is merely passive, to prevent mixture by diffusion and consequent loss of mechanical availability. In a mutual solution of two substances, it is usually only the one that is present in large excess that gets through the diaphragm in purity: if it should prove to be a general law that the dialyzing action is only complete when there is such large excess, it would be strong evidence for the view that the molecules dissolved in it form the nuclei of loose molecular complexes which are too large and permanent to get through, while the free solvent in which the molecules are not thus grouped is not so hindered.

When a solution is made more and more dilute, there comes a stage when it would

* The connexion between the various phenomena may be pictured in neutral terms, as POYNTING has recently done ('Phil. Mag.,' Oct. 1896), starting from a hypothesis that pressure increases the "molecular mobility" of a fluid according to an *assumed* law equivalent to VAN'T HOFF'S principle. In order to evade the hypothesis of partial dissociation in salt solutions, he restricts the sphere of action of a dissolved undissociated molecule to one or two or three definite molecules of the solvent, leading to correspondingly different amounts of osmotic pressure; thus a temporary chemical combination is dealt with instead of, or it may be in addition to, an extended sphere of influence. But the considerations given above show that it is the number of spheres of influence that is really effective, so that if there is chemical combination it must be in part with dissociated ions as in BRÜHL'S view. On the special assumptions involved in the extension of the methods of gas-theory to liquids, BOLTZMANN ('Zeits. für Phys. Chemie' vi, p. 478) has offered a demonstration (approved by LORENTZ) of the law of osmotic pressure, which seems to refer it to molecular bombardment, and require that the mean energy of translation of a molecule shall be the same in the liquid state as in the gaseous state at the same temperature.

seem impossible to imagine that its electrolysis, if it remains of normal type, is conducted through a mechanism like GROTTHUS' chains: the dissolved molecules are far out of each others' range of influence, and the very first stage of the working of a GROTTHUS' chain containing molecules of the solvent would produce that dissociation which it is the object of the chain theory to evade. Similar considerations apply to the velocity of chemical reactions. When a solution of K.HO neutralizes one of HCl, the heat generated is mainly that of the union of H and HO to form H_2O: when the solutions are very dilute this should take a considerable time to develope, even allowing for intimate mixture by stirring, if each H had to find its HO partner directly. The immediate reaction must therefore be due to a mobile equilibrium of dissociation being disturbed by the mixing of the solutions, and then re-establishing itself.* Thus in the progress of an ion H through the water under the electric force in electrolysis, it would not be the same H that is driven on, but that ion often gets fixed liberating another one in its place, so that it is the mean translation of a condition of matters in which there is a definite number of H ions in the element of volume that is given by KOHLRAUSCH'S law, not that of an individual ion. This accords with WHETHAM'S interpretation of his result, that in acetic acid solutions, in which the conductivity is abnormally low, the ionic velocity is abnormal to an equal extent.†

59. A principle quite analogous to the one on which VAN'T HOFF'S law has here been based, has already been applied to a cognate phenomenon in authoritative investigations. The transpiration of two different gases into each other across a porous partition establishes a difference of pressure; there is thus present a store of available energy, which would be run down in the mixing of the gases; its amount, as originally determined by Lord RAYLEIGH from the special properties of gases, is obtained by finding how much free energy runs down when the gases are each separately expanded to the volume of the mixture, and adding these amounts. This result, either in the present form or expressed with reference to entropy, has been sanctioned, explicitly or tacitly, as axiomatic by MAXWELL‡ and other authorities, when applied to gases whose molecules do not exhibit sensible mutual attraction: the change of configuration arising from the two mutually independent systems occupying the same space, instead of different equal spaces, is rightly held to involve no change in the available energy. The principle above employed is of precisely similar nature.

If we imagined two gases in which the molecular mass differed only infinitesimally,

* In the same way, if a gaseous reaction were of ternary type, so that three atoms or ions had to unite to form a molecule, it must proceed far more slowly than a binary reaction, and may not get established at all, except by the help of the catalytic action of some other substance, such as water vapour, in reducing it to binary stages or facilitating the simultaneous presence of the three kinds of atoms in the same molecular sphere of action.

† W. C. D. WHETHAM, "Solution and Electrolysis," 1895, pp. 142, 155.

‡ 'Encyc. Brit.,' Art. "Diffusion": Collected Papers, II., p. 644.

just the same amount of work could still be gained by mixing given volumes of them in a reversible manner as if they were gases wholly unlike; but the transpiration pressure would then be infinitesimally small and the time of transpiration infinitely great.* It is thus impracticable to proceed to a limit, and no paradox is here involved such as the assertion that a finite amount of work could be gained by mixing two gases which are practically identical in properties. A similar apparently paradoxical limiting case might be formulated as regards osmotic pressure of a dissolved substance very nearly identical with the solvent.

60. The law of HENRY that the density of dissolved gas is in a constant ratio s to its density as it exists free in the surrounding atmosphere, is involved in the osmotic law, and conversely may be employed to verify it. In circumstances of equilibrium the potential of free energy of the dissolved gas (in GIBBS' sense) must be the same in the liquid and the atmosphere; that is, the removal of an elementary portion of the gas from the liquid to the atmosphere must not alter the free energy of the whole. Thus the difference of the free energies of the dissolved gas per unit mass, when its partial pressure in the liquid is changed from p_1 to p_2, is equal to the difference of the free energies of the gas per unit mass in the surrounding atmosphere when its partial pressure is changed from p_1/s to p_2/s. The latter difference is by Lord RAYLEIGH'S principle, independent of what other gases may also be present in the atmosphere: it is thus $\int pdv$, where $pv = R'\theta$ for the unit mass of gas, and is therefore at constant temperature $R'\theta \log p_1/p_2$. This does not involve s, and therefore the *difference* of the free energies of the dissolved gas at two different densities is the same as if it existed in the free gaseous state at those densities: and this carries with it identity for the two states as regards all relations of available energy and work. Conversely, the law of HENRY follows as above, by the principle of available energy, from the circumstance that the molecules of the dissolved gas are outside each others' spheres of molecular action, independently of any picture that we may form of the process of exchanges in evaporation and absorption.

It is a confirmation of the soundness of this thermodynamic theory, that the law of osmotic pressure for dissolved gases is immediately involved in, and might have been predicted from, the equations given by VON HELMHOLTZ in 1883,† in a discussion of their energy relations in connexion with the theory of galvanic polarization.

* The assumption is involved that the gases are really different and that means exist for separating them. [The fact that the amount of available energy at our command depends on the control we have learned to exercise over physical processes does not detract from the objective validity of that conception as a deduction from general principles of molecular theory, as has often been suggested (*cf.* §49 *supra*): any more than our possible complete ignorance of some forms of total energy would give to the idea of energy itself a subjective aspect.]

† "Zur Thermodynamik Chemischer Vorgänge," III., 'Monats. Berl. Akad.,' May, 1883, especially equations (4) and (5); 'Abhandlungen,' III., pp. 101–114. The law had however been arrived at quite explicitly by WILLARD GIBBS as early as 1876 in his discussion of the general theory (p. 226). Recently the argument has been carefully formulated by Lord RAYLEIGH, 'Nature,' Jan. 14, 1897: *cf.* also a

61. It is the circumstance that the available energy A of § 51 is a function of the bodily configuration and constitution of the system, whose alteration by dilution is independent of the nature of the solvent provided the solution is sufficiently dilute, that makes osmotic pressure independent of the solvent and therefore the same as the corresponding gas-pressure. This is of course different from asserting that the whole available energy of a dissolved substance is the same as its available energy at the same density in the free gaseous state. In fact the difference between these energies may be estimated from a knowledge of the solubility: thus the available energy per unit mass of the gas in the solution at its actual density ρ' is equal to that of the same gas in the free space at the corresponding density ρ; so that the available energy per molecule of the dissolved gas is equal to that of free gas of its own density and temperature together with $R_1T \log \rho'/\rho$ and also R_1T for the volume occupied by the free gas. This makes in all for the excess of available energy, per molecule, of the dissolved gas $R_1T \log e\rho'/\rho$, or $R_1T \log es$, where s is the solubility and R_1 is a gas constant the same for all kinds of molecules. Like information is derivable from the ratio of partition of any dissolved substance between any two solvents which do not intermix: its available energy per unit mass must in the state of equilibrium be the same in both solutions.

62. The increase of available energy involved in molecules or atoms of given species appearing in the dilute solution during chemical change is, per molecule, $a - R_1T \log bN$ where N is the number of such molecules already there per unit volume and a is a function of the temperature, b being a constant which depends on the standard temperature of reference (§ 51). A reaction going on in the solution involves the disappearing by breaking up of molecules of some of the types present, and the appearing of molecules of other types to an equivalent extent: when chemical equilibrium is attained, the change of available energy arising from a slight further transformation of this kind must vanish: that is,

$$n_1 (a_1 + R_1T \log b_1N_1) + n_2 (a_2 + R_1T \log b_2N_2) + \ldots .$$

vanishes, leading to $R_1T \log b_1^{n_1}b_2^{n_2} \ldots \log N_1^{n_1}N_2^{n_2} \ldots = -(n_1a_1 + n_2a_2 + \ldots)$, where $n_1, n_2, \ldots$ are the numbers of the molecules of the different types that take part in the reaction, reckoned positive when they appear, negative when they disappear; so that $N_1^{n_1}N_2^{n_2} \ldots .$ is equal to K, a function of the temperature, which is the law of chemical equilibrium originally derived by GULDBERG and WAAGE from statistical considerations. Again, if A is the available energy of the whole solution, and δA, equal to $\delta A_0 + R_1T \log K'$, where $K' = Kb_1^{n_1}b_2^{n_2} \ldots .$, denotes its variation per

letter by GIBBS, March 18. The pressure difference is necessitated by the circumstance that the steady state would be brought about by interchange of individual molecules. But its amount is calculable *a priori* only when the dissolved molecules are practically out of each others' range: and then the argument in the text shows that it depends solely on the number of molecular aggregates with foreign nuclei that are present, irrespective of whether these nuclei are complete molecules or parts of dissociated molecules.

molecule of reaction without change of temperature, δA is null as above in the equilibrium state at each temperature, so that $\delta A_0 = - R_1 T \log K'$. And, with partial differentiation, $d/dT\ (\delta A/T) = d/dT\ (\delta A_0/T)$, hence it is equal to $- R_1\, d/dT\, .\log K'$ or $- R_1\, d/dT \log K$, and is independent of the unknown term A_0. Now reverting to the general theory, if E is the energy in a system, dH the heat imparted to it and dW the work done to it, $dE = dW + dH = dW + T\, d\phi$ and $A = E - T\phi + T\phi_0$, where A, E, T, ϕ are all analytical functions of the state of the system. Thus, employing total differentials, $d\,(A/T) = -E\, dT/T^2 + dE/T - d\phi = -E\, dT/T^2 + dW/T$: so that in the present case $d\,(\delta A/T) = - \delta E . dT/T^2 + d\,\delta W/T$. If the small amount of reaction represented by δ occurs so that no mechanical work is done on the system, δW is null; hence δE is equal to δH the amount of heat taken into the system from its surroundings per molecule of the reaction when it proceeds without work. Thus finally $\delta H = - T^2\, d/dT\ (\delta A/T) = R_1 T^2\, d/dT\ (\log K)$, which is the thermal relation developed by VAN 'T HOFF.*

On the Electromotive Forces established by Finite Diffusion.

63. The function of an osmotic partition in preventing by pure constraint the diffusive degradation of energy (§ 54) is illustrated by the theory of electromotive forces of diffusion. In the concentration-cells, of which the theory was established by VON HELMHOLTZ, the solution in each cell was homogeneous, and the influence of concentration was determined by balancing different cells against each other: there being no diffusion, the process was reversible, and thermodynamic formulæ were applicable. By forming an electrode of a metal surrounded by one of its insoluble salts, such as mercury surrounded by calomel, employing for the other one zinc immersed in zinc chloride solution, the net constitutive change at the mercury electrode when electricity passes through the cell is independent of the concentration of the solution, being simply the deposition of the equivalent quantity of mercury from undissolved calomel: hence that electrode accounts for a constant part of the electromotive force. On the other hand the change of free energy by dissolution of the equivalent of zinc is made up of a part arising from change of chemical constitution and another part depending on the concentration of the solution which receives the resulting chloride. The part of the electromotive force depending on the processes at the zinc electrode is thus in the case of a reversible electrode equal to const.—RT log p, where p is the osmotic pressure of the zinc chloride solution and R

* *Cf.* WILLARD GIBBS, *loc. cit.* p. 231, where the case of gaseous reactions was treated. More directly, we can form a reversible CARNOT cycle in which the constitutive change δA is made at temperature T and unmade at $T - dT$. The work of the cycle must be $\delta h\, .\, dT/T$, where δh is the heat absorbed in the change when the maximum amount of mechanical work is done in it by osmotic or other appliances: thus $\delta h\, .\, dT/T = dT\, .\, d\,\delta A/dT$, so that $\delta h = T\, d\,\delta A/dT$. When no work is done in the change, the heat absorbed is δH, equal to $\delta h - \delta A$, which is $- T^2 d/dT\ (\delta A/T)$ as above.

is now the gas-constant belonging to an electrical equivalent per unit volume, which is 8580 in c.g.s. units: this may be expressed in the form RT log P/p, where P depends on the metal of the electrode and on the solvent employed in the cell and on the temperature, but not on the concentration of the solution. This quantity P has been called by NERNST, on grounds of analogy, the solution pressure of the metal electrode in the solvent.* When the electrode is polarizable so that the processes are not reversible, the difference of potential must be less than this formula would give. If now we are dealing with a two-fluid cell, in which the fluids are separated by an osmotic partition and passage of the solvent is prevented by balancing the osmotic tendency by hydrostatic pressure, the processes are still reversible and the electromotive force of the cell will be RT (log P_1/p_1 − log P_2/p_2), while passage of a current will gradually polarize the faces of the partition. If however the ions could pass through the partition into the solution of different concentration without diffusion of the fluids in bulk, the part of this electromotive force depending on concentration would be cancelled, and there would remain RT log P_1/P_2 due solely to the affinity of the solvent for the materials of the electrodes. But if we are dealing with a cell, in which the fluids are in direct contact along an interface of finite dimensions so that steady diffusion at a finite rate is going on, or in which they are even allowed to diffuse steadily across an osmotic partition, there will be loss of availability owing to that diffusion, so that the back electromotive force arising at the junction of the fluids is less than the maximum value − RT log p_1/p_2. In the absence of knowledge of the rate at which the diffusive degradation of energy is proceeding and is affected by electric transfer, the principle of availability cannot supply a formula for this diminution of the back electromotive force, which will depend on the nature of the layer of transition: but a theory of the process of steady interdiffusion of two ionized fluids has been formulated by NERNST and PLANCK which involves an expression for its magnitude.† Thus, considering diffusion of a simple

* There appears a difficulty in imagining, in accordance with the view here taken, that the value of P can be dependent on a layer of the metallic ions extending into the solution, especially as the potential difference between dielectrics could not be so explained. *Cf.* § 56 *supra*.

† I find that applications similar to the above, but on a more extensive scale and with considerable differences in the argument, especially a more prominent use of entropy, are made in PLANCK's later important exposition "Ueber das Princip der Vermehrung der Entropie," 'Wied. Ann.,' 44, 1891, pp. 385–428. The general formula for the potential difference between two diffusing solutions is there obtained from the variation of an analytical function, which is really the available energy, on the hypothesis that the solutions are in a permanent state of diffusion, determined by NERNST's principles, in which the concentration varies from point to point so slowly that the diffusive dissipation other than electric may be neglected. *Cf.* also on the history of the subject NEGBAUR, 'Wied. Ann.,' 44, p. 737. In the text above the statements are confined to the case of binary electrolytes.

The development of the laws of chemical equilibrium given in § 60 has also been largely anticipated as to form by PLANCK, 'Wied. Ann.,' 32, 1887: his postulates are however different from those that enter here, where the analysis occurs as an outcome of a general view of the relations of molecules in bulk to the æther and to each other, §§ 11–12. [*Cf.* PLANCK, "Vorlesungen über Thermodynamik," 1897.]

solution across a layer in which the concentration varies, when the steady state is attained both ions must diffuse together at the same rate notwithstanding their different mobilities u and v, measured by KOHLRAUSCH as the values of their mean velocities due to unit electric force. Now the mean steady velocity of migration of a single ion is equal to this mobility divided by its electric charge e and multiplied by the force which causes its motion : this force consists of an electric part $-edV/dx$, where V is the electric potential set up during the transition to the steady state of diffusion, and of an osmotic part. To determine the latter, observe that when a solution is separated from the pure solvent by a permeable osmotic partition, the solvent is restrained from passing across only by an osmotic pressure acting against it : this means that to maintain the steady state without diffusion the osmotic partition must exert more pressure by the amount p on the solution than on the pure solvent. If we consider a layer of the actual solution, of cross-section unity and thickness δx, there would thus have to be a bodily force $dp/dx \,.\, \delta x$ exerted on it if the diffusion of its ions were prevented : therefore $-dp/dx \,.\, \delta x$ is the aggregate of the forces acting on the contained ions and producing diffusion, that arise from the gradient of concentration. If n be the number of ions per unit volume, the mean force per ion is thus $-n^{-1}dp/dx$: this is not a mere hypothesis founded on a vague analogy of osmotic pressure with ordinary hydrostatic pressure, but gives a precise measure of an actual force on a constituent of the medium. The number dN/dt of single ions of either kind that is driven across unit area of a geometrical interface in a solution of varying concentration by these forces is thus given, after NERNST, by

$$\frac{dN}{dt} = -nu\frac{dV}{dx} - \frac{n}{e}\frac{u}{n}\frac{dp}{dx}, \quad \text{also} \quad = nv\frac{dV}{dx} - \frac{n}{e}\frac{v}{n}\frac{dp}{dx}.$$

If D denote the coefficient of diffusion of the solution, $dN/dt = -D\,dn/dx$; and by the gaseous law which applies to osmotic pressure of very dilute solutions $p = neRT$. Hence immediately

$$D = \frac{2uv}{u+v}RT, \qquad \frac{dV}{dx} = \frac{v-u}{v+u}\frac{1}{ne}\frac{dp}{dx};$$

so that the integrated potential difference $V_2 - V_1$ across the diffusion layer is $RT\,(v-u)/(v+u) \,.\, \log p_2/p_1$. It follows that when steady diffusion is allowed to go on, the back electromotive force at the junction of the fluids is thereby reduced in the ratio of the difference to the sum of the ionic mobilities. The agreement with experiment of these expressions for $V_2 - V_1$, and for the ordinary diffusion coefficient D of a solution as thus determined electrically, constitutes two distinct tests of the general validity of this diffusion scheme, and of the hypothesis of independent mobility of the ions of which it is a corollary.

In the case of a solution only partially dissociated, like that of acetic acid referred to in § 58, provided the time of association of two paired ions is on the average large compared with the time of relaxation of the system, these expressions for D and

$V_2 - V_1$ will still hold for the dissociated portion, if u and v denote the actual velocities of the ions when free, not the abnormally small effective velocities as determined by WHETHAM. Thus the total diffusion would now consist of this part belonging to the ionized portion, with coefficient independent of the degree of ionization, together with the actual diffusion of the non-ionized portion. On the same hypothesis the potential difference between the fluids would depend, as might have been foreseen, only on the actual concentrations of the ions in the two solutions, the amount of non-ionized substance being immaterial except in so far as it gives rise to an ordinary contact difference (§ 56): but it may not be computed from the abnormal ionic velocities by the ordinary formula unless the degree of ionization is independent of the concentration.

Critique of VON HELMHOLTZ'S *Theory of Electric Stresses: Electrostriction not due to Mechanical Forcive.*

64. A theory of electrostatic stress in dielectric media, based on the method of energy, and avoiding molecular theory, has been originated by KORTEWEG,* formulated in general terms by VON HELMHOLTZ,† and further developed by LORBERG, KIRCHHOFF,‡ HERTZ§ and others: it is desirable to examine the relation in which it stands to the views here set forth. The investigation of VON HELMHOLTZ postulates a dielectric medium which is effectively continuous, not molecular; also a potential function, that namely of the distribution of uncompensated polarity which represents the electric state of the medium, satisfying a characteristic equation, that of the FARADAY-MAXWELL theory. The energy per unit volume is expressed in terms of this potential, in such form that the variation of the integral which represents the energy for the whole volume leads, *on integration by parts*, to this characteristic equation as one of the conditions of internal equilibrium; the integral is then asserted to be in the normal form, which would mean, in our order of ideas, that it represents the actual distribution of the energy in the medium as well as its total amount. Its variation with sign changed, owing to change of material configuration, should then give the extraneous forcive that must be applied in order to maintain mechanical equilibrium; the variation with respect to the electrical configuration being null, so that electric internal equilibrium is provided for, by the characteristic equation already satisfied. The variation without change of sign should thus give the mechanical forcive of electric origin that acts on the medium. But the *data* do not even on these assumptions suffice to lead to a definite stress-system for the material; a certain geometrical stress-system is merely assumed which yields on the element of

* D. J. KORTEWEG, 'Wied. Ann.,' 9, 1880.

† H. VON HELMHOLTZ, 'Wied. Ann.,' 13, 1882: 'Abhandlungen,' I., p. 798.

‡ G. KIRCHHOFF, 'Wied. Ann.,' 24, 25, 1885: 'Abhandlungen,' 'Nachtrag,' p. 91.

§ H. HERTZ, 'Wied. Ann.,' 41, 1890: "Papers on Electric Waves," English edition, pp. 259–268.

volume a mechanical forcive the same as the one thus deduced from the energy function. An infinite number of such stress-systems might in fact be specified, for there are six components of stress which need satisfy only three conditions. If however the stress system is required to be symmetrical with respect to the lines of polarization, there is in this respect no indefiniteness (§ 40); and the one given by VON HELMHOLTZ is of this kind. Thus the definite result really deduced by VON HELMHOLTZ from his energy hypothesis is an expression for the bodily mechanical forcive in the polarized medium, the (X, Y, Z) of equation (4) of his memoir; while correlative formulæ are applied by him and by KIRCHHOFF for the bodily forcive in a magnetized medium. These expressions, however, definitely contradict the formulæ of MAXWELL and of the other previous writers for the bodily mechanical forcive in a magnetized medium, which are in general agreement with those developed in this paper: in fact VON HELMHOLTZ translates his formulæ into MAXWELL's electric stress system, while MAXWELL himself had to invent for the case of magnetic polarization, which was the one he considered, a different stress-system, namely his magnetic stress. As recent writers have in the main tacitly accepted VON HELMHOLTZ's procedure, it is incumbent on us to assign the origin of this discrepancy; and for this purpose a summary of his method is given, the effect of alteration of the coefficient of polarization arising from strain in the material being for the present left out of account.

65. The organized electric energy in the polarized medium being assumed, from other considerations, to be

$$W = \int K/8\pi \, . \, (dV^2/dx^2 + dV^2/dy^2 + dV^2/dz^2)\, d\tau,$$

where

$$d/dx\,(K\, dV/dx) + d/dy\,(K\, dV/dy) + d/dz\,(K\, dV/dz) + 4\pi\rho = 0,$$

in which ρ is a constant associated with the element of dielectric matter, called the density of its free electric charge, the forces acting will be derived from the variation of W; variation with respect to V leads to the electric forces, and that with respect to the material configuration leads to the mechanical ones. The problem is to determine the mechanical forces when there is electric equilibrium, that is when variation with respect to V yields a null result. The form of W above expressed does not lead to this null result; we can however by integration by parts derive the form $W = \frac{1}{2}\int V\rho\, d\tau$, the essence of this transformation being that in the new integral the distribution of the energy among the elements of volume $d\tau$ of the medium has been altered. This form does not satisfy the above requirement either, but by combining the two forms we obtain

$$W = \int \{\rho V - K/8\pi \, . \, (dV^2/dx^2 + dV^2/dy^2 + dV^2/dz^2)\}\, d\tau,$$

whose variation with respect to V is null as required: although as integration by parts is employed, the variation is *not* null for each single element of mass. This integral is then taken to represent the actual distribution of the organized energy in the medium when in electric equilibrium, and not merely its total amount: and variation of it with respect to the material configuration should on that hypothesis give the actual bodily distribution of mechanical forcive, not merely its statical resultant on the hypothesis that the system is absolutely rigid. Now in finding the variation of W arising from a virtual displacement (δx, δy, δz) of the polarized material, we have to respect the conditions that the free charge $\rho\delta\tau$ is merely displaced, so that by the equation of continuity $\delta\rho + d(\rho\delta x)/dx + d(\rho\delta y)/dy + d(\rho\delta z)/dz = 0$, and also that each element of the material is moved on with its own K, so that $\delta K + dK/dx \, . \, \delta x + dK/dy \, . \, \delta y + dK/dz \, . \, \delta z = 0$; while things have been arranged so that a variation of V produces no result,—but only however no aggregate result on integration by parts. Unless the transitions at interfaces are supposed to be gradual, and the integration then to extend throughout all space, there will also be direct surface terms in the variation, because the virtual shift of the material leaves a space unoccupied on one side and occupies a new space on the other; thus finally by the ordinary process of integration by parts we obtain for any region

$$\delta W = \iiint \left[\left\{ \rho \frac{dV}{dx} + \frac{1}{8\pi} \frac{dK}{dx} \left(\frac{dV^2}{dx^2} + \frac{dV^2}{dy^2} + \frac{dV^2}{dz^2} \right) \right\} \delta x + \ldots . + \ldots . \right] \delta\tau$$
$$- \int \frac{K}{8\pi} \left(\frac{dV^2}{dx^2} + \frac{dV^2}{dy^2} + \frac{dV^2}{dz^2} \right) \delta n \, dS.$$

The coefficient of δx with sign changed has been taken to be the component of the bodily mechanical forcive. But to obtain the total mechanical forcive acting on an element we must retain all the terms in the variation that belong to it, so that it is illegitimate in this connexion to transmit a traction from it to the boundary of the medium by the process of integration by parts. If then we consider the single element of volume by itself, so that in the formula δS is an element of its surface, the forcive on it will be VON HELMHOLTZ's one together with a hydrostatic pressure $- K/8\pi \, . \, (dV^2/dx^2 + dV^2/dy^2 + dV^2/dz^2)$ acting over its surface; and this complete specification would agree with our previous results, *except* that we have $K - 1$ in place of K for reasons already assigned. But it would seem that the method thus described must be radically unsound; it would be valid if there were only one medium under consideration, of which W is the energy function: but there is here, in the same space, the æther with its stress and the polarized matter with its reacting mechanical forces, and (§ 6) there is no means of disentangling from a single energy function in this way the portions of energy which are associated with these different effects.

66. There are also subsidiary terms in VON HELMHOLTZ's formulæ, involving the rate of alteration of the inductive capacity of the fluid dielectric by compression, terms which are extended in the work of KORTEWEG, LORBERG, and KIRCHHOFF to include the alterations of the inductive capacity of a solid dielectric produced by the

various types of strain that it can sustain. Their *rationale* is best seen by the more elementary procedure of KORTEWEG, who first introduced them. He considered the following cycle; (i) move up a piece of the dielectric material from an infinite distance into an electric field, (ii) strain it and so alter its inductive capacity and therefore the electric energy, (iii) move it back to an infinite distance in the strained state, (iv) restore it to its original condition by removing the strain. In order to evade perpetual motions, the mechanical work done by electric attractions as it approaches must exceed the work absorbed as it recedes, by the loss of available electrical energy due to strain; and this leads KORTEWEG to terms in the mechanical forcive which depend on the rate of variation of inductive capacity with strain. The process is analytically developed for fluids by VON HELMHOLTZ, by adding on to δK, the variation of K, a part arising from the compression of the material which the virtual displacement involves, namely by adding $-dK/d \log s . (d\,\delta x/dx + d\,\delta y/dy + d\,\delta z/dz)$, where s denotes the material density: and KIRCHHOFF formulates it for isotropic solid media, replacing $dK/d \log s$ by KORTEWEG's two coefficients which express the actual rates of change of K due to elongations along the line of polarization and at right angles to it. But here again a process of integration by parts comes in, which removes part of the bodily forcive from the element of volume at which it is directly applied to the boundary, and so vitiates the result regarded as a specification of the forcive which produces the actual mechanical strain in the material.

67. Moreover, phenomena of this latter kind are more appropriately investigated as intrinsic changes of the equilibrium configuration of the material arising from molecular actions produced by the polarization, the forcive of the above argument being simply what would be originated if these changes were prevented by constraint. Such deformations of the elements of volume of the material, the result of electro-striction or magnetostriction, may not fit in with each other, and the strain thence arising will originate secondary mechanical stresses: but it appears preferable to keep these distinct from the regular stress which is the effect of the *direct* electric or magnetic action of different finite portions of the material on each other.

This separate procedure may be illustrated by an investigation of the change of intrinsic length of a bar of magnetic material, caused by its introduction into a magnetic field. Clamp the bar to its natural length when at a great distance; then introduce it into the magnetic field so as to lie along the lines of force; then unclamp in such way that it may do as much work as possible in pushing away resistances to its magnetic elongation; finally remove the unclamped bar again to a great distance. If this cycle is performed at a uniform temperature, it follows from CARNOT's principle that there can be no resultant work done in it. Now the work done by the magnetic forces in introducing the bar is $\int I\, dH$, that is $\int (\kappa + Q\, d\kappa/dQ + I\, d\kappa/dI)\, H\, dH$, per unit volume, where κ is the magnetic susceptibility which is presumably a function of the internal longitudinal pressure Q in the bar and of its intensity of magnetization I. The work done in unclamping is $\frac{1}{2}Q_1 l_1$ per unit volume, where l_1 is the intrinsic magnetic elongation and Q_1 the pressure corresponding to the strength H_1 of the part of the field in which it is unclamped. This is on the supposition that the bar is long, so that there are no free magnetic poles near together which would diminish Q by their mutual attraction. The work done per unit volume by the magnetic forces during the removal of the bar is $-\int(\kappa + \mathrm{I}\, d\kappa/d\mathrm{I})\,\mathrm{H}\,d\mathrm{H}$. The resultant work in the cycle being null, we have $d\kappa/dQ\,.\int QH\,dH = -\frac{1}{2}Q_1 l_1 = -\frac{1}{2}Q_1^2/M$, where M is YOUNG's elastic modulus. This can only be satisfied if Q is of the form λH^2, where λ is a constant, and it then gives $d\kappa/dQ = -2\lambda/M$, and the elongation l is $-\frac{1}{2}d\kappa/dQ\,.\,H^2$, that is $\frac{1}{2}\,d\kappa^{-1}/dQ\,.\,I^2$, or $-\frac{1}{2}\,d\log\kappa/dQ\,.\,HI$; while the corresponding stress Q is $-\frac{1}{2}d\kappa/dl\,.\,H^2$.* This result is of course valid only in the absence of hysteresis. A similar process applies where the field is transverse to the bar; and thus KIRCHHOFF's complete results may be obtained. A more complete enumeration of possible physical changes would also take cognizance of alteration of the elastic constants of the material due to the magnetic excitation; but this cause (*cf.* § 83) will not add terms of the first order to the energy-changes unless the bar is under extraneous stress, not merely constraint, while the cycle is being performed.

For dielectrics, direct experiments have not found any sensible dependence of inductive capacity on the pressure in the case of liquids; while the experimental discrepancies,† which these terms were introduced by VON HELMHOLTZ to reconcile, have since been cleared up.

68. In the paper above referred to, KIRCHHOFF remarks (§ 3) that an expression for the traction across an ideal interface in a uniform polarized medium might be arrived at by supposing a very thin film of air introduced along the interface, and computing the attraction between the two layers of opposed poles thus separated, a process which had been employed by BOLTZMANN. He concludes that this process must be at fault, on the ground that the specification of stress thus obtained does not satisfy a necessary property of mechanical stress-systems, namely that the tractions exerted over the surface of an infinitesimal element of volume of any form must balance each other; and he gives this as the reason for having to fall back on an energy-method in order to obtain a specification free from objection. The preceding considerations (§§ 44–48) indicate the direct reason of the illegitimacy of that process, while they also exhibit the logical basis of the application of the method of mechanical energy in problems of molecular physics.

* [In these differentiations I is constant; see § 83.]

† Namely, the differences in the values of K at first found by QUINCKE, by use of three different experimental methods (§ 78), which it is easy to see would on the usual theory involve perpetual motions.

Conservation of Energy in the Electric Field: Limited Validity of POYNTING'S *Principle.*

69. It has been explained (§ 6) that the agencies in an electric field may be in part traced by transmission through the æther after the manner of ordinary mechanical stress, and in part, namely as regards forces on the electrons, not so traced. As regards the former part, therefore, the increase of energy in any region must be expressible explicitly as a surface integral, representing work done by tractions exerted over its boundary. This theorem will thus have application in all cases in which the configuration of the electrons is not changing; for its strict application, the bodies inside the region which carry currents or electric charges or are polarized, must thus be at rest, and there must be no change in the state of electrification of any conductor in the region. Recurring for an illustration to the simpler circumstances of a perfect fluid containing vortex rings, it is easy to show analytically that the rate of increase of energy in any region is there expressible as a surface integral, involving the velocity and the pressure, only when there are no vortex rings in the region or when the rings in it are all supposed to be held fixed by constraint.* This illustration also emphasizes the point that the surface integral must be taken as a whole, that an element of it does not necessarily represent the activity across the corresponding portion of the surface.

Thus taking the material system, concerning which we need make no hypothesis as regards inductive quality or æolotropy, to be *at rest* in the electric field so that there are no changes of energy due to the mechanical forcives, and neglecting those due to convection currents which rearrange electrifications, if W and T denote the organized potential and kinetic electric energies in the region, and D the rate of dissipation of organized energy due to currents of conduction, $dW/dt + dT/dt + D$ must be expressible as a surface integral. Now W is made up of the energy of æthereal strain $(8\pi)^{-1}\int(P^2 + Q^2 + R^2)\,d\tau$, and that of material polarization, $\int\phi\,d\tau$ where $\phi = \int(P\,df' + Q\,dg' + R\,dh')$ which must be an exact differential when there is no dielectric hysteresis; thus in all $dW/dt = \int(P\,df''/dt + Q\,dg''/dt + R\,dh''/dt)\,d\tau$. Again the rate of dissipation arising from ionic migration in the conducting circuits is $D = \int\{P(u - df''/dt) + Q(v - dg''/dt) + R(w - dh''/dt)\}\,d\tau$. Hence we must have $dT/dt = dE/dt - \int(Pu + Qv + Rw)\,d\tau$, in which dE/dt is a surface integral; and this equation will give an *a priori* indication, independent of special hypothesis, of the distribution of organized kinetic energy in the medium, that of the potential

* The reason is simply that the form of the contained vortex rings is not a function merely of the state of the fluid inside the surface, but also in part determines the simultaneous velocity of the fluid throughout all space. So also the energy associated with each atom of matter is really distributed throughout the whole æther, and therefore the energy-changes associated with changes in the configuration of matter cannot be represented as propagated step by step across the æther.

energy and the dissipation being supposed known. Substituting from the kinematic relation $4\pi u = d\gamma/dy - d\beta/dz$, and integrating by parts,

$$dT/dt = dE/dt - (4\pi)^{-1}\int\{l(\beta R - \gamma Q) + m(\gamma P - \alpha R) + n(\alpha Q - \beta P)\}\, dS$$
$$+ (4\pi)^{-1}\int(\alpha\, da/dt + \beta\, db/dt + \gamma\, dc/dt)\, d\tau.$$

This equation of energy can however only apply to the case in which the energy of magnetic, as well as of electric, polarization is completely organized, and not mixed up with other molecular energy of the material, as it would be if there were hysteresis or permanent magnetism. When this condition is satisfied, the negation of perpetual motion requires that $\alpha\, da + \beta\, db + \gamma\, dc$ shall be an exact differential, say $d\psi$: thus we may tentatively assume $T = (4\pi)^{-1}\int\psi\, d\tau$, when the surface integral will remain as the value of dE/dt. In the case usually considered, in which the law of induced magnetization is linear, this gives MAXWELL's formula for the distribution of the energy, $T = (8\pi)^{-1}\int(a\alpha + b\beta + c\gamma)\, d\tau$; while the value of dE/dt expresses POYNTING's law of flux of electric energy corresponding to that hypothesis.

70. That this law of distribution of electrokinetic æthereal energy, for a magnetic medium of constant permeability, falls in with the present scheme may be verified as follows. Let $(\alpha', \beta', \gamma')$ be proportional to the velocity of the irrotational flow of the æther, due in part when there is magnetism to the AMPEREAN æthereal vortices, in such wise that the total kinetic energy is $(8\pi)^{-1}\int(\alpha'^2 + \beta'^2 + \gamma'^2)\, d\tau$; this is equal to $(8\pi)^{-1}\{\int\nu dV/dn\, dS - \Sigma\mathfrak{k}\int dV/dn d\sigma\}$, where $\delta\sigma$ is an element of a barrier surface closing a magnetic vortex of strength $\mathfrak{k}$, and δS is an element of the outer boundary of the region under consideration. As T is to include only the organized energy, it is given by this expression when in it V is restricted to be the potential of the magnetic force as ordinarily defined. In that case for an element of volume $\delta\tau$,

$$\Sigma\int\mathfrak{k} dV/dn d\sigma = \Sigma(\mathfrak{k}l dV/dx + \mathfrak{k}m dV/dy + \mathfrak{k}n dV/dz)\sigma = -4\pi(A\alpha + B\beta + C\gamma)\, d\tau;$$

and therefore

$$T = (8\pi)^{-1}\int(\alpha^2 + \beta^2 + \gamma^2)\, d\tau + \tfrac{1}{2}\int(A\alpha + B\beta + C\gamma)\, d\tau = (8\pi)^{-1}\int(a\alpha + b\beta + c\gamma)\, d\tau.$$

But although this expression locates the energy correctly as regards distribution throughout space, it still ignores the essential distinction between the energy of the translatory motions of electrons which constitute the current and that of their orbital motions which involve the magnetism; in a complete and consistent theory these two parts must be kept separate; *cf.* foot-note, § 38 *supra*.

On the Nature of Paramagnetism and Diamagnetism, as indicated by their Temperature Relations.

71. As a result of an extensive investigation of the magnetic properties of matter, the law has recently been formulated by CURIE* that in all feebly paramagnetic sub-

* P. CURIE, 'Annales de Chimie,' 1895.

stances, including gases, the coefficient of magnetization varies inversely as the absolute temperature, with a degree of accuracy which tends to perfection at high temperatures : that in strongly magnetic substances such as iron, nickel, and magnetite, the same law is ultimately reached when the temperature is sufficiently high : while in diamagnetic substances the coefficient is usually nearly independent of temperature and also of changes in the chemical state of the material. The inference is made by CURIE that this points to diamagnetism being an affair of the internal constitution of the molecule, having only slight relation to the bodily motions of the molecules on which temperature depends; which is in accordance with the modified WEBERIAN view necessitated by the present theory. On the other hand, paramagnetization is an affair of orientation of the molecules in space without change of internal conformation, so that alteration of the mean state of translational motion is involved in it, and we should expect a temperature effect. A striking and probably just analogy is drawn by CURIE between (i) the simple law of expansion of a gaseous substance at high temperature, and the sudden change which it undergoes on lowering the temperature beyond a critical point so that the mutual attractions of the molecules come into play and produce the liquid state, and (ii) the simple law of magnetization of a substance like iron or nickel at high temperatures, and the sudden change which it undergoes when the temperature is lowered beyond the point at which the material passes into its strongly magnetic or ferromagnetic condition. The relation between paramagnetization and temperature in the former state proves to be so simple and universal that it must be the expression of a theoretical principle. The following considerations in fact derive it from CARNOT's principle : the argument is precise so long as the induced magnetization is so slight that the exciting magnetic force on the separate molecules is practically that of the inducing field, but it loses exactness as soon as, owing to diminution of energy of agitation with falling temperature, the molecules begin to exercise sensible magnetic control over each other, and thus introduce the phenomena of grouping and consequent hysteresis that are associated with the ferromagnetic state.

72. Consider a mass of paramagnetic material, moved up from a place where the intensity of the magnetic field vanishes to a place where it is H. The aggregate per unit volume of the total magnetic energies of its molecules is thereby altered from null to $-$ IH or $-\kappa H^2$. The mechanical work done by the mass in virtue of its attraction by the field is $\frac{1}{2}$IH, for the magnetization is at each stage of its progress proportional to the inducing force. Thus there remains a loss in the total magnetic energy of the molecules, equal to $\frac{1}{2}$IH ; this can only have passed into heat in the material ; for we can work on the hypothesis that the field of force H is due to an absolutely permanent magnetic system, so that no energy is used up in producing magnetic displacements in the inducing magnets. Now let us apply CARNOT's principle to a reversible cycle in which the material is moved up into the field at temperature $T + \delta T$ and removed at temperature T, with adiabatic transition

between these temperatures. Let $h + \delta h$ be the thermal energy per unit volume which it must receive from without at the higher temperature, and h that which it must return at the lower, in order to perform the amount of work δW, equal to $\frac{1}{2} H^2 d\kappa/dT.\delta T$, in the cycle; then, by CARNOT's principle, $\delta W/\delta T = h/T$; and $h = -\frac{1}{2}\kappa H^2$ as above; so that $d\kappa/dT = -\kappa/T$, leading to $\kappa = A/T$ which is CURIE's law. Conversely, assuming CURIE's law we can deduce that in paramagnetic bodies magnetization consists in orientation of the molecules without sensible change in their internal energies. In an analytical form the argument will then run as follows: $dh = MdI + NdT$, and $dE = dh - \kappa^{-1}IdI$; whence by the thermodynamic formula $M/T = d/dT.\kappa^{-1}I$, so that $M/I = Td\kappa^{-1}/dT, = \kappa^{-1}$ by CURIE's law; hence $dh = HdI + NdT$, so that at constant temperature $h = \frac{1}{2}HI$, that is the heat that the material developes during magnetization is the equivalent of the magnetic energy that is not used up in mechanical work. This is precisely what we should expect if the material is a gas; for there is then no internal work by which this energy could be used up, and the magnetization arises from the effort of the magnetic field to orientate the molecules which are spinning about as the result of the gaseous encounters. The law of CURIE thus indicates that the same is sensibly true for all paramagnetic media at high temperatures: at lower temperatures they gradually pass into the ferromagnetic condition. It is the magnetization, so to speak, of an ideal perfect ferromagnetic, in which the controlling force that resists the orientating action of the field is practically wholly derived from the magnetic interaction of the neighbouring molecules, which for this purpose form elastic systems, that is illustrated by EWING's well-known model, which so clearly represents the hysteresis accompanying ferromagnetic excitation. In ordinary paramagnetic substances this mutual magnetic control of the molecules is insensible compared with the control due to other molecular causes; and our conclusion is that these causes are such that the magnetic energy expended in working against them is transformed into heat energy, not into internal energy of any regular elastic type.

But we have not taken account of the fact that the molecules of every substance are subject to both paramagnetic and diamagnetic influence, of which one or the other preponderates. The theoretical law should thus be $\kappa = -B + AT^{-1}$ or $\kappa T = A - BT$; so that in a diagram of the relation between κT and T each substance would be represented by a straight line. In paramagnetics the line should slope slightly down towards the axis: for diamagnetics it should pass not through the zero of temperature but on the positive side of it. According to CURIE, his experimental results are equally well represented by this formula, on account of the preponderant influence of the paramagnetism.

Similarly, should it turn out that for weakly electric media such as gases, $(K-1)/\rho$ is independent of the temperature, it would follow that the electric polarization is mainly an affair of change of internal constitution of the molecules: while were the polarization mainly an affair of molecular orientation, $(K-1)/\rho$ would vary inversely as the absolute temperature: in intermediate cases it would not vary so rapidly as this. The circumstance that in gaseous media and some others, the dielectric constant is exactly equal to the square of the refractive index, favours the former alternative (§ 21).

Mechanical Relations of Radiation reconsidered.

73. The results given in Part II., §§ 28–9, as to the mechanical forcive exerted on a material medium by a stream of radiation passing across it, require amendment in the light of these principles, of which they also form an apt illustration. Consider, as there, two media separated by the plane of yz, and a system of plane-polarized waves advancing across them, with their fronts parallel to that plane, the electric vibration parallel to the axis of y, and the magnetic one parallel to the axis of z; we may generalize by taking K and μ to be in each medium functions of x. The electrical equations are

$$4\pi v = -\frac{d\gamma}{dx}, \qquad \frac{dQ}{dx} = -\frac{dc}{dt}, \qquad v = \sigma Q + \frac{K}{4\pi} C^{-2} \frac{dQ}{dt}.$$

Applying the formulæ found above (§ 38), the force acting on the electric polarity comes out to be null, while the electromagnetic bodily force is, per unit volume, $X = v'\gamma$, being wholly parallel to the axis of x: its periodic part has double the frequency of the radiation. Now there is no mechanical elasticity associated with matter which is powerful enough to transmit in any degree the alternating phases of forcives connected with a phenomenon which travels so fast and with such short wave-length as radiation, long HERTZIAN waves being excluded. Consequently when X is wholly alternating it is not transmitted by material stress at all; and it is only when its value for each element of the medium contains a non-alternating part that we can have a material forcive. When the media are perfectly transparent, and are traversed by a steady train of waves, there is therefore no transmissible material forcive either on surfaces of transition or anywhere else, and the $\int X\,dx$ previously calculated has no relation to material stress. But if we consider a stream of radiation passing across a transparent medium into an opaque one, and for simplicity take the latter to be homogeneous so that for the transmitted waves $c = c_0 e^{-px} \cos(nt - qx)$, the expression for X, viz: $\left(v - \frac{1}{4\pi} C^{-2} \frac{dQ}{dt}\right)\gamma$, contains a non-periodic term $\left(1 - \frac{\mu n^2 C^{-2}}{p^2 + q^2}\right) \frac{pc_0^2}{8\pi\mu^2} e^{-2px}$. This when integrated over the medium gives a pressure on the opaque medium of intensity $\left(1 - \frac{\mu n^2 C^{-2}}{p^2 + q^2}\right) \frac{CqE}{2\mu n}$, where E is the energy per unit volume of the incident radiation absorbed. Unless the opacity is so great that the intensity of the light is diminished in the ratio e^{-1} in penetrating a few wave-lengths, that is when p is negligible compared with q,* this pressure will be

* This will not usually be the case for metallic media.

The sign of this mechanical force may be negative in a region of intense absorption.

practically $(1 - \mu m^{-2}) m\mathrm{E}/2\mu$, where m is the real part of the index of refraction of the medium, as measured by the ratio of the velocities in deviation experiments with prisms. And in general it appears that it is only absorption, not reflexion, of radiation that is accompanied by a mechanical forcive, the force on any absorbing mass being $(1 - \mu m^{-2}) m\mathrm{E}'/2\mu c$ in the direction of the radiation traversing it, where E' is the total radiant energy absorbed by the mass per unit time.

In the case of a transparent medium traversed by two systems of waves, direct and reflected, forming stationary undulations, the mechanical force is proportional to $\sin 2qx$, and vanishes at both the nodes and antinodes of the electric vibration.

74. The *rationale* of the mechanical forcive is that, owing to the absorption of energy, which can only occur when phase-differences exist, a difference of phase becomes established between the two factors of X, the electric current and the magnetic field, so that their product contains a non-alternating part. It is known that vapours of complex chemical constitution are very powerful absorbers of radiation; and in this case (if not in all cases) the absorption must be a property of the single molecule. By the argument just stated, there must then be difference of phase between the electric flux (displacement of electrons) in the molecule and the magnetic field, and this will give a mechanical forcive driving the molecule along the path of the radiation. As the tails of comets and the Solar Corona consist of very rare distributions of vaporous or other matter, in free space which exerts no retarding influence, a comparatively small absolute amount of absorption by them of the Solar radiation might account for their observed repulsion from the Sun; in this way a definite and actually existing physical agency may be made to take the place of vague electrical repulsion in BREDICHIN's important analysis of cometary appendages.

This mechanical action of waves on absorbing systems placed in their path may be roughly illustrated by an arrangement in which a system of sound waves traverses a space filled with resonators approximately in unison with them. The open mouth of each resonator is repelled,* so that in case there is any regularity in their orientation, the system as a whole will be subject to mechanical force. The resonators might be suspended so that the mechanical forces may themselves produce this orientation, and thus form a sort of medium polarizable by waves. A corresponding electric illustration is the action of long HERTZIAN waves in orientating and repelling mobile conducting circuits which lie in their path. The very considerable repulsion of the vanes in the radiometer arises of course from a mutual stress between the vanes and walls and the rarefied gas, and so has a null resultant as regards the system as a whole.

Stresses and Deformations in Electric Condensers.

75. The elastic deformation produced in the dielectric of a spherical condenser by

* *Cf.* Lord RAYLEIGH, 'Theory of Sound,' vol. 2, §§ 255*a*, 319.

the mechanical forcive may be readily calculated. If u denote the radial displacement, the normal and transverse principal tractions at any point in the spherical dielectric shell are

$$P = \lambda\left(\frac{du}{dr} + 2\frac{u}{r}\right) + 2\mu\frac{du}{dr}, \quad Q = \lambda\left(\frac{du}{dr} + 2\frac{u}{r}\right) + 2\mu\frac{u}{r},$$

where μ, $\lambda + \frac{2}{3}\mu$ are the moduli of rigidity and compressibility of the material. The electric force at any point is kr^{-2}, where kK is the charge on a coating: hence (§ 36) the mechanical bodily force is derived from the potential $-(K-1)/8\pi \,.\, k^2 r^{-4}$; and there is also an outward normal traction over each coating equal to $-K/8\pi \,.\, k^2 r^{-4}$. The equation of equilibrium of a conical element of volume is

$$\frac{d}{dr}(Pr^2) - 2Qr = \frac{K-1}{2\pi}\frac{k^2}{r^3}, \text{ so that } \frac{d}{dr}\left(r^2\frac{du}{dr}\right) - 2u = \frac{K-1}{2\pi(\lambda+2\mu)}\frac{k^2}{r^3},$$

giving

$$u = Ar + \frac{B}{r^2} + \frac{K-1}{\lambda+2\mu}\frac{k^2}{8\pi r^3},$$

and therefore

$$P = (3\lambda+2\mu)A - \frac{4\mu}{r^3}B - \frac{\lambda+6\mu}{\lambda+2\mu}\frac{(K-1)k^2}{8\pi r^4}, \quad Q = (3\lambda+2\mu)A + \frac{2\mu}{r^3}B - \frac{\lambda-2\mu}{\lambda+2\mu}\frac{(K-1)k^2}{8\pi r^4}.$$

The values of A and B are determined by the normal tractions at the coatings $r = r_0$ and $r = r_1$, and are, when the coatings are wholly supported by the dielectric,

$$A = \frac{1}{r_1 r_2(r_1^2 + r_1 r_2 + r_2^2)}\frac{\vartheta k^2}{8\pi(3\lambda+2\mu)}, \quad B = \frac{r_1^3 + r_1^2 r_2 + r_1 r_2^2 + r_2^3}{r_1 r_2(r_1^2 + r_1 r_3 + r_2^2)}\frac{\vartheta k^2}{32\pi\mu},$$

where ϑ represents $1 - (K-1) \,.\, 4\mu/(\lambda+2\mu)$.

It will suffice to state the results for the case of a thin shell of radius a with adhering coatings: then $A = \vartheta k^2/24\pi(3\lambda+2\mu)a^4$, $B = \vartheta k^2/24\pi\mu a$.

The coefficient of expansion of the radius of the sphere, due to the electric stress, is $u/a = (\lambda + 2\mu - \mu K)/8\pi\mu(3\lambda+2\mu) \,.\, F^2$, and the coefficient of expansion of the volume of the sphere is three times this. It is easily verified that when the shell is thin the stress in the material of the dielectric is made up of a pressure $KF^2/8\pi$ normal to the shell combined with a pressure $(K-2)F^2/8\pi$ in all directions tangential to it.*

[76†. The circumstance that these results are independent of the radius of the sphere suggests an extension of their scope. Whatever be the form of the dielectric shell provided it is of uniform thickness, F will be the same all over it; and the mechanical force acting on its substance, being derived from a potential $-(K-1)F^2/8\pi$, will be directed at each point along the normal δn to the shell. Consider the internal equilibrium of an element of volume $\delta S\,\delta n$, of which the opposite faces δS are elements of level surfaces bounded by lines of curvature for which R_1, R_2 are the principal radii: it will be maintained if a pressure of intensity P,

* This naturally differs from KIRCHHOFF's result, 'Wied. Ann.' 24, p. 52, § 4.

† Rewritten December 2.

equal to $KF^2/8\pi$, act on the element across the faces δS, and another pressure $-Q$ act on it, which is the same across all perpendicular faces. For, resolving the forces along δn, we must have for equilibrium

$$-\delta n \frac{d}{dn}(P\,\delta S) - Q\,\delta n\,\delta S\left(\frac{1}{R_1} + \frac{1}{R_2}\right) + \frac{K-1}{4\pi} F \frac{dF}{dn}\,\delta S\,\delta n = 0.$$

Now, by the constancy of the induction, we have $d/dn\,(F\,\delta S) = 0$, leading to $\frac{dF}{dn} = -F\left(\frac{1}{R_1} + \frac{1}{R_2}\right)$ and also to $\frac{d}{dn}(P\,\delta S) = \frac{K}{8\pi}\,\delta S F \frac{dF}{dn}$: thus, on substitution, we obtain $-Q = (K-2)\,F^2/8\pi$. The constancy of Q all round the edge of a flat element of volume $\delta S\,\delta n$ secures the balancing of the tangential components of the forces. Hence the mechanical stress in any condenser sheet of uniform thickness is the same as has been found above for the spherical case. If e and f denote the elongations of the material in the normal and tangential directions, $\lambda(e + 2f) + 2\mu e = -KF^2/8\pi$, $\lambda(e + 2f) + 2\mu f = -(K-2)\,F^2/8\pi$; hence $f = (2 + \lambda/\mu - K)/(3\lambda + 2\mu)\,.\,F^2/8\pi$, $e = (2\lambda/\mu - K)/(3\lambda + 2\mu)\,.\,F^2/8\pi$; so that the extension of the volume of the shell and the change in its thickness are the same as were found above for the spherical case. If the shell is an open one, the presence of its free edge will disturb these relations: but that influence will be mainly local, as the forces introduced by the edge will be almost wholly of the nature of local action and reaction.]

Various Practical Illustrations and Applications of the Stress Theory.

77. *Refraction of a Uniform Field of Electric Force.*—An arrangement by which these principles may be precisely verified is that of the refraction, at a plane interface AB, of a sheaf of parallel lines of electric force F, according to the FARADAY-MAXWELL law of tangents, $\tan\iota_1/\tan\iota_2 = K_1/K_2$, $F_1/F_2 = \operatorname{cosec}\iota_1/\operatorname{cosec}\iota_2$. This configuration of lines of force may be obtained and fixed by means of a condensing system having its plates P_1Q_1 and P_2Q_2 normal to the incident and refracted lines. Each plate may be protected from convective discharge into the fluid dielectric by a covering plate of glass or mica, which will itself produce no refraction. In such a case, when both the dielectric media are fluid, the total mechanical result of the electric excitation will be the same as that of normal tractions on the interface between them, of intensity $-2\pi n'^2 - \frac{1}{2}i'F$ towards each side, that is, in all $(K_2 - K_1)\,(2\pi n''^2/K_1K_2 - T^2/8\pi)$ towards the side 1. As the field of force in this condensing system is uniform except near its edge, the interface will simply be lifted up between the plates by the amount which corresponds to this traction, without ceasing to be horizontal. Thus the common surface will be elevated when $\tan\iota_1$ is less than $(K_1/K_2)^{\frac{1}{2}}$, while at greater incidences it will be depressed.

This principle supplies in fact a method of obtaining the inductive capacities of fluid media by angular measurement only, without the aid of an electrometer. When the condenser P_1Q_1, P_2Q_2 is charged, the interface between the fluids will usually cease to be horizontal; the upper plate P_1Q_1 is then to be rotated until horizontality is again obtained, as may be tested very exactly by reflexion of a beam of light; then the ratio of the tangents of the inclinations of the plates will be that of the inductive capacities of the media. The method would also apply to solids, as we might employ a prism of the material, over a horizontal face of which a sheet of a fluid dielectric is spread, and observe the deviations from horizontality of the upper surface of this sheet. An equivalent arrangement has been actually employed for solids by PÉROT,* who however adjusted the plates to uniformity of the electric field by the electric test that translation of a small piece of solid dielectric in the field between them should not affect the capacity of the condenser.

78. *Experiments on Electric Traction and Change of Pressure in Fluids.*—The direct experimental examination of the material forcives of polarized media is necessarily confined to fluids, for in the case of solids the strains produced by them could hardly be disentangled from the intrinsic changes of configuration due directly to the polarization. The field for fluids has been very fully explored by QUINCKE.† The inductive capacity of the fluid dielectric of a horizontal condenser was first determined by direct electrical measurement. The attraction between the plates was then weighed. Then, using a wide cylindrical air-bubble extending across the space between the plates, and connected through an aperture in the upper one with a manometer, the increase of air-pressure in the bubble produced by charging the condenser was measured. As half-way between the plates the capillary interface between air and liquid lies along the lines of force, there is by the previous formulæ (§ 37) no true surface traction on that part of the interface; so that the indication of the manometer would give exactly the change of pressure in the liquid due to the electric excitation, were it not that the different electric conditions over other parts of the interface change the value of its curvature and so introduce a capillary change of pressure. Finally, employing a flat bubble of air resting against the upper plate alone, and maintaining the pressure in it constant, the change of curvature of its lowest part produced by the electrical excitation was measured by the optical method; the surface tension operating through this change of curvature must balance exactly the direct traction on the surface and the change of pressure in the liquid below it. To compute these, we notice that the line of force through the middle of the bubble

* PÉROT, 'Comptes Rendus,' 1891; quoted by DRUDE, 'Physik des Æthers,' p. 299. The conjugate condensing system, in which namely the lines of force and the lines of equal potential are interchanged, so that the plates are now bent according to the law of tangents where they cross the surface of the liquid, has recently been brought into requisition by PELLAT ('Annales de Chimie,' 1895), in order to derive the law of the traction on a dielectric interface from the expression for the energy of the dielectric system. As in these cases the field of force is uniform in both media, the bodily part of the mechanical force vanishes, and the interfacial traction proper thus constitutes the whole forcive: but that would not generally be so.

† G. QUINCKE, 'Wied. Ann.,' 19, 1883; or an abstract in the paper below cited, 'Roy. Soc. Proc.,' 52, 1892, pp. 59–62.

is straight, so that if F denote the electric force in air and F/K that in the liquid, the traction on the surface due to these two causes is $(K-1)^2 F^2/8\pi K^2 + (K-1) F^2/8\pi K^2$, that is $(K-1) F^2/8\pi K$ upwards, which is the formula employed by QUINCKE; while F is determined by the relation $aF + bF/K = V$, provided the bubble is so broad that the middle tube of induction is practically cylindrical, a and b being the lengths of this tube that are in air and the liquid, and V the difference of potential between the plates. After an error in the direct determination of K, due to an experimental oversight whose existence was suggested by HOPKINSON, had been corrected, all the results showed substantial agreement, for thirteen liquids that were examined, with these theoretical formulæ. But the agreement was not quite complete; a subsequent examination* still showed that the attraction between the plates always came out less and the change of pressure in the liquids greater than the formulæ would give, though these discrepancies were within the limits of experimental error, except for the case of rape oil in which they amounted to as much as ten per cent. The neglect of the capillary correction above mentioned would account for a discrepancy in the same direction as the first; and the irregularity of the electric distribution near the edge of the plates would account for one in the same direction as the second.

In a paper on the bearing of the phenomena of electric stress on electrodynamic theory,† I had previously been led to inferences militating against the possibility of dielectric polarization being of molecular type, from a comparison of these experimental results with an electric traction formula including both the molar and the molecular forcives. According to the present argument (§ 44), the latter forcive being separately compensated, the difficulty there encountered does not exist. The remaining considerations in that paper retain their validity; they show for instance that the formulæ for the experimental reductions can be derived from a knowledge of the distribution of the organized energy alone. But in the light of the present views, we are no longer restricted or even allowed to consider the induction in a dielectric as all of one kind; the total circuital induction is in fact made up of a material polarity combined with an æthereal elastic displacement, giving an apparent but natural complexity which it had previously been an aim to evade.

79. *Experiments on Electric Expansion in Solids and Fluids.*—The results obtained in § 76 may be applied to the discussion of a very thorough series of experiments on electric expansion, made by QUINCKE,‡ which appear hitherto not to have been correctly interpreted. Following the early experiments of FONTANA, and more recent ones by GOVI and DUTER, a condenser of the form of a glass thermometer-bulb was used, and the expansion of volume arising from electric excitation was read directly on its tube. By employing a long cylindrical bulb, the

* G. QUINCKE; 'Wied. Ann.,' 32, 1887, p. 537.

† 'Roy. Soc. Proc.,' 52, 1892, pp. 65–6.

‡ Abstracted in 'Sitz. Akad. Berlin,' February, 1880, and 'Phil. Mag.,' July, 1880, pp. 30–39: in full in 'Wied. Ann.,' 10, 1880, pp. 161–202, and 513–553.

longitudinal expansion of the glass could also be measured microscopically at the same time. It was found by QUINCKE that the coefficient of volume expansion was always about three times that of this longitudinal expansion of the glass dielectric, just as the above theory indicates for the elastic strain in a bulb of uniform thickness. The erroneous deduction was however made from an imperfect theory, that electric expansion of solids is uniform in *all* directions, like expansion by heat, and so in no part due to mechanical forces of attraction.

By using the formula of § 76 along with various known physical constants, a test of the order of magnitude of QUINCKE's determinations may be obtained. Thus with a striking distance of ·4 centim. between brass balls 2 centims. in diameter, the expansion in volume of a flint glass condenser of thickness ·06 centim. was found to be $\frac{3}{4} \times 10^{-6}$.* According to BAILLE's experiments† this striking distance corresponds to a difference of potential of 47 c.g.s. The expansion of volume of the bulb due to the mechanical force is $\{2 - K + \lambda/\mu\}/(\lambda + \frac{2}{3}\mu).F^2/8\pi$, where, according to EVERETT's experiments‡ for flint glass, $\mu = 25 \times 10^{10}$ c.g.s. and $\lambda = \mu$, and according to HOPKINSON K is about 7. This gives for the thickness under consideration a coefficient of expansion equal to $-0{\cdot}24 \times 10^{-6}$, while the observed value was $0{\cdot}75 \times 10^{-6}$.§ The difference between them, in so far as it does not arise from experimental uncertainties, is an intrinsic superficial expansion of the glass, which arises directly from the transverse polarization itself, and is not due to the mechanical forces caused by it. That there is such intrinsic alteration due to electric excitation, is independently suggested by QUINCKE's observation that the values of the elastic constants of the material are slightly altered by that cause.

In the case of a fluid the effect of electric excitation is to diminish the hydrostatic pressure; consequently expansion should result when the plates of the condenser are fixed. This agrees with what happens for most fluids. But the fatty oils form an exception; thus for them at any rate there is an intrinsic electric contraction superposed on the expansion due to diminished pressure.

In both these cases the intrinsic change of volume is of order of magnitude not higher than the change due to the mechanical stress.

The observation of QUINCKE‖ that a thin glass-tube condenser, with walls thicker on one side than the other, becomes curved (in accordance with the theory above) when it is electrically charged, virtually affords a convenient method for studying

* *Loc. cit.* 'Wied. Ann.,' 10, p. 190.

† 'Annales de Chimie,' 1882: quoted in J. J. THOMSON's 'Recent Researches,' p. 87.

‡ 'Phil. Trans.,' 1868, p. 369.

§ According to QUINCKE's own determinations (*loc. cit.* p. 187) the value of K would be about 12, which is a great deal higher than HOPKINSON's results, and would give an expansion $-0{\cdot}30 \times 10^{-6}$. It seems possible that these determinations, which involve considerable unitary complexity, may be wrongly recorded, as QUINCKE's reduction of them sometimes appears to give values for K that are less than unity.

‖ *Loc. cit.*, p. 394.

the gradual rise of the charge of the condenser and its residual discharge. With such a "glass thread electrometer" it appears that the curvature takes place gradually on excitation, occupying for small charges sometimes as long as 30 seconds: and on discharge it is annulled with corresponding slowness. As part of this deformation is intrinsic, that is, due to molecular forces and not to the mechanical stress, it is a direct indication of gradual shaking down of the material into modified molecular groupings under the influence of the electric field.

80. *Influence of Polarization of a Fluid on Surface Ripples.*—The last illustrations belong to cases in which the field of force is uniform, so that the bodily mechanical forcive vanishes. A problem amenable to experimental examination, in which this is not the case, is the influence of electric polarization on ripple motion in fluids. The fluid, in a glass dish, might for example form part of the dielectric of a horizontal condenser, of which the upper coating is a wire grating separated from the fluid by a plate of glass or mica so as to prevent communication of electrification to its surface.

Taking the axis of y downwards into the fluid whose dielectric constant is K_2, and the axis of x along the interface, the electric potentials in the two fluids, of which the upper will usually be air, are $V_1 = A_1 y + B_1 e^{my} \cos mx$, $V_2 = A_2 y + B_2 e^{-my} \cos mx$, subject to the condition that at the interface $y = C \cos mx$ we must have $V_1 = V_2$ and $K_1\, dV_1/dn = K_2\, dV_2/dn$. Thus $A_1 C + B_1 = A_2 C + B_2$, and $K_1 (A_1 + mB_1 \cos mx) = K_2 (A_2 - mB_2 \cos mx)$ which latter involves both $K_1 A_1 = K_2 A_2$ and $K_1 B_1 = - K_2 B_2$. The velocity potentials of the wave-motions in the two fluids are

$$\phi_1 = m^{-1}\, dC/dt\, e^{my} \cos mx, \quad \phi_2 = - m^{-1}\, dC/dt\, e^{-my} \cos mx.$$

In addition to the hydrodynamical pressure-difference acting from the upper to the lower side of the interface, equal to $- g(\rho_2 - \rho_1) y + (\rho_2\, d\phi_2/dt - \rho_1\, d\phi_1/dt)$, that is to $- \{g(\rho_2 - \rho_1) C + m^{-1} (\rho_2 + \rho_1)\, d^2C/dt^2\} \cos mx$, there will act on it a downward capillary traction $T\, d^2y/dx^2$, or $- m^2 TC \cos mx$, and a downward electric normal traction* $(K_1 - K_2)(K_1 K_2^{-1} N_1^2 - T_1^2)/8\pi$, in which $N_1 = A_1 + mB_1 \cos mx$ while T_1 is of the second order. This electric traction is therefore equal to $K_1 (K_1 - K_2)/8\pi K_2 . (A_1^2 + 2mA_1 B_1 \cos mx)$, where $B_1 = (K_1 - K_2)/(K_1 + K_2) . CA_1$; while the intensity of the total displacement, material and æthereal, in the electric field is $i'' = - K_1 A_1 / 4\pi$. The balancing of these tractions at the interface $y = C \cos mx$ requires that, in addition to the mean statical elevation, we should have

$$(\rho_2 + \rho_1) \frac{n^2}{m^2} = (\rho_2 - \rho_1) \frac{g}{m} + mT - \frac{(K_2 - K_1)^2}{K_1 K_2 (K_2 + K_1)}\, 2\pi i''^2,$$

in which n/m is the velocity of wave-propagation. The effect of the electric polarization is thus for ripples of length $\lambda\ (= 2\pi/m)$ the same as would be that of a diminution of the surface tension by $(K_2 - K_1)^2 / K_1 K_2 (K_2 + K_1) . \lambda i''^2$.

* This is the statical equivalent as above (§ 37) of both the actual electric traction on the surface, and the electric pressure transmitted from the interior of the fluid to the surface.

If the lower medium were conducting, we should have had $A_1C + B_1 = 0$, and the electric downward traction would be $- K_1N_1^2/8\pi$, that is $- K_1A_1^2/8\pi + m/4\pi . A_1^2C \cos mx$. Thus in the equation giving as above the velocity of propagation, the electric term would be $- A_1^2/4\pi m$, or $- 4\pi\sigma^2/K_1^2m$, where σ is the density of the electrification on the interface. The effect of this electrification is thus the same as that of a diminution of the surface tension by $2\sigma^2\lambda/K_1^2$, where λ is the wave-length.*

81. *Relations of Electrification to Vapour Tension and Fluid Equilibrium.*—It has already been shown (§ 52) that the possibility of mechanical equilibrium between fluid dielectrics which do not mix requires that the electric tractions on the interface shall be in the direction of the normal. There are also other dynamical relations deducible from the fact that such forcives, when integrated round a closed circuit in fluid media, must give a null result, in order to avoid the establishment of cyclic perpetual motions. The earliest example which led the way to relations of this kind was Lord KELVIN's establishment of a connexion between the vapour tension of a liquid and the curvature of its free surface : and similar balances must independently hold good between vapour tension and other causes of surface traction.

Consider in the first place a volume of conducting fluid with a large horizontal free surface. Let an electric field be established over a portion of this surface ; there will be a surface density σ of electrification induced over that portion, which will vary from point to point ; while the electric forces will elevate the surface by an amount $h, = 2\pi\sigma^2/g\rho$ where ρ is the density of the fluid, above the level at a distance where there is no electrification. The vapour tension over the electrified part must thus be smaller by $g\rho_0 h$ than over the unelectrified part, where ρ_0 is the density of the vapour. This difference of tension must be the natural steady difference produced by the electrification of the surface ; for otherwise a process of distillation will set in and there could not be equilibrium, though there could theoretically be perpetual generation of work while the temperature remains uniform, as the electric charge does not evaporate with the fluid. It follows that an electrification of surface density σ must depress the equilibrium vapour tension by an amount $2\pi\sigma^2\rho_0/\rho$.†

Suppose again that the fluid is a dielectric of inductive capacity K, and has no free charge. A similar train of reasoning shows, by the formula of § 37, that when the polarization of the material dielectric, at the surface, is made up of a normal component n' and a tangential component t', its vapour tension is thereby diminished by an amount $2\pi (Kn'^2 + t'^2)/(K - 1) . \rho_0/\rho$. Conversely, we can argue that, as the change of vapour tension can depend only on the state of polarization or electrification at the part of the surface which is under consideration, the effect of the electric excitation must be completely expressible by a mechanical traction over the surface

* This result was given twice too large in 'Proc. Camb. Phil. Soc.,' April, 1890.

† This agrees with a result given by Prof. J. J. THOMSON, "Applications of Dynamics to Physics and Chemistry," 1888, § 86.

which must be wholly normal and depend only on the intensity of the field of force at the place. That this is the case for fluids, but not for solids, has already been shown. And this law of dependence of vapour tension on electric state only applies to fluids, not to solids like ice; for a flow of the medium is required to complete the cycle on which the argument is based. In the case of a solid with finite vapour-tension, electric excitation—as also gravity, strains, and other physical agencies—will promote evaporation, excessively slow of course, from some parts of its surface, and condensation on others, until a form suitable to equilibrium of vapour tension is attained.

In expressing conditions of equilibrium for fluid media, the above total electric normal traction over each interface is simply to be added to such other forces as would exist in the material system if there were no electric field. Thus if we take for example the case of a number of dielectric fluids superposed on each other in a tall jar under the action of gravity, the form of the upper surface is obtained by equating the electric traction to the pressure difference produced by difference of level alone; and for any interior interface the same statement holds good, the form of each interface depending only on the electric field at the place and the inductive capacities of the two fluids which it separates. And this procedure is quite general whatever extraneous forcives there may be; the form of each interface is always determined by equating the difference of electric tractions on its two sides to the difference of pressures due to other than electric causes.

82. *Tractions on the Interfaces of a divided Magnetic Circuit.*—An important practical deduction is that when a bar or ring, longitudinally magnetized temporarily or permanently, is divided by an air-gap, the force drawing together the two halves of it consists of the attractions of the uncompensated polarities which would remain if there were no air-gap, together with a traction on each face of the gap, at right angles to its plane, and of intensity $2\pi\nu^2$, where ν is the normal component of the magnetization. This traction is in other respects quite independent of the character of the magnetic field that may exist at the gap; when the gap is narrow it is simply the attraction between the free polarities on its two faces. When the gap is transverse, the total amount of the traction is $2\pi\int I^2\, dS$, that is $(8\pi)^{-1}\int(B - H)^2\, dS$, where B and H are the longitudinal components of the magnetic induction and force; when it is oblique the longitudinal pull between the halves of the bar varies as the square of the cosine of the obliquity. When the substance is magnetized by an electric coil, there may in addition be the attraction between the two halves of the coil. For the case of iron H is very small compared with B, unless the field is far greater than is required to saturate the iron; so that the part of the mechanical traction across a transverse gap which is due to the polarities on its faces is practically $(8\pi)^{-1}\int B^2\, dS$.

83. *Interaction of Mechanical Stress and Magnetization.*—Consider a wire, magnetized to intensity I by a longitudinal magnetic field H, and subject to an

extraneous tensile force of intensity Q per unit cross-section: let M denote the modulus of elastic extension of its material, which will be an even function of I. The mechanical work expended on the wire in a slight alteration of its circumstances is per unit volume

$$\delta W = (M^{-1}\,\delta Q + \delta\eta)\,Q + H\,\delta I = (M^{-1}Q + Q\,d\eta/dQ)\,\delta Q + (H + Q\,d\eta/dI)\,\delta I,$$

where η is its intrinsic magnetic elongation when magnetized to intensity I under tension Q, this magnetization practically not altering the extraneous field in the case of a wire. To avoid perpetual motions, δW must in the absence of hysteresis be a perfect differential of the independent variables I and Q: hence $Q\,dM^{-1}/dI = (dH/dQ)_I + d\eta/dI$. Here I is a function of H and Q, so that to determine dH/dQ when I is constant we have $(dI/dH)_Q + (dI/dQ)_H\,(dQ/dH)_I = 0$, the subscript denoting the variable that is constant in the differentiation. Thus on substitution $d\eta/dI = (dI/dQ)_H/(dI/dH)_Q + Q\,dM^{-1}/dI$; and the total expansion is $\eta' = \eta + Q/M$: so that on writing as usual κ for $(dI/dH)_Q$, we have $d\eta'/dI = -I\,(d\kappa^{-1}/dQ)_H + 2Q\,dM^{-1}/dI$. This is the exact equation which should be directly satisfied by series of observations of η', I, and M, formed with different constant values of H and Q, provided hysteresis is negligible. As η' must be an even function of I, it follows that when I and Q are small, $\eta' = -\frac{1}{2}I^2\,(d\kappa^{-1}/dQ)_H$, or $\frac{1}{2}I^2\,(d\kappa^{-1}/dQ)_I$, as in § 67.

For the case of a ring magnetized by a coil, there can be no free polarity except at an air-gap; thus there is no stress of magnetic origin in the material. The alterations of longitudinal and transverse dimensions of rings of iron and nickel* are thus wholly intrinsic changes due to the magnetic polarity and in no part due to mechanical stress such as Q. In the neighbourhood of the origin, where η' is proportional to I^2, the curves given by BIDWELL expressing the relation between η' and I should be parabolic, as in fact they are. At the magnetization corresponding to a maximum or minimum ordinate η' of the curve, the effect of a very small imposed tension on the magnetization should change sign, being null for that particular magnetization; the summit of the curve is therefore the VILLARI critical point. But if there is a tension Q so considerable that change of the elastic modulus by magnetization contributes appreciably to the elongation, the VILLARI point will be displaced from the summit of the curve, backwards when magnetization increases the modulus. It appears from the experiments of BIDWELL† that for iron tension increases the intrinsic elongation, for nickel it at first increases then diminishes and finally for stronger fields increases it, while for cobalt there is no sensible effect.

84. *Mechanical Stress in a Polarized Solid Sphere.*—The mechanical stress sustained by a sphere of soft iron situated in a uniform magnetic field H can be simply expressed. The well-known analysis of POISSON gives a constant field

* SHELFORD BIDWELL, 'Phil. Trans.,' A, 1888, p. 228; 'Roy. Soc. Proc., 1894.

† 'Roy. Soc. Proc.,' vol. 47, 1890, p. 480.

$H' = 3H/(\mu + 2)$ and uniform magnetization $I' = (\mu - 1)H'/4\pi$ inside the iron, whether the law of induction is linear or not. Thus for this case of a sphere the mechanical forces exerted on the iron involve no distribution of forcive throughout its volume, but simply an outward normal traction of intensity $\{(\mu^2 - 1)\sin^2\theta - \mu + 1\} H'^2/8\pi$ over its surface: that being so, the stresses agree with KIRCHHOFF's values, and the elastic strain produced in the sphere is given by his formulæ,* the result of course involving only very slight deformation. In fact, taking the axis of x along the direction of I', it is clear that an elastic displacement (u, v, w) of the type $u = ax^3 + bx(y^2 + z^2) + cx$, $v = w = a'x^3 + b'x(y^2 + z^2) + c'x$ satisfies the conditions of the problem for the case of a sphere, the constants being determined by satisfying the equations of internal equilibrium and adjusting the surface tractions. In addition to this mechanical deformation there will be the intrinsic deformation above determined (§ 83) arising from the molecular changes produced by the magnetic polarization.

Precisely similar formulæ express the mechanical stress in a sphere of solid dielectric matter situated in a uniform electric field.

I desire to express, as in previous Memoirs, my obligation to the friendly criticism of Professor G. F. FITZGERALD, which has enabled me to remove obscurities and in various places to make my meaning clearer.

* KIRCHHOFF, "Gesamm. Abhandl., Nachtrag," p. 124; *cf.* also LOVE, "Treatise on Elasticity," I., § 168.

INDEX SLIP.

X. Bakerian Lecture.—*On the Mechanical Equivalent of Heat.*

By Professor Osborne Reynolds, *F.R.S.*, *and* W. H. Moorby, *M.Sc.*, *late Fellow of Victoria University and* 1851 *Exhibition Scholar.*

Received March 10,—Read May 20, 1897.

[Plates 3–8.]

PART I.

On the Method, Appliances and Limits of Error in the direct Determination of the Work expended in raising the Temperature of Ice-cold Water to that of Water boiling under a pressure of 29·899 inches of Ice-cold Mercury in Manchester.—By Osborne Reynolds.

The Standard of Temperature for the Mechanical Equivalent.

1. The determination by Joule, in 1849, of the expenditure of mechanical effect (772·69 lbs. falling 1 foot) necessary to raise the temperature of 1 lb. of water, weighed *in vacuo*, 1° Fahr. between the temperatures of 50° and 60° Fahr. (at Manchester), together with the second, in 1878, 772·55 ft.-lbs., to raise the temperature of 1 lb. (weighed *in vacuo*) from 60° to 61° Fahr., at the latitude of Greenwich, established once for all the existence of a physically constant ratio between the work expended in producing heat and the heat produced; while the extreme simplicity of his methods, his marvellous skill as an experimenter, and the complete system of checks he adopted, have led to the universal acceptance of the numbers he obtained as being within the limits he himself assigned (1 foot), of the true ratio of work expended in his experiments in producing heat and the heat produced as measured on the scale of the thermometer on which he spent so much time and care.

The acceptance of J = 772, as the mechanical equivalent of heat, amounts to the acceptance of the scale between 50 and 60 on Joule's thermometer *b* as the standard of temperature over this range.

Joule's thermometers are now in the custody of the Manchester Literary and Philosophical Society (having been confided to its care by Mr. A. Joule); so that this material standard is available. But the standard of temperature actually established by JOULE is universally available wherever the British standard of length is available, together with pure water and the necessary means and skill of expending a definite quantity of work in raising the temperature of water between 50° and 60° Fahr., since in this way the scale on any thermometer may be compared with that on JOULE's.

The difficulty of access to JOULE's thermometer, and the inherent difficulty of making an accurate determination of the equivalent, have limited the number of such comparisons.

The most serious attempts have been made with the very desirable object of determining the mechanical equivalent of a thermal unit, measured on the scale of pressures of gas at constant volumes, first recognised by JOULE as the nearest approximation to absolute temperature.

The results of these comparisons have been various, all having apparently shown that JOULE's standard degree of temperature is less than the one-hundred-and-eightieth part between freezing and boiling points on the scale of pressure of gas at constant volume, the differences being from 0·1 to 1·0 per cent. JOULE himself contemplated comparing his thermometer with the scale of air pressures, but did not do so. So that only indirect comparisons have been possible.

HIRN, who was the first to follow JOULE, in one of his researches introduced a method of measuring the work done which afforded much greater facility for applying the work to the water than the falling weights used by JOULE in his first determination, and this was adopted by JOULE in his second determination. But notwithstanding the greater facilities enjoyed by subsequent observers, owing to the progress of physical appliances, the inherent difficulties remained. The losses from radiation and conduction could only be minimised by restricting the range of temperature, and this insured thermometric difficulties, particularly with the air thermometer, which, it seems, does not admit of very close reading. This, together with certain criticisms, of which some of the methods employed admit, appear to have left it still an open question what exact rise in the temperature in the scale of air pressures corresponds to the 772 ft.-lbs.

2. The research, to the method and appliances for which this paper relates, has been the result of the occurrence of circumstances which offered an opportunity, such as might not again occur, of obtaining the measure, in mechanical units, of the heat in water between the two physically fixed points of temperature to which all thermometrical measurements are referred, and of thus placing the heat as defined in mechanical units, on the same footing as the unit of heat as defined by temperature, without the intervention of scales, the intervals of which depend on the relative expansions of different materials such as mercury and glass.

It has been, so far as I am concerned, undertaken with considerable hesitation, on account of the responsibility even in attempting such a determination, and the harm to science that might follow from further confusion owing to error in what, in spite of opportunities, must be the extremely difficult task of making such complex determinations within less than the thousandth part. These considerations, together with my inability to find the large amount of time necessary for making the observations, prevented any attempt until July, 1894. At that time Mr. W. H. MOORBY offered to devote his time to the research, and so relieve me of all responsibility except that which attached to the method and the appliances; and having, from experience, the highest opinion of Mr. MOORBY's qualifications for carrying out the very arduous research, there seemed to be no further excuse for delay, particularly as after seeing the appliances in the laboratory both Lord KELVIN and Dr. SCHUSTER expressed strongly their opinions as to the value of the research.

The Opportunity for the Research.

3. This consisted in the inclusion in the original equipment, in 1888, of the laboratory of the following appliances:—

(1.) A set of special vertical triple-expansion steam-engines, with separate boiler, closed stoke-hold, and forced blast; these engines being specially arranged to give ready access to the shafts (3 feet) above the floor, and being capable of running at any speeds up to 400 revolutions per minute, and working up to 100 H.P. (Plate 3.)

(2.) Three special hydraulic brake dynamometers, on separate shafts, between and in line with the engine shafts, with faced couplings, so that one brake shaft could be coupled with the shaft of each engine, leaving each engine to work its own shaft; or the brakes on the high-pressure and intermediate engines could be removed, and their shafts coupled by means of intermediate shafts, so that all three engines worked on the brake connected with the low-pressure engine. These brakes, which are shown (Plate 3), are separately capable of absorbing any power up to a maximum of 30 horse-power at 100 revolutions, and increasing as the cube of the speed; so that a single brake is capable of absorbing the whole power of the engine at any speed above 100 revolutions a minute.

The whole of the work is absorbed by the agitation of the water contained in the brake, while the heat so generated is discharged by a stream of water through the brake, with no other functions than of affording the means of regulating, independently, the temperature of the brake and the quantity of water in the brake. The moment of resistance of the brake at any speed is a definite function of the quantity of water in the brake. And as, except for this moment, the unloaded brake is balanced on the shaft, the load being suspended from a lever on the brake at 4 feet from the axis of the shaft, if the moment of resistance of the brake exceeds the moment of the load, the lever rises, and *vice versâ*. By making the lever actuate the valve which regulates the discharge from the brake, and thus regulate the effluent stream, the quantity of water in the brake is continually regulated to that which is just sufficient to suspend the load with the lever horizontal, and a constant moment of resistance maintained whatever may be the speed of the engines.

(3.) Manchester town's water, of a purity expressed by not more than 3 grams of salts in a gallon, brought into the laboratory in a 4-inch main at town's pressure (50 to 100 feet), and distributed either direct from the main or at constant pressure from a service tank 10 feet above the floor of the laboratory.

(4.) Two tanks, each capable of holding 60 tons of water, one in the tower, 116 feet above the floor, the other 15 feet below the floor, connected by 4-inch rising and falling mains, each 500 feet long, passing in a chase under the floor. The rising main including a special quadruple centrifugal pump, 2 feet above the floor, capable of raising a ton a minute from the lower to the upper tank. (Shown in Plate 7.) Also a set of mercury balances, showing continually the levels of water in the two tanks, and the pressures in the rising, falling, and towns mains. (Shown in Plate 4.)

(5.) A special quadruple vortex turbine, supplied from the falling main and discharging into the lower tank, capable of exerting 1 h.p., and available for steady speed at all parts of the laboratory. (Shown in Plate 7.)

(6.) A supply of power to the laboratory by an engine and boiler, quite distinct from the experimental engine, and distributed by convenient shafting which is always running. (Shown in Plate 3.)

The Measurement of the Work.

4. Of the appliances mentioned, the brake on the low-pressure engine is the centre of interest, as it was by this that the work was measured, as well as converted into heat.

The existence of the appliances was largely due to the interest in educational work taken by Mr. William Mather, who, together with the other members of the firm of Mather and Platt, not only placed at my disposal the facilities of their works, but inspired the enthusiasm which alone rendered the execution of such novel and special work possible.

The development of the brake dynamometer, from its introduction by Prony, has an interesting and important history, but into this it is not necessary to enter. The purpose of these dynamometers is to afford continuous frictional resistance adapted to the power exerted by the prime mover in causing a shaft to revolve, and of a kind that is definitely measurable. To fulfil the first of these conditions, the mean moment of resistance of the brake must just balance the mean moment of effort of the engine, and the means of escape of heat from the brake must be sufficient to allow all the heat generated to depart without accumulating to an extent which may interfere with the action of the appliances. In the first brakes the resistance was obtained by the friction of blocks or straps pressed against a cylindrical wheel on the shaft, and, small powers being used, radiation and air-currents round the brake were found sufficient to carry off the heat, but, when larger powers were used, these sources of escape failed to keep the temperatures down to practical limits, which necessitated the application of currents of water to carry off the heat.

The measurement of the work was invariably accomplished by attaching the brake blocks, or straps, to a lever, or arm, so that the whole brake would be free to revolve with the brake-wheel, except for the moment of the weight of the parts which, adjusted to the power of the engine, was kept in balance by the adjustment of the pressure of the blocks on the wheel. Then, since the work done is equal to the product of the mean moment of resistance, over the angle turned through, multiplied by the angle, if the resistance is constant over time, the moment of the *brake*, multiplied by the whole angle, measured the work done.

It is however to be noticed that the assumption that the *time-mean* of the moment on the brake is the same as would be the *angle-mean* of this moment might involve an error of any extent, provided the resistance and the angular velocity varied in conjunction. And as steam engines invariably exert an effort within the period of the revolution while the friction and the pressure causing it are apt to respond to any variations of speed, it is probable that there has been some error from this cause in all such measurements although not previously noticed.

HIRN appears to have been the first to recognise that in a steady condition the resistance of fluid between the brake-wheel and the brake would answer instead of the solid friction, so that the mean time moment of effort exerted in turning a paddle in a case with bafflers containing water would be strictly measured by the mean time moment of the case. And although subject to the same error from periodic motion as the friction brake, the facility this fluid brake offered for cooling and regulating led to its simultaneous adoption and development by several inventors, for measuring power—the late WILLIAM FROUDE, for the purpose of measuring the work of large engines, inventing that arrangement of paddle vanes and bafflers which gives the highest resistance, regulating the resistance by thin sluices between the vanes and bafflers, and always working with the case full of water.

The brake under consideration differs from that of Mr. FROUDE in only one fundamental particular—the provision by which a constant pressure in the interior of the brake is secured by the admission of the atmosphere to that part of the brake where the dynamical effect of the water is to cause the lowest pressure—this admits of working the brakes with any quantity of water from nothing to full, and thus allows of the regulation of the resistance by regulating the quantity of water in the brakes without sluices.

The description of this brake has already been published, together with that of the engines,* but it will be convenient to give a short description.

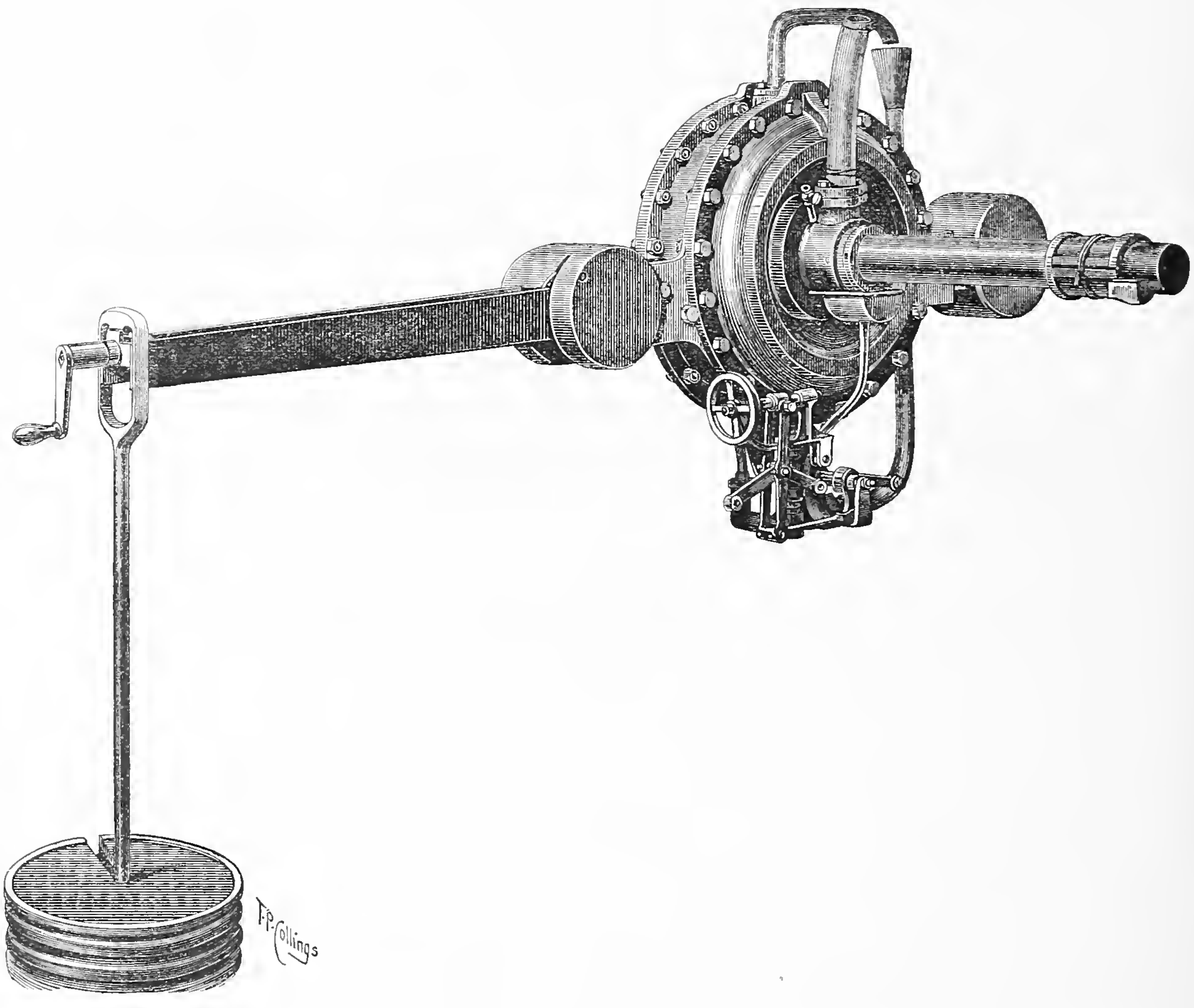
This brake consists primarily of (1) a brake wheel, 18 inches in diameter, fixed on

* "Triple Expansion Engines," by Professor OSBORNE REYNOLDS, 'Minutes of Proceedings, Inst. C.E.,' vol. 99, 1889, p. 18.

the 4-inch brake shaft by set pins, so that it revolves with the shaft (figs. 2 and 3), and (2) a brake (or brake case) which encloses the wheel, the shaft passing through *bushed* openings in the case which it fits closely, so as to prevent undue leakage of water while leaving shaft and brake-wheel free to turn in the case, except for the slight friction of the shaft (figs. 1, 2 and 3).

The outline of the axial section of the brake-wheel is that of a right cylinder, 4 inches thick. The cylinder is hollow—in fact, made of two discs which fit together, forming

Fig. 1.

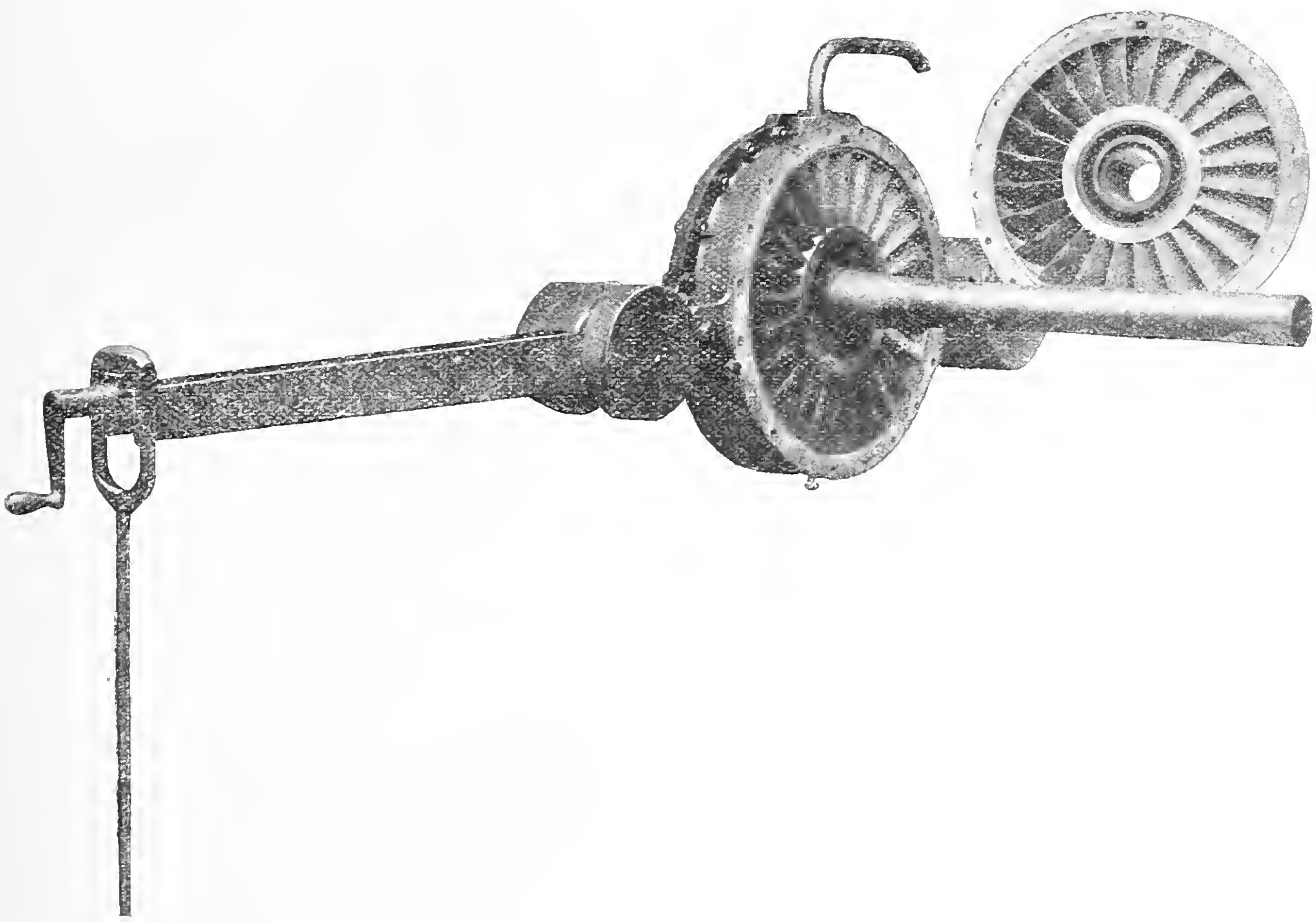


an internal boss for attachment to the shaft, and also meet together at the periphery, forming a closed annular box, except for apertures to be further described (fig. 3). In each of the outer disc faces of the wheel are 24 pockets (carefully formed), $4\frac{1}{2}$ inches radial and $1\frac{1}{2}$ inches deep measured axially, but so inclined that the narrow partitions or vanes ($\frac{1}{4}$ inch) are nearly semicircular discs inclined at 45° to the axis; the vane on one face being perpendicular to the vane on the opposite face (fig. 2).

The internal disc faces of the brake case, as far as the pockets are concerned, are the exact counterparts of the disc faces of the wheel (except that there are 25 pockets), so that the partitions in the case are in the same planes as the partitions meeting them in the wheel, there being $\frac{1}{64}$ inch clearance between the two faces.

The pairs of opposite pockets when they come together form nearly closed chambers, with their sections, parallel to the vanes, circular. In such spaces vortices in a plane inclined at 45° to the axis of the shaft may exist, in which case the centrifugal pressure on the outside of each vortex will urge the case and the wheel in opposite directions inclined at 45° to the direction of motion of the wheel,

Fig. 2.



which will give a tangential stress over the disc faces of the wheel of $1/\sqrt{2}$ of the sum of these vortex pressures. The existence and maintenance of these vortices is insured by the radial centrifugal force of the water in the pockets in the wheels owing to its motion.

This is the late Mr. W. FROUDE'S arrangement. But an essential feature of the brake is the provision which insures the pressure of the atmosphere at the centre of the vortices, even when the pockets are only partially filled.

The vortex pressure is greatest at the outsides of the vortices, which occurs all over the annular surfaces of the pockets, but the actual pressure on these surfaces is not determined solely by the vortex motion unless the state of pressure at the centre of the vortices is fixed, for the vortex motion only determines the difference between these pressures. To insure the constant pressure, and at the same time to allow of the pockets being only partially full—that is, to allow of hollow vortices with air cores at atmospheric pressure, it is necessary that there should be free access of air to the centres of the vortices, and as this access cannot be obtained through the water, which completely surrounds these centres, it is obtained by passages ($\frac{1}{8}$ inch diameter) within the metal of the guides, which lead to a common passage opening to the air on the top of the case (figs. 2 and 3).

To supply the break with water there are similar passages in the vanes of the wheel leading from the box cavity, which again receives water through ports which

Fig. 3.

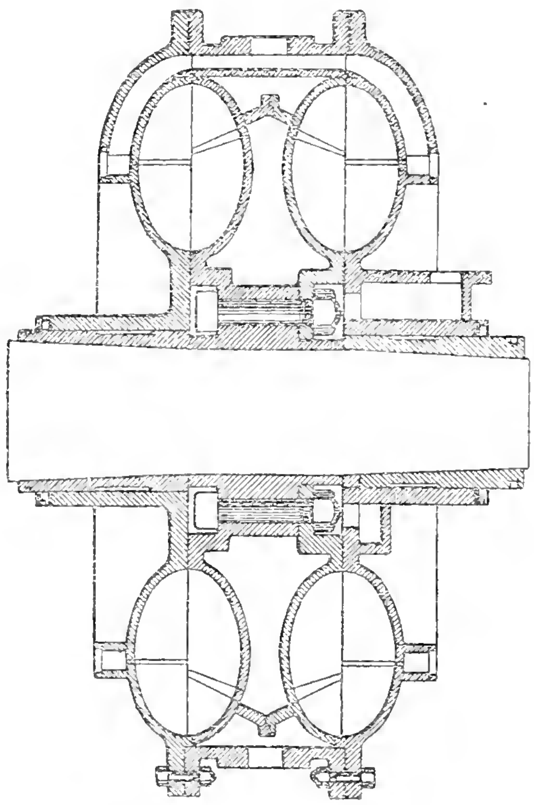
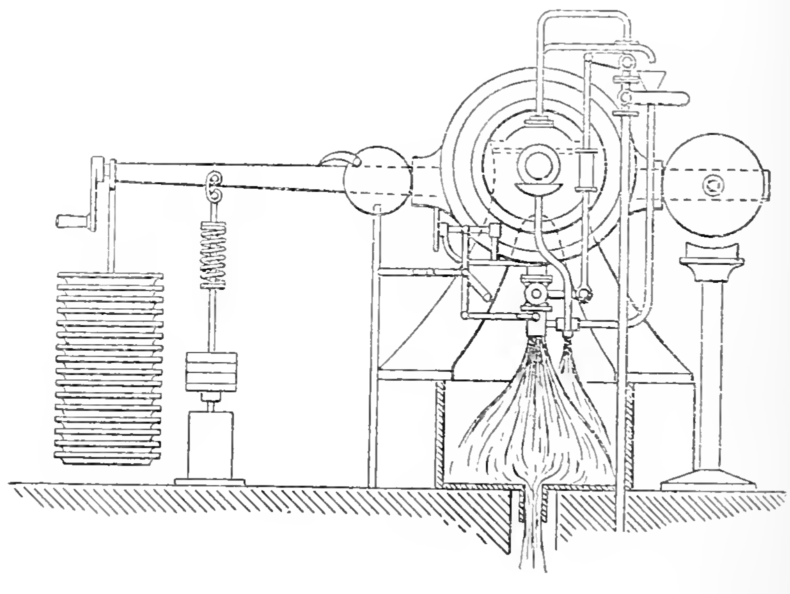


Fig. 4.



open opposite an annular recess in one of the disc faces of the case into which the supply of water is led, by means of a flexible indiarubber pipe from the supply regulating valve.

The water on which work has been done leaves the vortex pockets by the clearance between the disc surfaces of the wheel and case, and enters the annular chamber between the outer periphery of the wheel and the cylindrical portion of the case, which is always full of water when the wheel is running, whence its escape is controlled by a valve in the bottom of the case, from which it passes to waste.

By means of linkage connected with a fixed support and the brake case, an automatic adjustment of the inlet and outlet valves, according to the position of the lever, is secured without affecting the mean moment on the brake case. And this also affords means of adjusting the position of the lever. To admit of adjustment for wear the shaft is coned over that portion which passes through the bushes, the bushes being similarly coned, and screwed into short sleeves on the casing, so that by unscrewing them the wear can be followed up and leakage prevented.

The brake levers for carrying the load and balance weight, are such as to allow the load to be suspended from a groove parallel to the shaft, at 4 feet from the shaft, by a carrier with a knife edge, the carrier and the weights each being adjusted to 25 lbs. (shown figs. 1 and 4). In addition to this load, a weight is suspended from a knife edge on the lever nearer the shaft, this weight being the piston of a dash-pot in which it hangs freely, except for the viscous resistance of the oil. This weight being adjusted to exert a moment of 100 ft.-lbs., and again a travelling weight of 48 lbs., is carried on the lever and worked by a screw with $\frac{1}{4}$ inch pitch, so that one turn changes the moment by 2 ft.-lbs., while a scale on the lever shows the position. A shorter lever on the opposite side of the case carries a weight of 74·6 lbs., which is adjusted to balance the lever and sliding weight when the load is removed.

The Accuracy of the Brake.

5. The principle of these hydraulic dynamometers is that when moment of momentum is introduced into a fixed space without altering the moment of momentum within that space, the rate at which moment of momentum leaves the space must equal the rate at which it enters. The brake-wheel imparts moment of momentum to the water within the case, and the friction of the shaft imparts moment of momentum to the case. The water in the case, when its moment of momentum is steady, imparts moment of momentum to the case as fast as it receives it, and the time mean of the moment of the load is equal to the time mean of the moment of effort of the shaft.

This is not affected by water entering and leaving the case at equal rates, provided it enters and leaves radially.

The condition of steadiness is, however, essential, in order that the moment of effort shall be at each instant equal to the moment of resistance on the case; any change in the moment of momentum of the water in the case being the result of the difference of the moment of effort on the shaft and that of resistance on the case.

The Time-Mean of the Moment of Effort.

6. When, however, the shaft is run over an interval of time, the mean moment of resistance on the case, less the difference of the moments of momentum of the water, at

the end and beginning of the interval, divided by the time, is the time-mean moment of effort on the shaft.

The possible limit of this error may be estimated when the maximum moment of momentum of the water is known as well as the minimum moment of resistance, and the minimum interval of time.

Thus taking the limits to be 30 lbs. of water, with radius of gyration 0·66 foot at 300 revolutions a minute (< 14), the interval of running 3600 seconds, the moment of the load 400 ft.-lbs., the limit of the time-mean of change of moment of momentum of the water is 14/3600, and this divided by the mean moment of resistance gives as the limits of relative error, $\pm$ 0·00001. This is supposing the whole of the water to be absent at the beginning or end of the trial, while the actual difference never amounts to more than 2 or 3 lbs., so that the limits do not exceed 0·000001, which is neglected.

The Angle-Mean of the Moment of Effort on the Shaft.

7. As already pointed out in Art. 4, when both the angular velocity of, and the moment of effort on, the shaft are subject to fluctuations of speed, the time-mean of the moment of effort may differ from the angle-mean. This applies to all brakes, but in hydraulic brakes, in which the resistance is proportional to the square of the speed although lagging by an unknown interval, it becomes possible to estimate the possible limits of this error when the limits of fluctuation of speed are known.

Taking ω the angular velocity of the shaft and ω_0 the time-mean of the angular velocity, $2a^2\omega_0$ the extreme differences of speed, and assuming the variation to be harmonic,

$$\omega = \omega_0 \{1 + a^2 \cos n (t - T_1)\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

$$\omega^2 = \omega_0^2 \{1 + \frac{a^4}{2} + 2a^2 \cos n (t - T_1) + \tfrac{1}{2}a^4 \cos 2n (t - T)\} \quad . \quad . \quad (2).$$

Then to a second approximation neglecting a^6, if T_2 is the interval of lagging in the resistance and M the moment of resistance at the time t,

$$M = M_0 \{1 + 2a^2 \cos n (t - T_1 - T_2) + \tfrac{1}{2}a^4 \cos 2n (t - T_1 - T_2)\} \quad . \quad (3),$$

where M_0 is the time-mean of the moment of resistance. Also the rate at which work is done with uniform velocity, is $M\omega_0$, of which the mean is $M_0\omega_0$, and is the rate of work as measured by the mean moment on the case, multiplied by the mean-angular velocity.

To a second approximation the rate of work with varying speed is

$$M\omega = M_0\omega_0\{1+2a^2\cos n(t-T_1-T_2)+\tfrac{1}{2}a^4\cos 2n(t-T_1-T_2)\}\{1+a_2\cos n(t-T_1)\} \quad (4),$$

and from this it appears that the mean rate of work is

$$\omega_0 M_0 (1 + a^4 \cos nT_2),$$

which shows that the relative error in taking this as $M_0\omega_0$ is $+ a^4 \cos nT_2$. Thus the error arising from fluctuations in speed of $2a^2\omega$ is within the limits $\pm a^4$, when the resistance varies as the square of the speed, as in the hydraulic brakes.

Where, as in the brake under consideration, there is an automatic adjustment, by which the quantity of water in the brakes is adjusted to the speed, so as to maintain the resistance constant, there will be no error caused by such gradual variations of speed as result from changes in the boiler pressure, since the automatic adjustment can keep pace with them. But it takes time for the water to get in and out, and any variations, so rapid that, owing to the inertia of the brake case with its load, their effect has been reversed before the case has moved sufficiently to affect the water in the brake, will produce errors.

Such cyclic variations of speed attend all motions derived from reciprocating engines, and it is only these, and not the secular variations, that produce errors.

The Variations in the Speed of Rotation of the Steam Engine.

8. The cyclic variations all go through one or two complete periods in the time of revolution of the engine, and are approximately simple harmonic functions of the time.

They arise from three distinct causes :—

(1.) The varying energy of motion of the reciprocating parts ;
(2.) The varying moment of the effort of the steam pressures on the cranks ;
(3.) The effect of gravitation on the unbalanced parts in the engine.

In the case of a simple vertical engine, unbalanced and working with moderate expansion, these variations of speed may be severally estimated when I, the moment of inertia of the revolving parts, r the half-stroke of the reciprocating parts, and W the weight of these parts are known together with N the number of revolutions per minute, and U the work done per stroke.

For, considering the variations as existing separately, we may assume that the angular motion would be steady but for the particular effect, thus :

(1.) The moment of effort on the crank being constant, and the resistance constant, and equal to the effort, the energy of motion of all the parts is constant.

Putting $\omega = 2\pi N/60$, and $i = r^2 W/g$,

$$\tfrac{1}{2}I\omega^2 + \tfrac{1}{2}i\omega^2 \sin^2 nt = C,$$

where C is constant, t is the time since the axis of the crank-pin has crossed the axis of the cylinder and n is ω_0, the mean value of ω or $2\pi N/60$.

Whence neglecting i as compared with I, the extreme variation of ω is approximately

$$2a_1^2\omega_0 = \tfrac{1}{2}\frac{i}{I}\omega_0$$

whence

$$a_1^2 = \tfrac{1}{4}\frac{i}{I}.$$

(2.) In the same way, considering the effect of the crank effort alone, with a moderate expansion, the energy that has to be absorbed and given out by the revolving parts is about one-fourth part of the work per stroke, and

$$\tfrac{1}{2}I\omega^2 - \tfrac{1}{8}U \cos 2n(t - T) = C,$$

where nT, say $\frac{\pi}{3}$ is the angle of the crank at which ω^2 is a minimum.

The extreme fluctuations in velocity are

$$2a_2^2\,\omega_0 = \frac{U}{4}\frac{\omega_0}{I\omega_0^2}, \quad a_2^2 = \tfrac{1}{8}\frac{U}{I\omega_0^2},$$

$$\omega = \omega_0\left\{1 + \frac{U}{8I\omega_0^2}\cos 2\left(nt - \tfrac{1}{3}\pi\right)\right\}.$$

(3.) The effect of the weight of the reciprocating parts acting alone, causes a fluctuation on the revolving parts of $2rW$; thus approximately

$$\tfrac{1}{2}I\omega^2 - rW\cos nt = C,$$

and

$$\omega = \omega_0\left(1 + \frac{Wr}{I\omega^2}\cos nt\right)$$

giving an extreme fluctuation on the angular velocity of

$$2a_3^2\omega_0 = 2\frac{Wr}{I\omega^2}\omega_0.$$

The equation of velocity is thus approximately expressed by

$$\omega = \omega_0 \left[1 + \tfrac{1}{4}\frac{i}{I}\cos 2nt + \frac{U}{8I\omega^2}\cos 2\left(nt - \tfrac{1}{3}\pi\right) + \frac{Wr}{I\omega^2}\cos nt\right].$$

In the low-pressure engine used in these experiments, the values of the several quantities are, the units being linear feet, lbs, seconds.

$$I = 126, \quad i = 2{\cdot}47, \quad r = 0{\cdot}625, \quad W = 200, \quad rW = 125, \quad U = 1650,$$

$$\tfrac{1}{4}\frac{i}{I} = 0{\cdot}0049, \quad \frac{U}{8I\omega^2} = \frac{148}{N^2}, \quad \frac{rW}{I\omega^2} = \frac{90}{N^2},$$

whence, substituting

$$\omega = \omega_0\left(1 + 0{\cdot}0049\cos 2\omega_0 t + \frac{148}{N^2}\cos 2\left(\omega_0 t - \frac{\pi}{3}\right) + \frac{90}{N^2}\cos\omega_0 t\right),$$

from this the approximate joint error can be found. But it is sufficient here to show that the individual errors are negligible.

The first gives an error in the mean moment

$$\pm M\,(a_1^4 < 0{\cdot}000024).$$

The second and third are inversely proportional to N^4, if N is 300, which is the lowest value.

The second error is between

$$\pm M\,(a_2^4 < 0{\cdot}0000025).$$

The third

$$\pm M\,(a_3^2 < 0{\cdot}0000001).$$

These are all negligible quantities, and, as the corresponding effects in the high-pressure and intermediate engines, owing to the cranks being set at angles of 60°, would only be to compensate those of the low-pressure engine, the greatest error would not exceed $\frac{1}{40000}$th part.

9. Besides the errors resulting from the terminal differences in the moment of momentum of the water and the fluctuations of speed in the engine, error in the measurement of the work may arise from imperfect balance of the brake, from the frictional resistance of the automatic gear, from unequal resistance in rising and falling of the piston of the dash-pot, and from the end oscillation of the brake.

The Error of Balance of the Brake.

Although, when the shaft is running, the brake levers are perfectly free between the stops, yielding to the slightest force even when carrying a load of 400 pounds in addition to the weight of the brake-case of over 300 pounds, yet, when the shaft is standing, it requires a moment of some 40 ft.-lbs to move the lever in either direction, so that the balance can only be obtained as the difference of these moments, and this can only be obtained to about 1 foot pound. But, it is to be noticed that as long as the distribution of weights are unaltered and the lever is in the same position, any error of balance, whatever might be its cause, would be the same for all trials, no matter what might be the difference in the suspended load; so that, in taking the difference of the trials, the error would be eliminated, and, to insure this, the automatic adjustment was so arranged that, by a screw adjustment, the lever could be raised or lowered without affecting the automatic adjustment of the valves (see fig. 4, p. 308). Also an index was arranged adjacent to the end of the lever to which it might be always adjusted (shown in Plates 4 and 5).

The Error of Balance resulting from Friction of the Automatic Gear,

This had been a matter of serious consideration in designing the brakes, for, although it was obviously possible to so balance the parts of such gear that there should be no pressure against the fixed support arising from the weight of this gear, it was not obvious that the friction of these valves and their gear would not allow of a steady resistance to motion being maintained—would not allow the brake to lean against the fixed support within the limits of friction. However, after careful consideration of various contrivances, I came to the conclusion that, if the gearing between the support and the valve were inelastic, the joints being an easy fit, the tremor of the shaft and the brake, when running, might be depended upon to release any frictional resistance in this gear; so that, after any change, the gear would rapidly return to equilibrium. This proved to be the case, even to an unexpected extent, as was shown by the freedom of all the pins.

It was subsequently found by experiment that, even when the valves were so tight that it required a moment of 30 ft.-lbs. on the brake to move the automatic gear alone, with the shaft standing, in either direction, when the shaft was running any tendency to lean upon the support in either direction was the result of imperfect balance in the gear; and that, by adjusting this balance to an extent which would not cause a moment on the brake of 0·01 ft.-lb., the tendency of the brake to lean either in one direction or the other might be reversed—showing that, with a load of 600 ft.-lbs., the relative limits of error are $< \pm 0{\cdot}000016$, and in the difference of the trials would be zero.

The Work done in the Brake by End Play in the Shaft.

The clearance in the brake-case would allow of nearly $\frac{1}{32}$-inch end play along the shaft; and when the brake is running, owing to the slight *end* play of the engine-shaft, there is at times a slight backwards-and-forwards movement, in the period of the engine, of the brake-case on the shaft, but not more than the 64th of an inch at the greatest. This end play, when it existed at 300 revolutions and 1200 ft.-lbs. load, could always be prevented by an end pressure on the case of < 50 lbs. Hence the limit of work done on the brake is $< 2 \times 50 / 12 \times 64 = 0{\cdot}13$ ft.-lb., which, compared with the work in one revolution with a load of 1200 ft.-lbs., is $0{\cdot}13/1200 \times 2\pi = 0{\cdot}000017$. This would be the limit if the error is proportional to the load, while, if constant, the error on the difference of two trials would be zero; so that the greatest relative error is less than

$$+ 0{\cdot}000017.$$

The Error from the Dash-Pot.

Since the piston is suspended freely in the oil-cylinder, and the resistance of the oil is viscous and expressed by $\mu vs / a$, where μ is the coefficient of viscosity, v the velocity of the piston, s the area of surface, and a the distance between the surfaces, the total resistance is thus $\mu s/a$ multiplied by the total displacement (which never exceeds 0·1 ft.) divided by the time (3600 seconds). This is infinitesimal. Besides which, with 1200 or 600 ft.-lbs. load at 300 revolutions, the lever remains perceptibly steady, there being no vertical vibration perceptible to the finger on the lever. Hence, as long as there are no oscillations, the limit of error from the dash-pot, if any, is imperceptibly small.

The only circumstances under which the lever oscillates is when the water flowing through is less than about 4 lbs. a minute; then a slow oscillation appears, the lever moving some half-inch, which causes the automatic gear to lean on the fixed support, and may cause a small error.

The Development of the Thermal Measurements.

The appliances were originally designed, in 1887, solely for the purpose of the study of the action of steam in the engines, and certain problems in hydraulics and dynamometry, without any intention of their being used for the purpose of measuring the heat equivalent of the work absorbed, but rather the other way.

It was, of course, obvious that, as the primary purpose of the brakes was to afford accurate measurement of the work spent in heating water, it was only necessary to measure the change of temperature of the water between entering and leaving the brake, as well as its quantity, to obtain an approximate estimate of the heat equivalent of the work done. But the recognition of the extreme difficulty of obtaining any first-hand assurance as to the accuracy of scales of thermometers, and the fear of creating erroneous impressions as to the value of the equivalent, made me reluctant to allow such determinations. For this reason, as well as to avoid complicating the brake, in the first instance I made no provision for the introduction of thermometers, as may be seen in Plate 3.

But, after the engines and brake had been in use for two years, and had been found to possess attributes in steadiness of running, delicacy of adjustment and balance, beyond what I had dared to expect, and particularly in being able to work with an almost absolutely steady supply of water between steady temperatures, and the same temperatures for different powers, arising either from differences of speed, or differences of load, I realized that by working with the same thermometers on the same parts of their scales, and with the same loads and temperatures at different speeds, since the relative error of balance would be the same, if the surrounding temperatures were the same, the difference of two trials would afford the means of determining the loss of heat by radiation, and, this being determined, the difference of two trials made at the same temperatures as the previous trials, and both at the same speeds, but with different loads, would afford data for determining the error of balance without introducing the value of the equivalent or the use of the scales of the thermometers, except to identify equal temperatures.

I then yielded to the very general wish on the part of those who worked in the laboratory, and added such provision to the brake on the low-pressure engine as would admit of the measurement of the heat carried away by the effluent water, but only for the purpose of verifying the accuracy of balance as determined by mechanical means.

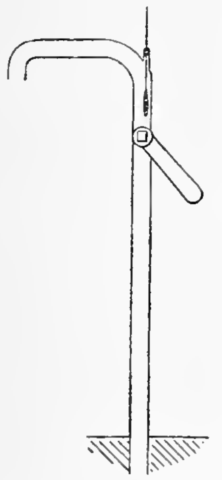
The Thermal Verification of the Balance of the Brakes.

10. The desirability of such independent determination of the balance arose in the first instance from the circumstances already described (Art. 9), viz., that the statical balance could only be determined to 1 ft.-lb., while the absence of effect from the friction of the automatic gear, &c., was only arrived at by somewhat complicated considerations.

The supply of water to the brake came from the service tank, 10 feet above the floor, and 7 feet above the shaft, the tank being supplied direct from the town main, and regulated by a ball-cock. The pipe from the tank passes beneath the concrete floor to a point conveniently close to the brake, whence a branch, in which is a hand-cock, rises vertically to a height of 4 feet above the floor, at which height is the automatic inlet valve, and from this the pipe is bent over, so that its mouth is directly over the inlet opening into the brake, with which the pipe is connected by a flexible indiarubber tube.

The first provision made for measuring the temperature of the entering water was an opening in the bend of the pipe over the inlet valve, with a vertical $\frac{3}{8}$-inch brass tube soldered in, about 4 inches long. This admitted of an indiarubber cork, through the centre of which a thermometer was passed into the pipe, as shown in fig. 5. This was afterward replaced by a glass thermometer chamber, as shown in the figure, Plate 5.

Fig. 5.



To measure the temperature of the water leaving the brake it was necessary, by means of a pipe fixed to the mouth of the outlet valve, to bring the effluent water above the balancing lever of the brake, and to one side of it. This pipe was arranged so as to admit the introduction of a vertical thermometer into the ascending pipe, much in the same way as the other. In the first instance the extension passage and the thermometer were all rigidly attached to the brake, and moved with it, which entailed a re-balance of the brake. Subsequently another arrangement was made. The thermometers used were divided to one-fifth of a degree Fahrenheit ; they were both immersed in the flowing water to within a few degrees of the top of the mercury. They were compared at equal temperature, but otherwise subjected to no tests for accuracy of scale.

In making the experiments the link connecting the inlet valve with the automatic gear was removed and the valve was set open, the supply being adjusted by the hand-cock below. The head on the inlet being constant, when the cock was set the flow was practically steady. The quantity of water in the brake then depended on the outlet valve, which, with the exception of a little trouble at starting and stopping, soon overcome, kept the brake lever steady.

To catch the water after leaving the outflow thermometer, the extension pipe turned horizontally over the lever and then turned downwards into a basin, the lip of which was above the mouth of the pipe, and from the basin flowed in a short trough, from which it was caught in buckets. In these it was taken to the scales and carefully weighed. This was a primitive arrangement, and required several assistants, but was found capable of considerable accuracy up to about 40 lbs. a minute.

In making these experiments the engines were kept running at nearly constant speed by keeping constant pressure in the boiler. The speed being indicated on the speed gauge as well as recorded on the counter.

The water entering the brake, coming, as it did, from the town's main, was at nearly constant temperature between 40° and 50° Fahr., according to the time of the year, and varying less than a degree throughout several trials.

The rise of temperature was adjusted by the quantity of water admitted, according to the work, so that the final temperatures as well as the initial were as nearly as possible the same in the different trials.

This rise was such as admitted of the temperature of the brake being the same as that of the laboratory, which could always be adjusted to about 70° Fahr., so that the rise was from 25 to 30 degrees. This, with 40 lbs. a minute, required from 25 to 30 h.p.

Before commencing the actual trial everything was adjusted, and the engines running with steady load and steady speed until the thermometer showed the heat to be steady at the desired temperature, then, at a signal, the counter was put in and the water caught, each of the thermometers, and one giving the temperature of the laboratory, being then read at minute intervals over 15 or 30 minutes, when, on a signal, the counter was removed and also the last bucket.

The results of these tests were very consistent, within about 0·3 per cent., which was within the limit of accuracy then aimed at.

Trials with equal loads and different speeds showed that the loss by radiation was very small, while those at the same speed with different loads showed the balance was within the limits determined by mechanical tests.

In these trials the only correction was that for the lubricating water which escaped from the brake bushes. This was caught at each bearing, and the temperature taken so that the heat might be added, this being seldom more than 3 per cent. It may also be noticed that in these trials the heat lost or gained by conduction to or from the shaft was included in the radiation. As the brake is on an overhanging shaft which extends no farther than the outer bush of the brake case (Plate 3), the only conduction is on the side at which the shaft is continuous, where the brake bush is only some 4 inches from the brass of the shaft bearing. As the temperature of the brake on this side, which is opposite to that at which the cold water enters, was kept by the lubricating water at the temperature at which the water left the brake, and this was at temperature of the laboratory, there would be no cause of conduction unless the friction of the shaft in its bearing caused its temperature to rise above that of the laboratory. When the lubrication was good this was small, although on one or two occasions it made itself felt.

The Idea of Raising the Temperature from 32 *to* 212.

11. These tests became an annual exercise in the laboratory, and a very instructive exercise. But, as the subject—the value of the equivalent—was attracting much attention, the desire to obtain measures of it from these trials, by those engaged in them, resulted in Mr. T. E. STANTON, M.Sc., then Senior Demonstrator, effecting, for his own satisfaction, a comparison of the scales of the thermometers used in the experiments with a thermometer used in the Physical Laboratory, which had been compared with the air thermometer, and introduced these corrections into the results of the trials, which so gave values very close to what might be expected. I could not see however that determinations made with thermometers so corrected

could have any intrinsic value, but, as the matter was exciting great interest in the laboratory, I carefully considered the conditions which would be necessary in order to render the great facilities, which this brake was thus seen to afford, available for an independent determination.

The institution of an air thermometer was carefully considered and rejected. But it occurred to me that it might be possible to avoid the introduction of scales of the thermometers, just as before, and yet obtain the result. If it could be so arranged that the water should enter the brake at the temperature of melting ice and leave it at the temperature of water boiling under the standard pressure, all that would be required of the thermometers would be the identification of these temperatures. At first the difficulties appeared to be very formidable. But on trying, by gradually restricting the supply of water to the brake when it was absorbing some 60 h.p., and finding that it ran quite steadily with its automatic adjustment till the temperature of the effluent water was within 3° or 4° of 212° Fahr., I further considered the matter and formed preliminary designs for what seemed the most essential appliances to meet the altered circumstances.

These involved—

(1.) An artificial atmosphere, or a means of maintaining a steady air pressure in the air passages of the brake of something like one-third of an atmosphere above that of the atmosphere.

(2.) A circulating pump and water cooler, by which the entering water (some 30 lbs. a minute) could be forced through the cooler and into the brake, at a temperature of 32°, having been cooled by ice from the temperature of the town main.

(3.) A condenser by which the effluent water leaving the brake at 212° Fahr. might be cooled down to atmospheric temperature before being discharged into the atmosphere and weighed.

(4.) Such alteration in the manner of supporting the brake on the shaft as would prevent excess of leakage from the bushes in consequence of the greater pressure of the air in the brake, since not only would the leaks be increased, but when the rise of temperature of the water was increased to 180°, the quantity for any power would be diminished to one-sixth part of what it would be for 30°, so that any leakage would have six times the relative importance.

(5.) Some means which would afford assurance of the elimination of the radiation and conduction, as, with a rise of 140° Fahr. above that of the laboratory, these would probably amount to two or three per cent. of the total heat.

(6.) Scales for greater facility and accuracy in weighing the water, with a switch actuated by the counter.

(7.) A pressure gauge or barometer, by which the standard pressure for the boiling point might be readily determined at 3° or 4° Fahr. above and below the boiling point, so as to admit of the ready and frequent correction of the thermometers used for identifying the temperature of the effluent water.

(8.) Some means of determining the terminal differences of temperature and quantity of water in the brake, which would be relatively six times larger with a rise of 180° than with 30°.

The Special Appliances and Preliminaries of the Research.

12. Having convinced myself by preliminary designs, not only of the practicability of the appliances, but also of the possibility of their inclusion in the already much occupied space adjacent to the brake, there still remained much to be done in the way of experimental investigation to obtain data from which the requisite proportions of these appliances could be determined, and these preliminary investigations were not commenced till the summer of 1894, when Mr. MOORBY undertook to devote himself to the research.

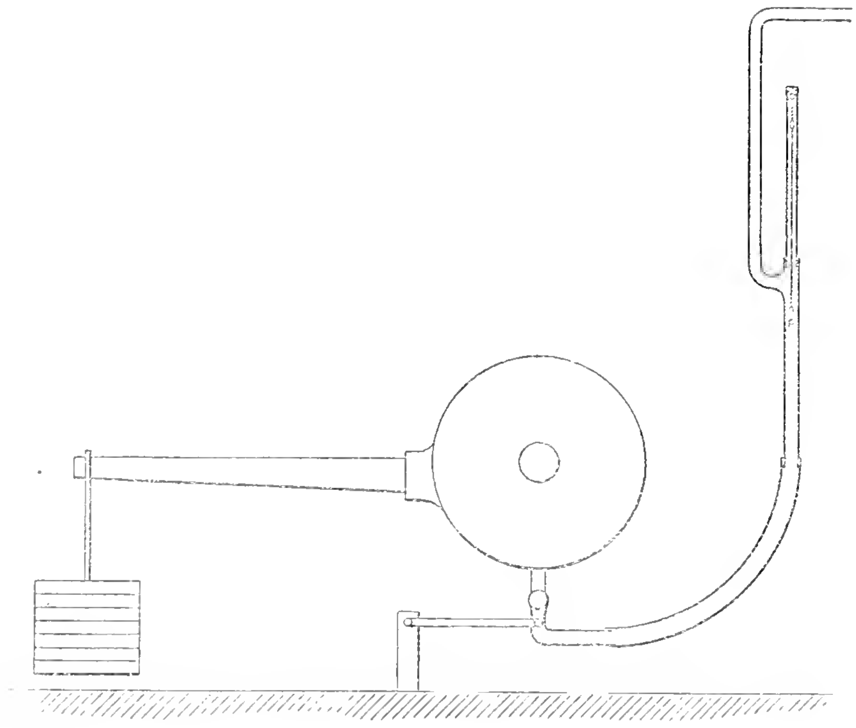
Weighing Machine and Tank.

13. The first step consisted in obtaining a somewhat special table weighing machine (Plates 4 to 6), having two rider weights on independent scales, one divided to 100 lbs. from 0 to 2200, the other to 1 lb. from 0 to 100. Also a galvanized iron tank, 5′ × 2′ 9″ × 2′ 9″, capable of holding above one ton of water, with a 4-inch screw valve at the bottom, opening inwards by a handle above the top of the tank, the top of the tank being covered with carefully fitted, but separate, ½-inch pine boards, previously steeped in melted paraffin-wax, to prevent adhesion or absorption of water. This machine and tank, which is a large affair, was placed in the only position available, opposite the end of the shaft and behind the standing pipes for supplying the condensing water to the engine, thus leaving the passage between these pipes and the end of the shaft open, an important matter, as this passage was the only place from which the observations on the brakes could be made. This entailed the carrying the outflow from the brake over the passage, about 6 feet 6 inches from the floor.

Design of the Outflow.

14. This extension of the pipe further entailed the necessity of making this pipe a fixture, and connecting it with the outlet below the automatic cock by a *bent* wire-bound flexible indiarubber pipe, so as to prevent any moment on the brake. (See fig. 6.)

Fig. 6.



The Thermometer Chambers.

15. A glass chamber for the outflow thermometer was introduced as shown (fig. 6), and another for the inlet, somewhat similar. These were arranged so that the bulbs of the thermometers were down in the full current while the scale was in the glass tube, through which a portion of the water was allowed to flow, that from the inlet thermometer being conducted away to waste, while that from the outlet was conducted back again into the outflow pipe. In this way, not only the bulbs of the thermometers, but the entire thermometers were immersed in the flowing water.

The Two-way Switch.

16. A switch, as shown in Plate 5, was also constructed for diverting suddenly the stream of effluent water from waste to the tank, or *vice versâ,* without exposing the stream for more than an inch, and without any splashing or uncertainty.

Experience in Making Observations.

17. When these arrangements were completed, and whilst the other appliances were progressing, Mr. Moorby commenced a series of experiments similar to those which had been previously made, using the water from the tank at the temperature of the town's water, and raising it to temperatures which were successively increased. This was with a view of testing the improved facilities, and also of gaining experience and facility in making and recording the observations.

The engines and brakes were occupied two or three times a week in the ordinary work of the laboratory, so that there were only one or two days a week available for these experiments, and every opportunity was valuable.

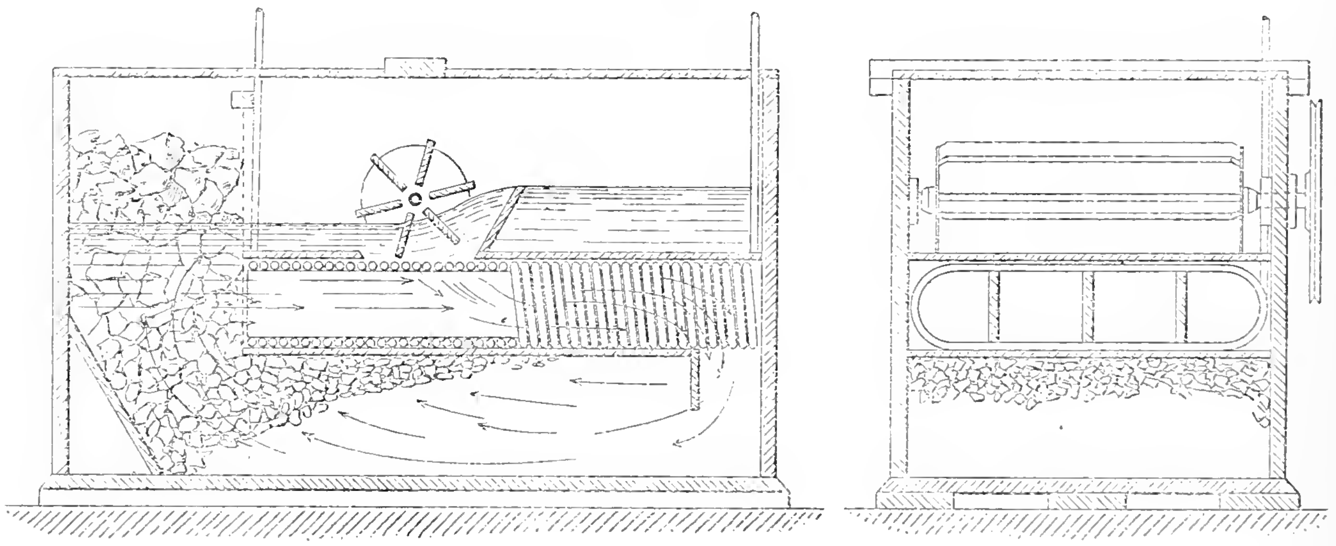
The Design of the Condenser.

18. At the same time he made experiments to determine the necessary length of pipe in order that the water flowing along it at the rate of 20 lbs. a minute would be cooled from 212° to 70°, when the pipe was jacketed by a stream of town's water at 50° Fahr.; by the result of which experiments the condenser in which the effluent water is cooled to 75° was designed (Plates 4 to 7).

Design of the Ice-Cooler.

19. To cool the water to 32°, or as near as practicable, I had, on account of the danger of some ice being carried through with the water if the ice were once put into the water, decided to pass the water through a long coil of ordinary water piping, immersed in water, towards the top of a tank with ice under the coil,

Fig. 7.



and from experiments made by Mr. Moorby, I decided on the coil and arrangements shown. The coil consists of $\frac{3}{8}$-inch composition pipe, 200 feet long, the tank being 2 feet 6 inches wide and deep and 4 feet long, the coil being placed near the surface of the water on a shelf with a wire netted space at the end for the introduction of the ice, which is pushed down under the shelf, and with a paddle which is kept in continual motion by a cord from the line shaft, thus securing a rapid circulation of the water. The tank is constructed of 1-inch pine saturated with paraffin wax, in preference to a metal tank.

In this design account had to be taken of the requisite head of water necessary to force some 20 lbs. a minute through the coil. It was estimated that this would require some 30 lbs. on the square inch, which, together with the 5 lbs. excess of pressure in the brake above the atmosphere, and a margin of some 25 lbs. in order to secure steadiness of flow, made a total of 60 lbs. on the square inch, or 122 feet of head.

The Circulating Pump.

20. It was essential that this head should be approximately steady, and under control during the trials, also that the water should be drawn as directly as possible from the town's mains, in order to secure both the low temperature and great purity of this water. This precluded the direct use of the water from the large tank in the tower, which would otherwise have just afforded this head. It also precluded the use of such head as there might be in the town's mains, as this was insufficient and continually varying, so that some special means of imparting the steady head to the water after drawing it from the tower mains was necessary. This involved pumping the water through the ice-cooler and brake. It might be done by pumping it from the service tank in the laboratory into an accumulator under a constant load, or by passing the water through a centrifugal pump, running at a steady speed on its way to the brake.

The facilities in the laboratory decided this question. There already existed the quadruple vortex turbine, with four three-inch wheels in series, worked from the water in the tower, which would work steadily up to 1 h.p., in a position which would be convenient for driving a centrifugal pump in the in-circuit of the pipe leading to the brake; I also had a quintuple centrifugal pump with five 1½-inch wheels in series which was adapted to the purpose. It was decided, therefore, to lead the water from the surface tank, 9 feet above the floor, into the quintuple pump, driven by the turbine under a constant and controllable head, so that the head would be raised to the required amount. Then, to lead the water through the cooling coils to a pressure gauge close to the brake, and thence through a regulating valve into the passage with the thermometer leading into the brake. (See Plates 6 and 7.)

The Outlet from the Condenser.

21. In order to prevent the formation of steam, owing to the presence of air in the water, before it had passed the outlet thermometer, it was necessary to maintain a certain pressure in the effluent water as it passed the bulb of this thermometer. At first it was thought that a head of 5 or 6 feet would suffice. In order to secure this, the level of the condenser being some 3 feet above the bulb, the pipe leading from the condenser was carried up vertically about 3 feet higher, then turned over and led down again to an orifice immediately over the switch, while from the top of the bend a vertical branch extended upwards about 3 feet, with its mouth open, to the air. This was subsequently raised. (See Plate 4.)

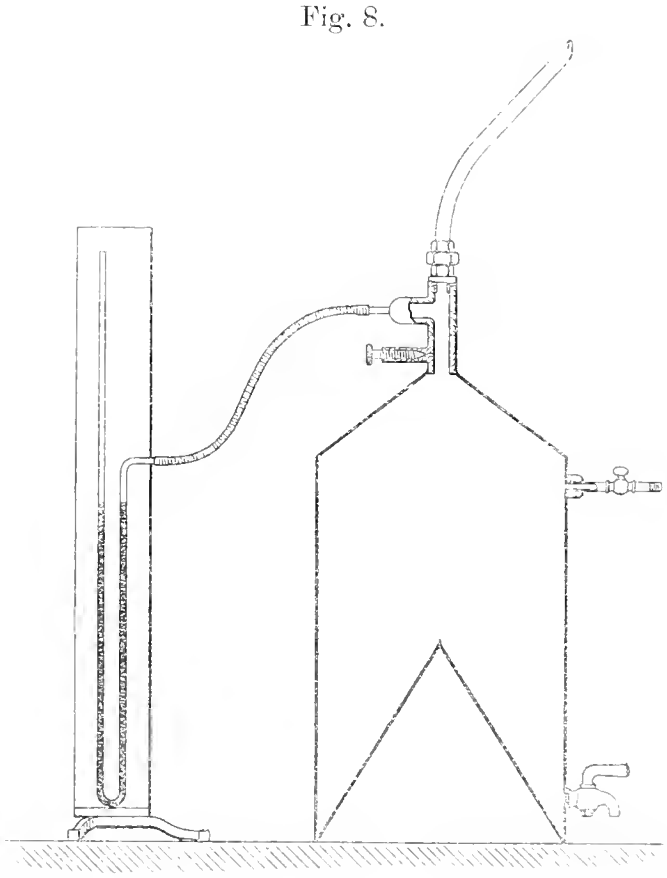
Preliminary Experiments at 212° Under Pressure.

22. The preliminary investigations and the construction of the appliances so far described, were not completed till May, 1895. It then became possible to make some experiments as to the working of the brake under pressure and at high temperature, so as to obtain guidance as to the artificial atmosphere and means of controlling the leakages at the bearings. From these experiments two things came out clearly. It was found that all that was necessary for an artificial atmosphere was to connect the outlet of the air passage on the top of the brake by means of a flexible india-rubber pipe capable of bearing the pressure to a vessel of very moderate capacity.

The Artificial Atmosphere.

23. A tin can, holding about 3 gallons, with the bottom and top coned upwards, and strong enough to stand the full pressure of 60 pounds, was adopted. The air connection with the can was at the top, at which there were also two side openings, one with a cock, to admit of air being pumped into the can, and the other with a fine

Fig. 8.



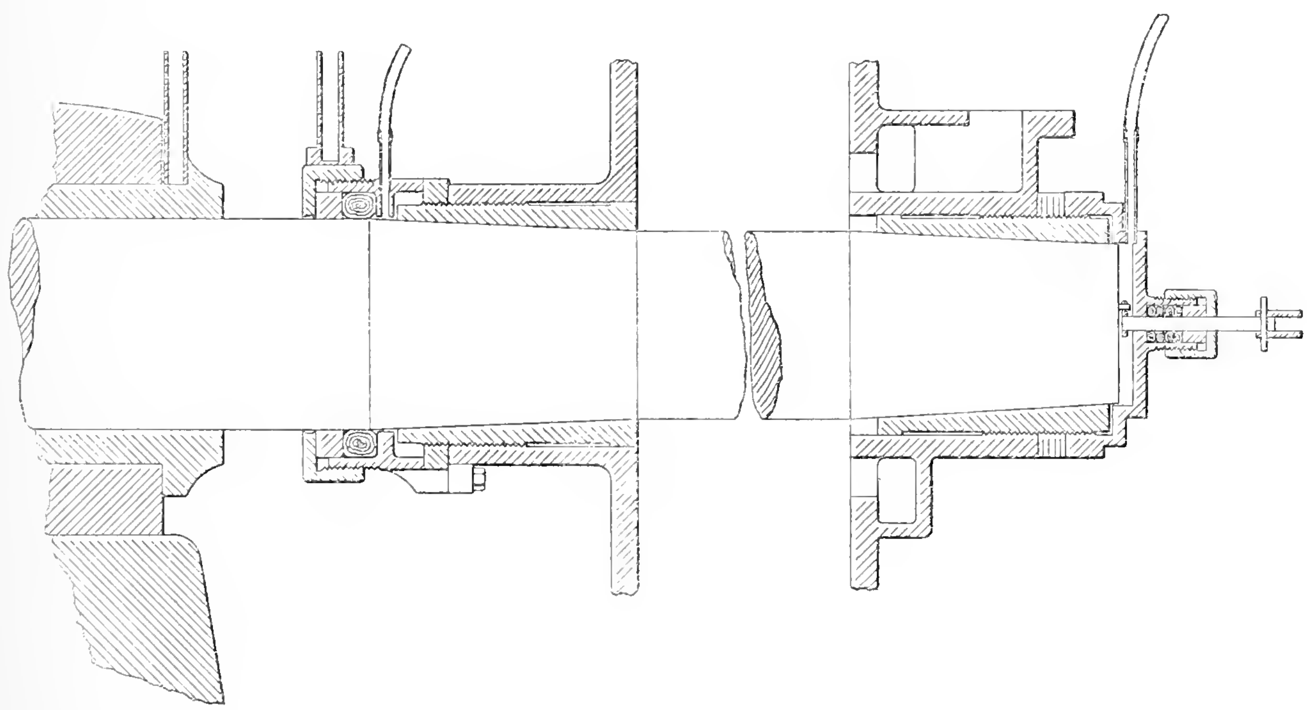
screw stop for allowing a slow and definite escape of air. An opening at the bottom, with a cock for drawing off water, was also provided. For forcing the air in, a syringe for inflating bicycle tyres was used in the first instance and proved ample; in fact, when once the pressure was raised, the small amount of air released from the water was more than sufficient to maintain the pressure, so that it was continually allowed to escape.

The Stuffing-box and Cap to prevent Leakage.

24. The thing that was revealed by the experiments at high temperatures was that the leakage of water at the coned bushes of the brake was so much increased by the pressure within the brake that even when these bushes were adjusted to run, as close as was practicable, on the cones of the shaft, this leakage was very considerable, so that some other method of controlling this escape became necessary.

This matter threatened to present great difficulties. It was apparently impossible to close in the bushes with stuffing-boxes and stop the leakage altogether, as that would prevent the lubrication of the shaft, and, apart from this, would cause the temperature on the shaft side of the brake to rise to the temperature of the brake, 212° Fahr., which would cause a large escape of heat along the shaft. Besides this, the adaptation of stuffing-boxes to the existing brake presented such difficulties that it almost seemed as though it would be necessary to have a new brake, which, besides the delay, would entail an addition of some £200 to the expenses, which were otherwise very considerable.

Fig. 9.



To avoid this I determined to try a stuffing-box on the shaft side, constructed in halves to be bolted together on the shaft, and then sweated into one, this stuffing-box to screw on to the exposed screw of the bush, and make a joint against the lock ring; then to open a passage through the box inside the packing-ring, with a tap to control the escape of water, and at the other end to screw a cap on to the bush, entirely inclosing the end of the shaft, with an aperture and a tap to regulate the water, also a small stuffing-box in the cap, to allow of a spindle for connecting the shaft with the counter.

These entailed very difficult and exceptional work, but were beautifully executed by Mr. FOSTER, in the Laboratory (fig. 9).

However, the result was very doubtful, as the water flowing from the brake through the aperture in the stuffing-box not only raised the temperature of the shaft, but was itself of uncertain temperature.

It was in July, 1895, that this experience was obtained, and for a time the success of the research seemed doubtful. During the vacation, however, an idea occurred to me which at once promised to do away with the whole difficulty.

The Cooling and Lubricating of the Bushes.

25. This idea consisted of what seemed to be a practicable plan of forcing a relatively small, but sufficient portion of the ice-cold water into the brake through each of the bearings, the quantities being strictly under control.

That this plan should not have presented itself as soon as the addition of the stuffing-box and the cap were contemplated, becomes intelligible when it is remembered that the main object in the invention of this brake had been to secure a constant pressure in the air space within the vortices, so that by admitting the water through passages in the vanes directly into this air space a constant resistance, whether that of the atmosphere, or artificial atmosphere, on the entering water would be secured, and that the possibility of maintaining an even flow through the brake, so essential to any success in the research, depended entirely on the realization of this constant resistance. Except the inlet passage, the interior of the wheel, and the air space in the vortices, all the spaces in the brake and brake-case are under the full vortex pressure, excepting where, as in the bush on the closed side of the brake, and that between the solid disc faces on the inlet side, the pressure is relaxed by the escape of the water. This vortex pressure depends on the load on the brake, and may be anything up to 25 pounds on the square inch greater than that in the air-cores. It thus seemed like starting *de novo* to interfere with this arrangement; and it was only when one came to realize that the possibility of preventing all leakage by the introduction of the stuffing-box and the cap had rendered it possible, by controlled subsidiary supplies under pressure, to reverse the flow of the lubricating water, and so to do away with leakage, and not only to secure lubrication, but also to cool the bushes, and then only after considering the amounts of water required, and the provision in the way of pumping appliances, separate supplies of water and thermometers, &c., that the altered facilities afforded by the circulating pump came to be recognized.

The By-channels and Regulator admitting Cooled Water to the Bushes.

26. Since the main supply must enter, as before, at the same pressure as the air within the vortices, while, in order to reverse the flow through the bushes, that entering the cap must enter at a little, but only a little, above that of the air within, while that entering on the brake side of the packing-ring in the stuffing-box must enter at any pressure up to 20 lbs., according to the load, above that of the air within, it was clear that there must be three supplies of water at different pressures under separate control; and it was equally clear that these supplies must all be at the same temperature.

Fortunately, the arrangements already made for the new supply afforded ready means of securing these conditions, as, in order to insure steadiness in the supply through the regulating valve, it had been provided, in arranging the pump, that there should be an excess of 20 lbs. on the square inch above that necessary to force the maximum water through the coil and to overcome the air pressure in the brake; also, as the regulating cock was only an inch or two from the thermometer chamber, the water would be subject to little heating by radiation after leaving the cock, while the effect of radiation to the by-channels would be of secondary importance, as it is eliminated with the rest of the radiation in the difference of the trials.

It thus became possible, by leading cooled water through two short by-branches, with separate regulators, from the supply pipe, before passing the main regulator, respectively into the aperture through the stuffing-box on the inside of packing-ring, and into the cap on the inlet end, to secure controlled inflows of ice-cold water between each of the bushes and the shaft, and so to adjust the temperature of the bearing and insure lubrication of the shaft (fig. 9).

In order to render such inflows steady and constant, it was desirable that the pressures before passing the regulator should be kept at a considerable and constant quantity above the vortex pressure in the brakes.

From the first preliminary trials made with the branches it appeared that the turbine and pump were capable of supplying sufficient pressure for this, so that the only additions necessary were the branches. These were made of $\frac{1}{4}$-inch brass pipe from the main pipe from the cooler as far as the branch regulators, and thence continued by $\frac{1}{8}$-inch indiarubber vacuum tube $\frac{3}{4}$ inch outside wrapped with tape. The branch regulators have cocks, with provision for fine adjustment, so that the very small quantities which passed might be definitely regulated to great nicety (Plate 5). With these it was found practical to maintain the temperature of the bushes from anything a few degrees above 32 to any required temperature.

It is to be noticed that the work done by pressure over and above the pressure p_a in the inlet thermometer chamber is that due to the difference between the pressure in the main pipe before passing the regulators and p_a, through whichever passage the water enters. And since in that water which passes into the thermometer chamber through the main regulator this work has been converted into heat, and is measured as entering heat by the inlet thermometer, the assumption that the water through the branches enters at the pressure p_a, and the temperature given by the inlet thermometer, involves no other error than that resulting from radiation, which is constant for all trials, and is eliminated in the difference.

The Regulation of the Temperature of the Bushes.

27. In the preliminary trials this temperature was only ascertained by touch, and regulated so as to be as nearly as possible that of the laboratory, the branch cocks being set with a definite opening, and the excess of pressure maintained as nearly as possible constant, a plan which was found to give consistent results. But it also appeared that in order to maintain the same temperature in the stuffing-box for the large and small trials with the same pressure in the main pipe, it was necessary to open the branch cock wider in the large trials. This was to be expected from the greater vortex pressure in the large trials. And as owing to the greater resistance of the cooler in the large trials there was difficulty in maintaining a great excess of pressure over the vortex pressure, it was decided to run both large and small trials with the same setting of the cock, and the same head in the cooling pipe, keeping a record until some means was obtained of estimating the comparative slopes of temperature in the shaft in the large and small trials.

The Measurement of the Comparative Slopes of Temperature in the Shaft.

28. The desirability of some more definite knowledge of the slope of temperature in the shaft between the brass of the nearest shaft bearing and the stuffing-box was strongly felt, but it was not at first apparent how this might be done, the shaft being 4 inches in diameter and the gap between the end of the stuffing-box and the brass of the bearing being only 3 inches.

However, as it became more evident, with the branch cocks set at a constant opening and the same pressures in the supply pipe, that the temperatures in the stuffing-box were greater in the large than in the small trials, and that a small difference in the adjustment of the branch cock to the stuffing-box affected the apparent loss of heat to the extent of some 0·1 or 0·2 per cent. of the total heat, I determined to try and obtain some definite evidence of the relative slopes of temperature in the two trials, by measuring the relative temperatures of the brass and the stuffing-box as far as was practicable. For this purpose, I had thick brass tubes, radiating outwards, sweated on to the end of the stuffing-box to hold thermometers. Two such tubes were necessary on account of the screwing-up of the box, which had to be done whenever it began to leak ; and although this was not done during a trial, one tube would sometimes face downwards, which was inconvenient. In a similar manner two tubes were attached, one to the top and one to the bottom brass of the bearing, holes being bored into the brass and the tubes screwed in. These tubes are shown in fig. 9.

In this way, with a thermometer in one of the tubes on the stuffing-box and one in each of the tubes on the bearing, although the thermometers might not give the actual temperatures of anything in particular, still the steadiness of the conditions of the brake warranted the conclusion that the differences in the readings of the thermometers would serve to identify similar conditions as to slope of temperature, and this turned out to be the case.

These thermometers threw a flood of light on to conditions which had before been hardly perceptible. Thus, after reading the thermometer during three large trials and three small trials, with the cocks set as before without having been displaced, and with the same pressures, it was found that the mean of the three large trials indicated 13° Fahr. greater slope from the stuffing-box to the brass than that indicated by the mean of the three small trials.

The Constants and Limits of Error of Conduction.

29. It thence became possible in the subsequent trials, by adjusting the cocks, to bring about a mean condition in which the mean slope in the large trials was the same as that in the small, and by comparing the mean results of those trials in which the difference of slope had been in one direction with the mean of those in which it had been in the opposite, to obtain a constant expressing the quantity of heat lost for each degree of the recorded slope.

These thermometers, read to 1° Fahr. 7 times during the trial of each sort, would give a limit of error of the $\frac{1}{7}$ of a degree which, taking 12 thermal units per hour as the loss per degree, would give as limit of relative error on 100,000 thermal units of, on one trial,

$$0{\cdot}00002,$$

and these being casual, when taken over 40 trials would be less than a millionth.

The Hand-Brake for Regulating the Speed of the Engines.

30. Although it had been found possible to maintain the speed of the engine constant within 2 or 3 per cent. when the engines were working with a considerable margin of pressure in the boiler, by maintaining the pressure in the boiler constant, the care and attention on the part of Mr. J. Hall, who had charge of the engine, became excessive when the engines were indicating over 80 h.p., particularly as he could not be attending to the fire and lubrication, and at the same time watching the speed indicator. To meet this difficulty, as there is no known automatic governor

which will regulate an engine working against a resistance which is independent of the speed, without fluctuations, I arranged a hand-brake to be applied to the rope pulley, 3 feet in diameter, on the brake shaft, by one of the assistants in the laboratory during the trial. The amount of power to be absorbed by this being less than 2 h.p. at the most, a $\frac{3}{8}$-inch cotton rope, with one end fast, passed round in one of the grooves of the pulley, the other end being attached to a spring balance, the position of which could be regulated with a screw, would answer the purpose (shown in Plate 5).

In this way, as the natural variations of speed of the engines are very slow, Mr. MATHEWS was able, after a little experience, to keep the speed to within something like one revolution, or 0·3 per cent.

The Corrections for the Terminal Heat of the Brake.

31. As the temperature of the effluent water could be continually regulated by regulating the supply of water to the brake, whatever might be the speed, the chief importance of keeping the speed regular arose from the errors (1) caused by small differences of temperature in the brake together with the water it contained at the commencement and end of the trial, and (2) by small differences in the weight of water in the brake at the commencement and end of the trial.

Such errors belong to the class of casual errors to be eliminated in the mean of a number of trials. Still, it seemed desirable to have some assurance that such elimination was effected, and, in order to obtain this, I proposed that the actual quantity of water in the brake for each of the loads used in the experiments should be determined experimentally at several speeds covering the range of variations likely to occur, and so to obtain a curve for each load, showing the water at each particular speed; this to be done by running the brake as in the trials, steadily, at a particular speed, the water passing as in the trial. Then, suddenly, by forcing down the lever, to close the automatic outlet valve, and, at the same time shutting the inlet valve and stopping the engines, and thus trapping the working charge of water in the brake. The water could then be drawn out and weighed.

Putting B for the capacity for heat of the metal of the brake, w for the weight of water, and T for the temperature observed on the effluent thermometer, the total heat in the brake is expressed by

$$(B + w)\, T^\circ,$$

and, if w_i, T_i° refer to the weight of water and temperature at starting, and w_f, T_f° to the corresponding quantities at the end of the trial, the correction which has to be subtracted from the heat observed is expressed by

$$(B + w_i)\, T_i^\circ - (B + w_f)\, T_f^\circ.$$

The Method of Conducting the Trials—Elimination of Radiation.

32. The entire system of working was designed to secure the most perfect elimination of radiation possible. Thus, it was arranged in the first place that the trials be made in pairs, one heavy trial and one light trial, made under circumstances as nearly similar as possible, except in respect of load and water. The loads in the first instance being 1200 and 600 foot pounds, and the quantities of water such that the final temperature should be as nearly as possible 212° Fahr., and, after the preliminary trials, 300 revolutions per minute was adopted as the speed for all the trials, 60 minutes as the time of running. The inlet and outlet thermometers to be read after the first minute, and every two minutes; also the temperature of the laboratory as shown by a thermometer in a carefully-chosen place. This temperature to be maintained as nearly constant as possible. The setting of the regulators during each trial to be recorded; also the pressure of the artificial atmosphere, and that in the supply pipe after passing the coil; and, subsequently, the reading of the thermometers in the stuffing-box and bearings taken every five minutes, and the speed gauge every two minutes. The observations and incidents being recorded by the rules in surveying, in ink, in a book, and distinct from any reductions. The initial and final reading on the scales and counter being included, as were also the initial and final readings of the inlet and outlet thermometers and speed gauge for the purpose of determining the terminal differences of the heat in the brakes.

As it was impossible to make trials simultaneously, and so secure similar conditions in the laboratory, it was at first arranged that the trials should be made in groups, including four pairs of trials.

The regular work in the laboratory monopolised the engines and brakes on all days in term time, except Mondays and Thursdays, so that the trials were confined to two days in the week. There was a certain likelihood of the state of temperature of the walls and objects in the laboratory being systematically different on the Mondays, after the laboratory had been without steam all Sunday, from what it would be on the Thursday, after the steam had been on for three days. And besides this, there would be a systematic difference in the temperature of all the objects during the first trial in the day, although the brake had been running for an hour before, from that which would hold in the following trials. In the first instance, therefore, it was arranged that a heavy and a light trial should be made on the same day, and a light and a heavy trial on the next available day, under as nearly similar circumstances as possible, except for the inversion of the order. Then again, a light and a heavy trial on the next day, followed by a heavy and a light on the following, so as to break the order and secure the same arrangement, in days of the week as well as in hours of the day, for the four light trials as for the four heavy trials.

As the results of any group of four pairs of trials would furnish a tolerably close approximation to the loss of heat by radiation, assuming this to be proportional to the observed mean difference of temperature between the laboratory and the *brake*, it was easy to obtain an approximate constant, R, for radiation for each degree of difference of temperature, and so to introduce a correction, $R(T_2 - T_a)$, in each trial for the radiation resulting from the observed mean difference of temperature of laboratory and brake, $T_2 - T_a$.

These corrections would serve two purposes—first, affording a better comparison of the results of the separate trials for future guidance, and secondly, by recording the mean difference of temperature, would show how far the mean differences of temperature in the large trials had differed from those in the small trials, and thus how far the radiation had been eliminated.

Lagging the Brakes.

33. In order to obtain still more definite assurance as to the elimination, it was arranged that after consistent results had been obtained in several groups of four pairs of trials, as above, with the brake naked, the brake should be covered with non-conducting material, in the best way practicable, so as greatly to reduce the radiation, at the same time leaving it definite, and then similar trials should be run.

If the coefficient of radiation could in this way be reduced to one-fourth that of the naked brake, such error as there might be remaining in the mean results with the naked brake would be reduced to one-fourth with the lagged brake.

In this, however, there was danger of introducing errors of other kinds.

The non-conducting material would absorb heat slowly and take a long time to arrive at a state of equilibrium, and during the interval the rate of loss of heat from the brake would be irregular. The total error that could result from this cause would be the product of the specific heat of the material used multiplied by the weight, and again by the 75°, or the half of whatever was the difference in temperature of the brake and the air. This decided the choice of the material to include cotton wool. Two pounds of this would, if not too tightly pressed, cover the brake about $1\frac{1}{2}$ inches thick, and the total heat it would absorb would be less than 0·4 lb. of water raised from 32° to 212° Fahr., and would then be only 0·0008 of the heat generated by 30 h.p. in an hour, while it would reduce the radiation to about $\frac{1}{7}$. But as the cotton wool would gradually collapse if subjected to any elastic pressure, it was decided only to use this to such thickness as it could be protected by light cotton strings extending in axial planes round the brake, and to prevent absorption of moisture by the cotton wool, to cover it with thick anti-rheumatic flannel, about 1 inch to $1\frac{1}{2}$ inches in thickness, as shown in Plate 5, which would raise the capacity for heat of the entire lagging to about $\frac{1}{600}$ that of the heat generated in the small trials, and as the brake was kept at steady temperature for about one hour or more before the trial commenced, the actual differences would not exceed some one ten-thousandth part.

The Conduction by the Levers.

This lagging only extended over the body of the brake covering all the brass-work, leaving the levers and balance weights on the levers bare.

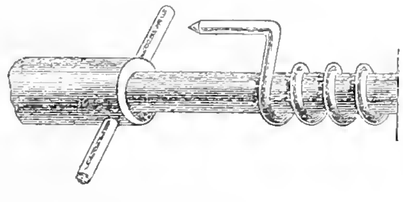
These levers being in metallic contact with the brass of the brake assumed at these points the temperature of the brake, and would conduct the heat along to the balance weights till it was lost by radiation. As the temperatures were constant in all the trials this loss of heat would merely form part of the radiation and be eliminated as the rest; but, owing to the masses of the balance weights and the length of the levers, it must take a long time for the balance weights and the further parts of the levers to arrive at a steady temperature, a fact which would account for a greater loss of heat in the first trial made in the day.

In order to obtain assurance that this delay produced no error it was arranged that after the completion of the series of trials with the brakes lagged, corresponding to that with the naked brakes, that the balance weights should be removed, and only the load at 4 feet from the brake left, and a third series of trials made.

Starting and Stopping the Trials.

34. Having adopted an hour as the length of each trial, and 300 revolutions as the normal speed, the engines having been running for an hour previously, while the water entering the brake was being adjusted, and afterwards, so as to insure the temperature, not only of the brake, but of the surrounding objects, having become approximately steady at the time of starting the trial, all that was necessary was that the counter should be pushed into gear, and at the same time the water-switch pushed over, and the reverse operation at the end of the trial. These operations, simple as they were, entailed errors, which arose partly from the impossibility of instan-

Fig. 10.



taneous engagement of the counter simultaneously with the switching of the water. In order to diminish these as far as possible, the spindle of a counter, on which was the worm which drove the worm wheel, was wrapped with a spiral spring of steel wire, which gripped the spindle so tight that it would not slip, the end of the wire being bent, so as to form a clutch standing off the shaft half-an-inch, the end of the wire being pointed, the shaft of the counter projecting a little beyond the wire. Facing the end of this shaft, and in line with it, was a socket in the end of the engine shaft, which was brought down to three-quarters of an inch diameter and carried two round pins, a sixteenth of an inch diameter, standing out radially, the engagement being effected by pushing the counter forward till the wire crank engaged on one of the pins. (Owing to the wire being pointed and the pins rounded, the chance of the wire striking plumb on to the pin and so preventing engagement was reduced to a minimum.)

This engagement was the result of a great deal of experience, and answered perfectly, but it involved the mean chance of a quarter of a revolution of the engine shaft after the wire had passed the pin before the actual engagement was effected, whereas on coming off the disengagement was instantaneous, the counter stopping by the friction of the worm before the momentum had carried it through any appreciable angle.

This would leave a mean error of the work done during one-fourth of a revolution on each trial, whence, the number of revolutions during the trial being 18,000, the relative mean correction would be one seventy-two thousandth part, or 0·000013. As, however, when the two operations were executed by different observers on a signal, the personal equations might amount to more than this, although it involved a difficult piece of linkage, an automatic connection was effected, as shown in Plate 5, the pushing of the counter into engagement shifting the switch, so that in making the trials no error was introduced.

The Leakage of Water.

35. As the loss of any of the water, which had entered the brake before it was weighed, would constitute a corresponding error in the results, the perfect tightness, not only of all the fixed joints, but of the casting and the pipes, was a matter of first consideration and of continual care. This was one of the reasons why the lagging was delayed till after consistent results had been obtained; for, as long as the brake and pipes were naked, such leakage could not fail to be observed on close inspection, and before lagging it was arranged to test the brake and pipes to an excess of pressure, so as to insure perfect soundness. Besides the fixed joints there were only two working joints, in addition to the openings into the switch and again into the tank.

(1) The working joints were: The stuffing-box on the main shaft and the stuffing-box on the automatic cock on the outlet from the brake.

Any leakage from these was open to observation both before and after lagging, as they were in no way covered; and arrangements were made so that such leakage could be separately conducted by pipes and caught in bottles. With care such leakage could be reduced to insignificant quantities.

The absolute loss of heat resulting from a leakage of $w_{\text{S.B}}$ lbs. of water from the stuffing-box on the shaft was equal to the product of the difference of temperature of the stuffing-box $T_{\text{S.B}}{}^{\circ}$, and inlet $(T_1{}^{\circ})$ multiplied by $w_{\text{S.B}}$:

$$w_{\text{S.B}}\,(T_{\text{S.B}}{}^{\circ} - T_1{}^{\circ}),$$

and in the few trials in which this became a sensible quantity it was to be added as a correction.

The Loss of Heat by the Leakage of Water from the Automatic Cock.

36. This was the product of (w_c), the weight of water which escaped multiplied by the total rise of temperature. Since the water passing the cock was on its way to the high temperature thermometer, where any such water was caught it was put into the tank, and so required no correction. This leakage was very small, at most 2 oz. in a trial, but as there must be some evaporation as the water escaped through the hot gland, which, though small, might be of some importance on account of the latent heat of evaporation, it was desirable in some way to enclose this stuffing-box in an indiarubber bag closing on the spindle, so that the vapour could not escape, and this was eventually accomplished very effectively and neatly by Mr. FOSTER, in a way which did not interfere at all with the free action of the cock (Art. 14, Part II.).

The result of this, besides preventing any subsequent loss of water, in this way, was to show that any error that had previously existed from evaporation was inappreciable.

The Loss of Water at the Switch.

37. Apart from evaporation which would result from the exposure to the air and in passing the air-gap into the switch, there was no loss, as the water descended almost tangentially on to the surface of the tube on the switch which received it, the switch itself being a vertical knife-edge extension of this surface, which passed through the vertically descending water at starting and stopping; and further, to prevent any minute drops of water going astray from the bursting of an occasional bubble in passing, a sheet brass hood was placed round the descending pipe directly the trial started.

The outside of the weighing tank is completely exposed to observation, and is perfectly tight. The valve in the bottom, being a 4-inch leather-faced screw-valve on a brass seat, is also tight, but for satisfaction it was arranged to place a clean tin dish under the valve before starting a trial, and only to remove it after the water was weighed, so that there should be absolutely no loss of water from any of these causes.

That there must be some loss of water by evaporation to the air as long as the temperature of the water, after leaving the condenser, was above that of the dew-point of the surrounding air, was certain. By using sufficient cooling water it would be possible to bring the temperature down to that of the dew-point; but it was found that this could not be done under all circumstances without a larger condenser, for which room was wanting, and, as long as the water lost by evaporation was the same in both trials, all error would be eliminated in the difference of the large and small trials. After careful consideration, it was arranged that the condensing water should be adjusted so that the water in all trials entered the tank at a temperature as nearly as possible 85°; it being probable, as the surface exposed to the air was nearly the same in the large and small trials, if the differences in temperature between the air and the water were the same, the evaporation would be the same, or would at least differ by a constant amount. In order to test this, it was further arranged that, after the trials were finished, the centrifugal pump should be temporarily re-arranged so that it could be used to draw water out of the tank and force it round through the condenser and switch, and so back again into the tank at rates corresponding to those of the large and small trials, and at the same temperature (85°), the water in the tank being at this temperature, the arrangement of the pump being such that, when stopped, all the water in the pipes would run back again into the tank. This would practically insure the same loss of water by evaporation during one hour's pumping as during one hour's trials, and any difference (w_ϵ) thus established between the large and small trials would then be treated as a standing correction on the difference of the heavy and light trials. This relative correction, taking W as the mean difference of water in the heavy and light trials, would be

$$\frac{w_\epsilon}{W}.$$

The Standards of Measurement.

38. In these experiments the expressions obtained for the work done in heating the water and the heat generated are, respectively,

$$2\pi N \,.\, RW_i \quad \text{and} \quad SW_\omega (T_2^\circ - T_1^\circ),$$

where R, W, T°, S are respectively length, weight, temperature, and capacity for heat.

Since these expressions both represent the same absolute quantity of energy, the difference in the numerical values of these expressions results only from the difference in the units in the two expressions. These units may be considered as the unit of work and the unit of heat respectively, as it is the inverse ratio of these units, measured in absolute quantities of energy, that is expressed by the ratio obtained from

$$\frac{2\pi NRW_i}{SW_\omega (T_2 - T_1)}.$$

But, as there are no actual standards either of work or heat with which quantities of work and heat can be respectively compared by a simple measurement, such comparisons can only be accomplished by the comparison of the several factors involved in each of these expressions with the several absolute standards which exist for such factors.

These standards are the standards of mass, length, and force, on the one hand, and of mass, quality of matter, and temperature, on the other.

Thus, work being defined as the mean product of force multiplied by the distance, and the standard of force being the force of gravitation on the unit of mass wherever it occurs, the work is represented by $W.h$, where W expresses the number of units of mass, and h the number of units of length through which it has been raised. Taking (M) and (L) as expressing these units, the unit of work is expressed as (ML).

Again, the unit of heat is defined to be one n^{th} part of that quantity which is required to raise one unit of mass (M) of a standard substance (pure water) from one definite state of temperature to another definite state. And calling this interval θ, the unit of temperature is defined to be θ/n. And, taking S to express the ratio of the number of units of heat required to raise W_ω units of mass of matter from $T_1°$ to $T_2°$ compared with $W_\omega(T_2° - T_1°)$, the heat expressed by $SW_\omega(T_2 - T_1)$ is in units $\left(M\frac{\theta}{n}\right)$.

So that, from the physical equivalence of the absolute energy expressed in the respective forms, it appears that the unit of heat, as defined by $\left(M\frac{\theta}{n}\right)$, is equivalent to

$$\frac{2\pi NRW}{SW_\omega(T_2-T_1)} \text{ units of work as defined by (ML),}$$

or that the heat required to raise one unit of mass of pure water through the definite interval of temperature θ is equivalent to

$$n\frac{2\pi NRW}{SW_\omega(T_2 - T_1)} \text{ units of work (ML).}$$

This is the definition of the mechanical equivalent of heat in Manchester, adopted by JOULE if $n = 1$, and θ is 1° Fahr. between 50 and 60, as determined on his thermometer. But, since the absolute kinetic value of the unit of force as here defined varies with the latitude and height of the place, while that of the unit of heat is constant, this mechanical equivalent varies from place to place with $1/g$, where g is the expression, in kinetic units, for the unit of force (M).

Thus, expressing the work in kinetic units, the unit of heat, as already defined, is equivalent to

$$g\frac{2\pi NRW}{SW_\omega(T_2 - T_1)} = C,$$

where the dimensions of C are $(L^2T^{-2}n\theta^{-1})$.

Whence, since g has dimension (LT^{-2}),

$$\frac{2\pi NRW}{SW_\omega (T_2 - T_1)} = \frac{C}{g},$$

where the dimensions of C/g are $(Ln\theta^{-1})$.

The object in this research being to replace the standard of temperature, as defined by the scale on a particular thermometer, by the standard obtained from the states physically defined by melting ice and by water boiling under a standard pressure, θ is here defined to express this interval, and S is, in accordance with the definition already given, used to express the ratio which the heat required to raise unit mass over any interval, per degree of rise, bears to that required to raise pure water over the interval θ, per degree of rise.

The Standards Involved.

39. It appears from the dimensions of C/g, as obtained in the last article, that the only general standards to which reference need be made are those of length and temperature.

It is, however, to be noticed that the determination of the work and the heat involve the determination of separate masses, and that the units only disappear on the condition that they are equal.

The Measurement of Mass.

40. Since it was not necessary to refer the mass to a general standard, the weights used were only referred to a Board of Trade standard for convenience.

Thirteen of the 25 lb. weights used for loading the brake were adjusted to the Board of Trade weight, then carefully balanced against each other, till, balanced in groups of four in any arrangement, there was less than 0·01 lb. difference. Four of these weights were then taken as the standard.

The compound lever machine, which had two scales on the same lever, one notched to each 100 lbs. for the position of the large rider, the other with a flat scale for every 1 lb. for the position of the small rider, was taken to pieces and the knife edges re-ground and re-set (by MR. FOSTER) till consistent results were obtained to the one-hundredth of 1 lb. Another rider was also made to work on the same scale as the small rider, being adjusted to one-hundredth of the weight, so as to read 0·01 lb.

The scales were then carefully surveyed by the standard 100 lb. weight, the original small rider being adjusted till the difference between its extreme positions on the scale balanced the standard to $<$ 0·01 lb., and the corrections for each V-notch into which the feather on the large rider fitted ascertained by balancing the standard to a like degree of accuracy.

The dead load on the scales, including the empty tank, came to 340 lbs., about, and between this and 2200 lbs. the scales would weigh any quantity with the lever swinging to 0·01 lb.

The weights to which the scales had been adjusted were then exclusively used on the brake. Thus the brake was balanced by the same weights as were used as the standard in weighing the water, with a sensitiveness which gave the error less than one forty-thousandth part of the weight of water in the smallest trials, while the casual error, which would not exceed this in a single weighing, would be eliminated in the mean of a large number of weighings. Thus the relative limits of error in weighing would not exceed ·000025.

The Correction for the Weight of the Atmosphere.

41. The balances being made in air it is necessary to add the weight of air displaced in each case.

As the relative weights only are concerned, if D_a is the weight of a unit volume of air, D_w that of water, and D_i that of cast-iron, the weights in air of unit masses are :—

$$1 - D_a/D_w \quad \ldots \ldots \quad \text{for water,}$$

$$1 - D_a/D_i \quad \ldots \ldots \quad \text{for cast iron.}$$

The load on the brake is therefore subject to the correction expressed by the factor $(1 - D_a/D_i)$, while that of the water balanced against cast-iron weights, has the correction factor

$$\frac{1 - D_a/D_i}{1 - D_a/D_w},$$

and the relative correction for the actual weight of water, as against the load on the brake in air, is

$$1\Big/\left(1 - \frac{D_a}{D_w}\right) \text{ or approximately } 1 + \frac{D_a}{D_w},$$

for the temperature 67° Fahr., $D_a = 0·0752$, $D_w = 624$.

Hence, the relative correction factor for the equivalent

$$(1 - 0·001205).$$

The Correction for g in Latitude of Greenwich and 45°.

42. Since the latitude of Manchester is 53° 29′, Greenwich 51° 29′, the value of g being (" Mémoires sur le Pendule," ' Soc. Française de Physique ')

$$g_{45^\circ}(1 - 0{\cdot}00259 \cos 2\lambda) = g_{45^\circ}(1 + 0{\cdot}0007558) \text{ at Manchester,}$$

$$\text{,,} \qquad \text{,,} \qquad = g_{45^\circ}(1 + 0{\cdot}0005814) \text{ at Greenwich,}$$

whence the correction factor is . . . . $(1 + 0{\cdot}0001746)$ for Greenwich,
and for 45° . . . . $(1 + 0{\cdot}0007558)$.

The Specific Heat of the Water.

The standard capacity for heat being that of distilled water, the obvious course would have been to have used distilled water in the trials, had this been practicable; but as it was apparent from the first that the quantity of water which would have to pass through the brakes during the trials would amount to some 20,000 gallons, or, say, 100 tons, all of which would have to be brought down to a temperature of 32° Fahr.; and that to do this, using distilled water, whether or not the water was used over again, the necessary appliance for producing, storing and cooling the water, were impracticable in the laboratory, the last 40° must be removed with ice, and this would require some 25 or 30 tons of ice. While using the town's water direct from the main, the average temperature, from February to June, would not exceed 45°, so that only 12° or 13° would have to be removed by ice, which would require from 7 to 10 tons, with no other appliances except the relatively small appliance for cooling.

The only practical course, therefore, was to use the town's water. And had it not been for the known purity of this, the research would never have been undertaken.

As affording definite assurance of the consistent purity of this water, as delivered in the college, Professor DIXON kindly undertook to furnish the mean results of the analyses which he makes periodically for the Manchester Corporation, of the water drawn from the supply in the college. These show that the impurities are almost negligible, taking 0·2 as the specific heat of the salts, the relative correction is $0{\cdot}8s$, where s is the relative weight of the salts.

The Effect of Air in the Water.

43. Even distilled water contains air unless special precautions are taken for its removal; so that any effect such air may have on the capacity for heat as measured would not have been avoided by using distilled water.

The direct effect of the same 0·00323 per cent. of air which water exposed to the atmosphere usually contains at normal temperatures, is so small as to be altogether negligible, and it would seem to be an open question whether the standard condition of water, as regards capacity for heat, does not involve the inclusion of this air. But the indirect effect of such air on the heat necessary to raise water from normal temperatures to near the boiling-point, is by no means negligible.

It does not appear that any definite study has hitherto been made of this effect; but it is a matter of common observation that as water reaches a temperature some 40° Fahr. below the boiling-point, bubbles appear on the sides and bottom of the vessel, which gradually increase in size and rise to the surface, increasing rapidly in size as they rise. The bubbles are usually referred to as bubbles of gas or air. But, a moment's consideration will show that, although the air or gas is the immediate cause of the premature formation and subsequent expansion of the bubble, it is none the less certain that the space occupied by the bubble is filled with saturated steam at the temperature of the water, the function of the air being merely that of balancing the excess of pressure of the surrounding water over the pressure of the saturated steam.

It thus appears that every bubble so formed represents a quantity of heat which is the latent heat of the volume of the saturated steam in the bubble over and above the heat of the weight of water in this steam.

Thus, if bubbles of air exist in water at a temperature of 212° Fahr., the weight of air per lb. of water being a, 0·0000323, and p the pressure, in inches of mercury, of the water, then, since the pressure of the air is $p - 30$, and the volume of 1 lb. of air at 212° Fahr. under 30 inches of mercury is 16·9 cubic feet, the volume of air per lb. of water is

$$V = \frac{16{\cdot}9 \times 30}{p - 30} \times a,$$

or, if $p = 40$,

$$V = 50{\cdot}7 \times a.$$

This is the volume, in cubic feet, of saturated steam at 212°; whence, since the latent heat per cubic foot is 36·6 at 212°, the excess of heat will be per lb. of water

$$V \times 36{\cdot}6 = 1855 \times a,$$

and this, divided by 180°, gives a relative error

$$10{\cdot}31 \times a.$$

If $a = 0{\cdot}0000323$, the error is

$$0{\cdot}00033, \text{ or } 0{\cdot}033 \text{ per cent.}$$

The water, after being exposed to the atmosphere in the service reservoir, where it discharges any excess of air, enters the brake cold with this normal air, there it is heated by work, under the pressure of the artificial atmosphere at pressure p, to maintain which it parts with some of the air, which, in passing out into the flexible pipe, carries out saturated steam, which is condensed by radiation from the pipe. The water, with the remainder of the air, is then carried by the centrifugal pressure into the outer chamber in the brake case under a pressure of about 50 inches of mercury. It then passes the automatic cock into the flexible pipe at 41 inches pressure, thence rising to the thermometer bulbs at 40 inches. In passing the automatic cock with a difference of pressure of 9 inches, the pressure will be further reduced, probably 9 inches below that in the pipe, so that any air that might have been retained would come out at that point, and expand further as it approached the thermometer bulb.

In the first instance, it was thought that a pressure of 5 feet of water would prevent the formation of bubbles, and the air gap in the pipe leading from the condenser was placed at this height above the thermometer. But many, and sometimes large, bubbles of air were observed passing up the thermometer chamber; and Mr. MOORBY observed that he could detect the passage of a large bubble by a fall in the thermometer before the bubble appeared in the glass chamber.

To prevent this, the air-gap was raised till it was 12 feet above the thermometer bulb; so that the error is limited to three ten-thousandths. Even so, as it is much larger than any of the errors of constant sign, it was important to try, by assimilating the conditions under which the water leaves the brake, to obtain experimental evidence which would narrow the limits.

It may appear at first sight as though these losses from the air in the water would, like the radiation, be eliminated in the difference of the large and small trials, but this is not so, since the quantity of heat so lost is proportional to the amount of water used, or it may be greater in the heavy trials.

The Standard of Length.

44. The measures of length that the research involves are—

(1.) The horizontal distance of the centres of gravity of the adjustable loads on the brake from the axis of the shaft.

(2.) The vertical heights of the barometer at which the boiling-points of the water were determined.

In order to secure a definite reference of these to the British standard, recourse was had to two carefully-preserved and independent measures derived from this standard.

(1.) A set of gauges by Sir JOSEPH WHITWORTH and Co., consisting of three steel bars, 9, 6, and 3 inches respectively, with parallel plane ends $\frac{3}{8}$ inch in diameter, adapted to a 20,000 of an inch measuring machine, which constitute the standards used in the engineering laboratory.

(2.) A brass bar by Elliott and Co., 39 inches long, and graduated in inches, used as the standard in the physical department in Owens College.

From the Whitworth gauges, two steel bars, $\frac{3}{4}$ inch in diameter and 9 inches long, with parallel plane ends, were made by Mr. Foster, and compared with the 9-inch Whitworth bar by the measuring machine.

With these and the Whitworth gauges, placed end to end, an outside gauge consisting of two surfaced angle-plates on a surfaced cast-iron bed was set out, and then a steel bar $\frac{3}{4}$ inch in diameter with plane ends fitted to these. Careful comparison showed that this bar did not differ from the sum of the lengths of the gauges by $\frac{3}{10000}$ parts of an inch. This length was then carefully laid off by the surfaced angle plates on the surface plate, and was so compared with the scale of the Elliott brass bar, account being taken of the temperature, and found to agree within less than $\frac{3}{10000}$ of an inch.

The 30-inch bar so obtained was then taken as the standard both for the levers of the brake and the barometer, to be carefully preserved.

Lengths of the Levers.

45. The V-groove, in which the knife-edge of the carrier, by which the load on the brake was suspended, rested, was originally made at a distance of four feet from the axis of the shaft at ordinary temperatures, and, as whatever the error might be when the brakes were hot, it would be the same for all the trials, since the temperatures were the same, it was decided to take this as the length of the levers in estimating the loads during the progress of the research, and to treat whatever error there might be as a standing correction on the final results. Such correction to be obtained by laying off four feet less the radius of the shaft from the carefully squared end of a steel plate 3 inches broad, $\frac{3}{16}$ inch thick, then placing this, flat, in a vertical plane perpendicular to the shaft, with its edge horizontal, as near as practicable to the knife-edge groove with the squared end touching the shaft. Then by means of a theodolite, set so that its line of collimation was in a vertical plane parallel to the axis of the shaft, and intersecting the vertical line on the plate, to observe the distance of the groove from the line on the plate, while the brake was running under the same conditions of temperature, and load as in the trials; but with the carrier temporarily displaced further along the shaft, so as to leave the bottom of the V-groove visible through the theodolite and in this way to obtain the actual distance of the groove from the axis of the shaft as affected by the expansion of the brake and any displacement of the bearing on the shaft which might result from the running.

By using a scale divided to the one-hundredth of an inch, and taking several readings, this could be determined to a thousandth of an inch, so that the limits of accuracy would be

$$\pm 0{\cdot}00002.$$

The Standard of Temperature.

46. As the most general standard is the difference between the two physically fixed points of temperature corresponding to the temperature of ice melting under the pressure of the atmosphere and that of water boiling under a pressure corresponding to 760 millims. of ice-cold mercury in the latitude of 45°, taking account of the variation of g, the standard in Manchester is the interval between melting ice and water boiling under a pressure of $760 \times 1{\cdot}0001721$ millim. of ice-cold mercury, which corresponds to 29·899 inches. And this interval divided by 180 is one degree Fahr.

According to REGNAULT's tables a divergence of one thousandth of an inch from the boiling point would correspond to an error of 0·0017° Fahr., and this would be less than the one-hundred-thousandth part of 180°.

In order to obtain this degree of accuracy in comparing the pressure of the vapour of pure water, in which thermometers could be placed, with the height of mercury over a range of two or three degrees above, and two or three below the point, at almost any time, irrespective of what might be the actual pressure of the atmosphere, it was necessary that the barometer, or pressure gauge, while in free communication with the vapour chamber should be shut off from the atmosphere, and at the same time so far removed that the temperature of the mercury should not be affected by the heat from the gas or boiling water. And, further, although in direct communication with the vapour, this must be such that no moisture could reach the mercury; and, such as involved no current in the passages which might affect the relative pressures, as would result by the interposition of a condensing vessel.

It was also necessary that the arrangements for reading the vertical distances between the upper and lower surfaces of the mercury should not only give absolute differences of height, but also that they should afford ready means of at any time determining the presence of vapour or gas, other than that of mercury, in the upper limb of the barometer.

The Barometer.

47. To meet these requirements, the barometer shown (Plate 8) was designed. The vessel which holds the mercury consists of a bottle-shaped casting of iron, 3 inches in diameter. Through a stuffing-box in the neck of this, the stem of the barometer tube passes. To admit of reading the level of the surface of the mercury in the bottle, two parallel plate-glass windows are arranged, $\frac{3}{4}$ inch diameter, having their axis $\frac{3}{4}$ inch from the axis of the bottle. These are sunk into the casting so as to leave the outer cylindrical surface of the bottle clear, the joints between the glass and the cast-iron being faced and made tight with a trace of beeswax, the other openings into the bottle being one for the admission and abstraction of mercury, fitted with a screwed valve, and one for the admission of air, with a mouthpiece for the attachment of a tube from the vapour chamber.

The glass stem of the barometer is drawn down into a neck towards the lower end, and this is bent through 180° so as to bring the mouth upwards, and thus admit of its introduction into the mercury in the bottle without letting in air. This bend has to be passed through the stuffing-box, then the tube is secured by screwing the gland on to the beeswax stopping. A brass guard tube is then screwed into the neck, to support the glass tube, to a height of 24 inches from the mercury in the vessel.

For reading the height of the lower limb, a cylindrical brass curtain, with a conical contraction on the top, the aperture in which is threaded internally at twenty threads to an inch to correspond to the screw on the outside of the neck of the bottle, is screwed on to this neck, the lip or bottom of the curtain being truly turned so that, when screwed down to the level of the mercury, it cuts off the light through the windows from a white sheet behind.

To the top of the brass casting, which forms the curtain, a brass cylindrical tube is rigidly attached coaxial with the curtain which fits over the brass guard round the barometer tube, this extends to a height of 26 inches from the lower lip, the internal diameter for the last inch being a little smaller and internally screwed at twenty threads to an inch. Into this is screwed a brass tube, externally screwed throughout its length, about 6 inches long, with parallel opposite slots $\frac{1}{8}$ inch wide extending to within an inch at either end, to form windows through which to see the light over the upper limb of the mercury. And on to the upper portion of this tube there is screwed a long cap, capable of screwing down to the bottom of the slot. The lower lip of this cap forms the curtain which cuts off the light when the lip is level with the upper limb of the mercury.

By this arrangement the variation of the distance between the lips of the lower and the upper curtains depends only on the change in their relative angular positions. For, since the slotted tube has a uniform thread, it can be turned, screwing into the lower curtain and out of the upper, both of which remain unmoved. Thus the position of the windows may be fixed, while the curtains are moved. So that for reading the distances it is only necessary to measure the relative angle.

This angle is measured by dividing the circumference of the cap just above the lip into five equal divisions, from 0 to 5, and these again into ten, then a turn through one of the smaller divisions means an alteration in the distance of one-fiftieth of one-twentieth of an inch. As this angle is measured relatively to the lower curtain, a vertical brass scale, divided to tenths and twentieths of an inch, is fixed externally to the top of the extension of the lower curtain, extending vertically just outside the graduated limb of the upper curtain, and thus serves for reading the angular distance of the index mark on the limb of the upper curtain on any particular thread and the number of threads from the index on the scale.

The Adjustment of the Indices on the Barometer.

48. The lower curtain, together with the slotted tube and cap, is unscrewed from the neck of the cast-iron bottle and lifted off over the tube. Then the 30-inch standard bar is set on end upright on a surface plate, and the lower curtain, &c., are lowered over the bar until the lower lip of the curtain rests on the surface plate, and the top of the bar is 30 inches from this lip. The cap is then screwed down until light is seen over the top of the bar through the slot just cut off. Then a vertical line, drawn on the cap just above the lip, at the edge of the scale is the index on the cap, and a horizontal line, drawn on the scale level with the lip of the cap, is the index point on the scale. And, when these two lines are brought into this position, the distance between the lips will equal the length of the bar.

In order to check this the curtain is raised, and two thin pieces of chemical paper are placed on the surface plate, one on each side of the bar, so as to leave a space between the paper and the bar. Then the curtain is replaced so that it rests on the paper, and light can be seen through the interval between the paper and the bar. Then light should be seen to an equal extent over the bar, and by screwing down the cap till the light disappears, the thickness of the paper will be measured by the angle turned through.

The construction of this barometer, the first of its kind, was undertaken by Mr. FOSTER, who has produced a very beautiful instrument by which direct reading can be taken to the ten-thousandth of an inch. The mercury having been re-evaporated for the purpose in an apparatus belonging to Dr. SCHUSTER by his assistant, Mr. S. STANTON.

This barometer could be used as a pressure gauge for pressure up to 34 inches and down to 26 inches, and by connecting the mouthpiece with a receiver in connection with a mercury or water syphon gauge, with the other limb open to the atmosphere, the differences of reading of the barometer for different pressures in the receiver can be readily compared with the corresponding differences in the syphon gauge, and by such comparisons, taken at intervals till the mercury reaches the closing in of the tube, a test is obtained as to the absence of anything but mercury vapour above the mercury.

When the barometer is in connection with the vapour chamber in which the thermometer is immersed, the passage of moisture back into the barometer is prevented by connecting the tube by a branch with an air receiver, in which the pressure is maintained higher than that in the vapour chamber ; the branch pipe communicating with the chamber through a piece of quarter-inch glass pipe, 3 inches long, plugged as tightly as possible throughout its length with cotton wool, through which the air has to pass from the receiver into the vapour chamber. In this way an indefinitely slow current of clean dry air can be maintained into the passage from the vapour chamber to the valve which controls the exit of the steam into the atmosphere, so that the air does not enter the vapour chamber in which the thermometers are, but directly passes out with the overflow steam.

There is necessarily some resistance to the air passing along the pipe to the vapour chamber, but this could easily be tested by removing the pipe from the vapour chamber, and leaving it open to the atmosphere, so that the barometer would adjust itself to that of the atmosphere, plus the pressure due to the resistance of the current in the pipe; then, stopping the current by closing the branch pipe, and reading again, the difference would give the pressure due to the current. With the plug as described this was so small as to be negligible, even when the pressure in the receiver was two atmospheres. As during the testing of the thermometers the pressure in the vapour chamber was generally greater than that of the atmosphere, in order to maintain this steady, a governor on the gas burner was necessary, as well as an accurately adjustable exit valve.

With these appliances the scale of high temperature thermometer could be tested at intervals, over a sufficient interval on each side of the boiling point (212° Fahr.), the corrections for surface tension, temperature, and gravitation being applied to within the thousandth of an inch of mercury.

This gives the limits of error $\pm$ 0·00001.

Correction of the Low Temperature Thermometer.

49. The correction on the thermometer for 32° would be at any time obtained in the usual way by immersing the thermometer vertically in a bath of soft snow, but as there was no ready means, as with the scale about 212°, of testing the scale at 32°, while this would be used for one or two degrees, this correction could only be made by comparison with a thermometer already corrected with the air thermometer, which comparison Dr. SCHUSTER allowed to be made in the physical department.

Corrections of the Thermometers for Pressure.

50. The pressures in the thermometer chambers of the brake being both some 10 or 15 inches of mercury above that of the atmosphere, it would be necessary to determine the corrections on each of the thermometers under the pressures and temperatures at which they had to work.

Thus, if e_1, e_2 are the corrections per unit of pressure in the initial and final thermometers, the correction for the heat is $(e_1p_1 - e_2p_2)$.

The Range of Temperature over which the Specific Heat would be Measured.

51. The temperature of the effluent water from the brake can be regulated either up or down to any required extent, and although there would necessarily be some

divergence from the boiling-point, with care and experience it would be possible to bring the mean result in a number of trials within a close approximation of 212° Fahr.

On the other hand, there has been no means provided of regulating the temperature of the water entering the brake. This is determined by the rate at which the water passes through the iced coil and the temperature at which it entered, as determined by the temperature in the town's mains, which varies from 38° in the winter to 55° in the summer. Thus the temperature in the light trials would be from half to a degree above 32°, and that of the heavy trials from a degree to two degrees.

In calculating the heat of each trial the actual difference with the correction for the thermometers is taken, but if, as is shown by previous investigations by REGNAULT and others, the specific heat at and near 32° is less than the mean specific heat between 32° and 212° by something like 0·5 per cent., there would be errors in taking the results so obtained as the mean specific heat between 32° and 212°.

Owing to the extreme difficulty of determining the specific heat over a very short range of temperature to such high degrees of accuracy as ·01 per cent., the experimental evidence as to the exact value of the specific heat within a few degrees of 32° is but vaguely surmised from the general fall of the specific heat with the temperature.

The law of the thermal capacity of water between 0° C. and t°, as deduced by REGNAULT from his experiments, is avowedly vague as to the lower temperatures. It shows no singular point at the maximum density, as would be expected; and RANKIN deduced another law from these experiments, making the minimum specific heat coincide with the point of maximum density. Also other experimenters have obtained higher specific heats near 32° than are given by REGNAULT's formula. It would seem probable, therefore, that the difference between the specific heat at 32° and the mean between 32° and 212°, as given by REGNAULT's formula, is too large.

In that case, the correction obtained by this formula in order to reduce the specific heat between the observed temperature in the trials to that between the standard points, would probably be too large, and thus afford an outside limit of error.

Thus, putting s for the mean specific heat between 32° and 212°, $s\,(1 + X)$ for the specific heat between $T_1°$ and 212°, when $T_1°$ is small compared with 180°, and, by REGNAULT, taking $s\,(1 - 0{\cdot}005)$ for the specific heat at $T_1°$, then the total heat from $T_1°$ to 212° is

$$s\,(1 + X)\,(212 - T_1°) = s\,\{180 - (T_1° - 32)\,(1 - 0{\cdot}005)\}$$

$$= s\,(212 - T_1°)\left(1 - \frac{T_1° - 32}{212 - T_1°} \times 0{\cdot}005\right),$$

or, neglecting $(T_1 - 32)^2$,

$$X = 0{\cdot}005\,\frac{T_1° - 32}{180} = 0{\cdot}000028\,(T_1° - 32).$$

Thus, taking the mean capacity of water between the temperatures of 32° and 212° as the standard capacity, the mean specific heat between $T_1°$ and 212° would be

$$1 + X = 1 + 0{\cdot}000028\,(T_1° - 32)\,;$$

and, if $T_1°$ is the mean initial temperature of the water of any number of trials, $1 + X$ is the mean specific heat of the water in all the trials. The mean specific heat of the difference of two trials would be $1 + X$; this appears as follows:—

Suppose $1 + X_1$ to be the mean specific heat for a set of heavy trials, and W_1 the mean weight of water, and $(1 + X_2)$ to be mean specific heat of a corresponding set of light trials, and W_2 the mean weight of water, $T_1°$, $T_2°$ being respectively the initial temperatures of W_1 and W_2, the difference of the total heats would be

$$(1 + X_1)\,(212 - T_1°)\,W_1 - (1 + X_2)\,(212 - T_2)\,W_2,$$

and the mean specific heat would be approximately

$$\frac{(212 - T_1°)\,W_1 - (212 - T_2°)\,W_2 + 180\,(X_1W_1 - X_2W_2)}{(212 - T_1)\,W_1 - (212 - T_2)\,W_2} = 1 + \frac{180\,(X_1W_1 - X_2W_2)}{180\,(W_1 - W_2)}\,;$$

and, as in the heavy and light trials $W_1 = 2W_2$ approximately, the mean specific heat by REGNAULT's formula would be

$$1 + 2X_1 - X_2 = 1 + 0{\cdot}000028\,[2\,(T_1 - 32) - (T_2 - 32)].$$

This result is obtained by merely summing the trials, but counting the water in the light trials as negative,

$$X = {\cdot}000028\,\Sigma\left\{\frac{W\,(T_1 - 32)}{\Sigma\,(W)}\right\}.$$

The Gradual Rising of the Indices of the Thermometer.

52. Where, as is generally the case, the indices of the thermometers are gradually rising, if they are used between the intervals at which they are corrected, the last observed correction being applied, there will be an error which will be negative, and of magnitude equal to the rate of rise during the interval multiplied by the interval. Thus, if the trials are uniformly distributed between the intervals of correction, the correction would be $0{\cdot}5a$, where a is the observed rise in the interval, hence the relative correction on the equivalent, taking $\bar{a}_1$ and $\bar{a}_2$, as the mean rises between the intervals of correction of the initial and final thermometers, would be

$$\frac{0{\cdot}5}{180}\cdot(\bar{a}_2 - \bar{a}_1).$$

The Work done by Gravity on the Water.

53. The difference of pressure on the bulbs of the initial and final thermometers which are at the same level, expressed in feet of water, is the work done by gravity per lb. of water. If p_1 and p_2 express these pressures in inches of mercury, the work done by gravity is

$$1{\cdot}14\,(p_1 - p_2),$$

which gives as the relative correction for the equivalent, approximately,

$$+\,0{\cdot}000008\,\Sigma\,[W\,(p_1 - p_2)]/\Sigma\,(W).$$

The Work absorbed in Wearing the Metal of the Bushes and Shaft.

[54. During the six years the brake had been in use, before the trial commenced, the shaft and bushes were occasionally lubricated with oil, chiefly to prevent oxidation of the shaft when standing, and, up to the commencement of the trials, there was hardly any appreciable sign of wear. After the closing of the bushes by the stuffing-box and cap, when the use of oil was purposely discontinued, there was no means of observing the wear of the metal as long as the brake worked satisfactorily, as it did during all the trials. But when, after the completion of the trials, the stuffing-box and cap were removed, in order to return to the original manner of working, the excess of leaking through the bushes showed that there had been considerable wear.

At that time it did not occur to me that the proportion of this wear, which took place during the actual running of the trials, would represent a certain amount of work absorbed in disintegrating the metal, or a certain amount of heat developed by the oxidation of the metal, and no attempt was then made to form a definite estimate of the amount of metal which had disappeared. As, however, the worn metal was replaced by a coating of white metal, the thickness of this (less than $\frac{1}{32}$nd of an inch) and the extent of surface (less than 124 square inches) subsequently showed that it could not be more than 1 lb.

This was after it occurred to me that however small might be the effect of this wear, since it was definitely observed to have taken place during the twelve months when the bushes were closed for the purpose of the trials, it was desirable, in order to complete the research, that some outside estimate should be obtained of the limits to its possible effect, whether from disintegration or from oxidation.

In as far as the loss of metal was due to the abrasion of the clean metal surfaces, it would be proportional to the number of revolutions, while in as far as it was owing to the oxidation of the metal surfaces, left bright after each run, it would be probably proportional to the number of runs.

The number of revolutions with the bushes closed, counting ordinary work as well as the trials, is found from the records to be less than 300 × 60 × 360, and the number of runs to be 80, the mean time being 4·5 hours. The revolutions during any one of the accepted trials were 300 × 60. And the trials were made in threes, so that the coefficient for oxidation would be $\frac{1}{240}$.

Hence, the metal worn by abrasion in a single trial would be less than $\frac{1}{360}$th of 1 lb. = 0·0028 lb., and the metal oxidised in one trial less than $\frac{1}{240}$th = 0·004 lb. So far the estimate is fairly definite, but, in order for its completion, it is necessary to arrive at some conclusion as to the work absorbed in disintegrating the metal, and of the heat developed by its oxidation.

There does not seem to be any reason why there should be more oxidation of the bright surfaces in a light trial than in a heavy trial, so that there would have been no error from this cause in their difference.

As regards the abrasion and the oxidation of the abraded metal, there would be a difference, as the weight on the shaft in a heavy trial is 1·23 of the weight in a light trial. Thus the differences of abrasion would have been

$$0·0006 \text{ lb.}$$

The work necessary to produce a state of disintegration, such as exists in the vapour of the metal, would be the total heat of vaporization, less the kinetic energy and work $-[\kappa_v/(T-32)+PV]$, and, although the heat of vaporization of the metal is not known, it would seem that it cannot greatly exceed, when subject to the deductions mentioned, the heat of vaporization of ice subjected to like deductions (1,000,000 ft.-lbs.).

Assuming this, since the difference in the work of two trials is about 70,000,000 ft.-lbs., the correction would be

$$-\ 0·00001,$$

which, considering that the disintegration would be very imperfect, may be taken as an outside limit, while the effect may have been even reversed by the oxidation of the degraded metal.—Nov. 9, 1897.]

Accidents.

55. In contemplating such an extensive and complex research, the result of which depends on the mean of a number of experiments, it was impossible to overlook the question as to how such accidents, as would probably occur, should be dealt with.

It was clear that, whatever the rule might be, it must be definite and rigorously applied.

Two other things were also clear, that, as in surveying, accidents might occur, say in reading the counter or the scales, which would only be apparent from the reduction of the results after the trial was finished. Also, that in these experiments there would be no such rigorous check on the results as in surveying; so that, without danger of sorting the results, anomalous results, the cause of which was not noted during the trial, could only be rejected when the results themselves contained evidence of the cause of the anomaly, say an abnormal difference between the mean speeds by the counter and the speed gauge.

It was therefore, from the first, decided to reject all trials in which there was definite evidence either during the trial or in the results, of uncertainty to which no definite limits could be assigned, in any one of the measurements, without regard for the apparent consistency of the results, and in the same way to retain all other trials.

56. The following table contains a summary of all those circumstances on which the accuracy of the result of the investigation depends, together with references to the several Articles in which they have been discussed. In line with each circumstance is placed the formula for the relative correction in the equivalent, necessary in consequence of the observed deviation from the conditions of equality between the heavy and light trials. In the same line with each circumstance are also given, to the millionth part, the limits of relative error as deduced in the corresponding Articles.

TABLE of Corrections (relative) and Limits of Error (relative) for all circumstances affecting the Accuracy of the Results.

Reference No.	References to Articles.	Circumstances affecting the accuracy of the results.	Formulæ for the relative corrections.	Limits of relative errors.	
1	6	Terminal differences in the moments of momentum of the water in the brake	0·00000	+0·000000	−0·000000
2	8	Cyclic-fluctuations in the speed of the engines	0·00000	0·000025	−0·000025
3	9	Work done on the water by end-play in the shaft	0·00000	0·000017	0·000000
4	9	" " dash-pot	0·00000	0·000000	0·000000
5	9	Effect of the automatic-gear on the balance of the brake	0·00000	0·000016	0·000016
6	10	Imperfect elimination of the error of balance	0·00000	0·000000	0·000000
7	29	" " " heat conducted by the shaft	$-C\Sigma(T_B^\circ - T_{SB}^\circ)/\Sigma(H)$	0·000000	0·000000
8	32	" " " " radiated from the brake	$-R\Sigma(T_2^\circ - T_a^\circ)/\Sigma(H)$	0·000010 (?)	0·000010 (?)
9	34	The engagement of the counter	0·000013	0·000000	0·000000
10	31	Terminal differences in speed and temperature	$-\Sigma\{(B+w_i)T^\circ_i-(B+w_f)T^\circ_f\}/\Sigma(H)$	0·000000	0·000000
11	35	Leakage of the stuffing-box	$-\Sigma w_{SB}(T_{SB}^\circ - T^\circ_1)/\Sigma H$	0·000000	0·000025
12	36	" at the automatic valve	0·00000	0·000000	0·000000
13	37	Imperfect elimination of the water lost by evaporation	0·00000	0·000000	0·000000
14	40	Limits of accuracy in weighing the water	0·00000	0·000025	0·000025
15	41	Weight of the atmosphere	−0·001205	0·0000000	0·000000
16	42	Correction for gravity at the latitude of Greenwich	0·000172	0·0000000	0·000000
17	42	" " " " 45°	0·000745	0·0000000	0·000000
18	43	Salts dissolved in the water	$+0·8s$	0·0000000	0·000000
19	43	Air " " "	$-10·3/a$	0·000100 (?)	0·000100 (?)
20	45	Length of the lever	?	0·000020	0·000020
21	50, 46	Effect of pressure on the thermometers, limits of error	$-\Sigma\{W(e_1p_1 - e_2p_2)\}/\Sigma(W) \times 180$	0·000010	0·000010
22	52	Rise of the standard readings of the thermometers in the intervals of correction	+ 0·5 × (the difference of rise / the number intervals) / 180	0·000000	0·000000
23	51	Differences between the initial temperature of water and freezing	$-0·000028 . \Sigma\{W(T_1^\circ - 32^\circ)\}/\Sigma(W)$	0·000000	0·000000
24	53	Work done by gravitation on the water	$-0·000008\,\Sigma\{W(p_1 - p_2)\}/\Sigma(W)$	0·000000	0·000000
25	54	Work absorbed in wear of the metal	0·00000	0·000010	0·000010
			Summary of limits of error	+0·000233	−0 000241

The quantities included under the signs Σ ( ) are to be taken positive for the heavy trials and negative for the light trials.

Significance of the Symbols in the Formulæ.—C, the constant for conduction obtained from the trials; R, constant for radiation, as defined in trials, with the lagged and naked brake; *s* and *a*, are weights of *salts* and *air* in unit weight of water; p_1 p_2, pressures in inches of mercury in the initial and final thermometer chambers; e_1 e_2, are the corrections, per inch of mercury, pressure on the initial and final thermometers; W, weight of water used in a trial; w_{SB}, weight lost at the stuffing-box; T°, temperature Fahr.; T_{SB}°, stuffing-box; T_B°, bearing; T_a°, air; T_i, temperature at beginning; T_f, at the end; w_i, water in brake at beginning; H, the heat generated during the trial; B, capacity for heat of the metal in the brake.

PART II.

On an Experimental Determination of the Mechanical Equivalent of the Mean Specific Heat of Water between 32° and 212° Fahr., made in the Whitworth Engineering Laboratory, Owens College, on Professor Osborne Reynolds' method.—By William Henry Moorby, M.Sc.

1. In view of the frequent and extremely careful and accurate determinations of the value of the mechanical equivalent of heat which have been made of late years by different experimenters using different methods the present series of experiments may on first thoughts seem superfluous. There did, however, seem to be sufficient disagrèement between the results previously published—more particularly between values of the equivalent, as derived from the direct methods described by Joule, Rowland, and Miculescu, and the indirect electrical methods of Griffiths, and Gannon, and Schuster, to warrant a new investigation into the value of this important constant, if the proposed new method of working should carry with it advantages not available in previous investigations. I was accordingly very glad to fall in with the wishes of Professor Reynolds that I should undertake a research bearing on this point on lines which he suggested to me in July, 1894.

2. In Part I., par. 3, a full description is given of the apparatus whose existence in the Whitworth Engineering Laboratory led up directly to the institution of this research into the value of the mechanical equivalent of heat.

The advantages which the proposed method offered were briefly :—

(1.) The possibility of obtaining a result which in no way depended for its accuracy on the value of the scale divisions of the thermometers used in the measurements of temperature (Part I., par. 11).

This was done by supplying a stream of water to the brake at a temperature of 32° Fahr., and there raising its temperature to 212° Fahr. before admitting it to the discharge pipe where its temperature was again taken.

(2.) A means of eliminating from the result all losses of heat due to radiation and conduction from the calorimeter employed (Part I., par. 32). The manner in which this elimination was accomplished is indicated below.

Let U and u represent the quantities of work done in two trials which differed only in the moment of resistance offered by the brake—the number of revolutions of the engine shaft and the duration of the trials being the same in each case.

Also let H′ and h' be the apparent quantities of heat generated in the brake in these trials. These quantities will be less than the true equivalents of the works U and u by quantities which represent the losses of heat from the brake by conduction, radiation, &c. These losses were made as nearly as possible equal by keeping the temperatures of the brake and its supports and surroundings at the same levels in the two trials.

Then the quantity of work (U — u) should be exactly equivalent to the quantity of heat (H′ — h'), and by dividing the first of these by the second, a value of the constant required is obtained.

The power available for the purposes of the investigation enabled me to deal with quantities approaching the following values in trials of one hour's duration :—

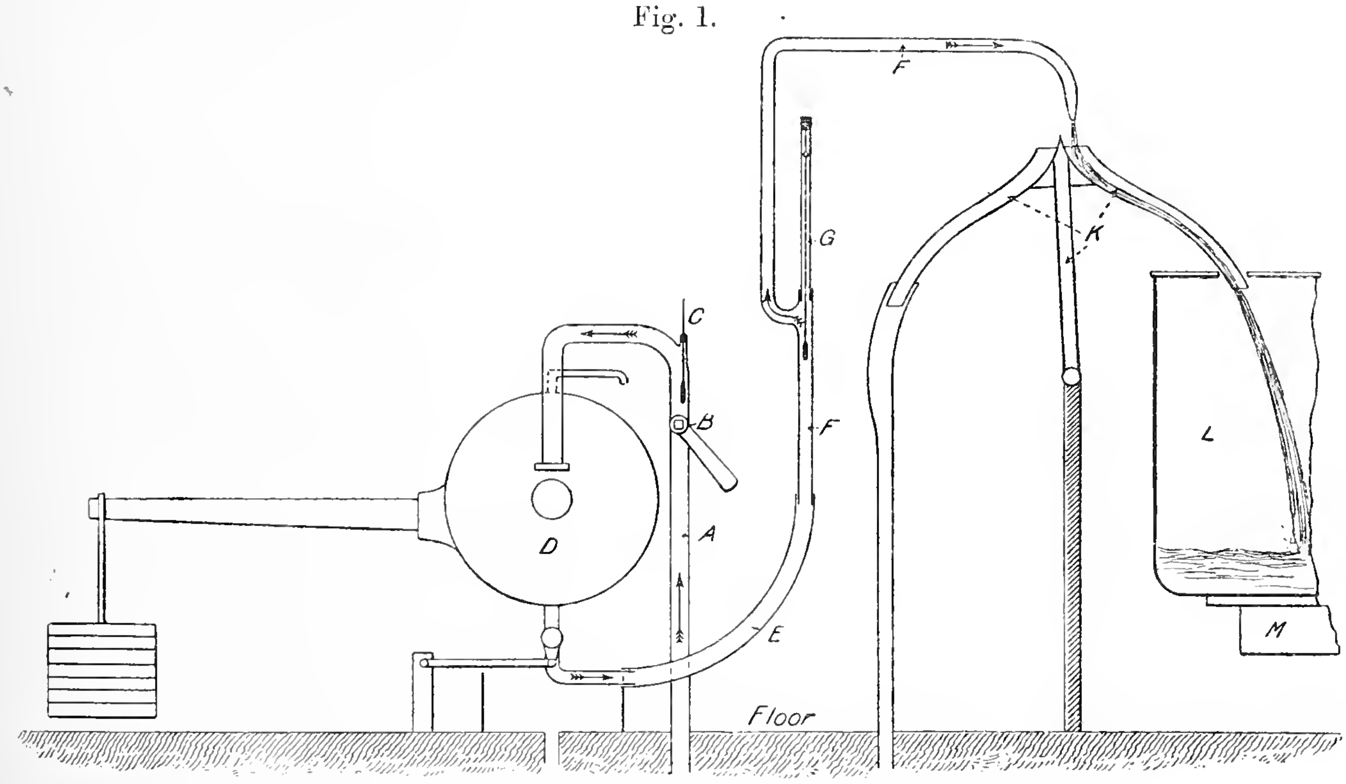
Revolutions, 18,000.
Total work done, 135,000,000 ft.-lbs.
Total weight of water raised 180° Fahr. = 960 lbs.
Total apparent heat generated = 170,000 B.T.U.

In quantities so large as these some of the small errors inevitable to all physical experiments became quite or nearly negligible.

Preliminary Apparatus and Trials.

3. It will, perhaps, be sufficient to indicate the general arrangement of the apparatus as first set up. This is illustrated in the annexed sketch. The water was

Fig. 1.



Preliminary Apparatus. Course of water shown by arrows.

supplied from the mains through the iron stand-pipe, A, and the regulating cock, B. Before it entered the brake its temperature was measured by means of the thermometer, C, inserted through a cork in the stand-pipe, the part of the stem on which readings were taken being exposed to the atmosphere. After being discharged from the brake, D, the water entered a flexible rubber pipe, E, bent through an angle of 90°, which connected a horizontal nipple at the bottom of the brake with a vertical one forming the lower end of a fixed line of copper piping, F. The temperature of discharge of the water was indicated by the thermometer, G, which was enclosed in a glass tube opening through a stuffing-box into the discharge pipe, the whole length of the stem being therefore kept at the temperature of discharge. On leaving the copper discharge pipe the water was directed at will by the two-way tipping switch, K, either to the left to waste or to the right into the tank, L, standing on the platform of the weighing machine, M.

A series of trials were made with this apparatus, the water being raised through varying intervals of temperature between 35° Fahr. and 100° Fahr. For obvious reasons the results were not satisfactory, and are therefore not published. Experience was gained, however, which helped very materially in the design of the final apparatus.

Common thermometers were used, and calibration errors on the comparatively small range of temperature through which the water was raised were of sufficient importance to vitiate all results. Again, the exposure of the stem of the thermometer, C, was a weak spot in the apparatus. I was much troubled also with leakage of water from the two bushed bearings of the brake.

In so far as could be judged, the bent rubber pipe, E, was found to be a satisfactory connection between the brake and the copper discharge pipe, and this has been retained in the subsequent apparatus.

Details of the Constituent Parts of the Final Apparatus.

Artificial Atmosphere.—(Part I., par. 23.)

4. To prevent loss of water by evaporation at the centres of the vortices formed in the brake, the ports in the vanes of the outer casing were connected through a flexible rubber tube some 4 feet long, with an artificial atmosphere formed in a tin receiver, the pressure in which was maintained by means of a cycle tyre inflator at about 9 inches of mercury, as measured on a U-gauge. The shape of this vessel is made clear in the sketch (Part I., fig. 8). The ends were made conical for greater strength. The receiver was also provided with an air valve, with which to relieve the pressure when too high, and a cock, with which water accidentally lodging inside could be drained away.

The Ice Cooler.—(Part I., par. 19.)

5. Some preliminary experiments indicated that a length of about 200 feet of $\frac{3}{8}$-inch diameter lead piping would, when immersed in a mixture of ice and water, be sufficient to cool a stream of some 16 lbs. of water per minute very nearly to 32° Fahr.

The ice cooler was accordingly made as follows: A wooden box, 4′ 0″ × 2′ 3″ × 2′ 0″, and lined inside with waxed cloth, was fitted with a horizontal wooden shelf about 2 feet 6 inches long, and on this was laid a flat oval coil of $\frac{3}{8}$-inch composition piping nearly 200 feet in length, the left-hand end of the coil and shelf stopping short at a distance of 1 foot from the end of the box, the right-hand end of the coil reaching the end of the box, but the shelf stopping some 6 inches short of that point. The coil was about 5 inches diameter, vertically, and over it were placed the wooden guide plates shown (Part I., fig. 7). An 8-inch diameter paddle, having 6 wooden floats, was placed about the middle of the box, at a height just sufficient to ensure the lower edges of the floats clearing the coil of pipe below it. A galvanized iron wire netting, extending from the shelf upwards to the top, separated the well at the left-hand end of the box from the compartment to the right containing the coil and paddle.

When working, the well and space beneath the shelf contained broken ice, well rammed in; while the level of the water was automatically kept at about 3 inches above the top of the coil. The paddle, driven by a cord from the line shafting in the engine-room, revolved in the direction shown by the arrow, and caused a circulation of water up through the ice in the well, and then horizontally through the coil and back to the ice under the shelf.

Circulating Pump.—(Part I., par. 20.)

6. In order to supply sufficient water to the brake against the resistance offered by the 200 feet of pipe in the cooler and the augmented pressure in the brake itself, it was necessary to use a circulating pump. This was a small Mather-Reynolds centrifugal pump with four $1\frac{1}{2}$-inch wheels, driven by a turbine available for this purpose in the engine-room. This pump was capable of supplying 16 lbs. of water per minute, against a pressure of 25 lbs. per square inch at the supply valve.

Some difficulty was encountered in the summer of 1896 with this combination, because the excessive demand for condensing water for the engine hardly left sufficient flow in the falling hydraulic main to work the turbine at the requisite speed to maintain the above pressure.

On the whole, however, the combination was exceedingly efficient, and with a graduated supply valve afforded a very delicate means of regulating the flow of water into the brake.

Water-tight Joints between the Brake and the Engine Shaft.

7. In (Part I., par. 24–29) the necessity of obtaining control over the leakage of water at the bearings of the brake, and the methods by which this was accomplished, are fully discussed. The bearing on the up-shaft end of the brake was provided with a stuffing-box, while the shaft end was covered with a cap. The annexed sketches show the general design of the stuffing-box and cap :—

A—The engine crank shaft.

B—The outer skin of the brake.

C—Conical brass bushes screwed into the outer skin of the brake.

D—Lock nuts on these bushes.

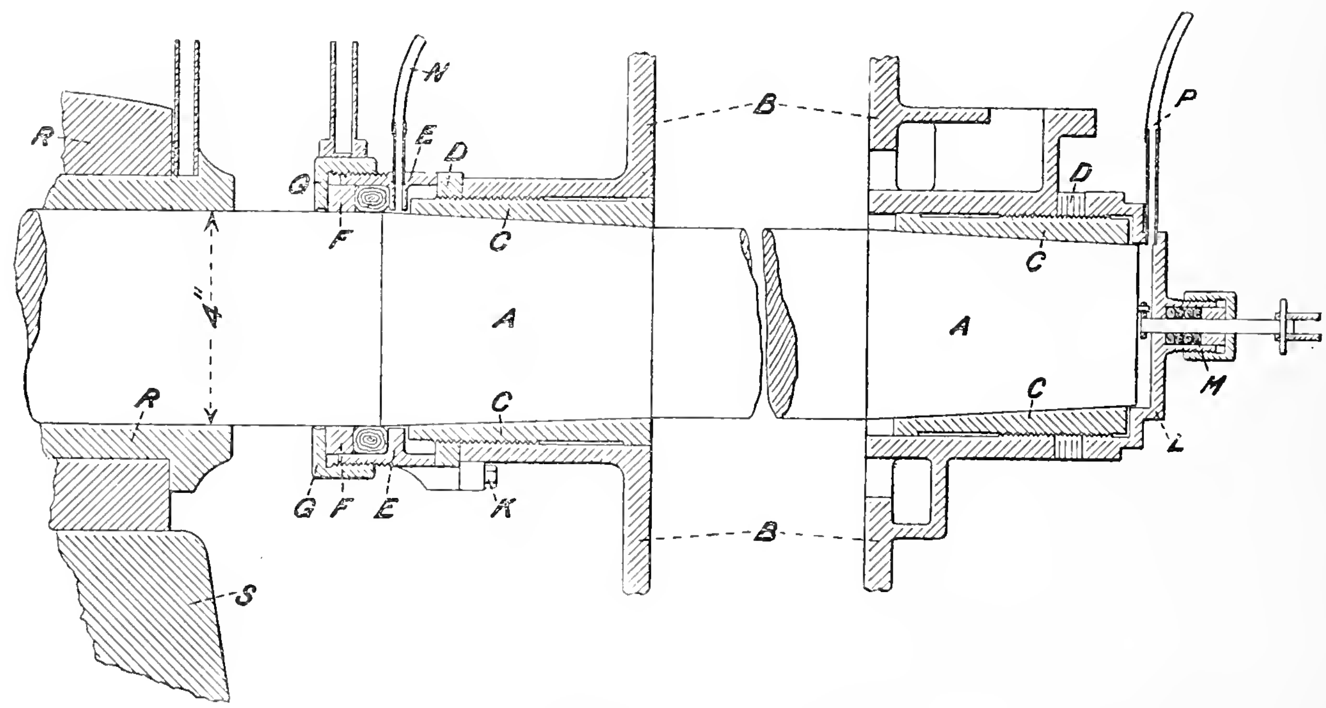
E, F, and G—Stuffing-box, ring and cover.

K—Set screws fastening stuffing-box to the lock nut.

L—Cap covering the end of the shaft.

M—Small spindle driven by a pin on the end of the engine shaft, passing through a stuffing-box on the cap, and required to drive the revolution counter.

Fig. 2.



Joints between brake and shaft.

The cap completely stopped all leakage from the bearing to which it was fixed, and, when the stuffing-box had worked for a short time, only a few drops of water escaped from the up-shaft bearing.

The brass bush bearings needed lubricating, and this was accomplished by supplying a small stream of water to each bearing through the pipes N and P, each provided with a regulating cock. This water came from the supply pipe between the ice cooler and the regulating valve controlling the main supply to the brake. It was consequently under considerable pressure and at a temperature very little over 32° Fahr. The water thus supplied had, of course, to enter the brake, and the amount supplied afforded a very convenient means of controlling the temperatures of the bearings.

At a distance of $2\frac{3}{4}$ inches from the cap of the stuffing-box was the end of one of the main bearings, R, carried on the cast-iron pedestal, S.

It was important that I should have some control over the loss of heat by conduction along this length of shaft. Accordingly, two pieces of brass pipe were soldered on to the cap of the stuffing-box, while two others were screwed, the one in the upper and the other into the lower brass forming the main bearing. Thermometers were placed inside the tube affixed to the stuffing-box cap, which happened to be uppermost at the time, and into the two pipes screwed into the main bearing. It was then assumed that the loss of heat along the shaft would vary with the difference of temperature between the stuffing-box cap and the bearing. In order that the losses of heat occurring in this way in any two trials should be identical, it was sufficient under the above assumption that this difference of temperature should be the same in both trials, and the temperature of the stuffing-box was regulated to this end by means of the amount of cold water passing into it.

Considerable difference of temperature was observed between the upper and lower brasses of the bearing, and as it seemed probable that the lower one approximated the more closely to the temperature of the shaft, that thermometer was the one used in determining the loss of heat by conduction.

In the later trials I endeavoured to keep the temperatures of the stuffing-box and the bearing at the same level, thus entirely eliminating this cause of loss from the experiments.

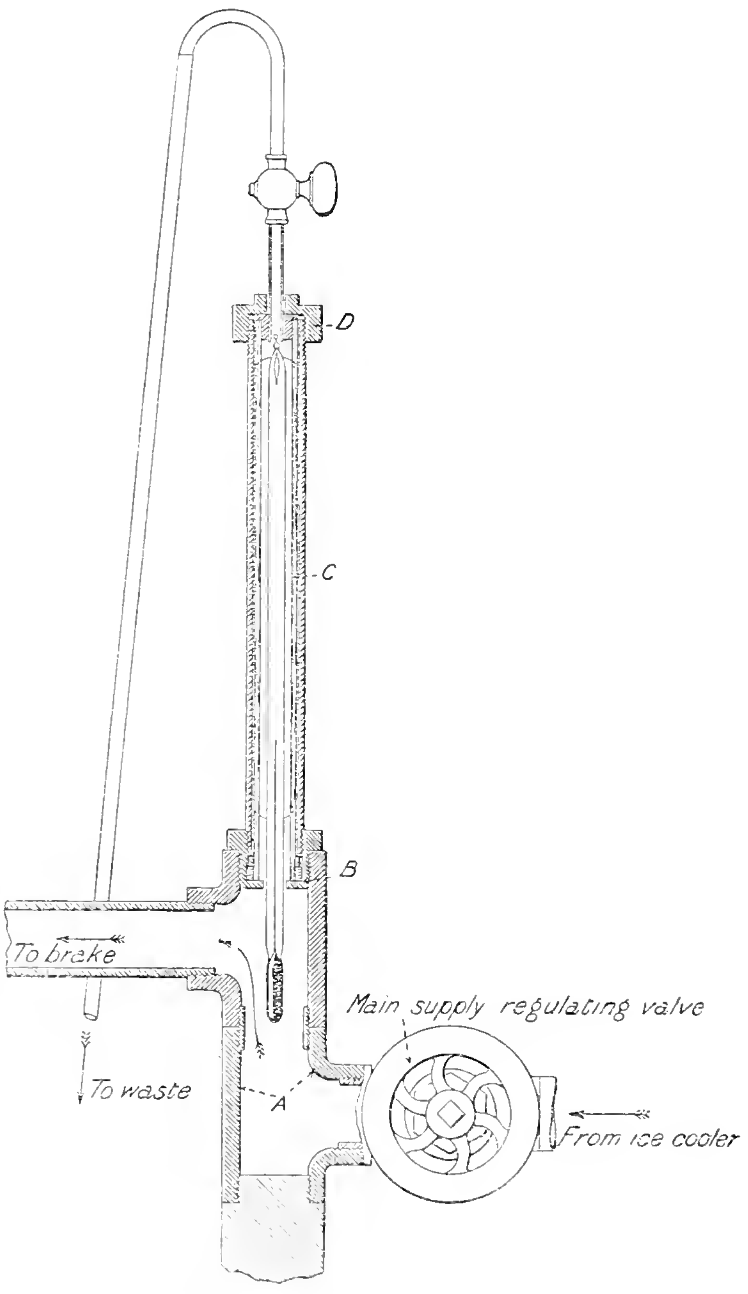
Water Jackets for the Low and High Temperature Thermometers.—(Part I., par. 15.)

8. It was evident that the temperatures of the water would be much more easily and accurately taken if the whole stem of each thermometer was kept at one temperature. To this end each of the principal thermometers was completely jacketted with a stream of the water whose temperature was required.

The arrangements adopted for this purpose are illustrated in the annexed sketches. (Figs. 3 and 4.)

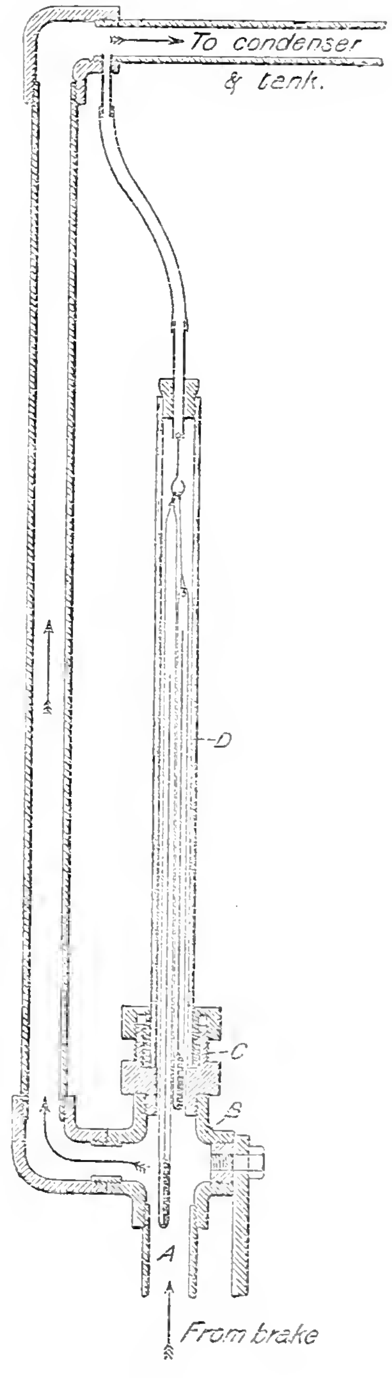
After leaving the main regulating valve the cold supply water entered a vertical brass T, shown at A. The main volume of the water flowed on to the brake through the horizontal arm of this T. At its upper end the T carried a small stuffing-box, B, into which was fixed a vertical $\frac{1}{2}$-inch diameter glass tube, C. This tube was closed at its upper end by means of a rubber stopper, held in place by the brass cap, D, screwed on to the upper end of a $\frac{3}{4}$-inch slotted copper pipe surrounding the glass tube. The stopper and cap were both penetrated by a short length of $\frac{1}{8}$-inch diameter brass tube, which carried a gas cock at its upper end. The thermometer was hung by a piece of string from the lower end of the $\frac{1}{8}$-inch pipe—the graduated part of the stem being all clearly visible through the glass walls of the chamber while the bulb was well in the main stream of water flowing through the brass T.

Fig. 3.



Cold water thermometer jacket.

Fig. 4.



Hot water thermometer jacket.

A small stream of water was allowed to run to waste through the small gas cock at the top, thus ensuring the whole of the stem of the thermometer being kept at the proper temperature.

The hot water discharged by the brake flowed from the bent rubber tube, previously mentioned, into the lower end of the vertical 1-inch diameter copper pipe, A. This pipe carried a brass cross, B, at its upper end, while fitted to the top of the cross was the stuffing-box, C, in which was fixed a piece of $\frac{3}{4}$-inch diameter glass tubing, D, forming the thermometer chamber. The upper end of this chamber was closed by a rubber stopper penetrated, as before, by a piece of $\frac{1}{8}$-inch diameter brass pipe, connected by a piece of rubber tubing to the main discharge pipe above.

The left arm of the cross carried an upward-turning elbow, and that again a $\frac{3}{4}$-inch diameter copper pipe, up which most of the water flowed.

The thermometers, two of which were used, were hung to the lower end of the $\frac{1}{8}$-inch pipe in the rubber stopper, so that the bulbs were immersed in the whole stream of water flowing up the 1-inch copper pipe from the brake. One of these thermometers was only used as a finder to indicate the temperature of the water as it rose after first starting the engine, and no record of its readings was kept.

The Condenser.—(Part I., par. 18.)

9. In order that there should not be a large loss of water before weighing, by evaporation from the tank into which it flowed from the brake, it was necessary to cool the stream to a temperature approaching that of the atmosphere.

For this purpose a condenser was constructed after the ordinary chemical pattern. It consisted of a length of 21 feet of $\frac{3}{4}$-inch diameter pipe inserted in an equal length of $1\frac{1}{4}$-inch diameter iron pipe.

Stuffing-boxes were used to form the joints between the two pipes. The hot water from the brake flowed through the inner tube, while a supply of condensing water flowed in the opposite direction through the annular space between the two pipes. By means of this condenser the water entering the tank was always cooled at least to 100° Fahr., and to lower temperatures in the earlier experiments when the water available in the mains was considerably colder.

The Rising Pipe.—(Part I., par. 21.)

10. The thermometer indicating the discharge temperature often gave readings more or less above 212° Fahr.

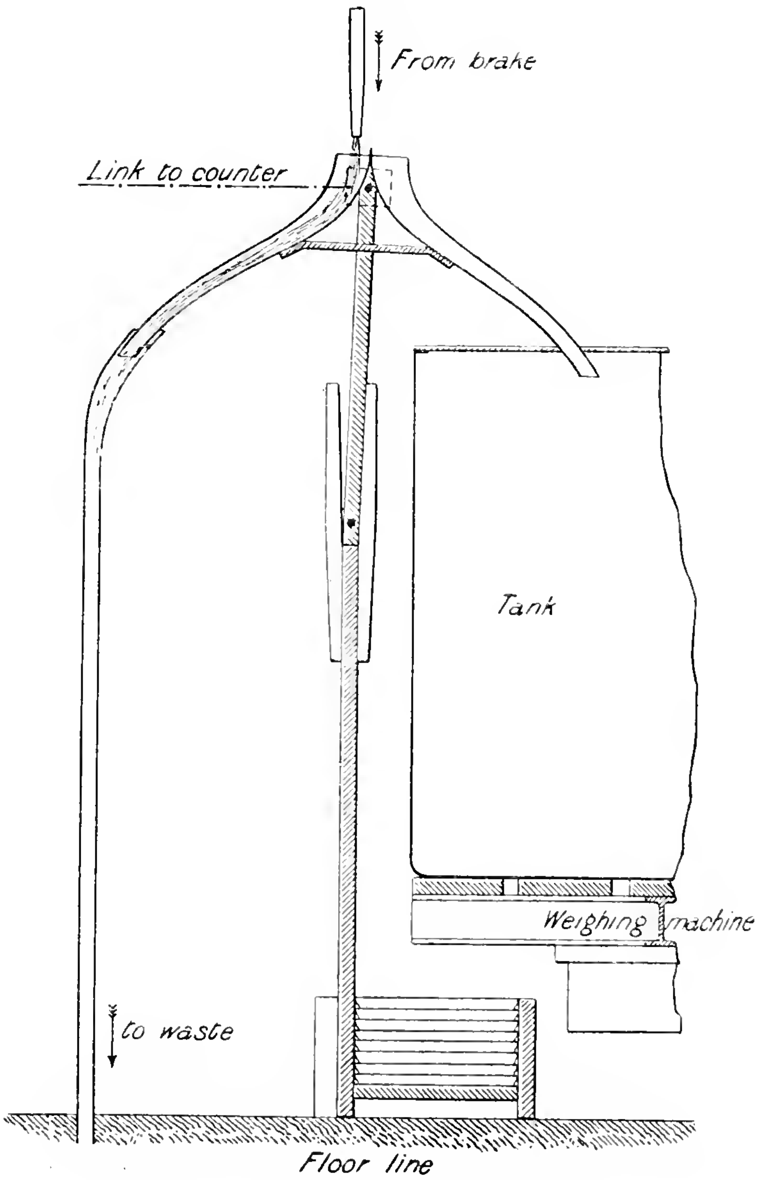
To provide against any fall in temperature at the thermometer bulb, which might occur by reason of the formation of bubbles of steam in the water, it was found desirable to keep some pressure on the water at that part of its course.

Accordingly, instead of discharging the water directly from the condenser into the tank, it was conducted up a vertical pipe, which was open at the top through a T to the atmosphere. The water then drained down another pipe provided with a nozzle at its lower end, opening into the two-way switch, to be described later. By this means a head of 11·3 feet of water was maintained at the thermometer bulb, and at a temperature of 220° Fahr. I had not much trouble with bubbles of vapour.

The Two-way Tipping Switch.—(Part I., par. 16.)

11. This was constructed to provide a means of rapidly diverting the water at will, either to waste or into the tank. It consisted, as shown in the sketch, of two curved copper pipes of rectangular section, meeting at their upper ends at an angle of about 30°. Their common side was produced for about $\frac{3}{4}$ inch, and formed into a knife-edge, separating the two orifices.

Fig. 5.



Tipping Switch.

These pipes were rigidly connected to a wooden link which worked about a horizontal axis, distant 25 inches below the knife-edge. Wooden stops were provided to limit the swing of the switch to rather less than 2 inches. One arm of the switch worked in a funnel forming the top of a pipe leading to waste, while the other worked through a hole in the cover of the tank. The whole arrangement was fixed so that when in the central position the knife-edge was $\frac{1}{4}$ inch vertically below the nozzle at the end of the discharge pipe.

This switch worked exceedingly well, diverting the stream of water almost instantaneously, without making any perceptible splash.

In the later trials this switch was connected by a chain of links with the revolution counter, so that when the latter was pushed into gear with the engine shaft the switch simultaneously directed the water into the tank, and *vice versa*.

Weighing Machine and Tank.—(Part I., par. 13.)

12. To facilitate the weighing, the stream of water was led during each experiment into a galvanized iron tank which stood on the platform of a weighing machine. The tank was 4 feet long by 2 feet 9 inches deep, by 2 feet 9 inches wide. During the experiments it was kept covered by a lid of thin boards, steeped in paraffin wax. These boards were always weighed with the tank, so that any water they might absorb was accounted for. A $2\frac{1}{2}$-inch valve in the tank bottom was used for discharging the water after weighing.

The weighing machine was graduated up to 2200 lbs., and was supplied with three rider weights.

No. 1, the largest, was provided with a knife-edge which fitted into grooves cut in the lever of the machine, each division representing 100 lbs.

No. 2 worked on another scale on the lever, each division representing 1 lb., and graduated up to 100 lbs.

No. 3 was made by Mr. Foster, in the laboratory, and indicated 0·01 lb. per division of the second scale. The lever was $32\frac{1}{2}$ inches long, and readings were taken only when the middle of the swing of a pointer fixed to the end of the lever coincided with a line marked on a brass plate alongside it.

It was quite easy in each individual weighing to set the machine to 0·01 lb., but owing, no doubt, to shifting of the platform, levers, &c., I do not think the readings taken were reliable beyond the $\frac{1}{50}$th of a lb.

This machine was not at first quite as sensitive as was necessary to attain the high degree of accuracy required for the purposes of the research. On examination this was found to be due to the slightly imperfect adjustment of the knife-edges attached to the graduated lever. The fault was rectified by Mr. Foster, and since then the performance of the machine has been highly satisfactory.

The Rubber Pipe Connections to the Brake.

13. On account of the very considerable pressure to which all the fittings of the

brake were subjected, it was found necessary to bind with tape the rubber pipes supplying the water to ensure them against bursting.

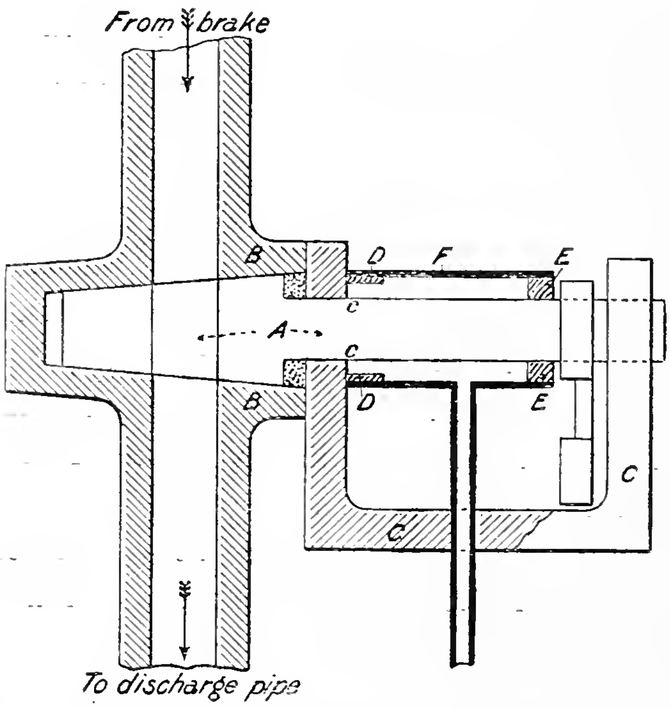
The extra stiffness thus given to these pipes did not much affect the free working of the brake, since none of them had a leverage of more than 4 inches from the centre of the shaft.

The case was, however, different with the bent rubber connection between the brake and the discharge pipe, since in this case the leverage is about 1 foot 6 inches. This pipe was eventually inserted in a cage consisting of a spiral of copper wire, $1\frac{1}{4}$ inches in diameter, through the coils of which were threaded two longitudinal wires to prevent elongation of the cage and rubber tube. By this arrangement the flexibility of the rubber tube was almost unimpaired.

The Device for Catching the Leakage at the Bottom Regulating Cock.—(Part I., par. 36.)

14. It was found impossible to prevent leakage taking place, generally to a small extent, from the automatic cock controlling the amount of water in the brake. It was, therefore, necessary to provide some means of catching this water, and it was very important that no impediment should be placed in the way of the free working of the cock spindle.

Fig. 6.



A tight joint was made between the valve seating, B, and the bracket, C, which carried the overhanging end of the valve, A. All the leakage, therefore, occurred along the valve spindle at *cc*. The method adopted to catch it was to solder a brass ring on to the bracket at D, and fit a ring of cork of the same diameter tightly on to the spindle at E. A piece of thin rubber tubing, F, was bound tightly to the ring, D, and the cork, E.

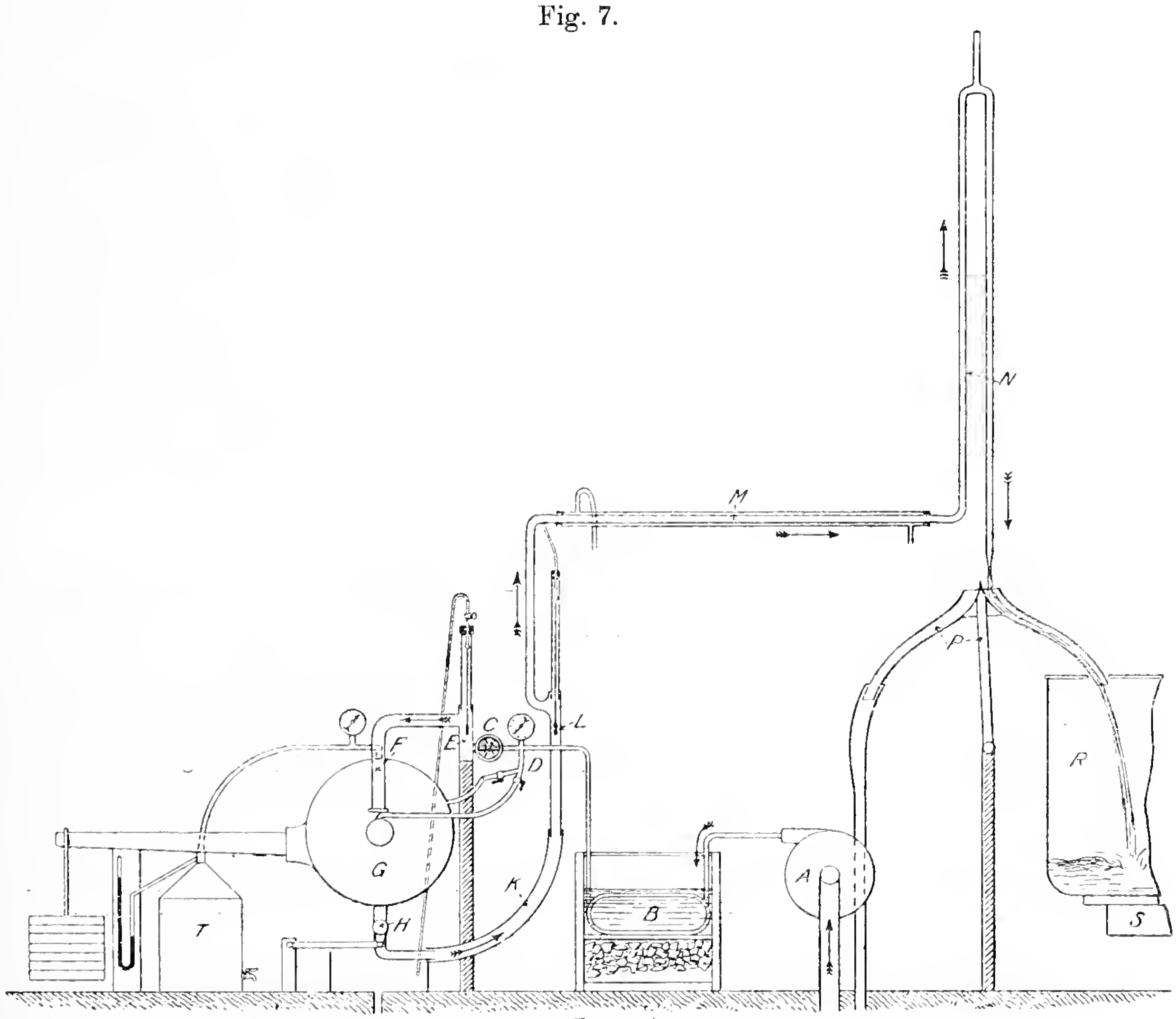
This tube caught all the leakage, which then drained down the smaller tube (shown in the sketch) into a bottle standing on the floor.

To prevent evaporation, the end of this small tube contained a short length of glass tube, the capillarity of which always kept the end closed by a bead of water.

General Arrangement of the Final Apparatus.

15. The general arrangement of the apparatus, as finally set up, is shown in the plates attached to Professor REYNOLDS' paper (Part I.), and in the annexed diagram. The course of the water was as follows :—

Fig. 7.



Final apparatus.

It was drawn from the mains by the circulating pump, A, and forced through the ice cooler, B, to the main regulating valve, C. Between the ice cooler and this valve there was a Bourdon pressure gauge and a branch-pipe, D, supplying water to the bearings of the brake. Entering the vertical stand pipe, E, the water flowed round the bulb of the initial temperature thermometer, a small stream being diverted to waste through the jacket. The straight flexible rubber pipe, F, then led the stream to the brake, G, from which the water flowed through the automatic valve, H, and the bent rubber pipe, K, to the vertical stand pipe, L, carrying the thermometer for measuring the temperature of discharge. Then passing through the condenser, M, and the rising pipe, N, the two-way switch, P, directed the water either to waste or into the tank, R, standing on the platform of the weighing machine, S. At T is shown the tin vessel forming the artificial atmosphere. A small Bourdon gauge was fitted on to the top of the brake because the mercury gauge, indicating the pressure in the air-vessel, was not visible to the observer when taking readings of the thermometers, and it was important that this pressure should be kept constant.

The Hand Brake and Speed Indicator.—(Part I., par. 30.)

16. In addition to the separate parts of the apparatus already mentioned there was a hand brake by which a moment of about 50 ft.-lbs. could be gradually applied to the engine shaft, and by this means a delicate adjustment of the speed of revolution was obtained.

To make this speed evident a small speed gauge was driven by a gut band from the engine shaft. It consisted of a paddle rotating about a vertical spindle in a cylindrical case. The case contained coloured water, and the pressure generated forced a column of the water up a glass tube, to a height which varied with the speed of revolution.

In Part I., Professor Reynolds has referred in one or two instances to the excellent manner in which various parts of the apparatus were constructed by Mr. Foster, to whom my thanks are also due for the valuable assistance he often rendered at critical moments in the research, and further for the advice and help he was always willing to give in the construction of apparatus for which I was mainly responsible.

The Method of conducting the Experiments finally adopted—using the Completed Apparatus.

17. During the progress of the experiments, I had at my disposal the services of two men and a boy. Of the men, the first, Mr. J. Hall, was fully engaged in attending generally to the needs of the engine and boiler, and had besides to maintain the boiler pressure at a point which ensured the steady running of the engine. I am bound to state that very much of the success met with must be attributed to the very admirable manner in which Mr. Hall's part of the work was performed.

The duties of the second assistant Mr. J. W. MATTHEWS consisted in regulating the engine speed by means of the hand brake, more particularly at the commencement and end of each trial, and also in keeping a constant pressure of 9 inches of mercury in the artificial atmosphere.

The boy's time was occupied in breaking up the ice and feeding it as required into the ice cooler.

In the last series of experiments three similar trials of 62 minutes duration each were made per day, and the engine having been once started was not stopped till the three trials were completed. Consequently what I say below as to the starting of the engine does not refer to every trial, for after emptying the tank at the close of any one all the necessary adjustments were ready made for the next.

I. The pump and engine were started simultaneously, the brake being therefore supplied with a stream of cold water through the ice cooler. The brake then automatically adjusted the weight of contained water till the load floated clear of the engine floor. The speed was then adjusted till the speed indicator gave the required reading, viz., in all recorded trials 300 revolutions per minute.

II. Since all the work done was expended on the stream of water passing through the brake, its final temperature rose more or less quickly, and by adjusting the regulating valve on the supply pipe the temperature of discharge finally remained steady at 212° Fahr. nearly. In the meantime the supply of water to the stuffing-box was regulated till the temperature of the cover was at the required level.

These adjustments took from a quarter to half an hour, and when made, the engine was allowed to run for some half-hour longer to ensure a steady condition being attained.

The water supply to the condenser had also been regulated till the stream of water issuing from the rising pipe and flowing to waste had the requisite temperature.

III. Readings were then taken of—

(*a*) The revolution counter.

(*b*) The weight of the empty tank and its cover.

IV. When a steady condition was reached, the revolution counter at a given signal was pushed into gear with the small spindle previously mentioned, making connection through the cap with the engine shaft, and simultaneously the two-way tipping switch, which had hitherto been directing all the water to waste, was pulled over and diverted the whole stream into the tank. In the later trials all leakage that did sometimes take place from the stuffing-box, and a slight leakage that always occurred at the automatic cock below the brake, were collected in two bottles kept for that purpose. These were put under the drain pipes in each case as soon as possible after the signal.

The speed of the engine as indicated by the gauge was read when the signal was given, and as soon as possible afterwards a reading was taken of the temperature in the discharge pipe.

V. At intervals of two minutes thirty observations were then taken of the temperatures of supply and discharge of the water to and from the brake, and also at each of these intervals a note was made of the reading of the speed gauge.

At intervals of four minutes fifteen observations were made of a thermometer registering the temperature of the room. Also at intervals of eight minutes readings were taken of the two thermometers in the stuffing-box and on the main bearing.

VI. When sixty-two minutes had elapsed the counter was freed from the shaft, at the same time the water being again diverted to waste.

The drain pipes from the stuffing-box and cock were removed from their respective bottles.

Readings were taken of the speed indicator and of the temperature of discharge.

VII. Fresh observations were made of—

(*a*) The reading of the revolution counter.

(*b*) The weight of the tank and water received during the trial, to which had been added the water caught from the regulating cock.

A record was also made of—

(*c*) The weight of water which had been caught from the stuffing-box.

18. These observations were afterwards reduced as follows :—

Let T_1 = mean temperature of water supplied to the brake.
T_2 = „ „ discharged by the brake.
W_1 = weight of tank and contents before the trial.
W_2 = „ „ after the trial.
w = weight of water caught from the stuffing-box.
t = rise of reading of the thermometer in the discharge pipe during the trial.
T_S = mean temperature of the stuffing-box cover.
T_B = „ „ lower brass of the main bearing.
T_A = „ „ air.
N_1 = reading of revolution counter before the trial.
N_2 = „ „ after the trial.
M = moment in ft.-lbs. carried by the brake.

Therefore we have for the total heat generated

$$H = (W_2 - W_1)(T_2 - T_1) + w(T_S - T_1) + t.X. + (T_S - T_B)C + (T_2 - T_A)R.$$

The determination of the quantity X and of the constants C and R, representing the losses by conduction and radiation will be dealt with later (pars. 30, 43 and 45).

Also the total work done

$$U = 2\pi(N_2 - N_1)(M + m),$$

where m = error in balance of the brake. This error will be dealt with subsequently (par. 29).

If the capitals H and U refer to trials with a large turning moment on the brake, and the small letters h and u refer to trials with a small turning moment, then for our value of the mean specific heat of water in mechanical units we have

$$K = \frac{U - u}{H - h}.$$

This quantity K is not strictly the same as the mechanical equivalent of heat, of which other determinations have been made, since we are here dealing with the mean specific heat of water between freezing and boiling-points.

For this reason it has been decided not to use the usual symbol J, at any rate at this stage of the research.

19. As an illustration of the method of tabulating and reducing the observations, I append all that were taken in trials 69 and 72 made on the 7th and 8th July, 1896, respectively.

It will be seen that all the observations of temperature, together with the readings of the speed indicator, which were made during the actual progress of each trial, are given on pages 373 and 375 respectively.

With the exception of the two readings of the speed indicator taken at the moments of starting and finishing each trial, and shown in brackets at the top and bottom of column No. 8, I was personally responsible for all observations recorded. These two observations were made by the assistant in charge of the hand brake and artificial atmosphere.

In the tables of temperature and speed observations

Col. 1 gives the times at which observations became due, the whole period of 62 minutes being divided into 31 two-minute intervals.
Col. 2 gives the temperatures of supply of the water to the brake.
Col. 3 ,, ,, discharge of the water from the brake.
Col. 4 ,, ,, the air in the engine room.
Col. 5 ,, ,, the stuffing-box cover.
Col. 6 ,, ,, the lower brass of the main bearing.
Col. 7 ,, fall of temperature between the stuffing-box and bearing, being the difference of Cols. 5 and 6.
Col. 8 gives the readings of the speed indicator.

Observations of the revolution counter and of the weight of the tank before and after each trial, are given on pages 372 and 374 respectively.

As I had to take all the observations myself, it was, of course, impossible to make them simultaneously at the times indicated in Col. 1. They were, however, always taken in the same order, as follows.

When the time for the next ensuing series of observations had arrived as given by a watch lying on the table at my side, I immediately read the temperatures of supply and discharge and the speed gauge in the order named, and after reading the three I entered them in the note-book. This generally took about a quarter of a minute. If then a reading of the atmospheric temperature was due, it was next taken and entered. After that the temperatures of the stuffing-box cap and of the bearing were noted in their turn, the whole series of observations being made in 1 or $1\frac{1}{4}$ minutes.

The interval which then elapsed before the next series of observations became due was often fully occupied in making adjustments of the regulating valve controlling the main water supply to the brake; of the cock regulating the supply to the stuffing-box; and of the speed of the turbine driving the pump, small alterations at all these points being frequently necessary.

At the head and foot of Cols. 3 and 8 will be seen observations in brackets. These observations were taken at the moments of starting and ending the trials, and were required in the calculation of a terminal correction to be referred to later.

At the close of each trial a mean of the observations occurring in Cols. 2, 3, 4, 5 and 7 was made, the two observations in brackets in Col. 3 being omitted in calculating these means.

On pages 372 and 374 additive corrections to the weights and to the mean temperatures of supply and discharge are given. These will be referred to later.

It will be noticed that in neither of the trials chosen was there any leakage of water from the stuffing-box.

The observations are given again in the partially reduced form which has been adopted for the final tabulation of the results on p. 376.

Cols. 1 to 8 should be self-explanatory.

Col. 9 gives the first approximation to the heat generated, obtained by multiplying the weight of water by its mean rise in temperature.

Col. 11 gives the difference of the temperature of the stuffing-box (supposed to be a measure of that of the water leaking from it), and the temperature of supply.

Col. 12 gives the loss of heat due to this leakage, and represents the product of Cols. 10 and 11.

Col. 13 gives the rise of temperature of the brake during the trial, and is assumed to be equal to the difference of the two temperatures given in brackets in the table of temperature observations (Col. 3).

Col. 14 gives the terminal correction to the heat required on account of the increase of heat in the brake itself during the trial.

Col. 15 gives the difference between the mean temperature of the stuffing-box and of the shaft bearing. As already explained the loss of heat by conduction has been assumed proportional to this difference, and a determination of its amount will be given later. At present it is sufficient to say that a loss of 12 thermal units occurred per trial per unit fall of temperature along the shaft.

Col. 16 gives, therefore, the product of this difference × 12, which represents the total loss by conduction.

Col. 17. The difference of temperature between the brake and the surrounding air was taken as being equal to the difference of the mean discharge temperature of the water and that of the air. The determination of the constant representing the loss of heat per unit difference of temperature is given later, and consequently,

Col. 18 gives the product of this constant × the difference of temperature in Col. 17.

Col. 19 gives the sum of the heat in Col. 9 added to all the corrections afterwards given.

A further Table (p. 376) gives the work done, and the corrected values of the heat generated in these two trials, and the differences between them.

The value of K in the last column is then found by dividing the difference of work in Col. 4 by the difference of heat in Col. 6.

A slight inaccuracy has been pointed out to me by Professor REYNOLDS in the method of finding the mean temperatures of supply to and discharge from the brake. It was originally intended that the trials should be of exactly one hour's duration, and that the first series of readings should be taken one minute after the start. It was found impossible to do this, on account of the number of points requiring attention in the first few minutes, and consequently I made all trials 62 minutes long, and took the first reading two minutes after starting. The mean used has not therefore been obtained strictly in accordance with the middle breadth rule. Any error introduced would be of the occasional type, and should be eliminated in the mean of a number of trials.

July 7, 1896.

Trial No. 69 (A).

Moment on the brake	600 ft.-lbs.

Trial began at 11.17 A.M., and ended at 12.19 P.M

Reading of revolution counter after trial	92,948.
,, ,, ,, before trial	75,400.
Number of revolutions during trial	17,548.

Weight of tank and water after trial	811·94 — ·5 lb.
,, ,, ,, before trial	342·16 + ·4 ,,
Weight of water discharged by brake during trial, including leakage from bottom cock	468·88 lbs.

Mean temperature of water in the discharge pipe .	212·007° F. + ·04.
,, ,, ,, supply pipe . .	33·595° — ·52.
Mean rise of temperature of the water	178·972° F.

Weight of water caught from stuffing-box	0 lb.

Temperature of water entering the tank	= 100° F.

1.	2.	3.	4.	5.	6.	7.	8.
Times.	Temperatures.					Fall of temperature between stuffing-box and bearing.	Readings of speed-gauge (revolutions per minute).
	Water supplied to brake.	Water discharged from brake.	Air.	Stuffing-box cover.	Lower brass of bearing.		
	°	°	°	°	°	°	
Began 11.17	..	(212)	..	..	..	..	(302)
19	33·57	211·9	74·4	..	..	..	300
21	33·5	212·0	..	..	..	..	302
23	33·57	212·3	75·7	107	107	..	302
25	33·58	211·3	..	..	..	..	303
27	33·58	211·5	76·0	..	..	..	302
29	33·58	212·2	..	..	..	..	304
31	33·57	211·1	76·4	109	110	−1	302
33	33·6	211·0	..	..	..	..	299
35	33·6	211·0	76·5	..	..	..	299
37	33·6	214·9	..	..	..	..	303
39	33·6	213·7	77·5	109	111	−2	301
41	33·62	213.3	..	..	..	..	301
43	33·6	213·2	76·8	..	..	..	299
45	33·59	212·2	..	..	..	..	301
47	33·64	211·5	77·0	110	111	−1	301
49	33·62	211·8	..	..	..	..	303
51	33·64	212·0	78·1	..	..	..	304
53	33·59	212·3	..	..	..	..	299
55	33·59	212·1	76·5	110	111	−1	301
57	33·58	212·2	..	..	..	..	301
59	33·6	211·8	77·8	..	..	..	301
12.01	33·62	211·9	..	..	..	..	302
3	33·61	212·0	78·3	115	113	2	301
5	33·62	211·5	..	..	..	..	300
7	33·6	212·0	79·0	..	..	..	300
9	33·57	211·6	..	..	..	..	300
11	33·59	211·6	76·8	112	113	−1	297
13	33·57	211·5	..	..	..	..	300
15	33·6	211·3	77·1	..	..	..	301
17	33·66	211·5	..	..	..	..	301
Ended 19	..	(212)	..	..	..	..	(302)
Means . . .	33·595	212·007	76·9	110·3	..	−·57	..

July 8, 1896.

Trial No. 72 (A).

Moment on the brake . . . . 1200 ft.-lbs.

Trial began 11.11 A.M., and ended 12.13 P.M.

Reading of revolution counter after trial		146,311
,, ,, ,, before trial		129,000
Number of revolutions during trial		17,311

Weight of tank and water after trial	1283·50	− 1·31 lbs.
,, ,, ,, before trial	347·21	+ ·4 lb.
Weight of water discharged by brake during trial, including leakage from bottom cock	934·58 lbs.	

Mean temperature of water in the discharge pipe .	212·46° F.	+ ·04
,, ,, ,, supply pipe .	34·706°	− ·55
Mean rise of temperature of the water	178·344° F.	

Weight of water caught from stuffing-box . . . . = 0 lb.

Temperature of water entering tank . . . . = 101° F.

1.	2.	3.	4.	5.	6.	7.	8.
Times.	Temperatures.					Fall of temperature between stuffing-box and bearing.	Readings of speed-gauge (revolutions per minute).
	Water supplied to brake.	Water discharged from brake.	Air.	Stuffing-box cover.	Lower brass of bearing.		
Began 11.11	..	(212·4)	..	..	..	..	(300)
13	34·74	212·3	72·0	..	..	..	302
15	34·8	211·5	..	..	..	..	300
17	34·71	212 8	73·7	97	99	− 2	304
19	34·7	212·9	..	..	..	..	305
21	34·69	211·7	74·0	..	..	..	299
23	34·72	212·0	..	..	..	..	302
25	34·7	212·6	73·3	101	101	..	303
27	34·77	212·8	..	..	..	..	307
29	34·78	213·5	74·4	..	..	..	302
31	34·77	214·0	..	..	..	..	300
33	34·69	213·2	74·7	101	102	− 1	301
35	35·0	213·2	..	..	..	..	299
37	34·6	214·0	75·6	..	..	..	303
39	34·7	214·4	..	..	..	..	307
41	34·76	214·0	74·7	104	103	1	302
43	34·79	212·8	..	..	..	..	304
45	34·66	213·0	74·8	..	..	..	301
47	34·75	212·3	..	..	..	..	300
49	34·66	211·6	75·7	105	104	1	297
51	34·68	211·2	..	..	..	..	302
53	34·68	212·0	75·4	..	..	..	302
55	34·66	211·6	..	..	..	..	299
57	34·66	211·0	75·3	104	105	− 1	297
59	34·58	211·3	..	..	..	..	302
12.1	34·6	212·3	76·0	..	..	..	305
3	34·59	212·9	..	..	..	..	299
5	34·67	211·8	76·0	107	106	1	301
7	34·7	211·4	..	..	..	..	302
9	34·69	211·9	75·8	..	..	..	304
11	34·68	211·8	..	..	..	..	302
Ended 13	..	(211·6)	..	..	..	..	(300)
Means . . .	34·706	212·46	74·8	102·7	..	− 0·14	

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.
Date.	Trial No.	Time of start.	Moment (ft.-lbs.)	No. of revolutions of engine shaft.	Work done (ft.-lbs.)	Weight of water discharged by brake (lbs.)	Rise of temperature in the brake (° F.)	Heat generated, less losses due to radiation, &c. (B.T.U.)	Weight of water caught from stuffing-box (lbs.)	Rise of temperature in the brake (° F.)	Loss of heat by leakage (B.T.U.)	Rise of temperature of brake during trial (° F.)	Terminal correction to heat (B.T.U.)	Fall of temperature along shaft between stuffing-box and bearing (° F.)	Loss of heat by conduction (B.T.U.)	Difference of temperature between brake and air (° F.)	Loss of heat by radiation (B.T.U.)	Corrected heat (B.T.U.)
7 July, '96	69	11. 7 A.M.	600	17,548	66,154,556	468·88	178·972	83,916	..	..	..	..	..	—0·57	—7	135·1	1078	84,987
8 July, '96	72	11.11 A.M.	1200	17,311	130,522,170	934·58	178·344	166,677	..	..	..	—0·8	—46	—0·14	—2	137·7	1099	167,728

1.	2.	3.	4.	5.	6.	7.
Determination No.	Trial No.	Works.	Diff. of works.	Heats.	Diff. of heats.	K.
	72 69	130,522,170 66,154,556	64,367,614	167,728 84,987	82,741	777·95

The Barometer.—(Part I., par. 47.)

20. Before dealing with the thermometers and their corrections, it becomes necessary to describe a combined barometer and manometer which was constructed to measure the pressures of steam employed in the determination of the boiling-points on the thermometer used to measure the discharge temperature.

The structural details of this instrument are given in Professor REYNOLDS' paper. At present it is sufficient to say that it consisted of a cast-iron, bottle-shaped reservoir, through the neck of which the glass tube holding the mercury column was carried in a stuffing-box, which made a perfectly air-tight joint between the glass and the reservoir. The pressure to be measured was introduced through a small iron pipe, which penetrated horizontally the cast-iron wall of the reservoir, and then turned vertically upwards till its open mouth stood above the level of the mercury inside. Two circular plate-glass windows in the reservoir walls provided a means of ascertaining the level of the mercury surface. In order to measure the height of the mercury column supported by any external pressure, a brass sleeve was made, which fitted outside the glass tube and the upper part of the reservoir. This sleeve consisted of a piece of $\frac{3}{4}$-inch diameter brass pipe fixed into a conical brass casting, which carried a truly-turned bevelled edge at its lower extremity. This conical casting engaged by an internal screw of twenty threads to 1 inch with the neck of the cast-iron reservoir. The upper part of the sleeve carried an internal thread of the same pitch, and into this was screwed a second piece of pipe through which two long narrow slits were cut at opposite extremities of a diameter. A third piece of brass pipe engaged with the upper end of the piece just mentioned, and was provided at its lower end with a truly-turned bevelled edge.

In use the bevelled edge on the conical brass casting was first adjusted to the surface of the mercury in the reservoir, and then the upper bevelled edge was adjusted to the surface at the top of the mercury column. Suitable horizontal and vertical scales were provided to enable me to measure the vertical distance between these two bevelled edges to $\frac{1}{1000}$ of an inch.

It was necessary to standardise this scale (Part I., par. 44). There is a Whitworth measuring machine in the laboratory, which is provided amongst others with standard end gauges of 9 inches and 3 inches long respectively.

Two new steel standards were made by Mr. FOSTER as nearly as possible of the same length as the 9-inch Whitworth, and by means of the measuring machine I determined their exact lengths as follows, three comparisons being made of the two new gauges with the standard. The table shows the readings obtained.

	Whitworth standard 9-inch gauge.	Laboratory standard gauge, No. 1.	Laboratory standard gauge, No. 2.
Readings on divided wheel of machine	0·0011 0·00112 0·00114	0·00105 0·0010 0·00097	0·00095 0·0009 0·00098
Mean readings . .	0·00112	0·001007	0·000943
True lengths . . .	9 inches	9 inches − 0·000113	9 inches − 0·000177

These three 9-inch standards, together with the 3-inch Whitworth, therefore gave a length when placed end to end of

$$30 \text{ inches} - 0·00029 \text{ inch}.$$

The next operation was to construct a single steel standard with a length of approximately 30 inches. This bar being made, and the measuring machine not being long enough to accommodate 30 inches, the measurements were made between the centres of a large lathe in the laboratory. Two centres were made with polished flat ends. The one was put in the fixed headstock, while the second was carried by the movable sleeve of the loose headstock which had previously been securely bolted to the lathe bed in a convenient position. A temporary wooden trough was made to carry our four short standards, and correctly line them between the two centres. The reciprocating centre in the loose headstock was then gradually screwed up till the gravity piece of the measuring machine just floated between the end of the adjacent standard and the centre. A mark on the hand-wheel actuating the centre was then fixed by means of a pointer. The four standards were then removed, and the 30-inch bar substituted for them, and the operation of bringing up the centre repeated. The circumferential distance then separating the pointer from the mark on the hand-wheel was then carefully measured.

A series of five of these observations were made, and the following readings taken, viz :—

(1) − 0·1 inch (3) + 0·09 inch (5) + 0·03 inch
(2) − 0·05 inch (4) + 0·02 inch.

$$\text{Mean} = - 0·002 \text{ inch}.$$

The hand-wheel had a diameter of $9\frac{1}{4}$ inches, and was fixed to a screw of $\frac{1}{5}$-inch pitch.

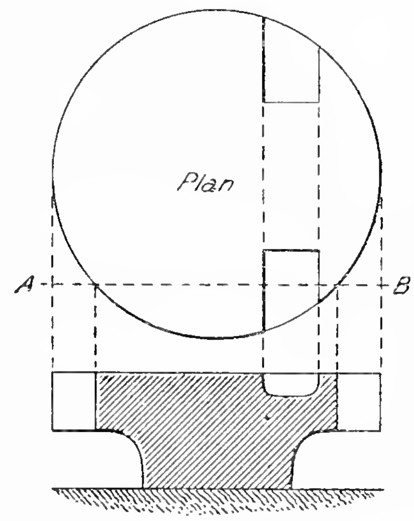
The 30-inch bar was therefore short of the length of the four steel standards by 0·0000138 inch.

Its correct length was, therefore,

30 inches − 0·0003 inch.

As the barometer was only graduated to 0·001 inch, no error was introduced in assuming the bar to be exactly 30 inches long.

(Part. I., par. 48).—For the purpose of transferring this standard 30 inches to the brass sleeve forming the scale of the barometer, a circular cast-iron surface plate was made. This plate had two pieces cut out of it, as shown in the sketch. The plate was fixed with its surface level, and then the brass sleeve was placed centrally upon it, standing upright on its lower bevelled edge. In this position the portion of the surface between the two grooves cut in the plate corresponded exactly to the surface of the mercury in the barometer between the two windows previously mentioned. As it was probable that in actual use the lower bevelled edge would be slightly above the mercury surface, the sleeve was packed up by means of some very fine sheets of tissue paper till a line of light could be seen under it. Four sheets were necessary to effect this; one of these was removed, and then the standard 30-inch bar was placed inside the brass tube, standing with one end on the surface plate. The upper bevelled edge was then adjusted till the line of light between it and the top of the steel standard was obscured, and the scale was made to read 30 inches in that position.



Section AB.

Together with Mr. FOSTER I made this adjustment a number of times, but after once fixing the 30-inch mark, the reading of the length of the steel standard never varied by as much as 0·0003 inch from 30 inches.

Unfortunately, the comparison was made at a temperature of 67° Fahr., while the standard temperature of the Whitworth gauges was 60° Fahr. A formula of reduction of the readings of the barometer therefore became necessary at all temperatures.

Taking for the coefficient of linear expansion of brass per ° Fahr.	0·000012
,, ,, ,, ,, steel ,,	0·0000066
,, ,, ,, ,, the mercury column of the barometer	0·0001.

Then at 67° Fahr. the true length of the brass barometer scale

$$= 30 \frac{1 + 35 \times 0·0000066}{1 + 28 \times 0·0000066}$$

$$= 30·000138 \text{ inches.}$$

To find T, the temperature at which the scale gives correct readings, we have, if $T = t + 32°$,

$$\frac{1 + t \times 0{\cdot}000012}{1 + 35 \times 0{\cdot}000012} = \frac{30}{30{\cdot}000138},$$

which gives $t = 31°$ and $T = 63°$ Fahr.

The coefficient of expansion of the mercury column relative to the brass scale is 0·000088.

Now if H_T = reading of barometer in inches at T° Fahr., and as before

$$t = T - 32,$$

then the corresponding corrected height of the column at a temperature of 63° Fahr.

$$= H_{63} = \frac{1 + 31 \times 0{\cdot}000088}{1 + t \times 0{\cdot}000088} H_T$$

$$= \frac{1{\cdot}002728}{1 + t \times 0{\cdot}000088} H_T,$$

and if H_0 = the corresponding pressure reduced to inches at the freezing-point, then

$$H_{63} = H_0 (1 + 0{\cdot}0031).$$

Therefore for any required pressure H_0 inches at a temperature of 32° Fahr., the corresponding reading at T° Fahr. is

$$H_T = \frac{1 + 0{\cdot}000088t}{1{\cdot}002728} H_0 \times 1{\cdot}0031,$$

or, allowing for the capillarity depression in a half-inch tube, this becomes

$$H_T = (1{\cdot}00037 + 0{\cdot}000088t)\, H_0 - 0{\cdot}009.$$

This formula has been used throughout to determine the steam pressures required for the verification of boiling-points to be discussed later (pars. 23 and 24).

The Thermometers.

21. The thermometers used for the measurement of the temperatures of supply and discharge of the stream of water passing through the brake were supplied by Mr. J. Casartelli of Manchester.

Their indications were read through the glass walls of their respective chambers by eye simply, parallax being avoided by the use of a small mirror placed behind the thermometer in each case.

Freezing-point Thermometers.

22. Two similar thermometers were obtained, one only of which was ever used during the experiments. This was a chemical thermometer, bearing the laboratory mark 2Q, with a $\frac{1}{4}$-inch diameter stem having its scale very plainly etched in black lines on the glass. The length was $11\frac{1}{2}$ inches over all, the bulb being $1\frac{1}{2}$ inches long, and then at a distance of $2\frac{1}{2}$ inches from the top of the bulb the graduations began. The scale extended from 30° to 45° Fahr., $6\frac{3}{8}$ inches of the stem being occupied by the 15° mentioned. Each degree was divided into tenths, and it was easy to estimate to the hundredth of a degree.

The index error of this thermometer was repeatedly checked during the whole period occupied by the research by being immersed in a mixture of pounded ice and water.

The table appended gives the corrections and the dates on which tests were made :—

Date.	Reading.	Correction.
5th December, 1895	31·7	+ 0·3
20th December, 1895	31·71	+ 0·29
9th January, 1896	31·67	+ 0·33
17th January, 1896	31·67	+ 0·33
31st January, 1896	31·57	+ 0·43
5th February, 1896	32·48	— 0·48
20th February, 1896	32·46	— 0·46
16th March, 1896	32·46	— 0·46
21st April, 1896	32·47	— 0·47
25th June, 1896.	32·47	— 0·47
7th July, 1896	32·52	— 0·52

Before making the test on January 31st the hot water from the brake backed up round this thermometer, so that the sudden alteration in the reading is accounted for to some extent.

Also up to this time part of the mercury had remained stuck in the upper bulb, but Dr. HARKER, of the Physical Department, now succeeded in bringing the separated mercury down into contact with the column below.

By permission of Dr. SCHUSTER the scale of this thermometer was compared by Dr. HARKER on the 27th April, 1896, with a standardised thermometer (BAUDIN, No. 12,771) in his possession between the points 32° and 35° Fahr.

This comparison showed that the correction of — 0·47 as obtained on April 21st was correct between 33° and 34°, which was the part of the scale used in most of the experiments up to that date.

At 35°, however, the correction increased to — 0·5, and consequently in the later experiments, when the temperature of supply in the heavy trials approached this point, a suitable correction was made to that already obtained by immersion in the mixture of pounded ice and water.

Boiling-point Thermometers.

23. In the first instance two similar thermometers were made to order to be ready for use in the discharge tube, but on one of these being broken, two additional ones were obtained. Only one of the four was, however, used in the research, viz., P1.

This was a chemical thermometer with a $\frac{1}{4}$-inch stem, having the scale engraved as already described. The length was $16\frac{1}{2}$ inches over all, the bulb being $1\frac{1}{2}$ inches long, and a blank space of $5\frac{1}{4}$ inches separating the top of the bulb from the first graduation. The scale extended from 200° to 220° Fahr., the 20° occupying $8\frac{3}{8}$ inches of the stem.

During the course of an experiment the reading of this thermometer was continually altering slightly. This fluctuation made it almost impossible to read the temperatures to $\frac{1}{100}$th of a degree. So that only the nearest $\frac{1}{10}$th of a degree has been recorded throughout.

The English standard boiling point, viz., 212° Fahr., is defined to be the temperature of saturated steam under a pressure which would sustain a column of mercury 29·905 inches long at the temperature of melting ice at the sea level in the latitude of Greenwich.

This corresponds exactly, on being corrected for the variation in the value of gravity, to the modern definition of the boiling point on the Centigrade scale, the pressure in this case being equivalent to a column of mercury 760 millims. long in latitude 45°, the other conditions being as before.

It was consequently possible to use REGNAULT's steam table in the neighbourhood of the atmospheric boiling point as a standard of comparison for the scale of this thermometer.

In order to conduct the comparison in Manchester, a knowledge of the relative value of gravity was necessary.

This was deduced from a formula given in 'Mémoires sur le Pendule' (Société Française de Physique), which is given below,

$$\frac{g\phi}{g_{45}} = (1 - 0{\cdot}00259 \cos 2\phi),$$

where $\frac{g\phi}{g_{45}}$ is the ratio of the value of gravity in latitude ϕ to its value in latitude 45°.

The latitude of Manchester being 53° 29′, this gives

$$\frac{g\phi}{g_{45}} = 1{\cdot}000756.$$

The altitude of the Owens College, Manchester, has no appreciable effect on the value given by the above formula.

I give below the table of steam pressures used in the calibration of the scale of the thermometer P1.

Temperature on Centrigrade scale.	Temperature on Fahrenheit scale.	Pressure of steam in millims. of mercury reduced to 0° C. and sea level in lat. 45°.	Pressure of steam in inches of mercury reduced to 0° C. and sea level in latitude of Manchester.
99°	210°·2	733·305	28·849
100	212·0	760·000	29 899
101	213·8	787·590	30·984
102	215·6	816 010	32·102

24. The general arrangement of the apparatus used to check the scale of the thermometer P1 will be gathered from the annexed sketch (fig. 8), and from Plate 6 attached to Professor REYNOLDS' paper, (Part I., par. 48).

A is an ordinary copper boiling-point apparatus, the steam from the boiling water passing up an inner tube in which the thermometer to be tested is hung, and then flowing down again so as to jacket this tube, finally escaping into the atmosphere through the cock shown. The top of the inner tube is closed by a cork having two holes, in one of which is fitted a half-inch brass tube for connection with the manometer, the other carrying the thermometer.

B is a glass flask containing an artificial atmosphere, of which the pressure is under control.

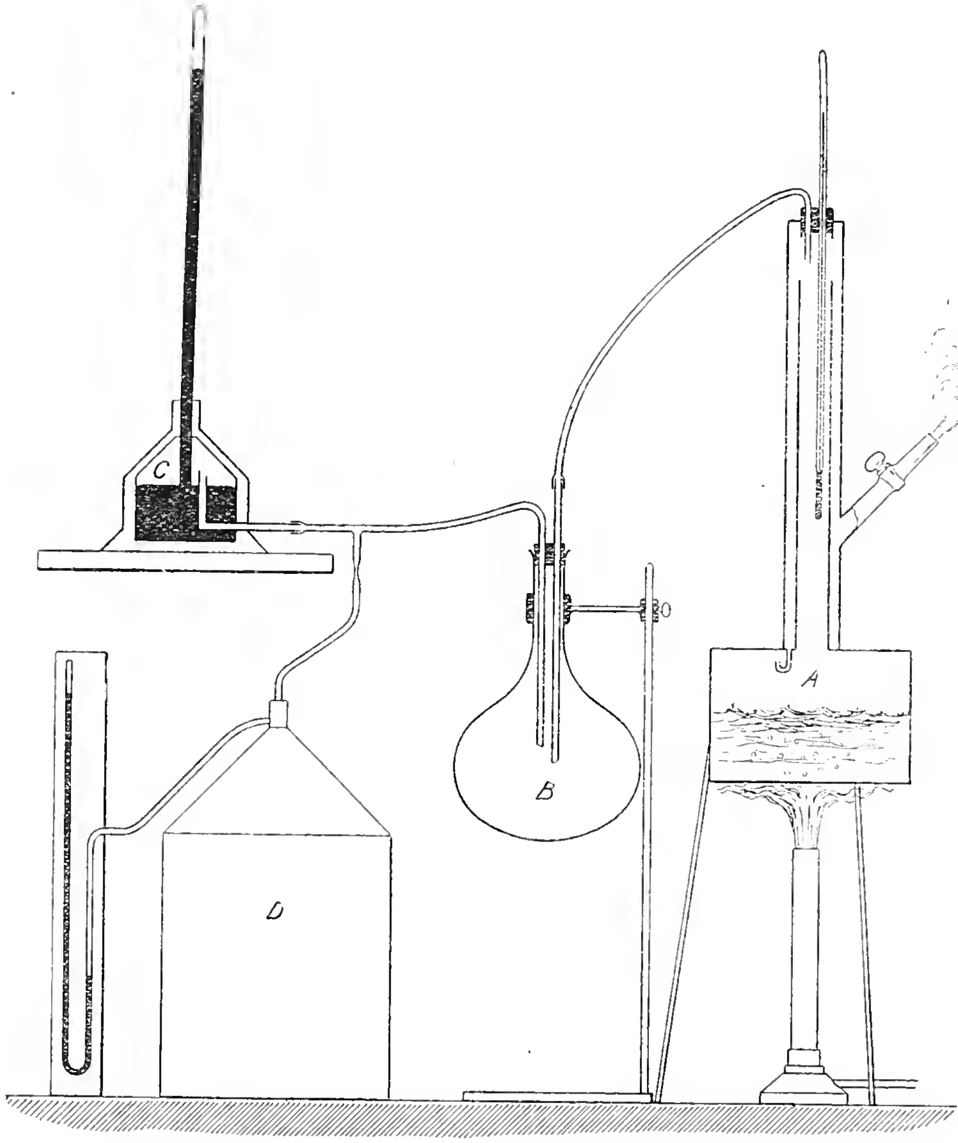
C is the combined barometer and manometer used to measure the pressure in A and B.

D is the tin receiver previously described, the pressure in which is kept at about 18 inches of mercury, as measured on a U-gauge. This receiver is in free communication through a capillary glass tube with the tube connecting the flask B and the manometer C.

The bore of the capillary tube just mentioned is just sufficient to admit a very small stream of air from the receiver through the flask B, and so out into the atmosphere by way of the cock on the boiler. The object of this stream of air was to counteract the tendency of the steam in the boiler to diffuse down the connecting rubber tube into the flask, where condensation would occur, and possibly some water might get into the barometer, it having been found quite impossible to keep a steady pressure in the apparatus whenever the steam made its way as far as the glass flask, B.

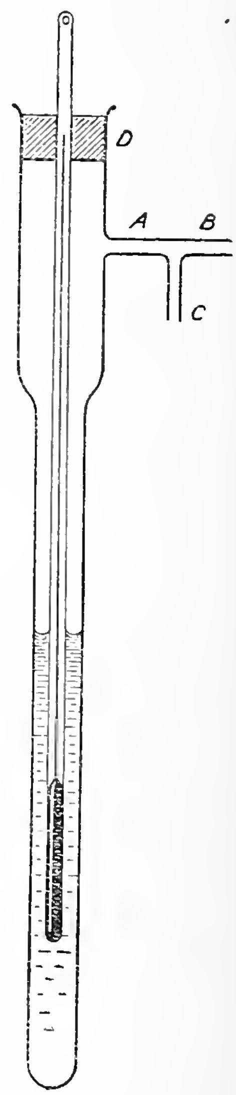
The boiler was well lagged and protected as far as practicable from draughts. A thermometer was hung alongside the brass scale tube of the barometer, and its reading was assumed to be the temperature of the barometer. Allowance having been made for this temperature, the steam escape cock was adjusted till the pressure inside the apparatus, as measured in the barometer, was at the required level. A reading was then taken of the thermometer under examination. The stem was

Fig. 8.



Apparatus for checking boiling-points.

Fig. 9.



pushed as far as possible into the boiler, the reading standing about a quarter inch above the top of the cork. Since there was always some escape of steam which blew up the hole in which the thermometer was inserted, it was not thought necessary to attempt to make any correction for the exposed part of the stem.

The annexed table gives the readings taken from this thermometer when immersed in steam of various known temperatures and the dates on which the tests were made :—

Date.	Readings obtained from thermometer P1 when immersed in steam at temperature			Correction used in experiments.
	212°	213°·8	215°·6	
28 Nov., 1895	211·43	213·26	215·01	+ 0·57
4 Dec., 1895	211·44	213·28	215·03	+ 0·56
5 Dec., 1895	211·5	213·33	215·07	+ 0·5
6 Dec., 1895	211·51 (rising)	..	..	+ 0·48
	211·53 (falling)	..	..	
12 Dec., 1895	At temperature 210°·46 reading was 210°·05			
9 Jan., 1896	..	213·38 (rising)	..	+ 0·44
		213·40 (falling)	..	
17 Jan., 1896	..	213·49	..	+ 0·34
23 Jan., 1896	..	213·49	..	+ 0·34
31 Jan., 1896	..	213·49	..	+ 0·34
8 Feb., 1896	211·76	213·57	215·3	+ 0·24
20 Feb., 1896	211·78	213·6	215·34	+ 0·22
	At 211°·34 reading was 211°·1			
16 Mar., 1896	211·86	213·66	215·4	+ 0·14
	At 211°·07 reading was 210°·87			
18 April, 1896	..	213·7	215·45	+ 0·11
15 June, 1896	211·94	213·74	215·5	+ 0·06
6 July, 1896	211·96	213·75	215·52	+ 0·04

25. In the case of each of these thermometers, viz., Q2 and P1, the water surrounding them was under a very considerable pressure, and it was therefore necessary to determine the effect of pressure on the reading given by each.

A piece of strong glass tube, fig. 9, about 1 foot in length and $\frac{3}{8}$ inch inside diameter, having one end fused up, was provided with a slightly wider mouth, in which was inserted a small branch pipe, A. This branch again split up into two arms, one of which, B, was connected through a rubber tube with an air receiver in which the pressure was indicated by a U-gauge, while the other, C, communicated directly with the atmosphere. Each of the branches B and C could be closed at will by means of a screw clip on the rubber tubing.

The pressure tube having been about half filled with water, the thermometer under consideration was fixed inside it by means of a cork, D.

In the case of the freezing-point thermometer, Q2, the pressure tube was then surrounded with pounded ice. After the contained water had cooled sufficiently for the thermometer inside to remain steady, the communication with the atmosphere was closed, and the full pressure of the air receiver put on the thermometer bulb by opening the clip on the tube, B. The rise in the reading due to the known rise of pressure was then noted. A number of these observations were made, using different additional pressure in each case. The result obtained was that for a rise in pressure on the bulb due to 1 inch of mercury, the rise in the reading was 0·0072°.

In the case of the boiling-point thermometer, P1, the pressure tube was immersed in the steam generated in the copper boiler previously alluded to. Similar procedure gave in this case a mean rise of 0·0066° per inch rise of pressure.

After applying corrections (to be dealt with later—par. 62), rectifying the thermometric indications on this account, I think that no error of greater magnitude than 0·01° can have existed in the calculated mean rise of temperature in any trial.

On 180° this gives accuracy of 1 part in 18,000.

26. In addition to the thermometers just dealt with, three others were used, on the readings of which depended the additive corrections to the heat already referred to. One of these indicated the atmospheric temperature, while two others were placed one on the stuffing-box and the other on the shaft bearing.

On the differences of heat which were used as the divisors in the determination of the equivalent from each pair of trials, these corrections all became extremely small quantities, and therefore it was of no importance that small errors should exist in these thermometers. Their scales were therefore never calibrated. Still another thermometer was used to determine the temperature of the stream of water entering the tank. As it was only necessary to keep this temperature in each pair of trials at the same level, errors in this thermometer were negligible.

Weighing Machine and 25-lb. Weights used on the Brake.—(Part I., par. 40.)

27. The absolute value of the unit used in the graduation of the lever of the weighing machine was a matter of indifference, but it was of vital importance that the same unit should be used for the weighing machine and for the 25-lb. weights used on the brake.

A set of iron weights were, however, sent down to the Manchester Town Hall, and there compared with the Board of Trade standards.

The comparison of the 25-lb. weights with our standard 25 lbs. was one of the first things undertaken in the course of the investigation. This was done by first balancing the standard placed on the platform of a small weighing machine in the laboratory by adjustment of the rider weights on the lever of the machine. The standard was then removed, and one of the 25-lb. weights substituted, a balance being made by adding to or drilling out some of the lead inserted in the weight.

This adjustment was accepted as perfectly satisfactory till towards the close of the experiments, when a small difference in the value of the equivalent as derived from trials in which different numbers of the weights were used, seemed to suggest an error in the weights themselves.

Accordingly, on the 9th June, 1896, I again compared the weights with the standard on a temporary balance, consisting of a simple lever with three knife-edges in a straight line, with the following result :—

Weight number.	True weight.
1	25·00
2	25·02
3	25·03
4	25·02
5	25·01
6	24·99
7	25·02
8	25·02
9	25·03
10	25·00
11	25·04
Hanger	24·99

And a lead balance weight to be referred to later, which weighed 13·98 lbs. instead of 13·97 lbs. as assumed

On the 17th of January, 1896, a set of four of these 25-lb. weights, at that time all supposed accurate, were used as a standard 100 lbs., by which a series of corrections to the 100-lb. scale of the weighing machine were obtained. These corrections have been used throughout the investigation, and are given below :—

Reading .	300	400	500	600	700	800	900	1000	1100	1200	1300
Correction	0·4	..	..	−0·12	−0·42	−0·5	−0·65	−1·12	−1·22	−1·31	−1·78

Rider weights Numbers 2 and 3 were at the same time made correct on their whole range.

In June another comparison was made, and the set of four weights, Numbers 2, 8, 9, and 10 were found to give substantially the same list of corrections as previously obtained.

The complete set of weights were then again weighed on the weighing machine, using the list of corrections given, together with the true value of the standard 100 lbs. The result was a verification of the list of their values already given.

The maximum error that might possibly be produced by using the weights on the brake in specially arranged groups was found to be—

In a pair of trials carrying moments of 1200 and 600 ft.-lbs. respectively, − 0·037 per cent. or + 0·043 per cent., and in a pair of trials run with moments of 1200 and 400 ft.-lbs. respectively, − 0·025 per cent. or + 0·03 per cent.

The value of the equivalent obtained from a set of six trials in which the weights had been specially arranged to eliminate the above possible error entirely, gave a result which did not differ at all from that previously obtained, and it may therefore be safely assumed that in the first series of trials this error did not occur to any sensible extent.

I think that, especially with the above result in view, the loading of the brake may be taken as absolutely accurate.

As to the limit of accuracy of the weighings in the 600 ft.-lb. trials, the weight of water dealt with was approximately 470 lbs. On this quantity the maximum probable error was 0·02 lb. in any trial. This gives greater accuracy than 1 part in 20,000.

The Adjustments of the Brake.

(1.) *Length of the Lever.*—(Part I., par. 45.)

28. This length was required between the centre line of the engine shaft traversing the brake and the V-groove carried by the lever.

It had been previously observed that both the shaft and the brake shifted a little horizontally when the engine was started, from the positions occupied with the engine stationary. It was therefore necessary to make the comparison between the length of the lever and our standard 4-feet with the engine running. Also, since the length of the lever varied with the temperature of the brake, this temperature was maintained, as in all the trials, at 212° Fahr.

Between the brake and the adjacent bearing the shaft is 4 inches diameter within $\frac{1}{1000}$ of an inch.

At a distance of 3 feet 10 inches from one of its square ends a fine line was scribed on a steel straight edge. This straight edge was then held with the square end aforesaid butting against the shaft, the length being horizontal and perpendicular to the line of shafting, and the distance between the straight edge and the lever being 10 inches. At a distance of 11 feet from the other side of the lever a theodolite was set up and adjusted so that the vertical plane of collimation of the instrument was parallel with the shaft and contained the line scribed on the face of the straight edge.

A steel scale, graduated to $\frac{1}{50}$ of an inch, was fixed firmly on to the lever, and a reading of this scale was taken through the telescope without altering the adjustments mentioned. This reading, of course, referred to the point on the scale just 4 feet distant from the centre line of the shaft. By a slight rotation about the vertical axis the line of collimation was then made to cut the centre line of the groove, and then a vertical rotation enabled a second reading of the scale to be taken.

A number of these observations were made while the brake was subjected to moments of 1200, 600, and 400 ft.-lbs., and they all indicated that the length of the lever in the trials made was $4' + 0{\cdot}02''$.

A correction to the value of the equivalent derived directly from the trials is therefore necessary on this account. It amounts to $+ 0{\cdot}0417$ per cent.

With this correction added, I think that the length of the lever can be assumed accurate to $\frac{1}{200}$ inch, or 1 part in 10,000 nearly.

(2.) *The Balance of the Brake.*—(Part I., par. 9.)

29. If a pair of trials are run, the one with a heavy indicated load, M_1, and the other with a lighter one, M_2, and if m be the moment carried by the brake on account of its initial want of balance, then the works done in the two trials are

$$U_1 = 2\pi N_1 (M_1 + m)$$
$$U_2 = 2\pi N_2 (M_2 + m)$$

where N_1 and N_2 are the revolutions in the two cases.

The difference of the work done

$$= 2\pi \{N_1 M_1 - N_2 M_2 + m (N_1 - N_2)\}$$

and the relative error involved in writing for this

$$2\pi (N_1 M_1 - N_2 M_2),$$

which has been done in these experiments, is

$$\frac{m (N_1 - N_2)}{N_1 M_1 - N_2 M_2}, \text{ very nearly.}$$

This error is 0 when $N_1 = N_2$.

The speed of the engine was therefore always regulated to the end that the number of revolutions in each of a pair of trials which were afterwards to be compared together should be approximately the same. As a general rule, this object was very nearly attained.

The maximum value of $N_1 - N_2$ was about 300, the values of N_1 and N_2 being approximately 18,000.

Under these circumstances, in trials carrying loads of 1200 and 600 ft.-lbs. respectively, the above error amounts to

$\dfrac{300}{18000 \times 600} = \dfrac{1}{36000} < 0{\cdot}003$ per cent. per ft.-lb. of error in the balance of the brake.

The method pursued to determine the want of balance was as follows :—

The lever was freed from all extraneous loads.

The brake and its pipe connections were then all filled with water, so as to be in the same condition as during the progress of a trial.

The lever was then lifted till its end was in its mean position opposite a pointer at a fixed height from the ground. A load was then gradually added to the front side of the brake till the friction of the bearings was overcome, and the lever fell. An observation of the moment required to cause the motion was then made. A series of twenty of these observations were made for the front and then a second series of twenty for the back of the brake, in which case the load on the back had to lift the lever from its mean position.

On taking the difference of the means of these two series of observations, the friction is eliminated and the resulting moment represents the error of balance of the brake.

Since in the course of a trial the lever oscillates a little from its mean position, the brake will, when in motion, be working against the resistance offered by the linkage connected with the regulating cock. When at rest, however, this resistance will not affect the load at all. In view of this fact, two determinations of the error in balance were made, the first with the brake working free of the linkage, by allowing the small motion to take place in the slack of the pin-joints, the second with the brake working against the resistance of the regulating apparatus. The results obtained were

In the first case, error in balance = 45·5 ft.-lbs.
In the second case, error in balance = 41·73 ,,

A mean of these two quantities would probably be approximately correct, viz., 43·615 ft.-lbs.

The lead balance weight previously mentioned, and weighing 13·97 lbs., was substituted for one of the 25-lb. weights, on the removal from the lever of the brake of a rider weight and a balance weight whose combined moment (par. 40) was calculated at − 44·12 ft.-lbs.

The actual uncompensated error in the balance appears therefore to be practically $\frac{1}{2}$ ft.-lb. This is so small, and the balancing of the brake such a very difficult operation to perform with any approach to accuracy, that any error there may be has been ignored, and the balance assumed perfect in all the calculations.

The end of the lever has always been kept at the level of the pointer indicated before, and by this means all error due to the varying horizontal position of the centre of gravity of the brake has been avoided.

Terminal Corrections to the Apparent Heat Generated.—(Part I., par. 31.)

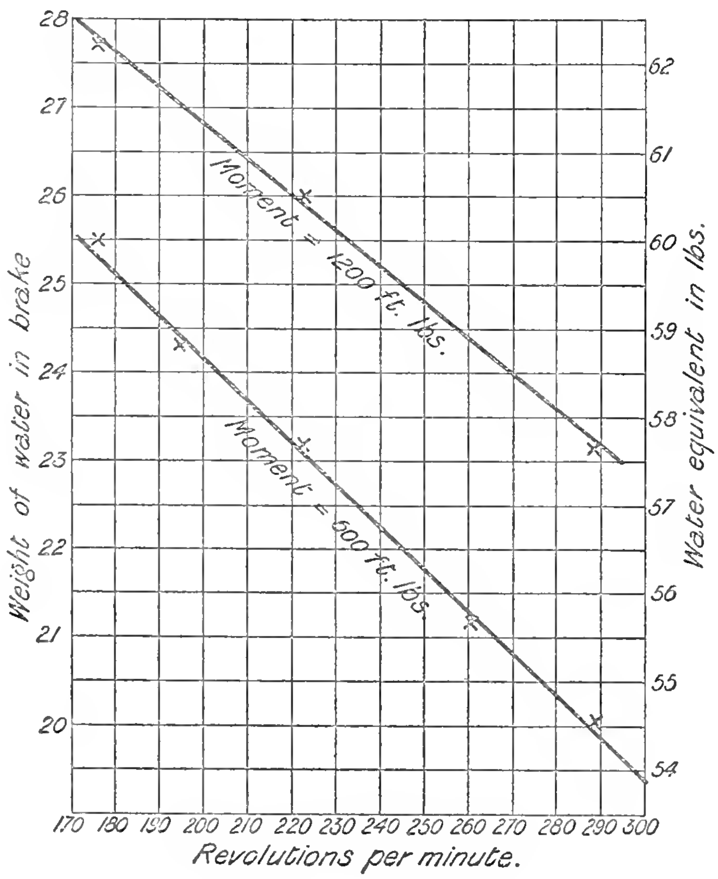
30. In order that the work done in any trial should be exactly equivalent to the heat generated in the water used, it was necessary that the total heat contained in the brake itself should be the same at the beginning and end of the trial.

This condition was rarely fulfilled, since it required that the weight of water in the brake, together with its temperature, should be unaltered at the close of the trial.

A determination was made of the amount of water contained by the brake at various speeds by suddenly stopping the engine when running at any given speed, simultaneously shutting off the water supply to the brake, and afterwards draining off and weighing the water shut in.

The results are shown in the annexed curves.

Fig. 10.



Curves showing water contained by and water equivalents of brake and contents at varying speeds.

The weight of brass in the brake is 368 lbs. Taking 0·094 for its specific heat, the water equivalent is 34·6 lbs.

To obtain a scale of weights representing the water equivalents of the brake at different speeds, we have to add 34·6 to the weights of water contained at the different speeds.

This scale is given at the right of the curves just alluded to (see above).

A correction to the heat obtained is now very easily deduced.

Let w_1 = water equivalent of brake at commencement of trial.
w_2 = ,, ,, end ,,
t_1 = temperature of water in discharge pipe at commencement of trial.
t_2 = ,, ,, ,, end ,,

Therefore, additional heat generated in the brake $= w_2t_2 - w_1t_1$, and this quantity is added to the heat already calculated as generated in the water.

The speed indicator which was used in the determination of the number of revolutions per minute required as the ordinate in the curve of water equivalents, was not reliable to one or two revolutions, and, therefore, unless a large difference of speed was indicated between the commencement and end of a trial, this difference was altogether ignored, and the rise in temperature was multiplied by the constant corresponding to any particular load at 300 revolutions to obtain the terminal correction.

The speed gauge required a negative correction of 11 at 300 revolutions, and, consequently, the curves give 57·6 and 54·6 as the water equivalent of the brake when loaded with 1200 and 600 ft.-lbs., respectively.

By interpolation from the above values 53·6 was obtained and used as the water equivalent in trials carrying a moment of 400 ft.-lbs.

Loss of Water by Evaporation and Leakage from the Discharge Pipe and Tank.— (Part I., par 37.)

31. In order to test the general efficiency of the discharge pipe as a conveyer of the water used, it was disconnected in June, 1896, from the brake, and the circulating pump was arranged to pump the water out of the tank and through the discharge pipe, which emptied itself again into the tank by means of the tipping switch.

The stream of water was regulated so as to correspond exactly with the quantities passed in trials carrying loads of 400, 600, and 1200 ft.-lbs. In a period of 62 minutes it was found that in each of these cases the loss approximated very closely to a quarter of a pound of water when its temperature was between 90° and 100°. Since this loss was the same in all the trials it has not been thought necessary to make a correction rectifying the heats on this account, for it would be completely eliminated in the differences of heat used in the calculation of the values of K given in the tables, if the interval of temperature through which the water was raised in the brake was the same in corresponding light and heavy trials.

When, however, I examined the results after the final reduction had been made, I found that the mean temperature of supply in the light trials was 0·7° lower than that in the heavy trials.

Consequently the mean difference of heat would require a slight correction, which, however, is less than $-$ 0·000002 relatively to the whole. This, being quite outside our limits of accuracy, has been ignored.

The Main Experiments.

32. In December, 1895, the apparatus, though not yet quite complete, was in a sufficiently advanced state to make it possible to commence the main K experiments.

The observations were taken and reduced in every experiment in substantially the same manner that I have described (paras. 17, 18, and 19). Some of the particulars mentioned were, however, omitted in the earlier trials, and were only recorded subsequently after their importance had come to be recognised.

In all, 80 trials were made on which any reliance has been placed, and these will be dealt with in different series, between any consecutive two of which some slight alteration had been made in the apparatus, the method of taking the observations, or of reducing the same; all these alterations leading up to the finally adopted methods which have been described.

33. I must first mention two sets of trials which do not appear in the tables. They were commenced in December, 1895, and were made mainly with the object of gaining experience in the behaviour of the apparatus, and of determining the most favourable conditions under which the experiments could be conducted.

The moments carried by the heavy and light trials in each set were 1200 and 600 ft.-lbs. respectively.

The speed was in the first set 230 revolutions per minute, and in the second set 180 revolutions per minute.

With the following exceptions the apparatus and methods were the same as described.

I. Omissions and faults in apparatus.

(1.) There were no thermometers on either the stuffing-box cover or on the main bearing, and consequently no effectual attempt could be made to keep these parts of the shaft at the same temperature in a pair of trials.

(2.) There was no means of catching the leakage from the stuffing-box, or from the bottom regulating cock.

(3.) The rising pipe at this time only maintained a head of about 5 feet of water over the thermometer in the discharge pipe.

(4.) The hand brake had not been fitted to the shaft.

II. Omissions and faults in the methods employed.

(1.) No corrections were added to the heat as given by the formula $(W_2 - W_1) \times (T_2 - T_1)$.

(2.) The heavy trials were of only half-an-hour's duration, in order that the second reading taken of the weight of the tank should lie on the same part of the scale of the weighing machine, which had not up to this time been corrected, in both heavy and light trials.

The results obtained were not very consistent, but, perhaps largely on that account, the trials admirably fulfilled the purpose for which they were made.

The importance of the terminal corrections was clearly indicated when the results were considered, and consequently means were at once taken to apply these corrections to the preliminary reduction of all subsequent trials. These included the provision of the hand brake, by means of which the engine speed on starting and finishing the trials could be easily controlled, and the observations of the speed of the engine and the temperature of the brake which were taken at the moments of starting and ending the trials.

Again, the terminal corrections and other incidental errors had very unequal weights when acting on the quantities obtained in the hour light trials and in the half-hour heavy trials—which latter quantities required doubling before the subtraction requisite to eliminate losses of heat could be effected.

It was, therefore, decided that in future all trials should be of equal duration (viz. 62 minutes), and this necessitated the immediate careful checking of the scale of the weighing machine, which was thereupon proceeded with. Furthermore, it was probable that many of the discrepancies which occurred were due to the small quantities of water it was possible to deal with at the low speeds hitherto used, and to remedy this defect a larger amount of work was done and heat generated by increasing the speed in all the recorded trials to 300 revolutions per minute. Incidentally this increase of speed was conducive to the steadier running of the engine.

I was much troubled with bubbles of steam in the discharge pipe, and to prevent their formation the rising pipe was lengthened till it gave a head of 11·3 feet over the thermometer bulb.

These trials also furnished information which led to the adoption of a pressure of 9 inches of mercury in the artificial atmosphere. It was found that with higher pressures than this the air by some means found its way into the discharge pipe, even with the lengthened rising pipe in position.

During the first few trials the only regulation of the water supplied to the bearings of the brake consisted of screw clips on the rubber pipes carrying the water. These were found to be very inefficient, and two cocks were substituted, each of which carried a scale which showed the amount to which it was open at any time.

34. Before dealing with the tables showing the final reduction of the experiments made, it is necessary to mention a preliminary reduction of trials Nos. 1 to 42 shown in Table A (p. 413), from which the constants used in the determination of the losses of heat by conduction along the shaft, and also by radiation, were deduced.

In this table the actual observations are as far as possible omitted, since they will appear later in the completely reduced tables.

It will be seen that the table consists of three similar parts, referring respectively to the heavy trials, the light trials, and the differences.

In each part

Col. 1 gives the number of the trial.

Col. 2 gives the work done, calculated in the ordinary way.

Col. 3 gives the heat generated, as calculated from the formula $(W_2 - W_1)(T_2 - T_1)$, all corrections being omitted.

Col. 4 gives the terminal corrections, for which, as I have said, the necessary observations were always taken.

Cols. 5 and 6 give respectively the mean differences of temperature observed between the stuffing-box and the top and bottom brasses of the main shaft bearing.

The quantities in brackets are not actually observed differences, but were deduced in the manner to be hereafter explained (par. 43).

These differences are + or − according as the stuffing-box was hotter or colder than the adjacent bearing.

Col. 7 gives the mean difference of temperature observed between the brake and the surrounding air. These differences are, of course, all positive.

The quantities given in the part of the table headed "differences" are in every case the remainders which are left on subtracting the corresponding quantities under the heading "light trials" from those appertaining to the "heavy trials."

In the last column are given the values of K, obtained by dividing the work occurring under the heading differences, by the heat, to which has first been added the terminal correction.

The conditions under which each series of trials given in Table A was run are enumerated below.

In every case the engine speed was 300 revolutions per minute, as read on the speed-gauge.

In all heavy trials the moment was 1200 ft.-lbs., with the exception of Series IV., in which the moment was 1244·12 ft.-lbs.

In all the light trials the load was 600 ft.-lbs.

Series I.

35. This series contains trials Nos. 1 to 11, No. 5 being omitted on account of an accident to the revolution counter.

In all these trials the outer brass skin of the brake was exposed directly to the atmosphere, and consequently the loss of heat by radiation was very large.

No attempt was made to catch the small quantities of leakage occurring at the stuffing-box and the bottom regulating cock.

The water supply to the stuffing-box was only regulated to the end that the bearing should not become unduly hot, and no record was kept of the temperature gradient along the shaft till trial No. 10 was reached.

In order to avoid any bias which might be given to the experiments by always combining a trial of one type with one of another type, trials of both of which types were always made at the same relative part of any day, the relative order of running was changed as indicated by the dates and times given in Table B. (Part I., par. 32). This method of combining the trials was adopted because at this time it was not as a rule possible to make more than two trials a day successfully, for breakdowns of a more or less serious nature were of fiequent occurrence.

Referring now to the preliminary reduction shown in Table A, Series I.:

The values of K, Nos. I., III., IV., and V. are seen to be in close agreement, notwithstanding the comparatively rough method of reduction used.

Determination No. II., however, stands out as very distinctly higher than the others, and the cause of this was fortunately evident.

In order to prevent the attempted rotation of the small handle shown in the illustrations at the end of the brake lever, one revolution of which altered the load on the brake by 1 ft.-lb., one of my assistants had tied it to the hanger carrying the load. The string making the connection was very tight, and the load was pulled perceptibly out of the perpendicular plane passing through the groove on the lever.

This fault was sufficient to condemn the two trials Nos. 3 and 4, and they do not appear in the final table on that account.

A wooden clip was subsequently added to prevent the rotation of the handle and its attached screw.

Lagging. (Part I., par. 33.)

36. The results given by the four accepted determinations of Series I. were so consistent that it was decided to proceed at once with the lagging of the brake, which, up to the present time, had been deferred on account of want of confidence in the apparatus generally.

The lagging consisted of a layer of about $1\frac{1}{2}$ inches of loose cotton wadding with which the whole of the exterior of the body of the brake was covered, together with the discharge pipe between the brake and the thermometer chamber. The cotton was all tied firmly in position, and the whole was enclosed in a covering of thick flannel.

As will be seen later, this lagging reduced the radiation by nearly 75 per cent. Its weight, about 2 lbs., was inappreciable, and, being evenly distributed, could not affect the balancing of the brake to any extent which it would be possible to detect.

The lagging was, I believe, of use, more especially in that it protected the bare metal from the strong draughts which often occurred in the engine-room. It required very careful attention, however, to protect it against dampness, and on this account I am not certain that better results would not have been obtained without it.

Series II.

37. With the exception of the addition of the lagging, no alteration was made in either apparatus or method between trials 11 and 12.

Sufficient experience and confidence in the apparatus had now been gained to enable me to make three trials per day, as a rule two being made in the morning and one in the afternoon, a stop of about one hour being made after the second trial. The brake was not allowed to cool down during this interval; the hot water contained on finishing the morning's run being shut in.

In Table A, the value 787·4 is given as the result of the combination of trials 12 and 14. There was evidently something amiss with this result, and as the combination of trials Nos. 13 and 14 gave the result 779·4, which agrees fairly closely with those given in Series I., the explanation which at once suggested itself was that the new lagging was damp when the day's running began and had dried before the commencement of trial 13. On this account, trial No. 12 has been expunged from the final Table B, and takes no further part in the investigation.

Series III.

38. As it had by this time been found possible to run three satisfactory trials per day, the most obvious way of combining them was to make three trials, all carrying the same load, on the first day; while the trials required to complete the three determinations were run on the next convenient day.

This method was pursued during the whole of the subsequent course of the investigation.

From this series onward I made an attempt to keep the temperature gradient along the shaft, between the brake and the adjacent bearing, the same in each pair of trials. In trial No. 21, I took observations for the first time of the temperature of the lower brass in the main bearing. In these trials also, the possible importance of the small leakage of water occurring along the spindle of the lower regulating cock, for the first time became apparent. The weight of water actually leaking away had not, I think, any appreciable effect, but owing to its high temperature it was nearly all evaporated, and, consequently, may have had a sensible effect in the lowering of the temperature of the water discharged from the brake. No successful means were yet devised for catching this water. So, in this series, it still remains as a possible source of error.

Series IV.

39. For use in the regular engine trials the brake is provided with a rider weighing 48 lbs., which can be traversed along a graduated scale on the lever by means of a leading screw. In order to maintain the balance of the brake, it carries at the back a second fixed load of 74·6 lbs.

These two large masses of iron had hitherto been left on the brake, but it seemed probable that they would very much affect the flow of heat away from it between any pair of consecutive trials (Part I., par. 33), for they continued to rise in temperature during the whole of any day on which experiments were made, and evidently they would absorb heat more rapidly when cold in the early part of the day than when hot later. It was therefore decided to remove them. Their combined moment about the engine shaft was − 44·12 ft.-lbs.

No allowance was made for this alteration in the loading of the brake, and, consequently, the moment in these trials was 1244·12 ft.-lbs., this figure having been used in the calculations given.

In order to bring the trials under some general denomination, this series has not been further reduced, nor combined with a corresponding set of light trials.

With the intention of stopping the leakage at the bottom cock, I had had some more packing placed in the gland surrounding the cock spindle. This did, to some extent, reduce the leakage, but it also had another effect which will be referred to under Series V.

Series V.

40. For the purpose of keeping the loads on the brake at the values carried by trials preceding the removal of the rider and balance weights, one of the 25 lb. hanger weights was removed, and for it were substituted some lead sheets weighing 13·97 lbs.

This lead weight then corresponded with the initial want of balance to a moment of 100 foot lbs., made up as follows :—

Want of balance	44·12	foot lbs.
Moment of lead weight . .	55·88	,,
	100	,,

After these trials had been made, I determined, with Professor REYNOLDS, by means of a spring balance, the force necessary to move the bottom cock. This was found to amount to a moment of 30 ft.-lbs. on the brake, and on this account this series of trials, though appearing in the final tables, have not been allowed any weight in the calculation of the final mean value of K. The preliminary reduction of Table A gave what were apparently very good values of K, but this only shows the small effect on the mean moment produced by variations in the resistance offered to the brake's motion, and this although its period of oscillation was very long.

Series VI.

41. These trials differ from those of Series V. only in the fact that the extra

packing had been removed from the gland on the cock spindle, while a means of catching the whole of the leakage, and at the same time preventing its evaporation, had been provided (par. 14). The whole of the leakage was credited with the temperature of the water in the discharge pipe, and was weighed with the main stream of water which had been caught in the tank.

Series VII.

42. These trials were made under similar conditions to those in Series VI. In the two last trials, however, viz., Nos. 39 and 42, some leakage was observed and caught from the stuffing-box.

An approximate estimation of the loss of heat due to this leakage is given in Table B, and has been included in the heats given in Table A.

Determination of the Loss of Heat by Conduction along the Shaft.

43. In the trials enumerated in Table A, the varying values of the temperature gradient, existing in the shaft leaving the brake, might evidently be a cause of comparatively large losses of heat which were not eliminated in the differences of heat, so far assumed to be equal to the corresponding differences of work.

It therefore became important to determine, at least approximately, what was the loss of heat by conduction along the shaft in each trial.

I have already said that the temperature of the shaft in the main bearing was assumed to be the same as that of the lower brass, while the temperature on leaving the brake was similarly taken as that of the stuffing-box cover.

Unfortunately, before trial No. 21, I had made no record of the temperature of the lower brass.

It was, however, found that in trials Nos. 21 to 41 the mean temperature of the lower brass exceeded that of the upper brass by about 7° Fahr.

Consequently, in Column 6, in the parts of Table A, where no observations had been taken, an estimation of the difference of temperature between the stuffing-box and the lower brass was made by subtracting seven from the difference occurring in Column 5. In this manner the differences entered in brackets were obtained for trials Nos. 10 to 20.

It appears that we have, therefore, 10 determinations, viz., V., VI., VII., VIII., IX., X., XI., XII., XIII., and XVIII., in which the differences of heat generated require a positive correction on account of the unbalanced conduction along the shaft, and four determinations, viz., Nos. XIV., XV., XVI., and XVII., in which those differences require a negative correction.

Assuming, as is very nearly the case, that the losses of heat by radiation are eliminated in the differences of the heats, it follows that by taking C = loss of heat per trial, by conduction along the shaft, per unit difference of temperature between the stuffing-box and lower brass,

Then C is given by the equation

$$\frac{675844869}{867995 + 75 \cdot 6\ C} = \frac{271143956}{348866 - 22 \cdot 5\ C} = K,$$

where the numerators represent the sums of the differences of work in the sets enumerated above, while the first terms of the denominators represent the sums of the differences of heat in the same sets, to which the terminal corrections have been added. The second term in each denominator represents the correction to be applied to the differences of heat for unbalanced conduction along the shaft.

On solving the equation we get

$$C = 12, \text{ very nearly.}$$

This agrees very closely with the value C = 13·61, which may be calculated from the dimensions of the conducting shaft, viz., 4 inches diameter and $2\frac{3}{4}$ inches long, and FORBES' value of the conduction coefficient for iron, viz.:

(0·1429 in C.G.S. unit).

Since nothing was known as to the internal thermal condition of the shaft, the figure 12 has been used throughout as a sufficiently close approximation to the constant required.

The corrections to the heat for conduction along the shaft in each trial were then obtained by multiplying the fall of temperature between the brake and bearing by 12.

The sign of the correction varies, of course, with the sign of the temperature gradient along the shaft.

Determination of the Loss of Heat by Radiation.

44. Under this heading are included all losses of heat not already dealt with under the headings "terminal corrections," "loss by conduction," and "loss by leakage of water."

Radiation in the Unjacketed Trials—Series I.

45. Determination No. II., consisting of a combination of trials 3 and 4, is omitted, for the reasons given. A constant R, representing the loss of heat by radiation per trial per unit difference of temperature between the brake and surrounding air is required.

In Tables B and C, the corrections to the heat are given for terminal errors and conduction along the shaft, the calculation of which has been explained.

The quantities given in the annexed table are sums obtained by adding together the corresponding quantities in Series I. of Tables B and C.

In trials 1, 6, and 9 the loss by conduction has been assumed the same as in trial 10; while in trials 2, 7, and 8 this loss has been given the same value as calculated for trial No. 11.

SERIES I.—Unjacketed Trials.

	Work done.	Heat.	Terminals.	Conduction.	Diff. of temperature between brake and air.
Heavy trials . .	542,876,020	677,309	+ 19	+ 116	556·4
Light trials . .	272,418,189	330,280	− 131	− 496	558·4

We have, therefore, the same value of K given by

$$K = \frac{542,876,020}{677,444 + 556 \cdot 4\,R} = \frac{272,418,189}{329,653 + 558 \cdot 4\,R},$$

and, solving for R, we get

$$R = 36 \cdot 86,$$

or, using this value of R and solving for K,

$$K = 777 \cdot 81,$$

which is the mean value deduced from this series of eight unjacketed trials.

Radiation Coefficient for Jacketed Trials, Nos. 12 *to* 42.

46. As in Series I., we get the sums of work, heat, &c., shown in the annexed table :—

	Work done.	Heat.	Terminals.	Conduction.	Diff. of temperature between brake and air.
Heavy trials . .	1,752,718,746	2,236,681	− 64	− 886	1862·6
Light trials . .	874,319,846	1,108,013	− 183	− 1369	1872·5

In this table the sums are given of the respective quantities in the trials used in Determinations VI. to XVIII. inclusive, Series No. V. being included, because no error was apparent in the quantities obtained; Series No. IV. being omitted, since the moment given could not be guaranteed correct with any certainty.

We thus get the following equation for R :—

$$\frac{874,319,846}{1,106,461 + 1872 \cdot 5\, R} = \frac{1,752,718,746}{2,235,731 + 1862 \cdot 6\, R},$$

which, on solution, gives

$$R = 9 \cdot 33,$$

and, substituting for R,

$$K = 777 \cdot 91.$$

47. The loss of heat by radiation from the brake, as given in the Tables B, C, &c., was determined by multiplying the difference of temperature between the brake and the air by the radiation constants, calculated as just described.

The Tables B, C, and D, giving the results of trials 1 to 42 inclusive, should now be self-explanatory.

The mean value of K given by the eight unjacketed trials I have mentioned was 777·81.

48. The best way of stating the values of K obtained throughout seemed to be as follows :—

The sums of the differences of the works and of the corrected heats were taken for each series of trials, and then a mean value of K for the series was found by dividing the first of these quantities by the second.

The values of K given as the mean for each series in Table D have been calculated in this way.

49. A mean value of K can be obtained from the jacketed trials contained in Series II., III., VI., and VII. (Series V. being kept out of the determination on account of the possible error already noticed), by finding the sums of the respective differences of work and heat given with each of these series in Table D, and then dividing the work by the heat so obtained.

The sum of the differences of work in Series II., III., VI., and VII.

$$= 676,259,560,$$

and the sum of the corresponding differences of heat

$$= 869,396\,;$$

therefore the mean value of K given by the accepted jacketed trials so far considered is

$$K = \frac{676,259,560}{869,396} = 777 \cdot 85.$$

From this mean none of the values obtained from any one of the above series differs by as much as 0·03 per cent.

Closer agreement than this could not possibly be expected, and it was consequently decided to vary the trials somewhat, in order to determine if any errors had been overlooked. For this purpose I made two fresh series of six trials each, the light trials carrying a moment of 400 ft.-lbs. only, none of the other conditions being altered in any way.

50. The full reduction of these Series (Nos. VIII. and IX.) is shown in the two Tables E and F.

As before, three trials were run on each day, but the last trial, on April 1, was not finished on account of an accident preventing me getting the correct weight of the water discharged by the brake. There are, consequently, only eleven trials in the tables. The radiation constant for these trials worked out to 8·16.

The mean value of K, given by the whole eleven trials, was 778·14, which is lower than the two means for the separate series in Table F, on account of the inclusion of the light trial No. 45, which does not appear in Table F.

This new value of K, viz 778·14, did not agree so closely with the former one of 777·85 as we had hoped, and, after reducing the last two series of trials, I devoted all my time to the checking of the whole of the apparatus anew.

It was a consequence of this stringent supervision of every separate part that the small errors in the 25-lb. weights, already noticed, were discovered (par. 27).

51. Calculation showed that this error might account for the discrepancy observed, and so it was decided to run a fresh series of trials with the weights so arranged that no error could appear on their account.

In order to have no known outstanding errors whatever, I made a small rectangular trough, fitted with a drain-pipe, by means of which all leakage from the stuffing-box was caught.

52. A series of fifteen trials, numbered 54 to 68 inclusive, was accordingly made, beginning on June 29, 1896. Owing, no doubt, to the long rest which the apparatus had had since Easter, a number of accidents were met with which completely spoiled the whole series.

The lagging of the brake was very damp when the series was begun, and, on account of the bursting of the various rubber-pipe connections, it did not thoroughly dry during the whole course of this series of trials.

For these reasons the results are not tabulated.

53. After remedying all the defects which had developed in the previous week's running I made two fresh series of six trials each between July 7 and 10 inclusive.

No further accidents occurred and the results were in every way satisfactory.

These are shown in Tables G and H.

The radiation constant worked out at R = 7·98.

The mean value of K, given by the two series, was

$$K = 777{\cdot}85,$$

which happens to be exactly the same as obtained previously from Series II., III., VI., and VII.

54. This last lot of trials afforded no explanation of the small difference (778·14–777·85)

$$= 0·3 \text{ ft.-lb. nearly,}$$

which occurred between the results give by the 1200–600 ft.-lbs. determination and the 1200–400 ft.-lbs. determination respectively.

The difference, of course, may be due to terminal errors, which, I think, have been mainly responsible throughout for the small discrepancies found to occur between individual determinations. It is more likely, however, that the small quantity of water dealt with in the 400 ft.-lbs. trials, and the consequent greater effect of the oscillations of the brake on the mean moment, may have introduced some error into these lightly-loaded trials. Further, some slight bias may have been given to the Series, Nos. VIII. and IX., by the long rest caused by the Easter Vacation, between trials 47 and 48.

55. In the annexed table I give the mean value of the work done and of the heat generated in the heavy and light jacketed trials respectively, against which no known sensible error can be placed.

Trials Numbers.	Mean work per trial.	Mean heat per trial.
Heavy trials : (13, 17, 18, 19, 20, 35, 36, 37, 38, 39, 46, 47, 48, 49, 50, 72, 73, 74, 75, 76, and 77)	134,337,403	172,685
Light trials : (14, 15, 16, 21, 22, 23, 33, 34, 40, 41, 42, 43, 44, 45, 51, 52, 53, 69, 70, 71, 78, 79 and 80)	61,355,503	78,867
Differences	72,981,900	93,818

and, dividing the mean difference of work by the mean difference of heat we have

$$K = 777·91.$$

This mean value of K deduced from the experiments requires correcting on a few counts, which are due to the method of working. These will be dealt with later.

56. The table given below illustrates the almost perfect manner in which losses of heat were eliminated on the mean result, by the method adopted throughout the investigation of always working on the differences of the quantities of work done and heat generated in a pair of trials.

	No. of revolutions of shaft.	Work done.	Heat generated, less losses due to terminals, conduction, &c.	Loss of heat by leakage of water.	Terminal corrections.	Difference of temperature between stuffing-box and bearing.	Difference of temperature between brake and air.
Means for 21 accepted heavy trials	17,817	134,337,403	171,510	4	− 1	− 3·9	140·5
Means for 23 accepted light trials	17,832	61,355,503	77,710	1	− 7	− 5·4	141·5
Differences	− 15	72,981,900	93,800	3	6	1·5	− 1·0
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

A value of K can be obtained by dividing the difference of work in Column 3 by the uncorrected difference of heat in Column 4. This operation gives

$$K = 778{\cdot}06.$$

The various corrections which this number requires are as follows :—

I. Correction due to difference in number of revolutions of shaft between light and heavy trials.

Since the difference in the number of revolutions is only 15, this correction, as previously indicated, when dealing with the balance of the brake, will be zero (par. 29).

II. Correction due to loss by leakage of water from the brake.

This correction amounts to $-\dfrac{3}{93{,}800} = -0{\cdot}000032.$

III. Correction due to terminal differences of temperature of the brake.

This correction amounts to $-\dfrac{6}{93{,}800} = -0{\cdot}000064.$

IV. Correction due to loss of heat by conduction along the shaft.

This correction amounts to $-\dfrac{1.5 \times 12}{9{,}3800} = -0{\cdot}000192.$

V. Correction due to loss of heat by radiation.

Assuming 9 for the value of the radiation constant, this becomes

$$= +\frac{9}{93{,}800} = +0{\cdot}000096.$$

The total correction factor is therefore $(1 - 0{\cdot}000192)$, which gives as before

$$K = 777{\cdot}91.$$

Corrections to the Mean Value of K *given by the Experiments.*

I.—Length of Brake Lever.

57. In dealing with the calibration of the measurements of the brake (par. 28), I have already mentioned that the value of K given by the experiments would require a correction factor of (1 + 0·00042).

II.—Salts Dissolved in the Manchester Water.

58. Professor DIXON kindly furnished Professor REYNOLDS with the results of a number of analyses of the town's water made during the College session, 1894–95. The dissolved salts were

$$\left.\begin{array}{ll}\text{Common Salt,} & 14{\cdot}4 \\ \text{Calcium Carbonate,} & 27{\cdot}7\end{array}\right\}\text{milligrammes per litre,}$$

therefore the proportion of salts by weight is 0·0000421. Taking their specific heat at 0·2, we get for the correction factor required, due to the lowering of the specific heat of the water,

$$1 + (1 - 0{\cdot}2) \times 0{\cdot}0000421 = (1 + 0{\cdot}00003).$$

III.—Air Dissolved in the Water Used.—(Part I, par. 43.)

59. Being rain water, it probably contained about $2\frac{1}{2}$ per cent. by volume of dissolved air. As affecting the specific heat of the water, this air would not have of itself any sensible influence.

It did, however, influence the resulting final temperature, as it was most probably all boiled out of the water, and the bubbles of expelled air would all be saturated with water vapour at a temperature of 212°, which vapour could not be formed without extracting its latent heat from the surrounding water.

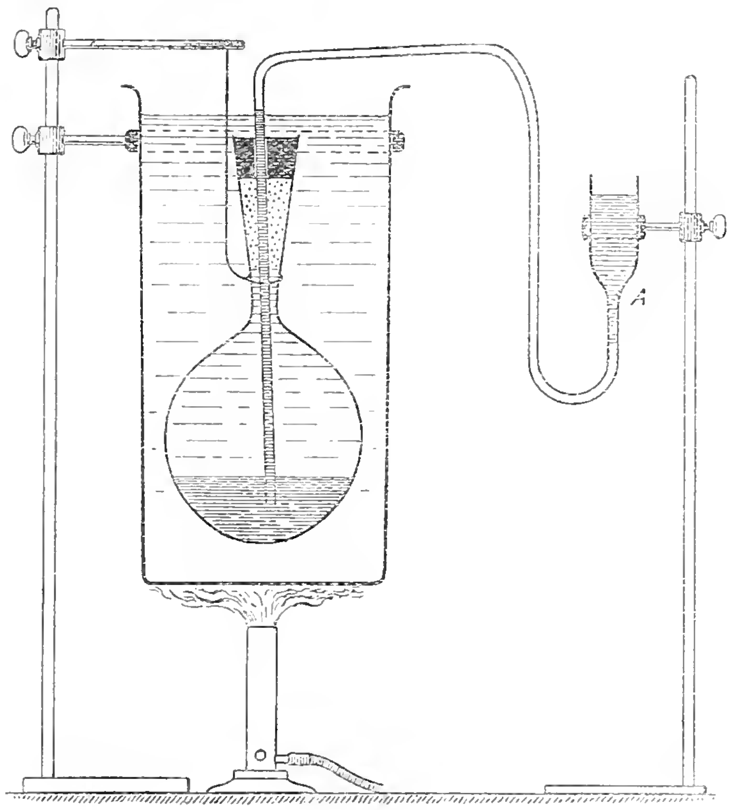
I made some experiments in December, 1896, with the object of determining the actual volume occupied by the bubbles of mixed air and water vapour under the conditions obtaining in the trials. The pressure on the water in the discharge-pipe was 10 inches of mercury very nearly.

The method adopted was as follows :—

I put a depth of about two inches of mercury into the bottom of a strong bolt-head flask, and above the mercury I poured in $1\frac{1}{4}$ lbs. of water. This filled the flask nearly to the brim. A rubber stopper, through which passed a glass tube, was then pressed into the neck of the flask, the glass tube being of such a length that the insertion of the stopper displaced mercury only up the tube, care being taken that no bubbles of air were included under the stopper. The stopper was then firmly tied into the neck, and the flask was hung inside a large glass beaker, which was then filled with water to a depth which covered the top of the rubber stopper.

One end of a piece of strong rubber tube was then fastened on the glass tube protruding from the flask, while its other end was fixed to the vessel shown at A, which was open to the atmosphere.

Fig. 11.



Mercury was poured into the glass funnel at A, and it was raised till there was a solid column of mercury from the bottom of the flask to the surface in A. The water in the beaker was then heated by a Bunsen flame till it boiled. This boiling was continued during a whole day, the water in the beaker being replenished as required. By adjusting the level of the free surface of the mercury at A, any required pressure could be put on the vapour column which formed over the water in the flask neck and displaced some of the mercury from the bottom. Also, by suddenly raising the pressure, the vapour was compressed and cold mercury flowed down into the flask, condensing the vapour in the neck as it descended. By this means the water in the flask could be made to boil briskly for a few moments now and then, so as to facilitate the escape of the air. At the close of the day the levels of mercury and water were adjusted so as to give the requisite pressure on the vapour column. The length of this column was then measured, and knowing the diameters of the flask neck and tube, it was easy to calculate the volume of vapour.

This was 2·2 cubic inches.

If this be reduced to a temperature of 32° and atmospheric pressure, the proportion of air by volume appears to be 1·6 per cent.

This number is considerably less than the 2·5 per cent. already mentioned, but as it was determined under conditions which approximated closely to those which held in the main trials, it was used in the calculation of the correction given below.

The weight of water vapour at a temperature of 212° per cubic foot = 0·03797 lb.

Therefore the correction due to the loss of the latent heat necessary to evaporate this weight of water, is, relatively to the 180 thermal units generated per lb. of water discharged by the brake,

$$\frac{4}{5} \times \frac{2 \cdot 2}{1728} \times \frac{0 \cdot 03797 \times 966}{180} = 0 \cdot 00021.$$

The correction factor is therefore $(1 - 0 \cdot 00021)$.

IV.—Reduction of the Weighings to Vacuo.—(Part 1, par. 41.)

60. Taking the density of water = 62·425,

and of air at 32° Fahr. = 0·08073,

and also assuming 70° Fahr. as the mean temperature of the engine-room during the trials, the correction factor becomes

$$1 - 0 \cdot 08073 \times \frac{493}{531} \times \frac{1}{62 \cdot 425} = 1 - 0 \cdot 00120.$$

In the calculation of this factor it must be borne in mind that the density of the air causes errors of equal magnitude in the measurement of both work and heat on account of the alteration of apparent density of the cast-iron weights used on the brake and on the lever of the weighing machine.

V.—Varying Specific Heat of the Water.—(Part 1, par. 51.)

61. According to REGNAULT the mean specific heat of water between freezing and boiling points is 1·005, assuming the specific heat unity at the lower temperature. If his formula for the specific heat be correct, then a correction factor of $(1 - 0 \cdot 00006)$ is necessary to make the value of K derived from the trials represent this mean specific heat. This factor is introduced because it was not strictly the whole range of temperature between freezing and boiling points which was dealt with in the trials, for the cold water supplied to the brake had various temperatures ranging from 32·7° to 34·3°. This correction would only just affect the second decimal place, and in consideration of the uncertainty that exists as to the exact value of the specific heat of water at any temperature, I do not propose to use a correction factor on this account.

VI.—Corrections due to the Fall in Pressure between the Supply and Discharge Pipes.

62. From observations taken on October 1st, 1896, I determined the pressure on the thermometer in the supply pipe to be :—

In the 1200 ft.-lb. trials	. . . . .	15 inches of mercury.
„ 600 „ „	. . . . .	11 „ „
„ 400 „ „	. . . . .	9·7 „ „

I have already stated that the pressure on the thermometer in the discharge pipe was 11·3 feet of water in all trials.

From these varying pressures two corrections are obtained as follows :—

(*a*.) ELEVATION of Temperature Readings by the Pressure on the Thermometers.

	1200 ft.-lbs.	600 ft.-lbs.	400 ft.-lbs.
Pressure on thermometer bulb in supply pipe in inches of mercury	15·0	11·0	9·7
Consequent elevation in readings of temperature (0°·0072 per inch)	0°·108	0°·0792	0°·0698
Pressure in discharge pipe in feet of water	11·3	11·3	11·3
Consequent elevation in readings of discharge temperature (0°·0066 per inch of mercury)	0°·066	0°·066	0°·066
Percentage correction to heat obtained	$\frac{0·042}{1·8}$ = 0·0233	$\frac{0·013}{1·8}$ = 0·0072	$\frac{0·004}{1·8}$ = 0·0022

If we now confine our attention to the combination of 1200 and 600 ft.-lb. trials, the relative correction to the difference of heat is

$$\frac{0·000233 - \frac{1}{2} \times 0·000072}{\frac{1}{2}} = 0·000394,$$

i.e., the correction factor to K on this account is

$$(1 - 0·000394).$$

Considering next the 1200–400 ft.-lb. determinations, the relative correction to the difference of heat is

$$\frac{0·000233 - \frac{1}{3} \times 0·000022}{\frac{2}{3}} = 0·000339,$$

which makes the correction factor

$$(1 - 0{\cdot}000339).$$

On the mean value of K deduced from the trials, I propose to make this factor

$$(1 - 0{\cdot}00037).$$

63. (*b.*) Generation of Heat in the Water on account of the Loss of available Head between the Supply and Discharge Pipes. (Part I., par. 53.)

	1200 ft.-lbs.	600 ft.-lbs.	400 ft.-lbs.
Head in supply pipe in feet of water	17·0	12·45	10·98
Loss of head before reaching the discharge pipe in feet	5·7	1·15	−0·32
Correction required by the work given in the tables per cent.	$\frac{5{\cdot}7}{1{\cdot}8 \times 777}$ = 0·0041	$\frac{1{\cdot}15}{1{\cdot}8 \times 777}$ = 0·0008	$\frac{-0{\cdot}32}{1{\cdot}8 \times 777}$ = −0·0002

Therefore the correction factors required are—

(α) For the 1200–600 ft.-lb. determinations

$$1 + \frac{0{\cdot}000041 - \frac{1}{2} \times 0{\cdot}000008}{\frac{1}{2}} = (1 + 0{\cdot}000074).$$

(β) For the 1200–400 ft.-lb. determinations

$$1 + \frac{0{\cdot}000041 - \frac{1}{3} \times 0{\cdot}000002}{\frac{2}{3}} = (1 + 0{\cdot}000063).$$

This factor also I propose to give the value

$$(1 + 0{\cdot}00007),$$

when applied to the mean value of K deduced from all the trials.

VII.—Correction due to the manner of Engagement of the Revolution Counter with the Engine Shaft. (Part I., par. 34.)

64. The spindle of the counter carried a wire pin parallel with the axis of revolution, which pin was driven by another carried by, and passing at right angles through, the axis of the spindle making connection with the engine shaft.

The mean chance was therefore that at every engagement of the counter with the shaft one-fourth of a revolution would be lost by the instrument, while on disengaging the counter stopped the instant it was withdrawn.

The work in every trial should therefore be increased to compensate for this loss.

The number of revolutions was approximately 18,000.

The correction factor is therefore

$$1 + \frac{1}{72,000} = (1 + 0{\cdot}00001).$$

65. A summary of these corrections is appended.

Cause of correction.	Magnitude and sign.	
	+	−
I. Length of lever	0·00042	
II. Dissolved salts	0·00003	
III. Dissolved air	..	0·00021
IV. Weight of atmosphere	..	0·00120
V. Varying specific heat of water	Neglected.	
VI. (a) Effect of pressure on thermometers	..	0·00037
(b) Loss of head in the water	0·00007	
VII. Engagement of revolution counter	0·00001	
Totals	0·00053	0·00178

Therefore the final correction factor is

$$(1 - 0{\cdot}00125).$$

66. Applying this correction factor to the value obtained from the experiments, we get for the value of the mean specific heat of water between freezing and boiling-points, expressed in mechanical units, at Manchester,

$$777{\cdot}91\,(1 - 0{\cdot}00125),$$

776·94.

APPENDIX.

Although no part of this research, it may be interesting to notice that reduced to the latitude of Greenwich this becomes

777·07,

and reduced to latitude 45° at sea level

777·53.

Expressed in metre-grammes and the centigrade unit of heat this last value becomes

426·58.

The value of g being

980·63,

we have for the mean value of the specific heat of water between 0° and 100° C., expressed in absolute C.G.S. units,

41,832,000 ergs.

Making use of REGNAULT's formula for the specific heat of water at different temperatures, this would give the mechanical equivalent of the heat required to raise 1 lb. of water at 60°·5 Fahr. through 1° Fahr. at Manchester as

773·74 ft.-lbs.,

and taking water at 32° Fahr., this gives

773·07 ft.-lbs.

Similarly expressing the result in absolute C.G.S. units, we have for the mechanical equivalent of the heat necessary to raise 1 gramme of water through 1° C. in latitude 45° and at sea-level

(*a*) From a temperature 15°·8 C. . . . . . 41,660,000 ergs.
(*b*) ,, ,, 0° C. . . . . . 41,624,000 ,,

TABLE A.—Table Showing the Preliminary Reduction of Trials, 1 to 42 Inclusive.

K Determination number.	Heavy trials. Moment, 1200 ft.-lbs.							Light trials. Moment, 600 ft.-lbs.							Differences.						Preliminary value of K obtained.
	Trial number.	Work done.	Heat generated.	Terminal correction.	Difference of temperature between stuffing-box and upper brass.	Difference of temperature between stuffing-box and lower brass.	Difference of temperature between brake and air.	Trial number.	Work done.	Heat generated.	Terminal correction.	Difference of temperature between stuffing-box and upper brass.	Difference of temperature between stuffing-box and lower brass.	Difference of temperature between brake and air.	Work done.	Heat generated.	Terminal correction.	Difference of temperature between stuffing-box and upper brass.	Difference of temperature between stuffing-box and lower brass.	Difference of temperature between brake and air.	
Series Number I.																					
I.	1	134,201,612	167,191	+ 11	..	..	139·3	2	68,310,950	82,626	..	..	..	137·4	65,890,662	84,565	+ 11	..	..	..	779·1
II.	4	138,446,542	172,957	− 63	..	..	140·5	3	68,182,773	83,090	− 6	..	..	140·7	70,263,769	89,867	− 57	..	..	..	782·4
III.	6	135,935,775	169,686	+ 31	..	..	137·6	7	67,926,419	82,432	−91	..	..	140·8	68,009,356	87,254	+122	..	..	..	778·3
IV.	9	136,063,953	169,859	− 10	..	..	138·9	8	68,096,065	82,725	−29	..	..	139·1	67,967,888	87,134	+ 19	..	..	..	779·9
V.	10	136,674,680	170,573	− 13	+ 9·4	(+ 2·4)	140·6	11	68,084,755	82,497	−11	− 3·3	(−10·3)	141·1	68,589,925	88,076	− 2	..	(+12·7)	..	778·8
Series Number II.																					
	12	133,628,584	169,519	− 40	+10·1	..	144·4		Combined	with tri	al 14.	..	..	..	..	..	..	..	..	..	787·4
VI.	13	135,392,907	172,591	+ 12	+ 9·3	(+ 2·3)	143·8	14	67,933,958	86,054	− 5	−11·2	(−18·2)	142	67,458,949	86,537	+ 17	..	(+20·5)	..	779·4
VII.	17	135,098,853	172,408	+ 6	− 3·9	(−10·9)	140·2	16	67,677,604	85,819	+55	−10·9	(−17·9)	140	67,421,249	86,589	− 49	..	(+ 7)	..	779·1
								15	67,658,754	85,737	−22	− 1·9	(− 8·9)	140·8							
Series Number III.																					
VIII.	18	133,734,142	170,604	− 69	+ 6·3	(− 0·7)	141·0	21	66,580,557	84,173	− 5	+ 3	− 2·6	145·3	67,153,585	86,431	− 64	..	(+ 1·9)	..	777·5
IX.	19	133,892,479	170,867	+ 63	+ 1·1	(− 5·9)	140·9	22	67,142,275	85,012	−38	− 1·9	− 7·9	144·3	66,750,204	85,855	+101	..	(+ 2)	..	776·6
X.	20	135,332,588	172,666	− 29	+ 0·3	(− 6·7)	140·8	23	66,765,283	84,703	−55	− 2	− 7·7	144·4	68,567,305	87,963	+ 26	..	(+ 1)	..	779·3
Series Number IV.																					
	24	139,870,565	178,183	+104	+ 2·1	− 6	141·7														
	25	139,448,444	177,847	+ 40	− 1·4	− 7·4	139·1														
	26	140,073,809	178,984	− 40	− 3·1	−10·2	139·3														
Series Number V.																					
XI.	30	134,073,435	171,054	− 12	+ 7·9	− 1·3	145·8	27	67,353,391	85,344	− 5	− 2·6	−11·1	149·4	66,720,044	85,710	− 7	..	+ 9·8	..	778·5
XII.	31	134,623,843	171,793	− 12	− 6·3	−11·7	145·4	28	67,146,045	85,147	+ 5	−11·8	−18·3	144·2	67,477,798	86,646	− 17	..	+ 6·6	..	778·9
XIII.	32	135,257,190	172,618	..	− 4·0	−10·3	141·4	29	67,315,692	85,406	−16	− 6·2	−13·7	140·4	67,941,498	87,212	+ 16	..	+ 3·4	..	778·9
Series Number VI.																					
XIV.	35	134,744,481	171,995	+ 6	− 2·9	− 9	144·9	33	67,692,684	85,724	−55	+ 2·1	− 5·1	145·7	67,051,797	86,271	+ 61	..	− 3·9	..	776·7
XV.	36	135,702,040	173,226	− 6	− 3·1	− 9	143·4	34	66,765,283	84,625	..	+ 0·9	− 5·0	144·2	68,936,757	88,601	− 6	..	− 4·0	..	778·1
Series Number VII.																					
XVI.	37	134,819,879	172,059	− 6	+ 8·9	− 0·3	145·9	40	67,703,993	85,555	−27	+24·9	+11·6	146·5	67,115,886	86,504	+ 21·0	..	−11·9	..	775·7
XVII.	38	135,151,632	172,550	− 17	+ 1·1	− 4·4	144·3	41	67,112,116	85,135	−16	+ 4·0	− 1·7	143·0	68,039,516	87,415	− 0·9	..	− 2·7	..	778·4
XVIII.	39	134,895,277	172,250	..	− 0·3	− 5·9	144·8	42	67,130,965	85,316	−21	..	−16·6	143·1	67,764,312	86,934	+ 21·0	..	+10·7	..	779·3

TABLE B.

Date.	Trial No.	Time of start.	Moment (ft.-lbs.).	No. of revolutions of engine-shaft.	Work done (ft.-lbs.).	Weight of water discharged by the brake (lbs.).	Rise of temperature in the brake (° F.).	Heat generated, less losses by radiation, &c. (B.T.U.).	Weight of water caught at stuffing-box (lbs.).	Rise of temperature in the brake (° F.).	Loss of heat by leakage (B.T.U.).	Rise of temperature of brake during trial (° F.).	Terminal correction to heat (B.T.U.).	Fall of temperature along shaft between stuffing-box and bearing (° F.).	Loss of heat by conduction (B.T.U.)	Difference of temperature between brake and air (° F.).	Loss of heat by radiation (B.T.U.).	Corrected heat (B.T.U.).
								Series No. I.										
Feb. 5, '96	1	11.5	1200	17,799	134,201,612	935·53	178·713	167,191	..	..	..	0·2	11	(2·4)	29	139·3	5135	172,366
Feb. 12, '96	6	1.57	1200	18,029	135,935,775	948·39	178·92	169,686	..	..	..	−0·2	31	(2·4)	29	137·6	5072	174,818
Feb. 13, '96	9	2.8	1200	18,046	136,063,953	952·09	178·406	169,859	..	..	..	0·2	−10	(2·4)	29	138·9	5120	174,998
Feb. 19, '96	10	11.21	1200	18,127	136,674,680	954·46	178·711	170,573	..	..	..	−0·6	−13	(2·4)	29	140·6	5183	175,772
								Series No. II.										
Feb. 28, '96	13	2.14	1200	17,957	135,392,907	965·37	178·782	172,591	..	..	..	0·2	12	(2·3)	28	143·8	1342	173,973
Mar. 5, '96	17	2.52	1200	17,918	135,098,853	964·71	178·715	172,408	..	..	..	0·1	6	(−10·9)	−131	140·2	1308	173,591
								Series No, III.										
Mar. 6, '96	18	10.22	1200	17,737	133,734,142	952·88	179·04	170,604	..	..	..	−1·2	−69	(−0·7)	− 8	141·0	1316	171,843
Mar. 6, '96	19	11.53	1200	17,758	133,892,479	955·85	178·759	170,867	..	..	..	1·1	63	(−5·9)	−71	140·9	1315	172,174
Mar. 6, '96	20	2.26	1200	17,949	135,332,588	969·38	178·12	172,666	..	..	..	−0·5	−29	(−6·7)	−80	140·8	1314	173,871
								Series No. V.										
Mar. 20, '96	30	10.13	1200	17,782	134,073,435	959·88	178·204	171,054	..	..	..	−0·2	−12	− 1·3	− 16	145·8	1360	172,386
Mar. 20, '96	31	11.39	1200	17,855	134,623,843	959·29	179·083	171,793	..	..	..	−0·2	−12	−11·7	−140	145·4	1357	172,998
Mar. 20, '96	32	2.15	1200	17,939	135,257,190	968·3	178·269	172,618	..	..	..	..	..	−10·3	−124	141·4	1319	173,813
								Series No. VI.										
Mar. 25, '96	35	11.1	1200	17,871	134,744,481	962·18	178·756	171,995	..	..	..	0·1	6	− 9·0	−108	144·9	1352	173,245
Mar. 25, '96	36	2.7	1200	17,998	135,702,040	967·85	178·98	173,226	..	..	..	−0·1	− 6	− 9·0	−108	143·4	1338	174,450
								Series No. VII.										
Mar. 27, '96	37	10.27	1200	17,881	134,819,879	963·21	178·631	172,059	..	..	..	−0·1	− 6	− 0·3	− 4	145·9	1361	173,410
Mar. 27, '96	38	11.44	1200	17,925	135,151,632	965·05	178·799	172,550	..	..	..	−0·3	−17	− 4·4	−53	144·3	1346	173,826
Mar. 27, '96	39	2.28	1200	17,891	134,895,277	961·57	179·072	172,190	..	..	60	..	..	− 5·9	−71	144·8	1351	173,530

TABLE C.

Date.	Trial No.	Time of start.	Moment (ft.-lbs.).	No. of revolutions of engine-shaft.	Work done (ft.-lbs.).	Weight of water discharged by the brake (lbs.).	Rise of temperature in the brake (° F.).	Heat generated, less losses by radiation, &c. (B.T.U.).	Weight of water caught at stuffing-box (lbs.).	Rise of temperature in the brake (° F.).	Loss of heat by leakage (B.T.U.).	Rise of temperature of brake during trial (° F.).	Terminal correction to heat (B.T.U.).	Fall of temperature along shaft between stuffing-box and bearing (° F.).	Loss of heat by conduction (B.T.U.).	Difference of temperature between brake and air (° F.).	Loss of heat by radiation (B.T.U.).	Corrected heat (B.T.U.).
								Series I.										
Feb. 5, '96	2	3.4	600	18,120	68,310,950	462·15	178·787	82,626	..	..	..	..	..	(−10·3)	−124	137·4	5065	87,567
Feb. 13, '96	7	10.12	600	18,018	67,926,419	459·03	179·578	82,432	..	..	..	−0·9	−91	(−10·3)	−124	140·8	5190	87,407
Feb. 13, '96	8	11.30	600	18,063	68,096,065	461·58	179·221	82,725	..	..	..	−0·5	−29	(−10·3)	−124	139·1	5127	87,699
Feb. 19, '96	11	2.30	600	18,060	68,084,755	459·66	179·475	82,497	..	..	..	−0·2	−11	(−10·3)	−124	141·1	5201	87,563
								Series II.										
Feb. 28, '96	14	3.36	600	18,020	67,933,958	480·87	178·954	86,054	..	..	..	−0·1	− 5	(−18·2)	−218	142	1325	87,156
Mar. 5, '96	15	10.34	600	17,947	67,658,754	479·29	178·883	85,737	..	..	..	−0·4	−22	(− 8·9)	−107	140·8	1314	86,922
Mar. 5, '96	16	11.50	600	17,952	67,677,604	479·3	179·05	85,819	..	..	..	1·0	55	(−17·9)	−215	140	1306	86,965
								Series III.										
Mar. 12, '96	21	10.33	600	17,661	66,580,557	469·17	179·408	84,173	..	..	..	−0·1	− 5	− 2·6	− 31	145·3	1356	85,493
Mar. 12, '96	22	11.48	600	17,810	67,142,275	474·84	179·034	85,012	..	..	..	−0·7	−38	− 7·9	− 95	144·3	1346	86,225
Mar. 12, '96	23	2.38	600	17,710	66,765,283	473·33	178·951	84,703	..	..	..	−1·0	−55	− 7·7	− 92	144·4	1347	85,903
								Series V.										
Mar. 19, '96	27	10.22	600	17,866	67,353,391	476·05	179·275	85,344	..	..	..	−0·1	− 5	−11·1	−133	149·4	1394	86,600
Mar. 19, '96	28	11.36	600	17,811	67,146,045	474·95	179·276	85,147	..	..	..	0·1	5	−18·3	−220	144·2	1345	86,277
Mar. 19, '96	29	2.19	600	17,856	67,315,692	477·32	178·928	85,406	..	..	..	−0·3	−16	−13·7	−164	140·4	1310	86,536
								Series VI.										
Mar. 23, '96	33	11.38	600	17,956	67,692,684	478·3	179·226	85,724	..	..	..	−1	−55	− 5·1	− 61	145·7	1359	86,967
Mar. 23, '96	34	2.27	600	17,710	66,765,283	472·18	179·221	84,625	..	..	..	..	..	− 5	− 60	144·2	1345	85,910
								Series VII.										
Mar. 30, '96	40	10.49	600	17,959	67,703,993	478·23	178·899	85,555	..	..	..	−0·5	−27	11·6	139	146·5	1367	87,034
Mar. 30, '96	41	11.59	600	17,802	67,112,116	474·96	179·246	85,135	..	..	..	−0·3	−16	− 1·7	− 20	143	1334	86,433
Mar. 30, '96	42	3.29	600	17,807	67,130,965	477·11	178·772	85,294	..	..	22	−0·4	−21	−16·6	−199	143·1	1335	86,431

Table D.

Determination No.	Trial No.	Work.	Difference of Work.	Heat (corrected.)	Difference of heat.	K.
			Series No. I.			
I.	1	134,201,612	..	172,366		
	2	68,310,950	65,890,662	87,567	84,799	777·02
III.	6	135,935,775	..	174,818		
	7	67,926,419	68,009,356	87,407	87,411	778·04
IV.	9	136,063,953	..	174,998		
	8	68,096,065	67,967,888	87,699	87,299	778·56
V.	10	136,674,680	..	175,772		
	11	68,084,755	68,589,925	87,563	88,209	777·58
			Mean valne = 777·81			
			Series No. II.			
VI.	13	135,392,907	..	173,973		
	14	67,933,958	67,458,949	87,156	86,817	777·02
VII.	17	135,098,853	..	173,591		
	16	67,677,604	67,421,249	86,965	86,626	778·3
			Mean value = 777·66			
			Series No. III.			
VIII.	18	133,734,142	..	171,843		
	21	66,580,557	67,153,585	85,493	86,350	777·69
IX.	19	133,892,479	..	172,174		
	22	67,142,275	66,750,204	86,225	85,949	776·63
X.	20	135,332,588	..	173,871		
	23	66,765,283	68,567,305	85,903	87,968	779 46
			Mean value = 777·94			
			Series No. V.			
XI.	30	134,073,435	..	172,386		
	27	67,353,391	66,720,044	86,600	85,786	777·75
XII.	31	134,623,843	..	172,998		
	28	67,146,045	67,477,798	86,277	86,721	778·1
XIII.	32	135,257,190	..	173,813		
	29	67,315,692	67,941,498	86,536	87,277	778·46
			Mean value = 778·1			
			Series No. VI.			
XIV.	35	134,744,481	..	173,245		
	33	67,692,684	67,051,797	86,967	86,278	777·16
XV.	36	135,702,040	..	174,450		
	34	66,765,283	68,936,757	85,910	88,540	778·59
			Mean value = 777·89			
			Series No. VII.			
XVI.	37	134.819,879	..	173,410		
	40	67,703,993	67,115,886	87,034	86,376	777·02
XVII.	38	135,151,632	..	173,826		
	41	67,112,116	68,039,516	86,433	87,393	778·55
XVIII.	39	134,895,277	..	173,530		
	42	67,130,965	67,764,312	86,431	87,099	778·01
			Mean value = 777·86			

TABLE E.

Date.	Trial No.	Time of start.	Moment (ft.-lbs.).	No. of revolutions of engine-shaft.	Work done (ft.-lbs.).	Weight of water discharged by brake (lbs.).	Rise of temperature in the brake (° F.).	Heat generated, less losses by radiation, &c. (B.T.U.).	Weight of water caught at stuffing-box (lbs.).	Rise of temperature in the brake (° F.).	Loss of heat by leakage (B.T.U.).	Rise of temperature of brake during trial (° F.).	Terminal correction to the heat (B.T.U.).	Fall of temperature along shaft between stuffing-box and bearing (° F.).	Loss of heat by conduction (B.T.U.).	Difference of temperature between brake and air (° F.).	Loss of heat by radiation (B.T.U.).	Corrected heat (B.T.U.).
							Series No. VIII.											
April 1, '96	46	10.23	1200	17,997	135,688,500	967·92	179·102	173,356	..	..	..	−0·5	−29	−3·4	−41	146	1191	174,477
April 1, '96	47	11.39	,,	17,990	135,641,722	969·39	178·73	173,259	..	..	..	−0·3	−17	−5·9	−71	143·8	1173	174,344
							Series No. IX.											
April 17, '96	48	10.43	1200	17,735	133,719,062	955·6	178·505	170,579	..	..	..	..	..	−5·3	−64	143·5	1171	171,686
April 17, '96	49	12.00	,,	18,033	135,965,935	974·92	178·162	173,694	..	..	..	..	..	−7·0	−84	142·5	1163	174,773
April 17, '96	50	2.17	,,	17,601	132,708,724	950·31	178·352	169,490	..	..	..	..	..	−9·6	−115	141·8	1157	170,532
							Series No. VIII.											
March 31, '96	43	10.30	400	17,958	45,133,482	317·85	179·188	56,955	..	..	..	−0·3	−16	−2·9	−35	145·7	1189	58,093
March 31, '96	44	11.45	,,	18,005	45,251,606	318·84	179·158	57,123	..	..	..	+0·1	5	−5·6	−67	143·4	1170	58,231
March 31, '96	45	2.30	,,	17,704	44,495,109	311·81	179·495	55,968	..	..	..	0·8	43	−7·4	−89	145·4	1186	57,108
							Series No. IX.											
April 20, '96	51	11.12	400	18,009	45,261,660	318·46	178·777	56,933	..	..	..	−0·1	− 5	−6·1	−73	145	1183	58,038
April 20, '96	52	12.20	,,	17,819	44,784,136	316·04	178·686	56,472	..	..	..	0·3	16	−9·1	−169	142·1	1160	57,539
April 20, '96	53	2.39	,,	17,919	45,035,464	317·84	178·926	56,870	..	..	..	−0·3	−16	−8·1	−97	142·4	1162	57,919

TABLE F.

Determination No.	Trial No.	Work.	Difference of work.	Heat (corrected).	Difference of heat.	K.
			Series No. VIII.			
XIX.	46	135,688,500	..	174,477		
	43	45,133,482	90,555,018	58,093	116,384	778·07
XX.	47	135,641,722	..	174,344		
	44	45,251,606	90,390,116	58,231	116,113	778·47
			Mean value	= 778·27		

Determination No.	Trial No.	Work.	Difference of work.	Heat (corrected).	Difference of heat.	K.
			Series No. IX.			
XXI.	48	133,719,062	..	171,686		
	51	45,261,660	88,457,402	58,038	113,648	778·35
XXII.	49	135,965,935	..	174,773		
	52	44,784,136	91,181,799	57,539	117,234	777·78
XXIII.	50	132,708,724	..	170,532		
	53	45,035,464	87,673,260	57,919	112,613	778·54
			Mean value	= 778·22		

TABLE G.

Date.	Trial No.	Time of start.	Moment (ft.-lbs.).	No. of revolutions of engine-shaft.	Work done (ft.-lbs.).	Weight of water discharged by brake (lbs.).	Rise of temperature in the brake (° F.)	Heat generated less losses by radiation, &c. (B.T.U.).	Weight of water caught at stuffing box (lbs.).	Rise of temperature in the brake (° F.).	Loss of heat by leakage (B.T.U.).	Rise of temperature of brake during trial (° F.).	Terminal correction to heat (B.T.U.).	Fall of temperature along shaft between stuffing-box and bearing (° F.).	Loss of heat by conduction (B.T.U.).	Difference of temperature between brake and air (° F.).	Loss of heat by radiation (B.T.U.).	Corrected heat (B.T.U.).
								Series No. X.										
July 7, '96	69	11.17	600	17,548	66,154,556	468·88	178·972	83,916	..	..	..	..	..	−0·57	− 7	135·1	1078	84,987
July 7, '96	70	12.24	600	17,807	67,130,965	474·78	179·474	85,211	..	..	..	..	..	−0·86	−10	135·0	1077	86,278
July 7, '96	71	1.41	600	18,095	68,216,702	482·62	179·608	86,682	..	..	..	..	..	0·57	7	135·6	1082	87,757
								Series No. XI.										
July 10, '96	78	10.45	600	17,436	65,732,325	464·59	179·393	83,344	..	..	..	..	..	−0·71	− 9	136·5	1089	84,424
July 10, '96	79	11.55	600	17,602	66,358,132	470·46	179·269	84,339	..	..	..	−0·4	−22	−4·43	−53	134·3	1072	85,336
July 10, '96	80	1.11	600	17,894	67,458,948	477·08	179·597	85,682	..	..	..	0·3	16	1·14	14	135·1	1078	86,790
								Series No. X.										
July 8, '96	72	11.11	1200	17,311	130,522,170	934·58	178·344	166,677	..	..	..	−0·8	−46	−0·14	− 2	137·7	1099	167,728
July 8, '96	73	12.29	1200	17,528	132,158,316	950·07	177·719	168,845	..	..	..	1·1	63	−0·43	− 5	135·0	1077	169,980
July 8, '96	74	1.43	1200	17,737	133,734,142	956·46	178·559	170,785	0·31	82·3	26	0·3	17	1·0	12	135·5	1081	171,921
								Series No. XI.										
July 9, '96	75	10.35	1200	17,529	132,165,855	945·95	178·39	168,748	..	..	..	0·6	35	0·86	10	134·1	1070	169,863
July 9, '96	76	11.50	1200	17,858	134,646,463	969·82	177·47	172,114	..	..	..	−0·6	−35	−1·71	−20	131·2	1047	173,106
July 9, '96	77	1.6	1200	17,954	135,370,287	974·37	177·566	173,015	..	..	..	0·4	23	−0·57	− 7	130·3	1040	174,071

TABLE H.

Determination No.	Trial No.	Work.	Difference of work.	Heat (corrected).	Difference of heat.	K.
			Series No. X.			
XXIV.	72	130,522,170	..	167,728		
	69	66,154,556	64,367,614	84,987	82,741	777·95
XXV.	73	132,158,316	..	169,980		
	70	67,130,965	65,027,351	86,278	83,702	776·89
XXVI.	74	133,734,142	..	171,921		
	71	68,216,702	65,517,440	87,757	84,164	778·44
			Mean value = 777·74.			
XXVII.	75	132,165,855	..	169,863		
	78	65,732,325	66,433,530	84,424	85,439	777·56
XXVIII.	76	134,646,463	..	173,106		
	79	66,358,132	68,288,331	85 336	87,770	778·03
XXIX.	77	135,370,287	..	174,071		
	80	67,458,948	67,911,339	86,790	87,281	778·07
			Mean value = 777·88.			

DESCRIPTION OF THE PLATES.

PLATE 3.

From a photograph in 1888. Is a front view of the triple expansion engines (100 H.-P.) and brakes, as they existed in the engineering laboratory, Owens College, before any modifications for the determination of the equivalent. The engine-shafts are disconnected from each other, and are working on three separate brakes. In the trials the three large pulleys (5 feet in diameter) were removed with the brakes on the high-pressure and intermediate engines, and the engine-shafts coupled by intermedial shafts, the work being all absorbed by the brake on the low-pressure engine—seen, on the right hand of the plate, overhanging the last bearing of the brake-shaft. On this shaft are two heavy 3-feet pulleys, which served as fly-wheels during the trials.

It was the facilities afforded by this brake and its appurtenances (§ 11) that suggested the research and rendered it possible: and, although the manner of admitting the water and air to the brake were necessarily modified in the experiments, the brake remained essentially the same. Part of the trials were made with the brake uncovered, as seen in this plate; and it was after the brake was covered that subsequent photographs were taken.

The vertical pipe supplying the town's water from the service tank to the brake, with the hand-cock and the automatic inlet-cock above, leading through the bowed pipe and flexible indiarubber tube to the inlet passage over the bush of the brake, are seen on the immediate right. Immediately on the left and a little behind and lower, is another bowed pipe leading from the top of the brake, with a gap in it; this is the air passage leading through the vanes to the centres of the vortex chambers, to secure atmospheric pressure there. The suspended and riding loads on the lever, the dash-pot, the front stop on which the lever rests (not being at work), are also seen. The hand wheel for adjusting the height of the lever when at work, the linkage connecting the automatic inlet and outlet-cocks with each other and with the front stop, together with the outlet-cock, the receptacle for waste, and the drip-can for the water escaping from the front bush, can be traced, though they are obscure in this plate.

Up high on the photograph is seen a shaft with two large pulleys; these are for connecting the separate engine-shafts by belts and ropes (seen), and have no place in the trials. But the bright shaft immediately below, seen as driven by a rope pulley from behind the wall of the engine-room, is the line shaft driven by the separate engine, always running, which afforded most important facilities for the research.

PLATE 4.

From a photograph, 1896. Also shows a front view of the engine-room, but, taken more to the right; it includes only the low-pressure engine. It shows a general front view of the appliances in the condition in which they were during the final experiments, as well as some of the standing appliances not included in Plate 3.

Low down, immediately on the right, is the front of the weighing-machine, with the tank resting on it; and immediately behind this, against the wall, are seen the mercury balances for the pressures of water in the mains; also the town's main to the service tank (out of sight on the right), in front of which is the 3-inch quadruple turbine which drives the ($1\frac{1}{2}$-inch) quintuple centrifugal pump (out of view, behind the tank) supplying the brake through the ice-cooler (§ 20). On the left of the tank, and passing through its cover, is the water-switch; and over this is the nozzle of a vertical pipe, straight almost to the roof, then horizontal, with an open vertical branch, to form an air-gap, then down again into the lower of the two horizontal pipes; this is the stand-pipe on the outlet from the condenser, for securing pressure in the final thermometer chamber (§ 22). The upper of the two horizontal pipes is the water-jacketed out-flow pipe or "condenser," which passes to the end of the room, and returns as the lower horizontal pipe to the stand-pipe. Immediately on the left of the plate, standing on the floor, is the frame for the hand-brake (§ 30). Besides the appliances mentioned, as seen, in this plate, nearly all the appliances are seen in front view; but many are better seen in the following plates, though this plate affords the best view of the general arrangement, and the best idea of the circumstances under which the observations were made. The passage between the brake and the 3-inch pipe supplying condensing water to the engine afforded the only post of observation for the counter, thermometers, speed-gauge, and pressure-gauges. The centrifugal speed-gauge, with its scale, is seen rising vertically from behind the small pressure-gauge on the brake.

PLATE 5.

This is a nearer and simplified front view of the more special appliances shown in Plate 4. Proceeding from the right is the switch and outlet nozzle from the condenser, with the water flowing into the tank over the thermometer. From the switch may be traced the linkage forming the automatic connection of the switch with the counter, immediately in front of the covered bush of the brake. Supported by the original supply pipe to the brake (the hand cock being shut) is seen the new inlet pipe from the ice-cooler, behind the brake. The pipe, rising on the right from behind the brake, passes a branch to the by-channels leading to the bushes (not seen) and a branch to the large pressure-gauge, then to the regulator; thence the water flows upwards past the bulb of the inlet thermometer, some of it passing up through the glass thermometer chamber, and so to waste through the small pipe at the top, but the main stream passing through the covered horizontal branch, and down the flexible indiarubber pipe into the brake. On the top of the brake is seen the new air-passage, of flexible indiarubber, leading to the vessel in which is the artificial atmosphere, which is connected with the large mercury-gauge on the left, also with the syringe. The automatic outflow cock is clearly seen under the brake, also the curved flexible pipe, covered with cotton wool, which receives the water from the outflow cock, leading to the fixed pipe behind the regulator, also covered, in which is the bulb of the outflow thermometer, and immediately over this the glass thermometer chamber, with its indiarubber continuation leading back into the main outflow channel which rises up behind the inlet thermometer chamber, till it turns at right angles into the condenser. Behind and on the left of the brake are seen protruding the stems of the thermometers for measuring the difference of temperature in the stuffing-box and the near bearing. Of the two bottles standing on the floor, that on the left is collecting the leakage from the stuffing-box, and the other the leakage caught in the indiarubber bag enclosing the automatic outflow cock.

PLATE 6.

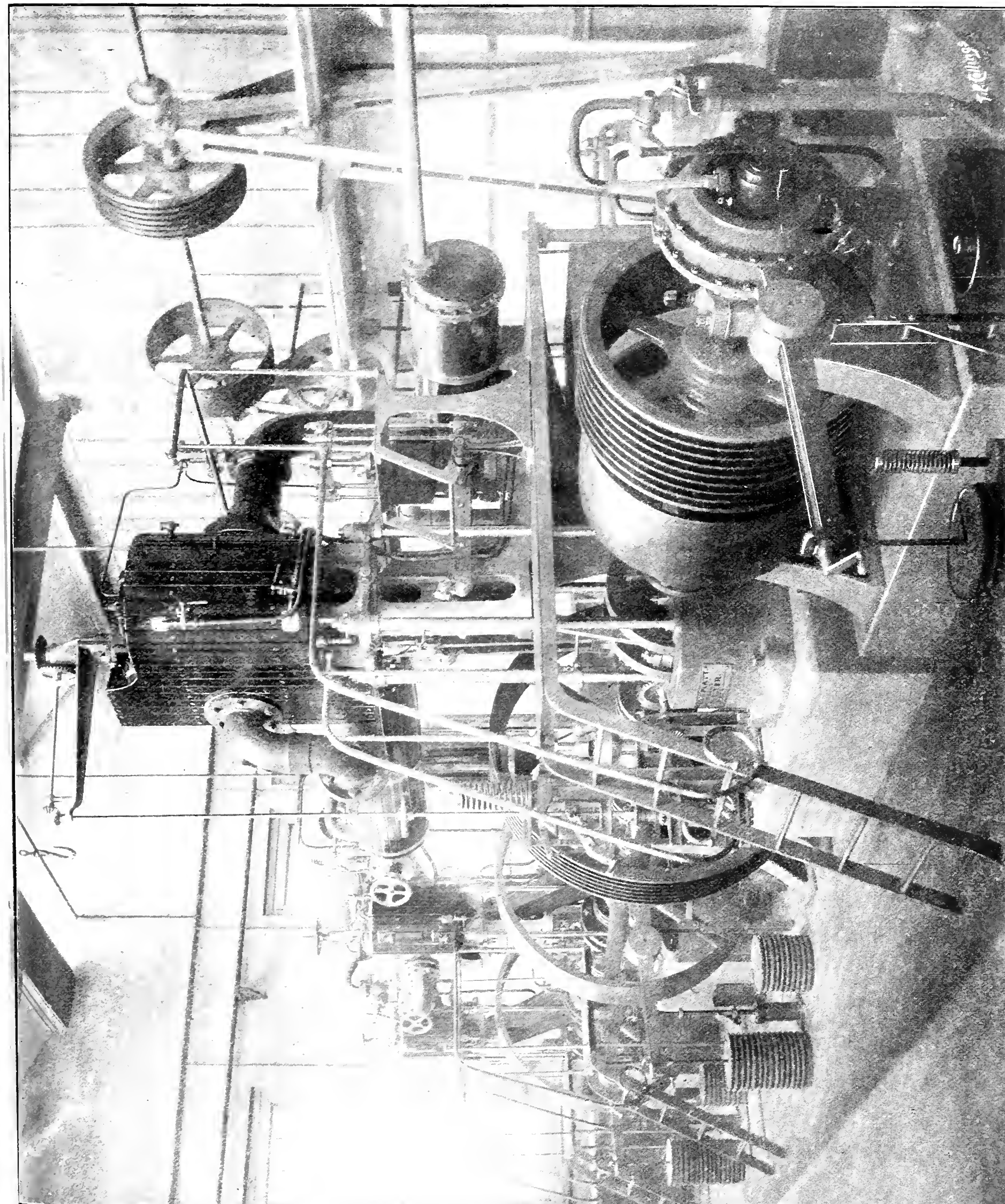
This is a back view. On the left, close in front of the tank on the weighing machine, over which is the condenser leading to the switch, is seen the $1\frac{1}{2}$-inch quintuple centrifugal pump, with its driving gear and the pipe supplying it from the service tank. On the other side of the 3-inch pipe for condensing water for the engines, and partly behind it, is seen the pipe leading from the pump up and along behind the 3-inch pipe, then down again into the ice-tank (on the extreme right of the plate); through this it passes in a coil, emerging from the cover again as the covered pipe rising obliquely to the regulator and inlet thermometer chambers (not seen), with the branch to the pressure-gauge. The small horizontal branch coming through from beneath the pressure-gauge, continued by the covered indiarubber pipe, passing behind the vortex vessel of the speed-gauge to the stuffing-box, is one of the by-paths taking ice-cold water to the bushes; that on the left is behind the brake. The outlet thermometer chamber, with its indiarubber continuation to the main outflow channel into the condenser, is also clear; as are also the belt and pulley driving the paddle in the ice-tank.

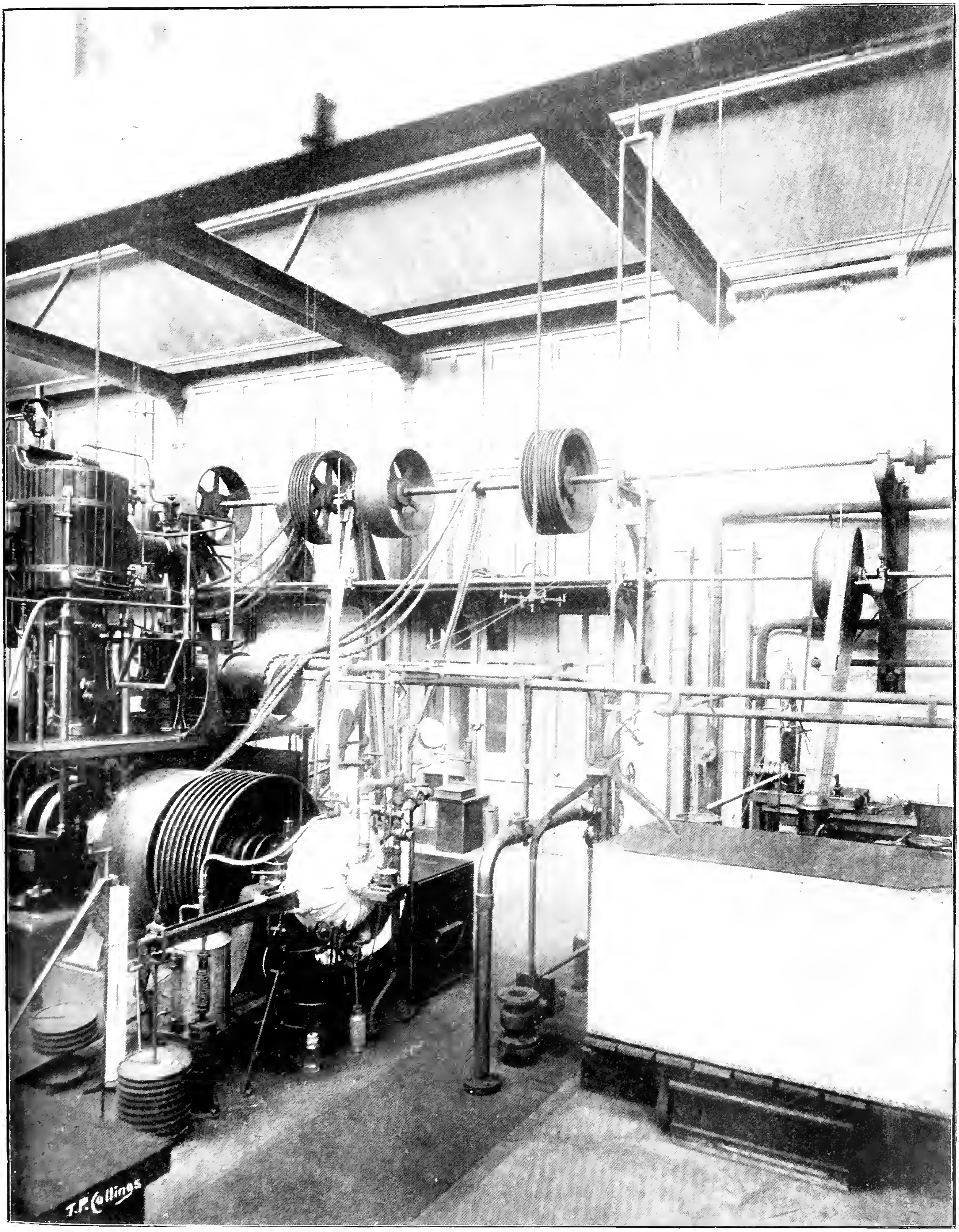
PLATE 7.

This is again a back view, but taken so as to show the appliances up to the end of the engine-room, not seen in the previous plates. In the middle front is seen the 6-inch quadruple centrifugal pump in circuit, with the rising 4-inch main from the lower tank to the tank in the tower (§ 3), together with the belt from the line shaft by which this pump is driven. Immediately on the left of this plate, standing on a bench, is the end of the 3-inch quadruple vortex turbine, driven by water from the tower, and driving by a cord the $1\frac{1}{2}$-inch quintuple centrifugal pump. The standard, the lever, and the large riding weight of the weighing-machine, with the tank behind, are completely in view; and over these again appears the condenser for cooling the effluent water, passing to the end of the room and returning underneath to the stand-pipe and thence to the switch.

PLATE 8.

This is from a photograph of the apparatus for correcting the high temperature thermometer. On the table is the barometer, and to the right is the vapour chamber, in which the thermometer is immersed through the cork on the top as far as to leave the top of the mercury visible. The escape passage and regulator are seen on the right. The pipe leading from the top is the connection of the vapour chamber with the lower mercury chamber in the barometer. This, after passing through the flask, receives by the branch (seen) a slight current of air from the pressure reservoir, with the top of which it is connected by a restricted pipe, so that the current is so slow that the resistance is negligible, though sufficient to prevent the vapour passing to the barometer; the pressure of air in the reservoir is shown by the large mercury-gauge, and is maintained by occasional pumping with the syringe seen in connection. The nozzle on the barometer, to which the air-passage is connected, leads into the cast-iron bottle which forms the mercury-chamber, above the surface of the mercury. The level of this surface is observed through the circular windows, of which that which is in front is shown to the left of the axis of the barometer, above the nozzle. Immediately above this window is seen the cylindrical brass curtain, which screws on to the neck of the bottle, by which the light through the windows over the mercury can be eclipsed. Attached to this curtain, and co-axial with it, is the outer brass tube extending up to the gap, with a vertical scale attached reaching past the gap. Behind the vertical scale, and screwed into the tube on the lower curtain, is a tube screwed throughout its length, and having two parallel slots, as windows, some 5 inches long, through which the upper limb of the mercury may be observed. From the top of this windowed tube downward is screwed the cap, the lower limb of which forms a cylindrical curtain for eclipsing the light over the upper limb of the mercury (§ 48).

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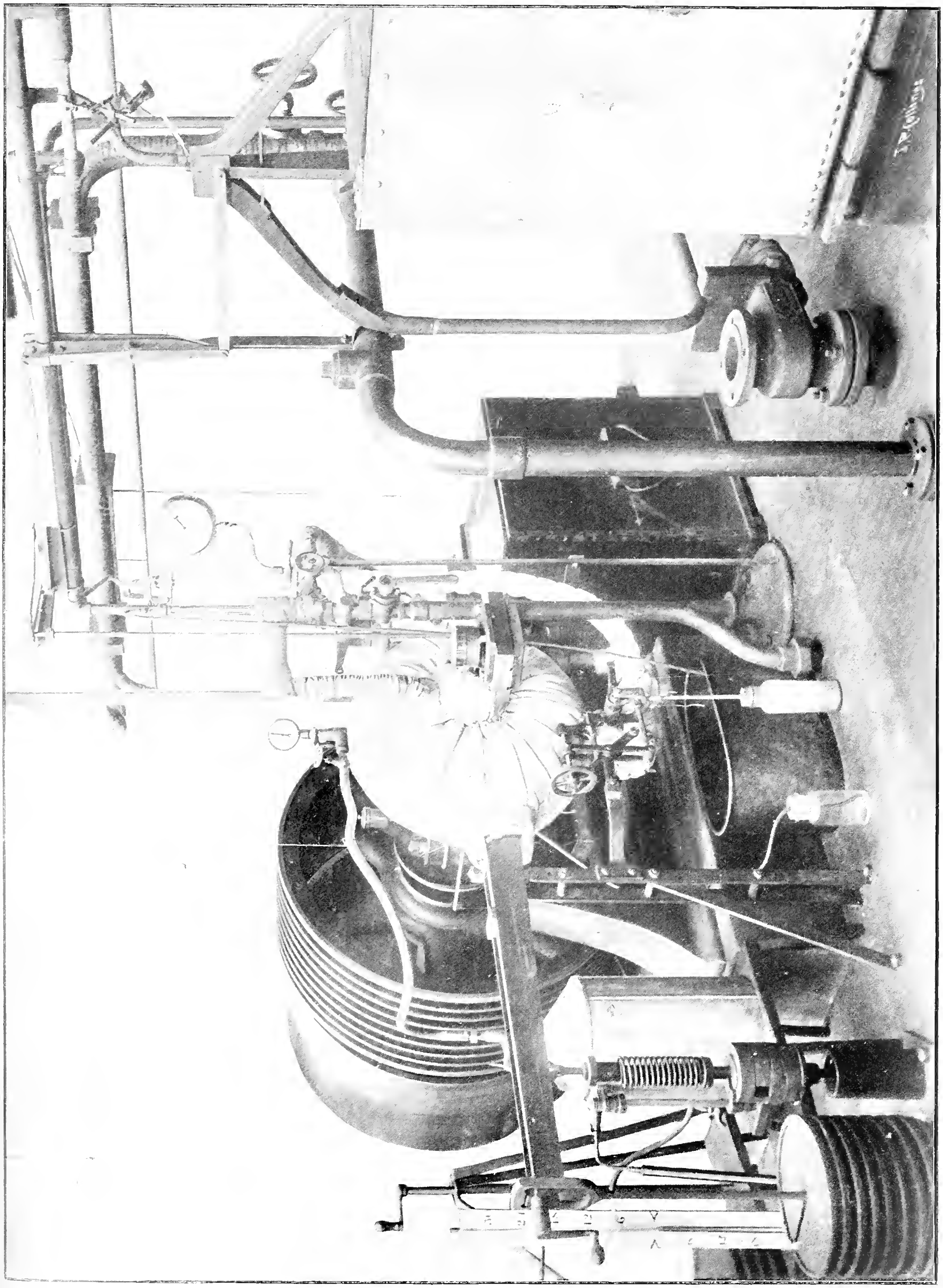

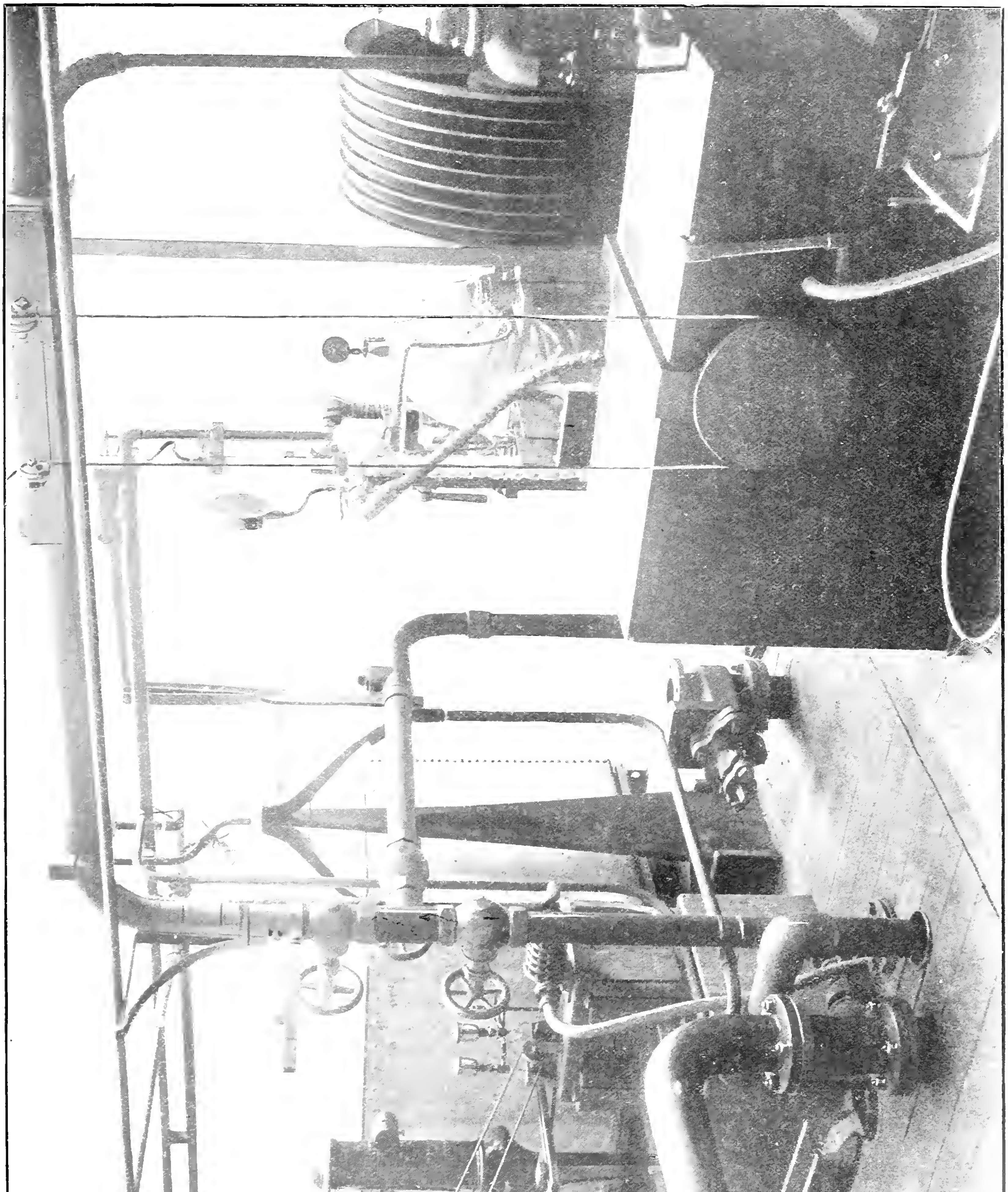

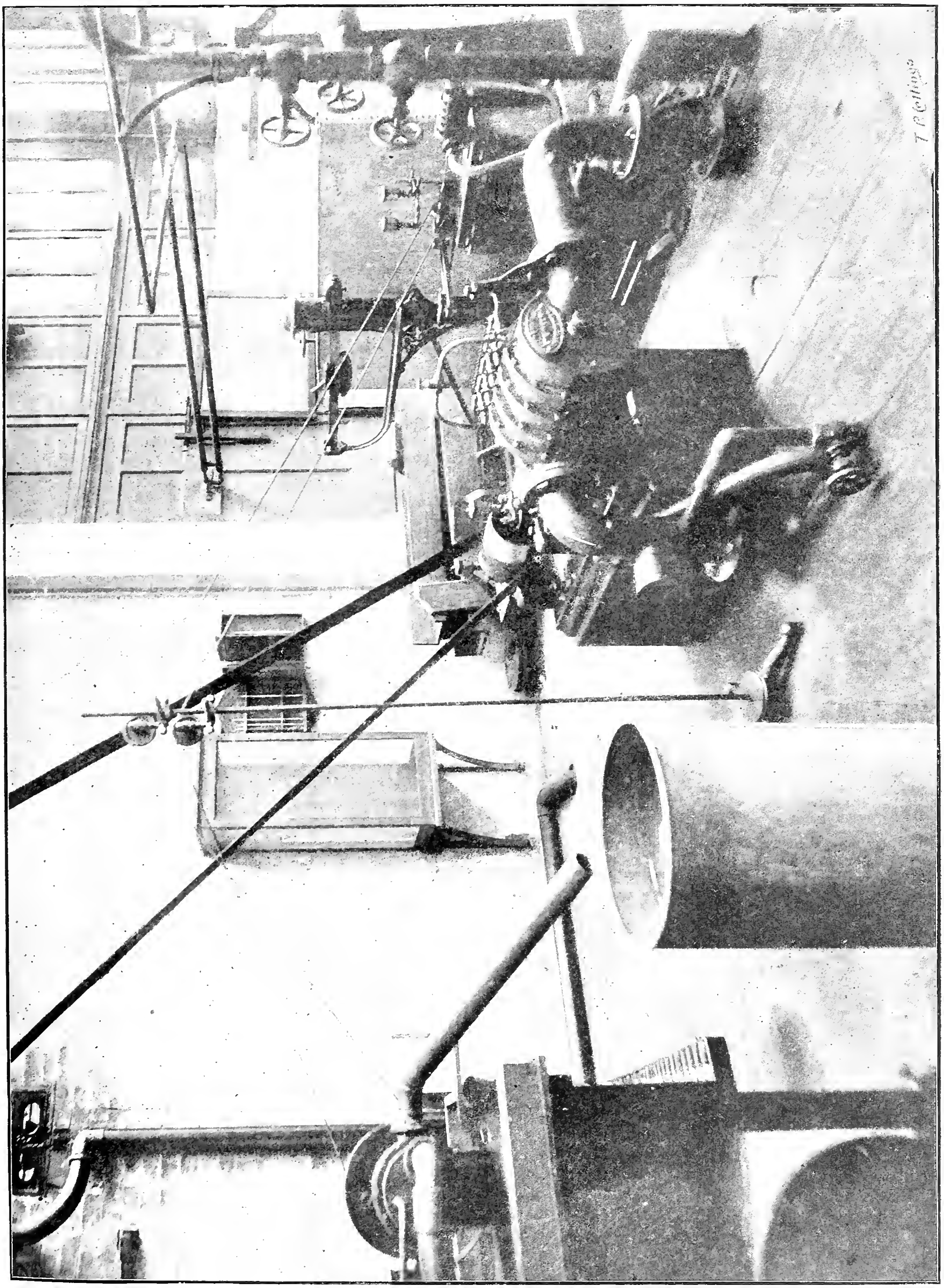

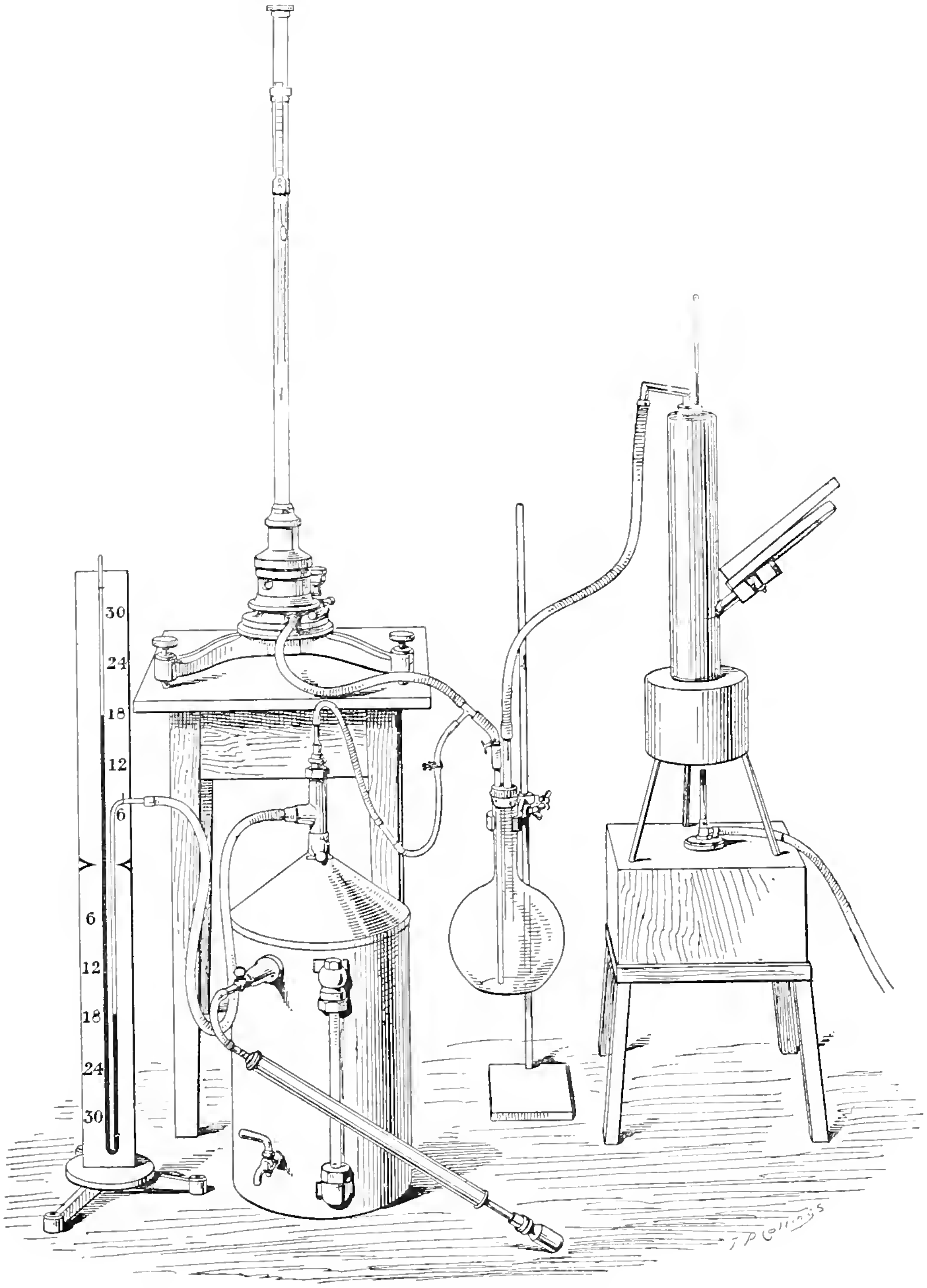
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INDEX SLIP.

XI. *On the Distribution of Frequency (Variation and Correlation) of the Barometric Height at Divers Stations.*

By KARL PEARSON, *F.R.S.*, *and* ALICE LEE, *B.A.*, *B.Sc.*

Received June 15.—Read June 17, 1897.

[PLATES 9–17.]

I.—ON THE FREQUENCY AT DIVERS STATIONS OF THE SEVERAL BAROMETRIC HEIGHTS.

1. *Introduction.*

LET a curve be formed such, that if y be the ordinate falling between the abscissæ x and $x + \delta x$, the area $y\delta x$ represents the frequency of the barometer with height lying between x and $x + \delta x$ for any locality. This curve will be spoken of as the *barometer frequency curve* for the given locality. The curve in any series of actual observations will be represented by a polygonal line; in the present case the element δx has been throughout taken as $\frac{1}{10}$ inch of barometric height.

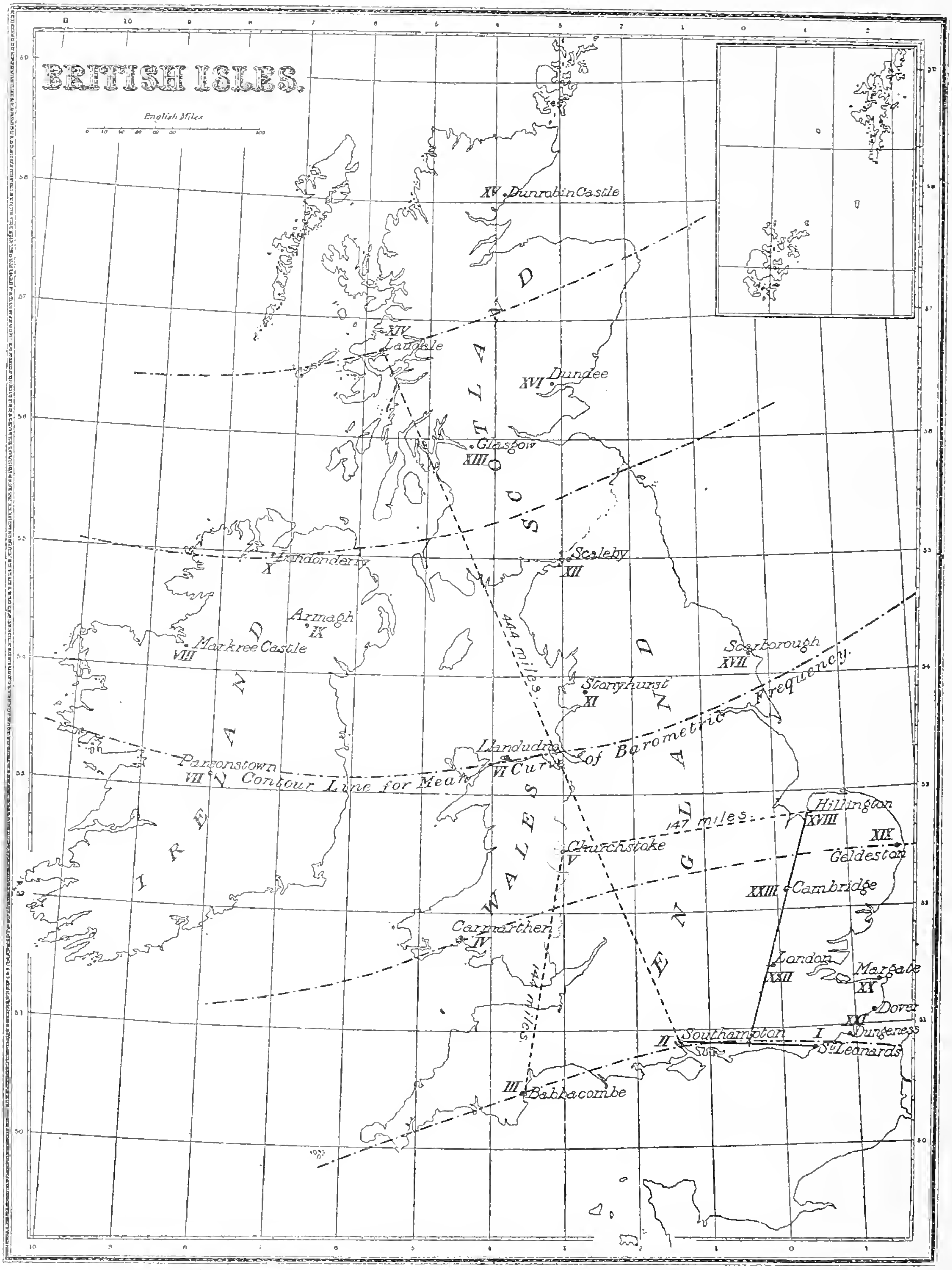
Such frequency curves occur in innumerable physical, anthropological and economic investigations, and can in many cases be fairly accurately represented by the normal curve of frequency, *i.e.*, LAPLACE's curve of errors. The barometric frequency curve is, however, a marked exception to this rule. The mean barometric height is very far from coinciding with the 'mode' or height of maximum frequency. While barometric frequency curves are remarkably smooth when a very large number of observations are dealt with, the distribution of frequency does not obey the normal law,* but some other law which up to the present has not been fully discussed.

In a memoir published in the 'Phil. Trans.,' A, vol. 186, pp. 343–414, a series of generalised frequency curves are introduced, and it is shown, pp. 351 and 382, that the asymmetry of the barometric frequency curve can probably be dealt with by one or other of these generalised curves.

The importance of this conclusion lies in the fact that the distribution of barometric frequency in any locality can then be fully described by the statement of the values of three or four well defined constants.

* This seems first to have been pointed out by DR. VENN, in a letter to 'Nature,' September 1, 1887.

7.3.98.


BRITISH ISLES.
English Miles
SCOTLAND
IRELAND
WALES
ENGLAND
XV Dunrobin Castle
XIV Laudale
XVI Dundee
XIII Glasgow
X Londonderry
XII Scaleby
IX Armagh
VIII Markree Castle
XI Stonyhurst
XVII Scarborough
VII Parsonstown
VI Llandudno
V Churchstoke
IV Carmarthen
III Babbacombe
II Southampton
I St. Leonards
XVIII Hillington
XIX Geldeston
XXIII Cambridge
XXII London
XX Margate
XXI Dover
Dungeness
444 miles.
147 miles.
144 miles.
Contour Line for Mean Curve of Barometric Frequency.

Accordingly, in order to test this theory of barometric frequency, series of observations have been reduced and the frequency distributions for divers localities fitted with generalised probability curves. On the basis of these curves the attempt has been made to answer the following questions :—

(*a.*) Is there any one type of curve especially characteristic of barometric frequency?

(*b.*) If so, what are the constants by which the distribution of this frequency can best be described?

(*c.*) Does there appear to be any numerical or geographical relation between these constants? and

(*d.*) Does a knowledge of their values for a variety of localities enable us to make any statement with regard to the physics of atmospheric pressure?

2. *Data of Observation.*

Our data were taken from the annual 'Meteorological Observations at Stations of the Second Order,' kindly placed at our disposal by Mr. R. H. Scott, F.R.S., Secretary to the Meteorological Council. The heights dealt with are 9 o'clock morning heights. Twenty stations were selected, sixteen in England and Scotland and four in Ireland. The stations selected are given on the accompanying map (page 424) of the British Isles. In their choice we were guided by (i.) the desire for a fairly uniform distribution and if possible a coast distribution, (ii.) the extent of data available, and (iii.) its continuity. The minimum number of years of continuous observation is five, the maximum thirteen. At one or two suitable places there were gaps in the record; at others less than five years were available. In all cases the records were only dealt with for whole years, to remove any irregularity which might arise from an annual periodicity in frequency. It is hoped that the fraction of a monthly periodicity—if there be a lunar influence on the frequency—will not be very sensible, when the records deal with 65 to 170 lunar periods. Any long period, such as a 19-year period, would of course render less general a frequency distribution based on five to thirteen years of observation only. The possibility of such a periodicity would render it very desirable to recalculate the frequency distributions for the same localities after, say, another ten years of observations.

Observations at the selected stations, when unreduced, were reduced by formulæ kindly provided by the Meteorological Office.

The following is what we shall hereafter term the *geographical order* of selected stations, with the years of observation and dates :—

Place.		Years of Record.	Date.
South Coast	St. Leonards	6	Jan. 1, 1880–Dec. 31, 1885
	Southampton	13	,, 1878– ,, 1890
	Babbacombe	13	,, 1878– ,, 1890
Wales	Carmarthen	13	,, 1878– ,, 1890
	Churchstoke	11	,, 1878– ,, 1888
	Llandudno	8	,, 1878– ,, 1885
Ireland	Parsonstown	13	,, 1878– ,, 1890
	Markree Castle*	10	,, 1880– ,, 1882 ,, 1884– ,, 1890
	Armagh	5	,, 1886– ,, 1890
	Londonderry	7	,, 1879– ,, 1885
North-west Coast	Stonyhurst	7	,, 1884– ,, 1890
	Scaleby	6	,, 1880– ,, 1885
	Glasgow	5	,, 1886– ,, 1890
	Laudale	11	,, 1880– ,, 1890
North-east Coast.	Dunrobin Castle	8	,, 1883– ,, 1890
	Dundee	11	,, 1880– ,, 1890
	Scarborough	5	,, 1881– ,, 1885
South-east Coast.	Hillington	13	,, 1878– ,, 1890
	Geldeston	6	,, 1880– ,, 1885
	Margate	8	,, 1883– ,, 1890

The frequency distribution† has been calculated for every $\frac{1}{10}''$. In reckoning the frequency, any height falling midway between two-tenths inches (*e.g.*, 28·75) has been split between those two-tenths (*e.g.*, half to 28·7 and half to 28·8).

Table I. of frequencies (p. 428) embraces the data from which the constants of the probability curves have been calculated. The lower or Roman figures in this table give the total number of days in which, during the years of observation, the barometer was at each particular height. The upper, or old-style figures, give the number of days per year at which the barometer (on the basis of these observations) may in the given locality be expected to be at each particular height.

We do not consider that the Stations of the Second Order give the best possible returns. Far from it; the irregularities which occur in the frequency distributions at several of these stations seem to us to indicate that either the methods of observation, the instruments, or the formulæ of reduction are not entirely satisfactory. It is probable that the Telegraph Stations would give far better results, and for a greater number of identical years. The ideal data would cover twenty or more well-distributed stations for identical periods of four "lustra," or twenty years. The observations of the Stations of the Second Order, however, were readily available in print, and our object has not been so much to make a contribution

* Omissions occur in the record for 1883, so that year is not included.

† Besides the above twenty stations, three telegraph stations, Cambridge, London (Brixton), and Dover-Dungeness will be found to have their constants recorded in our tables. The very great labour, however, of copying the manuscript records discouraged a larger selection of these stations. Our returns are for the thirteen years 1878–1890 inclusive.

to the meteorology of the British Isles as to illustrate what we believe to be a novel method of dealing with barometric frequency, in the hope that it may hereafter be taken up by professed meteorologists having at their disposal trained computators. We need not stay to lay stress on the large amount of arithmetical work involved in calculating the constants for even the short periods dealt with in the present memoir, it will be obvious to any one who has undertaken any statistical work of a like character. In order to save a portion of this arithmetical labour, a self-recording frequency barometer has been devised by Mr. G. A. YULE, some account of which will be found in an Appendix to this paper. We believe that with due precautions,* such an instrument would be a valuable addition to those already in use at the various meteorological stations, and serve for the ready calculation of the frequency constants.

Yet, admitting all the disadvantages of the non-contemporary character and short period of some of the data which we have used, we still believe that an examination of our diagrams will serve to convince the reader that our method is really capable of considerable service.

Putting aside one or two anomalies, we find a continuous and gradual change of the frequency constants as we pass from station to station round the coast, and it appears as easy to draw the contour lines of equal frequency as it has been hitherto supposed to be to draw isobars.

3. *Theory Applied.*

Let $y\,\delta x$ be the frequency of a deviation lying between x and $x+\delta x$ from the mean, then $\mu_n = \mathrm{S}(x^n y\,\delta x)\,\mathrm{S}(y\,\delta x)$ may be termed the n^{th} power of the mean n^{th} deviation. In the case of the Gaussian or normal distribution of the frequency of N observations

$$y = \frac{\mathrm{N}}{\sqrt{2\pi\mu_2}}\,e^{-x^2/(2\mu_2)}. \qquad \text{(i.)}$$

Here all the odd μ's vanish and we have the relation $\mu_{2\nu} = (2\nu-1)(2\nu-3)\ldots 5.3.1\mu_2^2$ for the even μ's. In particular $\mu_4 = 3\mu_2^2$.

When, therefore, the odd μ's are not nearly zero, and the relation $\mu_4 = 3\mu_2^2$ is not nearly satisfied, it is impossible on both counts to represent the frequency by a normal distribution.

Let $\beta_1 = \mu_3^2/\mu_2^3$, $\beta_2 = \mu_4/\mu_2^2$, then if β_1 be not zero and β_2 be not equal to three, a normal distribution is theoretically impossible.

Now a skew frequency distribution may be deduced from the skew-binomial $(p+q)^n$ as a limit, in the same way as it is possible to deduce the normal frequency distribution as a limit from the symmetrical binomial $(\frac{1}{2}+\frac{1}{2})^n$.

* Notably, frequent comparison with the standard mercurial barometer.

TABLE I.—Observed Annual Frequencies of the Several Barometric Heights

Old style figures, annual frequencies. *Roman figures, gross frequencies.*

	Twenty Stations in the British Isles.											
Station	St. Leonards.	Southampton.	Babbacombe.	Carmarthen.	Churchstoke.	Llandudno.	Parsonstown.	Markree Castle.	Armagh.	Londonderry.	Stonyhurst.	Scaleby.
Years of Record	6	13	13	13	11	8	13	10	5	7	7	6
Total Observations	2192	4748	4748	4748	4018	2922	4748	3653	1826	2557	2557	2192
Height, in inches.												
27·7	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	0·10 / 1	... / ...	... / ...	... / ...	... / ...
27·8	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	0 / 0	... / ...	... / ...	... / ...	... / ...
27·9	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	0 / 0	0·20 / 1	... / ...	... / ...	... / ...
28·0	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	0·08 / 1	0 / 0	0 / 0	... / ...	... / ...	... / ...
28·1	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	0 / 0	0·10 / 1	0 / 0	0·14 / 1	... / ...	... / ...
28·2	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	0 / 0	0 / 0	0 / 0	0 / 0	0·07 / 0·5	... / ...
28·3	... / ...	... / ...	... / ...	... / ...	... / ...	... / ...	0 / 0	0 / 0	0 / 0	0 / 0	0·21 / 1·5	0·17 / 1
28·4	... / ...	... / ...	... / ...	0·08 / 1	0·09 / 1	0·125 / 1	0·15 / 2	0·10 / 1	0 / 0	0 / 0	0 / 0	0 / 0
28·5	... / ...	0·08 / 1	... / ...	0·08 / 1	0·045 / 0·5	0·125 / 1	0·15 / 2	0·20 / 2	0 / 0	0 / 0	0 / 0	0·25 / 1·5
28·6	... / ...	0·15 / 2	0·19 / 2·5	0·23 / 3	0·14 / 1·5	0·125 / 1	0·15 / 2	0·25 / 2·5	0·6 / 3	0·36 / 2·5	0·14 / 1	0·08 / 0·5
28·7	... / ...	0·15 / 2	0·42 / 5·5	0·35 / 4·5	0·45 / 5	0·125 / 1	0·58 / 7·5	0·60 / 6	0·40 / 2	0·71 / 5	0·14 / 1	0·50 / 3·0
28·8	... / ...	0·31 / 4	0·38 / 5·0	0·65 / 8·5	0·68 / 7·5	0·875 / 7	0·50 / 6·5	1·05 / 10·5	0·40 / 2	1·29 / 9	1·14 / 8	1·42 / 8·5
28·9	0·33 / 2	0·66 / 8·5	0·46 / 6	0·81 / 10·5	1·14 / 12·5	1·06 / 8·5	0·77 / 10	1·90 / 19	1·30 / 6·5	2·00 / 14	1·21 / 8·5	2·50 / 15
29·0	1·17 / 7	1·04 / 13·5	0·69 / 9	0·88 / 11·5	1·23 / 13·5	2·69 / 21·5	1·62 / 21	2·40 / 24	3·10 / 15·5	2·93 / 20·5	1·50 / 10·5	3·25 / 19·5
29·1	2·25 / 13·5	1·65 / 21·5	1·88 / 24·5	2·69 / 35	3·09 / 34	3·00 / 24	3·85 / 50	5·40 / 54	3·20 / 16	6·14 / 43	2·36 / 16·5	4·50 / 27
29·2	3·67 / 22	2·85 / 37	2·81 / 36·5	4·54 / 59	4·23 / 46·5	6·19 / 49·5	6·23 / 81	7·35 / 73·5	8·30 / 41·5	7·71 / 54	5·64 / 39·5	5·92 / 35·5
29·3	4·42 / 26·5	6·08 / 79	6·04 / 78·5	6·62 / 86	6·50 / 71·5	7·06 / 56·5	9·38 / 122	12·15 / 121·5	7·20 / 36	10·86 / 76	6·93 / 48·5	11·50 / 69
29·4	8·12 / 49	8·31 / 108	9·31 / 121	12·23 / 159	12·00 / 132	13·81 / 110·5	12·58 / 163·5	14·60 / 146	13·80 / 69	13·57 / 95	10·86 / 76	11·75 / 70·5
29·5	11 / 66	13·96 / 181·5	15·15 / 197	15·46 / 201	16·45 / 181	19·25 / 154	19·19 / 249·5	19·50 / 195	21·20 / 106	21·36 / 149·5	16·00 / 112	25·17 / 151
29·6	20·33 / 122	19·58 / 254·5	18·62 / 242	24·00 / 312	23·45 / 258	26·75 / 214	23·46 / 305	26·30 / 263	25·60 / 128	26·79 / 187·5	26·57 / 186	27·00 / 162
29·7	26·50 / 159	26·81 / 348·5	26·85 / 349	28·12 / 365·5	29·91 / 329	31·25 / 250	29·81 / 387·5	31·75 / 317·5	31·90 / 159·5	30·29 / 212	29·07 / 203·5	31·75 / 190·5
29·8	36·42 / 218·5	35·65 / 463·5	36·12 / 469·5	36·50 / 474·5	32·77 / 360·5	32·375 / 259	35·12 / 456·5	33·85 / 338·5	34·00 / 170	35·07 / 245·5	33·57 / 235	33·08 / 198·5
29·9	44 / 264	42·19 / 548·5	41·27 / 536·5	38·96 / 506·5	38·73 / 426	37·125 / 297	38·65 / 502·5	37·25 / 372·5	33·00 / 165	36·29 / 254	41·50 / 290·5	39·42 / 236·5
30·0	49·83 / 2·99	46·35 / 602·5	45·92 / 597	42·81 / 556·5	41·64 / 458	44·81 / 358·5	39·15 / 509	37·10 / 371	39·40 / 197	39·57 / 277	39·21 / 274·5	37·25 / 223·5
30·1	48 / 288	47·65 / 619·5	46·96 / 610·5	44·00 / 572	44·05 / 484·5	40·00 / 320	40·08 / 521	39·95 / 399·5	38·90 / 194·5	33·07 / 231·5	44·21 / 309·5	34·83 / 209
30·2	40·75 / 244·5	38·46 / 500	38·88 / 505·5	35·88 / 466·5	34·27 / 377	32·50 / 260	35·23 / 458	32·70 / 327	31·30 / 156·5	30·29 / 212	34·36 / 240·5	34·83 / 209
30·3	27·25 / 163·5	29·38 / 382	30·19 / 392·5	28·46 / 370	29·91 / 329	25·50 / 204	27·23 / 354	26·55 / 265·5	33·10 / 165·5	26·07 / 182·5	30·00 / 210	26·33 / 158
30·4	18·08 / 108·5	18·27 / 237·5	18·23 / 237	18·00 / 234	18·86 / 207·5	18·625 / 149	19·31 / 251	17·40 / 174	18·10 / 90·5	21·36 / 149·5	21·93 / 153·5	16·50 / 99
30·5	12·25 / 73·5	14·58 / 189·5	13·65 / 177·5	12·31 / 160	12·09 / 133	11·00 / 88	11·62 / 151	10·85 / 108·5	11·40 / 57	9·50 / 66·5	9·64 / 67·5	8·92 / 53·5
30·6	6·83 / 41	6·81 / 88·5	7·31 / 95	7·31 / 95	8·50 / 93·5	6·50 / 52	6·88 / 89·5	4·20 / 42	6·70 / 33·5	5·93 / 41·5	6·14 / 43	5·17 / 31
30·7	2·58 / 15·5	3·35 / 43·5	3·12 / 40·5	3·46 / 45	4·18 / 46	3·25 / 26	2·85 / 37	1·30 / 13	2·00 / 10	3·07 / 21·5	2·71 / 19	2·42 / 14·5
30·8	0·67 / 4	0·54 / 7	0·38 / 5	0·50 / 6·5	0·59 / 6·5	0·50 / 4	0·38 / 5	0·25 / 2·5	0·10 / 0·5	0·79 / 5·5	0·14 / 1	0·50 / 3
30·9	0·58 / 3·5	0·31 / 4	0·31 / 4	0·23 / 3	0·18 / 2	0·625 / 5	0·19 / 2·5	0·15 / 1·5	... / ...	0·14 / 1	... / ...	0·33 / 2
31·0	0·25 / 1·5	0·08 / 1	0·08 / 1	0·08 / 1	0·09 / 1	... / ...	0·04 / 0·5	... / ...	... / ...	... / ...	... / ...	... / ...

at Twenty Stations in the British Isles, and Three Supplementary Stations.

Old style figures, annual frequencies. *Roman figures, gross frequencies.*

Twenty Stations in the British Isles.								Supplementary Stations.			
Glasgow.	Laudale.	Dunrobin Castle.	Dundee.	Scar-borough.	Hillington.	Geldeston.	Margate.	Dover-Dungeness.	London.	Cambridge.	Station.
5	11	8	11	5	13	6	8	13	13	13	Years of Record.
1826	4018	2922	4018	1826	4748	2192	2922	4748	4748	4748	Total Observations.
											Height, in inches.
...	...	...	...	...	...	...	...	...	...	...	27·7
...	...	...	...	...	...	...	...	...	...	...	
...	...	...	...	...	...	...	...	...	...	...	27·8
...	...	...	...	...	...	...	...	...	...	...	
...	0·09	...	...	...	...	...	...	...	...	...	27·9
...	1	...	...	...	...	...	...	...	...	...	
0·20	0·09	0·125	...	...	...	...	...	...	...	...	28·0
1	1	1	...	...	...	...	...	...	...	...	
0	0	0	0·18	...	...	...	...	...	...	...	28·1
0	0	0	2	...	...	...	...	...	...	...	
0	0	0	0·18	...	...	...	...	...	...	...	28·2
0	0	0	2	...	...	...	...	...	...	...	
0·20	0·045	0·25	0	...	0·12	...	...	...	...	0·08	28·3
1	0·5	2	0	...	1·5	...	...	...	...	1	
0	0·23	0·125	0	...	0·04	...	0·125	0·08	...	0	28·4
0	2·5	1	0	...	0·5	...	1	1	...	0	
0·20	0·18	0·19	0·27	0·50	0·08	...	0	0	...	0	28·5
1	2	1·5	3	2·5	1	...	0	0	...	0	
0·20	0·41	0·06	0·18	0·50	0·08	...	0	0	0·115	0·08	28·6
1	4·5	0·5	2	2·5	1	...	0	0	1·5	1	
0·60	0·77	0·625	0·68	0	0·15	0·33	0·25	0·08	0·04	0·15	28·7
3	8·5	5	7·5	0	2	2	2	1	0·5	2	
0·80	1·09	1·50	1·36	0·50	0·54	0·26	0·375	0·15	0·115	0·50	28·8
4	12	12	15	2·5	7	1·5	3	2	1·5	6·5	
1·50	2·95	2·25	2·64	1·90	0·96	0·12	0·69	0·35	0·77	0·81	28·9
7·5	32·5	18	29	9·5	12·5	7	5·5	4·5	10	10·5	
3·80	4·32	3·625	3·27	2·50	1·77	1·58	1·50	1·885	1·42	1·77	29·0
19	47·5	29	36	12·5	23	9·5	12	24·5	18·5	23	
5·60	6·55	7·06	6·32	4·50	2·62	2·42	1·44	2·15	2·04	1·85	29·1
28	72	56·5	69·5	22·5	34	14·5	11·5	28	26·5	24	
6·40	7·95	8·81	7·64	4·40	4·96	4·67	3·06	3·31	0·385	4·885	29·2
32	87·5	70·5	84	22	64·5	28	24·5	43	44	63·5	
11·70	11·41	13·31	10·91	8·50	6·15	6·42	5·875	5·73	6·46	6·23	29·3
58·5	125·5	106·5	120	42·5	80	38·5	47	74·5	84	81	
14·70	17·05	15·75	15·95	12·70	10·58	10·25	9·44	10·35	9·23	9·77	29·4
73·5	187·5	126	175·5	63·5	137·5	61·5	17·5	134·5	120	127	
22·30	23·23	23·375	21·00	20·10	17·54	14·42	12·125	13·115	14·615	16·385	29·5
111·5	255·5	187	231	100·5	228	86·5	97	170·5	190	213	
22·90	27·64	27·94	27·05	26·40	22·54	21·67	18·56	22·00	20·58	22·23	29·6
114·5	304	223·5	297·5	132	293	130	148·5	286	267·5	289	
34·40	31·50	34·375	34·32	35·80	29·81	31·92	26·81	28·92	28·85	29·85	29·7
172	346·5	275	377·5	179	387·5	191·5	214·5	376	375	388	
35·80	35·82	34·50	36·14	32·40	37·08	37·00	36·125	38·50	35·50	36·885	29·8
179	394	276	397·5	162	482	222	289	500·5	474·5	479·5	
38·00	34·59	37·56	36·64	40·20	41·92	40·75	44·56	43·85	42·23	41·35	29·9
190	380·5	300·5	403	201	545	244·5	356·5	570	549	537·5	
37·10	36·36	39·44	32·77	40·50	43·85	47·42	44·94	49·58	46·77	45·08	30·0
185·5	400	315·5	360·5	202·5	570	284·5	359·5	644·5	608	586	
30·60	32·82	33·44	37·00	41·60	42·69	44·75	49·625	46·50	45·92	42·31	30·1
153	361	267·5	407	207	555	268·5	397	604·5	597	550	
31·80	34·18	29·19	32·14	32·40	35·06	36·75	40·875	36·08	37·615	37·54	30·2
169	376	233·5	353·5	162	467·5	220·5	327	469	489	488	
29·30	24·23	22·81	24·55	25·10	26·58	27·67	29·44	26·08	27·46	26·96	30·3
146·5	266·5	182·5	270	125·5	345·5	166	235·5	339	357	350·5	
16·70	16·55	16·00	17·77	16·40	18·77	16·25	18·875	17·00	18·81	18·92	30·4
83·5	182	128	195·5	82	244	97·5	151	221	244·5	246	
9·90	8·55	7·31	8·50	9·40	10·81	9·83	12·625	11·00	12·19	11·54	30·5
49·5	94	58·5	93·5	47	140·5	59	101	143	158·5	150	
6·00	4·59	4·00	5·23	4·90	6·12	6·58	4·31	5·96	6·54	6·58	30·6
30	50·5	32	57·5	24·5	79·5	39·5	34·5	77·5	85	85·5	
2·50	1·91	1·625	1·95	3·40	2·69	2·17	3·25	1·92	2·81	2·69	30·7
12·5	21	13	21·5	17	35	13	26	25	36·5	35	
...	0·18	...	0·64	0·40	0·62	0·42	0·125	0·385	0·50	0·58	30·8
...	2	...	7	2	8	2·5	1	5	6·5	7·5	
...	...	...	...	0·40	0·23	0·67	...	0·27	0·19	0·19	30·9
...	...	...	...	2	3	4	...	3·5	2·5	2·5	
...	...	...	...	...	...	...	...	...	0·08	0·04	31·0
...	...	...	...	...	...	...	...	...	1	0·5	

Referred to its modal (or maximum) ordinate this frequency distribution is given by

$$y = y_0 (1 + x/a)^{\gamma a} e^{-\gamma x} \quad \text{(ii.)}$$

where

$$\gamma = 2\mu_2/\mu_3, \qquad a = 2\mu_2^2/\mu_3,$$

and if* $p = \gamma a$,

$$y_0 = \frac{N}{\sqrt{2\pi\mu_2}} \frac{\sqrt{2\pi(p+1)}\, e^{-p} p^p}{\Gamma(p+1)} \quad \text{(iii.)}$$

In order that this curve should fit a skew frequency distribution it is theoretically necessary that

$$6 + 3\beta_1 - 2\beta_2$$

should be zero, or at least that it should in statistical practice be small.

The theory of this curve is discussed, 'Phil. Trans.,' A, vol. 186, p. 373. It is shown in the same memoir that if

$$6 + 3\beta_1 - 2\beta_2 > 0$$

the generalised probability curve

$$y = y_0 (1 + x/a_1)^{m_1} (1 - x/a_2)^{m_2} \quad \text{(iv.)}$$

will be found suitable, whereas if

$$6 + 3\beta_1 - 2\beta_2 < 0$$

the generalised probability curve

$$y = y_0 \frac{1}{\{1 + (x/a)^2\}^m} e^{-\nu \tan^{-1}(x/a)} \quad \text{(v.)}$$

will, in a great variety of cases, represent the frequency. Methods for determining the constants of these curves are fully described, and these methods have been adopted in the present paper.

Of the curves (ii.), (iv.), and (v.), it may be noted that (ii.) has a range of frequency limited in one direction, (iv.) a range limited in both directions, while (v.), like the normal distribution (i.), has a range unlimited in both directions.

When a frequency distribution can be satisfactorily described by a curve of form (ii.), then it is generally possible to obtain a skew binomial representing, with a fair degree of accuracy, this distribution.†

In the 'Phil. Trans.' memoir above referred to, some Cambridge barometric data are dealt with, and it is shown for this case:

(i.) That $6 + 3\beta_1 - 2\beta_2$ is > 0, and this barometric frequency therefore fits a curve of limited range.

* The mean ordinate is given by $y_1 = y_0 \left(\frac{p+1}{p}\right)^p \frac{1}{e}$.

† The rule is not general, just as it is sometimes, but not always, possible to represent a normal distribution by a symmetrical binomial.

(ii.) That this expression not being large, a curve like (ii.) above describes with an equally small percentage error (p. 383) the frequency distribution.

(iii.) That a point-binomial could be found to fit the observations also with a close degree of accuracy.

The discussion of the thirteen years of Cambridge observations was not therefore conclusive as to the best means of describing barometric frequency, and more than one set of constants seemed capable of performing this function with an equal degree of accuracy. It was sufficient to indicate that the generalised probability curve in one type or another was fully capable of supplementing the obvious and admitted inadequacy of the normal curve.

The first stage of the present investigation was accordingly an inquiry as to the best type of generalised probability curve for barometric frequency.

4. *On the Value of the Criterion* $6 + 3\beta_1 - 2\beta_2$ *for Divers Localities.*

TABLE II.

Place.	Number of observations.	β_1.	β_2.	$6 + 3\beta_1 - 2\beta_2$.
St. Leonards . . .	2192	0·07123	3·04212	+·12944
Southampton . . .	4748	0·12260	3·36529	−·36277
Babbacombe . . .	4748	0·13110	3·34150	−·28970
Carmarthen	4748	0·12575	3·26479	−·15231
Churchstoke . . .	4018	0·12578	3·18891	−·00049
Llandudno	2922	0·08777	3·11777	+·02778
Parsonstown . . .	4748	0·15202	3·23306	−·01006
Markree Castle . .	3653	0·16380	2·98237	+·52664
Armagh	1826	0·20204	3·31567	−·02522
Londonderry . . .	2557	0·13185	3·06986	+·25581
Stonyhurst	2557	0·10401	3·42101	−·53001
Scaleby	2192	0·15122	3·12878	+·19610
Glasgow	1826	0·18256	3·21205	+·12359
Laudale	4018	0·21973	3·19784	+·26350
Dunrobin Castle . .	2922	0·16714	3·16067	+·18008
Dundee	4018	0·17885	3·23219	+·07217
Scarborough . . .	1826	0·13441	3·30247	−·20172
Hillington	4748	0·13942	3·26642	−·11458
Geldeston	2192	0·11654	3·27888	−·20815
Margate	2922	0·21562	3·50141	−·35596

This table shows us at once that there is a comparatively small range of values for the constants β_1 and β_2 for very diverse localities. The mean values of the constants, weighted with the number of years over which the observations extend, are as follows :—

$$\beta_1 = \cdot 14621, \quad \beta_2 = 3\cdot 24179, \quad 6 + 3\beta_1 - 2\beta_2 = - \cdot 04495.$$

It will thus be seen that the mean value of the criterion is small. Actually, it would mark a frequency distribution corresponding to a curve like (v.), but for practical statistical purposes a curve like (ii.), which fits closely when the criterion is even as large as 0·38421 (see Plate 10, fig. 6, of 'Phil. Trans.' memoir cited above), will suffice to describe the distribution of barometric frequency. The standard deviations of β_1, β_2, and $6 + 3\beta_1 - 2\beta_2$ are 0·03673, 0·12022, and 0·2068; these give for the probable errors of the means of the same three quantities (duly weighted), 0·00568, 0·01860, and 0·03200 respectively. Thus the probable errors are about 3·9, 0·5, and 71 per cent. of the observed means. Thus, while it is exceedingly improbable that the mean values of β_1 and β_2 differ much from their observed values, it is very possible that the true mean value of the criterion is zero and not − 0·04495.* The comparatively large and opposite values of the criterion at Markree Castle and Stonyhurst must, of course, be duly regarded, and a longer series of observations at these two stations may some day serve to indicate how far their barometric conditions are peculiar to local conditions of climate or of observation.

As a matter of fact, the curves corresponding to equations (iv.) or (v.) above were originally calculated for all the stations, and drawn upon the diagrams as well as those corresponding to (ii.), but the mean percentage error in frequency, when tested by a planimeter, showed no very sensible improvement. An examination of Plates 9–17, giving the graphical representation† of the observed frequency by the theoretical distribution (the negative direction of x being towards high barometer)

$$y = y_0 (1 + x/a)^p e^{-\gamma x},$$

amply demonstrates that this limit to the skew binomial suffices to satisfactorily describe barometric frequency. The closeness of the approximation is one rarely met with in the usual representation of variation by the normal curve of errors.

To illustrate how closely curve (ii.) corresponds to curve (v.), even with a value (− ·28970) of the criterion relatively large compared with the majority of those with which we are dealing, we give in the following table the frequencies *per annum* for Babbacombe as observed, and as calculated from (ii.) and (v.) respectively :—‡

* The probable error of the criterion for skew variation will be discussed in a forthcoming memoir by one of the present authors, and it will there be seen that this probable error is frequently large.

† We have to thank Mr. C. JAKEMAN, Demonstrator in University College, for much aid in the preparation of our diagrams.

‡ Another instance of the same closeness is exhibited in the 'Phil. Trans.' memoir above cited for a curve like (iv.) with (ii.). *Cf.* Curves I. and II. of fig. 6, Plate 10. The criterion is here still larger, *e.g.*, 38·421.

Height in inches.	Frequency *per annum.* Observed.	Calculated From (ii.).	Calculated From (v.).	Height in inches.	Frequency *per annum.* Observed.	Calculated From (ii.).	Calculated From (v.).
28·6	0·19	0 07	0·09	29·9	41·27	40·99	40·99
28·7	0·42	0·14	0·17	30·0	45·92	44·35	44·37
28·8	0·38	0·29	0·32	30·1	46·96	43·93	43·64
28·9	0·46	0·58	0·60	30·2	38·88	39·21	38·66
29·0	0·69	1·09	1·09	30·3	30·19	31·21	30·16
29·1	1·88	1·99	1·94	30·4	18·23	21·79	20·75
29·2	2·81	3·50	3·34	30·5	13·65	13·08	12·40
29·3	6·04	5·86	5·58	30·6	7·31	6·61	6·38
29·4	9·31	9·31	8·91	30·7	3·12	2·76	2·80
29·5	15·15	14·21	13·67	30·8	0·38	0·87	1·04
29 6	18·62	20·41	19·82	30·9	0·31	0·21	0·32
29·7	26·85	27·56	27·11	31·0	0·08	0·03	0·09
29·8	36·12	34·94	34·64				

In view of the above remarks we shall confine our attention to curves of the form (ii.). We can now discuss the relations between the constants of this curve and the physical quantities associated with barometry.

5. *Constants of a Local Distribution of Barometric Frequency.*

It is convenient to term the height of the barometer, corresponding to the maximum frequency, the *mode.* This mode never coincides with the mean, and we shall represent them by M_o and M_e respectively. The divergence of the mode from the mean marks the skewness of the frequency distribution, but it is fitting to measure this divergence in terms of some standard of variation of the local distribution. It is convenient to select as this standard, σ, the standard deviation of the distribution, or $\sqrt{\mu_2}$. We shall represent the skewness of the frequency by *Sk.* Another interesting physical constant of the distribution is indicated by the theoretical law of frequency—this is the *maximum possibile* of barometric height in the locality. It is directly obtained by adding to the mode or mean the possible range above either. This maximum height we will indicate by H_p, while H_o shall indicate the maximum observed height.

The following relations hold between the constants of the theoretical distribution and the above physical quantities

$$\sigma = \sqrt{\mu_2} = \sqrt{p+1}/\gamma \quad \text{(i.)},$$

$$M_o - M_e = 1/\gamma = \tfrac{1}{2}\mu_3/\mu_2 = \tfrac{1}{2}\sigma\sqrt{\beta_1} \quad \text{(ii.)},$$

$$H_p - M_e = a' = 2\mu_2^2/\mu_3 = 2\sigma/\sqrt{\beta_1} = (p+1)/\gamma \quad \text{(iii.)},$$

$$Sk. = (M_o - M_e)/\sigma = \tfrac{1}{2}\mu_3/\sqrt{\mu_2^3} = \tfrac{1}{2}\sqrt{\beta_1} \quad \text{(iv.)}.$$

Table III. contains the values of the mean and the moments calculated directly from the observed frequencies. In determining the moments, the observation polygons were regarded as trapezia ('Phil. Trans.,' A, vol. 153, p. 350). Table IV. then gives the constants of the theoretical distribution and the physical constants of the frequency deduced from them. The constants y_o, p, γ and the mean M_e suffice to determine the form of the frequency distribution. But any other three constants would serve equally well. Thus, we might take the three physical constants, y_o the frequency of the modal height, σ the variation, and *Sk.* the skewness of the distribution. Or, we might use y_1 the frequency of the mean instead of the frequency of the mode. In a memoir, which one of us hopes to shortly publish,* the probable errors are worked out for the constants of any frequency curve, and it is shown that these errors are not, as in the case of a normal frequency distribution, uncorrelated. An error, for example, in γ, marks a correlated error in p and in the mean, while in the case of the normal frequency curve there is no correlation between its position (mean) and shape (standard deviation). It becomes accordingly of considerable importance to determine which are the quantities in barometric frequency which have the least percentage probable errors, and then to adopt these as our standard constants of barometric frequency.

TABLE III. (The unit of μ_2, μ_3, μ_4 is $\frac{1}{10}''$.)

	Station.	M_e.	μ_2.	μ_3.	μ_4.
Selected stations of the second order.	St. Leonards	29''·9834	10·1350	8·6111	312·4840
	Southampton	29·9814	10·8126	12·4493	393·4473
	Babbacombe	29·9787	10·9012	13·0321	397·0938
	Carmarthen	29·9518	12·0724	14·8749	475·8197
	Churchstoke	29·9545	12·6642	15·9835	511·4453
	Llandudno	29·9229	13·0338	13·6743	529·1235
	Parsonstown	29·9271	13·1640	18·6224	560·2627
	Markree Castle	29·8861	15·4049	24·4703	707·7482
	Armagh	29·9134	13·7001	22·7935	622·3322
	Londonderry	29·8905	14·7902	20·6538	666·3570
	Stonyhurst	29·9406	12·2702	13·8613	515·0601
	Scaleby	29·8862	14·0539	20·4879	617·9683
	Glasgow	29·8859	14·3712	23·2779	663·3914
	Laudale	29·8570	15·0354	27·3287	722·9168
	Dunrobin Castle	29·8457	14·3639	22·2559	652·1105
	Dundee	29·8695	14·7373	23·9262	701·9974
	Scarborough	29·9028	12·9644	17·1137	555·0735
	Hillington	29·9429	11·7452	15·0299	450·6021
	Geldeston	29·9476	11·1784	12·7585	409·7187
	Margate	29·9745	10·3937	15·5596	378·2518
Telegraph stations.	Dover-Dungeness	29·9558	10·3496	10·7772	—
	London	29·9660	10·8159	11·1621	—
	Cambridge	29·9524	11·4491	13·3671	—

* See PEARSON and FILON, "On the Probable Errors of Frequency Constants." (Abstract) 'Roy. Soc. Proc.,' vol. 62, p. 173.

TABLE IV.—Constants of the Frequency Distribution.

	Station.	Frequency of mean (y_1).	Frequency of mode (y_0).	a'.	p.	γ.	σ.	Sk.	M_e.	M_o. Calc.	M_o. Observ.	H_p.	H_a.	$M_o - M_e$.	$H_p - M_o$.
				"			"		"	"	" "	"	"	"	"
Selected stations of the second order.	St. Leonards	45·596	45·887	2·3857	55·1592	23·5395	0·3184	0·1334	29·9834	30·0259	30·0→30·1	32·3692	30·974	0·0425	2·3433
	Southampton	44·184	44·876	1·8782	31·6258	17·3706	0·3288	0·1751	29·9814	30·0390	30·1→30·0	31·8596	30·974	0·0576	1·8206
	Babbacombe	43·997	44·732	1·8238	29·5097	16·7298	0·3302	0·1810	29·9787	30·0385	30·1→30·0	31·8024	30·976	0·0598	1·7639
	Carmarthen	41·825	42·688	1·9596	30·8075	16·2319	0·3474	0·1773	29·9518	30·0134	30·1→30·0	31·9114	30·969	0·0616	1·8980
	Churchstoke	40·845	41·522	2·0068	30·8016	15·8466	0·3559	0·1773	29·9545	30·0176	30·1→30·0	31·9613	30·974	0·0631	1·9437
	Llandudno	40·546	40·996	2·4215	44·5717	18·8194	0·3587	0·1481	29·9229	29·9760	30·0→30·1	32·3444	30·926	0·0531	2·3684
	Parsonstown	40·032	40·930	1·8611	25·3121	14·1378	0·3628	0·1949	29·9271	29·9978	30·1→30·0	31·7882	30·949	0·0707	1·7904
	Markree Castle	37·005	37·783	1·9396	23·4206	12·5907	0·3925	0·2024	29·8861	29·9655	30·1→30·0	31·8257	30·890	0·0794	1·8602
	Armagh	39·190	40·163	1·6469	18·7976	12·0211	0·3701	0·2247	29·9134	29·9966	30·0→30·1	31·5603	30·750	0·0832	1·5637
	Londonderry	37·790	38·424	2·1183	29·3381	14·3221	0·3846	0·1816	29·8905	29·9603	30·0→29·9	32·0088	30·885	0·0698	2·0485
	Stonyhurst	41·616	42·166	2·1723	37·4596	17·7042	0·3503	0·1612	29·9406	29·9971	30·1→30·0	32·1129	30·870	0·0565	2·1158
	Scaleby	38·672	39·452	1·9281	25·4517	13·7192	0·3732	0·1944	29·8862	29·9591	29·9→30·0	31·8143	30·891	0·0729	1·8552
	Glasgow	38·332	39·278	1·7745	20·9105	12·2348	0·3791	0·2130	29·8859	29·9669	29·9→30·0	31·6604	30·710	0·0810	1·6935
	Laudale	37·398	38·494	1·6544	17·2041	11·0034	0·3878	0·2344	29·8570	29·9479	30·0→29·9	31·5114	30·796	0·0909	1·5635
	Dunrobin Castle	38·316	39·140	1·8541	22·9323	12·9079	0·3790	0·2044	29·8457	29·9232	30·0→29·9	31·6998	30·730	0·0775	1·7766
	Dundee	37·825	38·693	1·8155	21·3651	12·3190	0·3839	0·2115	29·8695	29·9507	30·1→29·9	31·6850	30·824	0·0812	1·7343
	Scarborough	40·342	40·910	1·9642	28·7600	15·1509	0·3601	0·1833	29·9028	29·9688	30·1→30·0	31·8671	30·869	0·0660	1·8983
	Hillington	41·652	42·882	1·8569	27·6901	15·6291	0·3427	0·1867	29·9429	30·0069	30·0→30·1	31·7998	30·930	0·0640	1·7929
	Geldeston	43·487	44·128	1·9588	33·3244	17·5231	0·3343	0·1707	29·9476	30·0047	30·0→30·1	31·9064	30·924	0·0571	1·9017
	Margate	44·997	46·212	1·3886	17·5512	13·3598	0·3224	0·2322	29·9745	30·0493	30·1→30·0	31·3631	30·770	0·07485	1·3138
	Mean	40·682	41·4675	1·9204	28·5996	15·1580	0·3581	0·1894	29·9221	29·9902	..	31·8425	30·879	0·0681	1·8523
	S.D	2·606	2·583	0·2310	9·0044	2·8927	0·0226	0·0257	0·0417	0·0338	..	0·0761	0·085	0·0118	0·2396
	P.E. of mean	0·393	0·390	0·0348	1·3581	0·4363	0·0034	0·0039	0·0063	0·0051	..	0·0115	0·0128	0·0018	0·0361
	Coefficient of variation	6·45	6·22	12·03	31·48	19·08	6·31	13·57	0·14	0·11	..	0·24	0·275	17·33	12·94
Supplementary stations.	Dover - Dungeness	45·182	45·789	1·9883	37·1792	19·2066	0·3217	0·1554	29·9558	30·0079	30·0→30·1	31·9441	30·94	0·0521	1·9362
	London	44·216	44·772	2·0961	39·6221	19·3798	0·3289	0.1569	29·9660	30·0176	30·0→30·1	32·0621	30·95	0·0516	2·0445
	Cambridge	42·839	43·487	1·9613	32·5973	17·1303	0·3384	0·1721	29·9524	30·0108	30·0→30·1	31·9137	30·96	0·0584	1·9029

The following are some of the formulæ given in the memoir above referred to:

Probable error of mean . $= \cdot 67449\sigma/\sqrt{n}$,

Probable error of γ . . $= \dfrac{\cdot 67449\gamma}{\sqrt{2n}}\sqrt{\dfrac{\frac{1}{2}+S}{S}}$,

Probable error of p . . $= \cdot 67449p/\sqrt{nS}$,

Probable error of y_0 the modal frequency $= \dfrac{\cdot 67449y_0}{\sqrt{2n}}\sqrt{1+\dfrac{2T^2}{S}}$,

Probable error of y_1 the mean frequency $= \dfrac{\cdot 67449y_1}{\sqrt{2n}}\sqrt{1+\dfrac{2}{S}\left(p\log\dfrac{p+1}{p}-\dfrac{p}{p+1}+T\right)^2}$,

Probable error of σ . . $= \dfrac{\cdot 67449\sigma}{\sqrt{2n}}\sqrt{1+\frac{1}{2}\dfrac{1}{(p+1)^2 S}}$,

Probable error of skewness $= \dfrac{\cdot 67449}{2\sqrt{n}}\dfrac{p}{p+1}\dfrac{1}{\sqrt{(p+1)S}}$,

Correlation of errors in p and γ . $= \sqrt{\dfrac{1}{1+2S}}$,

Correlation of errors in p and mean $= 0$,

Correlation of errors in γ and mean $= \sqrt{\dfrac{2}{p+1}S\Big/(\frac{1}{2}+S)}$.

Here S is the series

$$\frac{B_1}{p}-\frac{B_3}{p^3}+\frac{B_5}{p^5}-\dots$$

and T

$$\frac{B_1}{2p}-\frac{B_3}{4p^3}+\frac{B_5}{6p^5}-\dots$$

the B's being the BERNOULLI numbers.

Now a consideration of these formulæ gives the following results—which will be found to be fully verified by the Table V. of calculated values—

(*a.*) The errors in the mean, standard deviation and skewness are very small. The errors in both mean and modal frequencies are small, but the error in the modal frequency is considerably smaller than that in the mean frequency. This follows from the fact that the frequency curve is horizontal at the mode, but slopes at the mean, and accordingly any bodily shift of the curve produces far more effect on the mean than on the modal frequency.

(*b.*) The errors in p and γ are large in some cases very large. The question then arises how is it possible to determine the frequency curve correctly. The answer lies in the fact that p and γ are so highly correlated, that given a large error in p, there

TABLE V.—Probable Errors of the Chief Constants of the Frequency Distributions for the British Isles.

Station.	P.E. of mean.	P.E. of skew-ness.	P.E. of standard deviation.		P.E. of modal frequency.		P.E. of mean frequency.		P.E. of constant p.		P.E. of constant γ.		Correlation of errors in p and γ.	Correlation of errors in mean and γ.
			Gross.	Per cent.	Gross.	Per cent.	Gross.	Per cent.	Gross.	Per cent.	Gross.	Per cent.		
St. Leonards . .	0·0046	0·0172	0·0033	1·0451	0·4678	1·0195	0·4807	1·0542	14·4568	26·2092	3·0941	13·1441	0·9970	0·0146
Southampton . .	0·0032	0·0114	0·0024	0·7224	0·3110	0·6931	0·3236	0·7324	4·2648	13·4853	1·1774	6·7781	0·9948	0·0253
Babbacombe . .	0·0032	0·0114	0·0024	0·7243	0·3108	0·6931	0·3234	0·7350	3·8441	13·0265	1·0958	6·5499	0·9944	0·0271
Carmarthen . .	0·0034	0·0114	0·0025	0·7231	0·2959	0·6931	0·3067	0·7367	4·1004	13·3097	1·0860	6·6908	0·9946	0·0259
Churchstoke . .	0·0038	0·0124	0·0028	0·7860	0·3128	0·7534	0·3256	0·7973	4·4561	14·4670	1·1524	7·2725	0·9946	0·0259
Llandudno . . .	0·0045	0·0148	0·0033	0·9103	0·3620	0·8831	0·3730	0·9199	9·0954	20·4062	1·9273	10·2412	0·9963	0·0180
Parsonstown . .	0·0036	0·0113	0·0026	0·7291	0·2838	0·6933	0·2967	0·7412	3·0539	12·0650	0·8585	6·0721	0·9935	0·0314
Markree Castle .	0·0044	0·0129	0·0033	0·8343	0·2987	0·7905	0·3142	0·8490	3·0989	13·2313	0·8389	6·6626	0·9930	0·0339
Armagh . . .	0·0058	0·0179	0·0044	1·1938	0·4493	1·1186	0·4774	1·2181	3·1519	16·7677	1·0167	8·4578	0·9912	0·0419
Londonderry . .	0·0051	0·0155	0·0038	0·9873	0·3629	0·9445	0·3786	1·0019	5·1926	17·6991	1·1856	8·8997	0·9944	0·0272
Stonyhurst. . .	0·0047	0·0157	0·0034	0·9784	0·3981	0·9442	0·4121	0·9903	7·4914	19·9985	1·7782	10·0436	0·9956	0·0214
Scaleby	0·0054	0·0167	0·0040	1·0728	0·4026	1·0203	0·4217	1·0906	4·5318	17·8056	1·2294	8·9609	0·9935	0·0313
Glasgow . . .	0·0060	0·0180	0·0045	1·1868	0·4393	1·1183	0·4636	1·2094	3·6978	17·6841	1·0904	8·9122	0·9921	0·0378
Laudale. . . .	0·0041	0·0120	0·0031	0·8089	0·2903	0·7542	0·3090	0·8263	1·8505	10·8146	0·6007	5·4594	0·9905	0·0457
Dunrobin Castle .	0·0047	0·0143	0·0035	0·9338	0·3460	0·8839	0·3642	0·9504	3·3571	14·6392	0·9516	7·3726	0·9928	0·0346
Dundee	0·0041	0·0122	0·0031	0·7992	0·2919	0·7543	0·3079	0·8141	2·5745	12·0502	0·7480	6·0719	0·9923	0·0371
Scarborough . .	0·0057	0·0184	0·0042	1·1692	0·4573	1·1177	0·4788	1·1868	5·9640	20·7371	1·5800	10·4284	0·9943	0·0277
Hillington . . .	0·0034	0·0114	0·0025	0·7263	0·2973	0·6932	0·3072	0·7375	3·4941	12·6187	0·9920	6·3472	0·9940	0·0288
Geldeston . . .	0·0048	0·0169	0·0035	1·0610	0·4501	1·0200	0·4676	1·0753	6·7891	20·3728	1·7939	10·2372	0·9950	0·0240
Margate . . .	0·0040	0·0141	0·0031	0·9474	0·4087	0·8844	0·4352	0·9676	2·2481	12·8087	0·8637	6·4649	0·9906	0·0448
Mean . .	0·0044	0·0143	0·0033	0·8670	0·3618	0·8736	0·3784	0·9317	4·8357	16·0098	1·2530	8·0534	0·9937	0·0302
Dover-Dungeness	0·0031	0·0115	0·0023	0·7182	0·3173	0·6929	0·3285	0·7270	5·4360	14·6210	1·4104	7·3432	0·9956	0·0216
London	0·0032	0·0115	0·0024	0·7167	0·3102	0·6929	0·3206	0·7250	5·9804	15·0936	1·4687	7·5785	0·9958	0·0203
Cambridge . . .	0·0033	0·0115	0·0024	0·7215	0·3014	0·6930	0·3133	0·7314	4·4628	13·6907	1·1786	6·8803	0·9949	0·0245

is little variation possible in γ; it also has a large *correlated* error, and these errors tend very closely to balance each other, *i.e.*, to retain the same shape for the frequency curve.*

Indeed it can be shown, both theoretically and empirically, that very considerable changes may be made in the constant p, and the frequency curve will not sensibly change its shape provided the correlated error be made in γ.

But these results show us at once that while the frequency curves as determined from p and γ may fit—as indeed they do—the observations very accurately, still p and γ are not good constants on which to base (when treated separately) any discussion of the relative distribution of barometric frequency with geographical position. The best constants are, as we might hope they would be, the physical constants, namely the mean or modal height, the mean or modal frequency, the standard deviation or variability, and the skewness. It is accordingly to these constants, as the fundamental constants, that we have specially directed our attention. All the other quantities involved can be expressed in terms of them. If q be written for the skewness *Sk.* we have:

$$p = \frac{1}{q^2} - 1, \qquad M_o - M_e = \sigma q,$$

$$\gamma = \frac{1}{\sigma q}, \qquad H_p - M_e = \sigma / q.$$

In Table V. the whole system of probable errors and of correlation between errors is given for the 23 stations, and an inspection of them will show at a glance the degree of accuracy which may be expected from any number of years of barometric observation. Curves will be found drawn on Plate 9, giving the probable percentage errors of the modal frequency, and the standard-deviation together with the probable errors of the mean and the skewness for any number of years up to 20, for a station having the mean values of σ and p. They will, we believe, be found of service by the practical meteorologist for estimating how closely any series of observations will suffice to determine the theoretical frequency of the station. It is true that these errors are not merely functions of the number of years of observation; they depend also upon the local values of p and σ, as an inspection of Table V. will illustrate. Compare, for example, Southampton with Babbacombe, or Armagh with Glasgow. The deviations of the probable errors from the graphical values owing to the variation in p and σ may, however, be looked upon as a second order deviation, and a reasonable, if rough, appreciation of the magnitude of the errors made may be obtained from Plate 9.

It will be seen from the Table V. as well as from Plate 9, that the value of the *physical* frequency constants are, for the years under consideration, given by the calculated values with a very close degree of accuracy.

* It should be noticed that the term involving p in the frequency, *i.e.*, $\left(1 + \frac{\gamma x}{p}\right)^p$, becomes more and more independent of p as p increases.

6. *On the Standard Barometric Frequency Curve for the British Isles.*

In order to reach a fair appreciation of the manner in which the distributions of frequency differ at any two local stations, it is desirable to have a standard frequency curve with which either of them may be compared. This curve was obtained in the following manner. The mean values of y_1, a', p and γ were found; they are recorded with their standard deviations, probable errors and coefficients of variation below the "selected stations" in Table IV. Naturally these values did not satisfy the relation $(p+1) = \gamma a'$, which is necessary if the curve is to be of the required type. Accordingly small alterations δp, $\delta\gamma$, $\delta a'$ were made in their values, proportional in each case to the corresponding probable error of p, γ, a', so that the relation

$$(p + \delta p + 1) = (\gamma + \delta\gamma)(a' + \delta a')$$

was satisfied. The alterations in no case amount to 1 per cent. of the corresponding value, being 0·179 of the probable errors of those values. The curve thus determined has for its equation, the unit of y being a day, and of x one-tenth inch :—

$$y = 40{\cdot}682\left(1 + \frac{x}{1{\cdot}9267}\right)^{28{\cdot}3556} e^{-15{\cdot}2364x},$$

the origin being at the mean height 29″·9221. This curve we shall in future speak of as the Standard Frequency Curve for the British Isles. It may be at first looked upon as an arbitrary curve, artificially obtained with a view to having some standard of comparison, but when it has once been plotted on the diagrams of the barometric frequency, we can give the standard curve a physical meaning. On examining Plate 15, fig. xvii. ; Plate 11, fig. vi. and Plate 12, fig. vii., it will be at once seen that the Standard Frequency Curve expresses the barometric frequency of places on a line running somewhat south of Scarborough, a trifle south of Llandudno, and slightly north of Parsonstown. Approximately we may state that the climate of Llandudno very nearly represents the standard of barometric frequency for the British Isles. Probably there would be hardly any sensible deviation at all between the standard frequency curve and the distribution of barometric frequency for places like Hull and Chester.

A comparison of the mean frequency contour as sketched in on the map, page 424, with Plate 13 of the 'Meteorological Atlas of the British Isles,' issued in 1883 by the Meteorological Office, shows that this contour is not very divergent from the isobar for 29″·90 ; it lies sensibly above the isobar given in that plate for 29″·92. The important suggestion now made is that a series of contour lines, generalised isobars, could be constructed, along which not only the mean barometric height would be the same, but practically all the constants which determine the distribution of barometric frequency. Our data are very far from sufficient to enable us to draw such a series satisfactorily, but an examination of Plate 10 will show that another such generalised isobar passes very close to St. Leonards, Southampton and Babbacombe, where in each case the fitted distribution diverges almost to the same extent from the standard ; Plate 13, figs. x. and xii., and Plate 14, fig. xiii. shows Londonderry and some place between Glasgow and Scaleby marking a third, while Laudale, and a place about midway between Dunrobin and Dundee give a fourth ; lastly, Geldeston and Carmarthen mark a fairly satisfactory fifth generalised isobar. These isobars are roughly sketched in on the map (page 424) to indicate the general grouping of stations with approximately identical frequency distributions. The accurate determination of these isobars is a problem for the future ; it will require a knowledge of the frequency at a much larger number of stations and a delicate process of interpolation.

Turning now to the diagrams on Plates 10–17, the reader will find the standard frequency curve marked in strokes and dots, and the local frequency curve given by a broken line. Comparing these two curves, and neglecting, for the time, the irregular observation-polygon on which the latter is based, a continuous and regular change in the divergence of these two will be observed as the diagrams are taken in succession round, say, the coast line of England and Scotland. There are slight local deviations from uniformity, but on the whole there can be no doubt that the distribution of barometric frequency is a perfectly uniform and continuous phenomenon over the district treated. A fairly accurate distribution for any station not included among the twenty dealt with could be obtained by graphical interpolation from the generalised isobars we have roughly sketched.

7. *On the Modal Height of the Barometer.*

Attention has already been drawn by FECHNER, MAZELLE, H. MEYER in a series of papers to the importance of modal heights, which they term *Scheitelwerthe.** It seems, however, impossible to accurately determine the modal heights, even if the observations be grouped in very small ranges, until a theory of skew frequency for the barometer has been adopted. A glance at the diagrams for Churchstoke, Carmarthen, Dunrobin, &c., will sufficiently illustrate how delusive is the peak of the observation polygon. The true position of the mode depends, like that of the mean, on the *whole* series of observations, and not on the observational maximum only, which must always be largely the result of the grouping selected, and the elementary range taken as basis of the grouping. In Table IV., under the heading M_0, observed value, will be found nearly all our observation polygon by itself could tell us of the modal height. We should be able to determine the nearest tenth to

* See FECHNER: "Ueber den Ausgangswerth der kleinsten Abweichungssumme," &c. 'Abhandlungen der math.-phys. Classe der k. Sächsischen Gesells. der Wissenschaften,' vol. 11, No. 1, 1874; MAZELLE: 'Wiener Denkschriften,' vol. 60, p. 433, 1893, vol. 62, p. 57, 1895; dealing with air temperatures; H. MEYER: 'Anleitung zur Bearbeitung meteorologischer Beobachtungen,' Berlin, 1891, pp. 12–27; but compare JUL. HANN: 'Die Klimatologie,' 2te Ausgabe, Einleitung.

the mode and to say on which side of this tenth the mode lies. Thus in Table IV., for example, Glasgow 29″·9 ⇾ 30″, means that the Glasgow modal height has 29″·9 for its nearest tenth, but lies on the 30″ side of this.

A closer approximation to the position of the mode may be obtained by dealing with the three chief frequencies and finding the vertex (i.) of a parabola with vertical axis passing through their tops, or (ii.) of a normal curve of errors with vertical axis passing through the same three points, the latter, according to our experience, giving the better approximation to the true mode.

Let c be the unit of grouping, let y_2 be the maximum frequency, and y_1 and y_3 the frequencies on either side of it. Let z be the distance of the mode from y_2 towards y_3, then we have

(i.) For the parabola :

$$z = c\frac{\Delta_{21} + \Delta_{32}}{2(\Delta_{21} - \Delta_{32})}, \text{ where } \Delta_{rs} = y_r - y_s.$$

(ii.) For the normal curve :

$$z = c\frac{\delta_{21} + \delta_{32}}{2(\delta_{21} - \delta_{32})}, \text{ where } \delta_{rs} = \log y_r - \log y_s.$$

A third method, which is generally far more accurate, as it depends on all the observations, has been given in the memoir on skew variation in the 'Phil. Trans.' already cited (see pp. 375–6). This depends upon the principle that the distance of the mode from the mean is, with a close degree of approximation, thrice the distance of the median* from mean. It may be as well to illustrate these methods on an actual example.

Modal Height of the Barometer at Southampton.

(i.)	By inspection of observation polygon	30″·1 ⇾ 30″·0
(ii.)	By using a parabola through three ordinates . . .	30″·0625
(iii.)	By using a normal curve through three ordinates .	30″·0615
(iv.)	By the principle of the modal third, as above . .	30″·0372
(v.)	By actual determination of the frequency curve. .	30″·0390

By adding up the frequencies for Southampton in Table I., and then interpolating, it will be found that the median height of the barometer there is almost exactly 30″. But by Table III., the mean height is 29″·9814, the third of the distance accordingly between mean and mode or the modal third = 0·0186, whence we obtain 30″·0372 for the mode. It is clear that this method gives a close approximation to the true result, and is one which can be used by any ordinary observer. The value obtained will be,

* The median height of the barometer is the height given by that observation out of $2n + 1$ observations, which has the heights of n observations less and the heights of n observations greater than its own height, *i.e.*, it is the middle height of the series of observations arranged in order of magnitude.

as a rule, remarkably closer to the true mode than any application of methods such as (ii.) or (iii.).

If we invert the process and calculate from Table IV. the median values at our three southern stations, we find :—

Median,	St. Leonards	29″·9976
,,	Southampton	30″·0006
,,	Babbacombe	29″·9986

Thus we see that the median height of the barometer along the south coast of England approaches extremely closely to the 30″, so commonly adopted by physicists as a measure of the "standard atmosphere."

There is another convenient method of looking at this standard atmosphere of 30″. If we turn to the modal heights in Table IV., we notice that the mean modal height for all our stations is 29″·9902, with the very small probable error of 0·0051. Hence the mean modal height for the British Isles differs by less than twice its probable error from the customary standard atmosphere. On the other hand, the mean mean height differs by more than 12 times its probable error from the standard atmosphere. We may accordingly look upon the standard atmosphere of 30″, either as corresponding very closely to the mean modal height of the barometer for the British Isles, or as representing the median height of the barometer along the English southern coast.

Another interesting feature of the modal height is that it is less variable than the mean as we pass from station to station, and the probable error in the determination of the mean of the modes is accordingly less than that of the mean of the means. Whereas up and down the British Isles we find a coefficient of variation of 0·14 per cent. for the mean barometric height, the corresponding quantity is only 0·11 per cent. for the modes. On this account, and because the mode—as the most frequent barometric height—has a more direct physical interpretation than the mean, it seems to us that a record of local modes would be of greater significance than a record of local means.

Owing to the property we have already noticed, *i.e.*, that the distribution of barometric frequency is constant along contour lines, differing, at any rate, not very widely from the isobars in the narrower sense of the word, it follows that mean and mode oscillate, although not without deviations, in general accordance. Thus, both mean and mode are least at Laudale and Dunrobin Castle, the most northerly stations we have dealt with ; both means and modes are greatest at the four southernmost stations, Margate, St. Leonards, Southampton, and Babbacombe, although it is characteristic of some peculiarity of observation or climatological individuality, that while out of the four the means are greatest at St. Leonards and Southampton, the modes are greatest at Southampton and Margate. In particular we look upon the modal value at Margate (30″·0493) as standing geographically between Geldeston (30″·0047) and St. Leonards

(3″·0259) as not beyond suspicion, and accordingly open to revision when a wider range of data is available. The great value of the "skewness" in the Margate distribution is also unsatisfactory.

8. *On the Variability of the Barometric Height.*

The only method hitherto used by meteorologists to express briefly the variability of the atmospheric pressure is, so far as we are aware, the statement of the maximum and minimum heights reached during any given period. The fallacy of this method has been illustrated by one of us elsewhere.* It gives no real impression whatever of the manner in which the *bulk* of the variation is distributed, yet, for most climatological purposes, this is precisely what we require. Judged by such a test as this (namely, the range from maximum to minimum height observed) Hillington has a more variable climate than Scarborough, and Southampton than Babbacombe, but, as a matter of fact, Hillington is considerably less variable than Scarborough, and Southampton is slightly less variable than Babbacombe.

Another striking illustration of the defects of this method of measuring the variability has been mentioned to us by Mr. R. H. SCOTT, namely, that in 23 years of barometric observations at Valencia, the maximum was only reached in the *last* year.

Whatever be the form of the frequency distribution, the problem of determining how the bulk of the variability is distributed about either mean or mode, is exactly similar in character to the problem of determining how the inertia of a plate is distributed about any axis in its plane. One satisfactory and useful measure in both cases is the swing-radius, or radius of gyration. This is the quantity which, for distribution about the mean, appears under the heading σ the standard deviation in our Table IV.

Judged by this test the following is the order of variability in barometric pressure at our 20 stations :—

(1).	St. Leonards	[1].	(11).	Scarborough	[13].
(2).	Margate	[4].	(12).	Parsonstown	[11].
(3).	Southampton	[2].	(13).	Armagh	[12].
(4).	Babbacombe	[3].	(14).	Scaleby	[15].
(5).	Geldeston	[7].	(15).	Dunrobin Castle	[20].
(6).	Hillington	[8].	(16).	Glasgow	[16].
(7).	Carmarthen	[6].	(17).	Dundee	[18].
(8).	Stonyhurst	[9].	(18).	Londonderry	[14].
(9).	Churchstoke	[5].	(19).	Laudale	[19].
(10).	Llandudno	[10].	(20).	Markree Castle	[17].

* In an essay on "Variation in Man and Woman;" see 'The Chances of Death, and other Studies in Evolution,' vol. 1, p. 275.

In square brackets we have inserted the order of mean barometric pressures. It will be seen at once that the 10 stations of least variability are the 10 stations of highest pressure. Thus, there is a correlation between high pressure and small variability.* Some changes in the two orders may well be due to the doubt which attaches to the reduction to sea level, but taken as a whole the list illustrates the local character of the climate at the various stations, so far as it depends upon the height and variability in height of the barometer. This method of appreciating variability seems to us more satisfactory than a mere measurement of maximum to minimum ranges, which, with our data, while leaving St. Leonards first for steadiness of climate, would place Geldeston quite close to it, and make both that town and Scarborough superior to Southampton!

Instead of taking the variability about the mean, we might equally well have taken it about the mode, the only difference being that we should now have to calculate $\sqrt{p+2}/\gamma$ instead of $\sqrt{p+1}/\gamma$; see p. 433, Equations (i.) and (ii.). The comparatively large values of p, however, do not allow of any widely divergent differences in the results. The more interesting problem of the variabilities in excess and defect, which, owing to the skewness of barometric frequency curves, are not the same, will be dealt with in the next section.

9. *On the Skewness of Barometric Frequency.*

The comparative closeness of the mean to the mode enables us to easily find a formula for the probability that the barometric height in any locality shall be in excess or defect of the modal height. Using the property of the modal third we have to integrate

$$y = y_0\left(1+\frac{\gamma x}{p}\right)^p e^{-\gamma x} = y_0\, e^{-\frac{1}{2}p\left(\frac{\gamma x}{p}\right)^2 + \frac{1}{3}p\left(\frac{\gamma x}{p}\right)^3 -}$$

from 0 to $\dfrac{2}{3\gamma}$.

Hence the area as far as terms of the order $1/p^2$

$$= \frac{2}{3}\frac{y_0}{\gamma}\left(1 - \frac{2}{27}\frac{1}{p} + \frac{4}{135}\frac{1}{p^2} + \ldots\right).$$

Thus the total area on the mean side of the mode

$$= 0\cdot5\mathrm{N} + \frac{2}{3}\frac{y_0}{\gamma}\left(1 - \frac{2}{27}\frac{1}{p} + \frac{4}{135}\frac{1}{p^2}\right), \text{ nearly,}$$

$$= \mathrm{N}\left\{0\cdot5 + \frac{2}{3}\frac{p^p}{e^p\Gamma(p+1)}\left(1 - \frac{2}{27}\frac{1}{p} + \frac{4}{135}\frac{1}{p^2}\right)\right\},$$

[* It has been shown for this skew curve (PEARSON and FILON, 'Roy. Soc. Proc.,' vol. 62, p. 175) that the mean is *negatively* correlated with the standard deviation. Thus we have a theoretical indication that high pressure is correlated with small variability. The actual correlation for the mean value of p is ·25, approximately, this being for a random variation from the standard curve.]

$$= \mathrm{N}\left\{0\cdot5 + \frac{2}{3}\left(\frac{1}{2\pi p}\right)^{\frac{1}{2}}\left(1 + \frac{1}{12p} + \frac{1}{288p^2}\right)^{-1}\left(1 - \frac{2}{27}\frac{1}{p} + \frac{4}{135}\frac{1}{p^2}\right)\right\},$$

$$= \mathrm{N}\left\{0\cdot5 + \frac{2}{3}\frac{1}{\sqrt{2\pi}}\frac{1}{\sqrt{p+1}}\left(1 + \frac{37}{108\,(p+1)} + \frac{2309}{12960}\frac{1}{(p+1)^2}\right)\right\},$$

$$= \mathrm{N}\{0\cdot5 + \cdot26596\,\mathrm{S}_k\,(1 + \cdot3426\,\mathrm{S}_k^2 + 0\cdot1782\,\mathrm{S}_k^4)\},$$

where S_k is the skewness recorded in Table IV.

Since the skewness is in our case small, it is sufficiently close for barometry to take the chance of a reading of the barometer being less than the mode, as:

$$0\cdot5 + 0\cdot266\,\mathrm{S}_k,$$

and greater than the mode, as

$$0\cdot5 - 0\cdot266\,\mathrm{S}_k.$$

The corresponding probabilities for the barometric height less and greater than the mean will be found to be:

$0\cdot5 - 0\cdot133\,\mathrm{S}_k$ and $0\cdot5 + 0\cdot133\,\mathrm{S}_k$ respectively.

The following is the table of stations arranged according to their respective chances of the barometer exceeding its modal and mean heights :—

TABLE VI.

Station.	Skewness.	Chance of Height being in excess of	
		Mode.	Mean.
St. Leonards	0·1334	0·465	0·518
Llandudno	0·1481	0·463	0·519
Stonyhurst	0·1612	0·457	0·521
Geldeston	0·1707	0·455	0·523
Southampton	0·1751	0·453	0·523
Carmarthen	0·1773	0·453	0·524
Churchstoke	0·1773	0·453	0·524
Babbacombe	0·1810	0·452	0·524
Londonderry	0·1816	0·452	0·524
Scarborough	0·1833	0·451	0·524
Hillington	0·1867	0·450	0 525
Scaleby	0·1944	0·448	0·526
Parsonstown	0 1949	0·448	0·526
Markree Castle	0·2024	0·446	0·527
Dunrobin Castle	0·2044	0·446	0·527
Dundee	0·2115	0·444	0·528
Glasgow	0·2130	0·443	0.528
Armagh	0·2247	0·440	0·530
Margate	0·2322	0·438	0·531
Laudale	0·2344	0·438	0·531

This list would present in general the same features, as we have already noted—of a continuous change, distributed in contour lines running a little north-east to south-west of the parallels of latitude—were it not for the anomalous positions of Llandudno, Stonyhurst, and Margate. These stations appear to us, especially the last, to have anomalies in the values of their constants, which can hardly be entirely due to local peculiarities in climate.

It will be observed that the mode is here again more suitable than the mean as a method of recording high barometer. It might, if the point were only superficially considered, be deemed a climatological advantage to have the frequency of the barometer above its mean value as great as possible. But this is not really so, for the simple reason that climates which have an extreme range of low barometer have a low mean, and, other things being equal, places with low mean have most frequency above the mean. On the other hand, places with high means, as a rule, give the greatest frequency above the mode. Of course, this relationship is not invariable; it follows from the mode being more steady than the mean. Thus, there is a greater chance of the barometer standing above the mode in St. Leonards than Laudale, but a less chance of its standing above the mean.

The inequality of the frequencies above and below the mode is not the only point of interest connected with the skewness of the barometric frequency-curves. We have already noticed that the standard deviation about the mean is a good measure of the local variability of the barometer, and may well be used to replace the maximum to minimum range.* But a further question arises owing to the skewness, namely, what is the variability above, and what is the variability below, the modal height? These two will not be equal, and an appreciation of their value is of considerable importance.

Some idea of the range above the mode may be obtained by considering the columns marked H_p, H_a and $H_p - M_o$ in Table IV., which give the theoretical maximum height, the observed maximum height and the total theoretical range above the mode. The disadvantage of using H_a has already been referred to; it may make the range above the mode depend on a single observation in the last year of the whole series. H_p is calculated from the whole sweep of observations, and hence, although it may never be reached in actuality, it is a far better measure of range.

The order of the stations is given in the following diagram:—

* An arbitrary multiple of the standard deviation, σ, may be conveniently taken as the range, if required. Thus 6σ practically covers the whole range of barometric-frequency at any station.

Range above the mode according to

$H_p - M_o$	H_p	H_a
Llandudno	St. Leonards	Babbacombe
St. Leonards	Llandudno	St. Leonards
Stonyhurst	Stonyhurst	Southampton
Londonderry	Londonderry	Churchstoke
Churchstoke	Churchstoke	Carmarthen
Geldeston	Carmarthen	Parsonstown
Scarborough	Geldeston	Hillington
Carmarthen	Scarborough	Llandudno
Markree Castle	Southampton	Geldeston
Scaleby	Markree Castle	Scaleby
Southampton	Scaleby	Markree Castle
Hillington	Babbacombe	Londonderry
Parsonstown	Hillington	Stonyhurst
Dunrobin	Parsonstown	Scarborough
Babbacombe	Dunrobin	Dundee
Dundee	Dundee	Laudale
Glasgow	Glasgow	Margate
Armagh	Armagh	Armagh
Laudale	Laudale	Dunrobin
Margate	Margate	Glasgow

An examination of these lists seems to show that, even allowing for some disturbances in the Stonyhurst, Llandudno, and Margate returns, there is no geographical fact closely represented by range above the mode thus measured. There are but few and small changes introduced into the order of stations, whether we consider $H_p - M_o$ or H_p only. But H_a differs so widely from H_p that we must consider one or other of them as of little value in the determination of the nature of the frequency above the mode. In general the range above the mode dealt with in this manner appears to be more closely correlated with local conditions than with geographical position.

It would undoubtedly be most satisfactory in order to appreciate the skewness of the range of frequency to calculate the values of the standard deviation from the mode for the two portions of the frequency curve, which fall respectively above and below the mean. The extremely slow convergence, however, of the series which express incomplete Γ-functions, renders this calculation extremely tedious, and such mechanical methods as AMSLER's Integrator do not, in our experience, give very good results,* when the curves for which the second moment is to be found have, as is the case with nearly all frequency-curves, considerable "tails."

Fortunately, Mr. DE FORREST, in a paper published in the 'Analyst' (vol. 10, p. 69; Iowa, 1883), has found for a series of values of p the probable deviations in excess and defect of the mode, *i.e.*, the values of x on either side of the mode for which the corresponding verticals, y, cut off the half areas. Unfortunately, although he has interpolated for a considerable number of values of p, his values of the probable errors are only calculated for the small series $p = 4, 5, 7, 10, 20, 50$, and 200, doubtless on account of the great amount of arithmetic involved. Accordingly his table is only as strong as these seven values, which are as follows:—

DE FORREST's *Table of Probable Deviations.*

e_1 = probable deviation in excess of mode, *i.e.*, along negative x.
e_2 = ,, ,, defect ,, ,, positive ,,

p.	$-\gamma e_1$.	γe_2.
4	0·822	1·613
5	0·955	1·788
7	1·289	2·085
10	1·654	2·450
20	2·561	3·359
50	4·334	5·133
200	9·121	9·920

The completion of the Table would undoubtedly be a useful, if laborious, piece of work.

We have calculated the values of e_1 and e_2 for the twenty stations by interpolation

[* An AMSLER's Integrator, especially constructed for me, to determine μ_1, μ_2, and μ_3 is fairly satisfactory for "oval" sections; it gives a passable value of μ_1, but is not accurate enough to give working values of μ_2 and μ_3 for "tailed" areas.—K.P.]

from this Table. The results will, of course, only be correct to a corresponding degree of accuracy. The positive sign is given to e_1.

TABLE VII.—Skewness in Variability and Range. Unit, one inch.

Station.	e_1.	e_2.	$e_1 + e_2$.	σ.
St. Leonards	0·191	0·225	0·416	0·318
Southampton	0·187	0·233	0·420	0·329
Babbacombe	0·187	0·234	0·421	0·330
Carmarthen	0·197	0·246	0·443	0·347
Churchstoke	0·202	0·252	0·454	0·356
Llandudno	0·213	0·256	0·469	0·359
Parsonstown	0·203	0·260	0·463	0·363
Markree Castle	0·219	0·283	0·502	0·392
Armagh	0·204	0·270	0·474	0·370
Londonderry	0·217	0·273	0·490	0·384
Stonyhurst	0·207	0·248	0·455	0·350
Scaleby	0·210	0·268	0·478	0·373
Glasgow	0·214	0·279	0·493	0·379
Laudale	0·210	0·282	0·492	0·388
Dunrobin Castle	0·212	0·274	0·486	0·379
Dundee	0·214	0·279	0·493	0·384
Scarborough	0·203	0·256	0·459	0·360
Hillington	0·193	0·244	0·437	0·343
Geldeston	0·191	0·237	0·428	0·334
Margate	0·175	0·235	0·410	0·332
British Isles	0·200	0·253	0·453	0·356

It will be found that the following empirical formulæ give the values of e_1 and e_2 with an accuracy quite as great as that of their determination by interpolation from DE FORREST'S table:—

$$e_1 = \sigma\,(0·6520 - 0·4728\ \mathrm{S}_k),$$
$$e_2 = \sigma\,(0·6488 + 0·3343\ \mathrm{S}_k).$$

These formulæ must, of course, only be applied with caution beyond the British Isles, and still less to other problems in skew frequency, when the skewness does not fall within the range of barometric skewness considered in this paper. Within this range, however, they give remarkably good results.

The above formulæ, or the table, show at once that e_1 and e_2 are quantities which follow the system of generalised isobars, and thus $e_1 + e_2$ and e_1, e_2, are good measures of the range and its skewness. It is, accordingly, these quantities which ought to be calculated for the purpose of obtaining an appreciation of the range above and below the mode at any station.

For example:—At St. Leonards half the frequency of the barometer falls into a range of ·416″, namely, from 29″·801 to 30″·217, *i.e.*, the mode being 30″·026, from 30″·026 $- e_2$ to 30″·026 $+ e_1$. Further, the relative scattering of the frequency above and below the mode is given by the ratio of $e_1 : e_2$ or 0·191 : 0·225. At Markree Castle, on the other hand, it requires more than half-an-inch to cover half the frequency, *i.e.*, from 29″·6825 to 20″·1845, and the ratio of 0·219 : 0·283 gives the relative ranges of half the frequencies above and below the mode.

The comparatively small amount of labour necessary to determine *Sk* and σ,—

in fact, *Sk* may, by the rule of the modal third be determined from the median and the mean—and, thence, e_1 and e_2 by the above formulæ, make this a very convenient, as well as scientifically accurate, method of appreciating the range and the skewness of the variability at any station.

Our discussion has now led us to the following general conclusions :—

(1.) The mode, the standard deviation, and the skewness fully define barometric frequency. These three constants depend, in the first place, on geographical position, and appear to be constant along certain lines—the generalised isobars.

(2.) A knowledge of these three constants enables us, by means of very simple formulæ, to describe the chief physical features of the barometric frequency at any station.

(3.) By aid of the twenty stations dealt with in this paper, a fair appreciation can be obtained of the barometric frequency at any place whatever in the British Isles by means of interpolation.

For example :—A line from Hillington on the Wash to a point midway between St. Leonards and Southampton strikes the south coast between Littlehampton and Worthing, cuts the generalised isobars so that they make approximately equal angles with it, and passes very nearly at one-third and two-thirds distances through Cambridge and London. Thus, if the St. Leonards constants were based on a longer period and so somewhat more satisfactory,* we might fairly accurately predict the constants of the Cambridge and London frequencies from those of Hillington, Southampton, and St. Leonards. We find, as a matter of fact, by interpolation :

	Mean height.	Standard deviation.	Skewness.
	"	"	
London	29·969	0·330	0·165
Cambridge	29·956	0·336	0·176

Actual calculation for thirteen years at these stations corresponding to the years used for Southampton and Hillington gives :—

	Mean height.	Standard deviation.	Skewness.
	"	"	
London	29·966	0·329	0·157
Cambridge	29·952	0·338	0·172

* An attempt was made to replace the St. Leonards returns by the same thirteen years as have been dealt with for the other stations recorded at a suitable "telegraph station." Unfortunately, during these thirteen years the most suitable station, namely, that at Dover, was changed to Dungeness—a station some considerable distance off and subject to probably somewhat different conditions. A short missing period between the two sets of observations was most kindly supplied by Mr. R. H. SCOTT, by interpolation from the Meterological Office Records. The result of the calculations showed a considerable increase of variation, probably due to the combination of two stations, and it was very doubtful whether the result was of greater weight than the six years returns from St. Leonards.

These values are not widely divergent from the previous interpolated values, and would for many climatological purposes suffice to describe the barometric frequency; indeed, graphically it is hardly possible to show the difference between the frequencies curves corresponding to these two sets of constants on the scale of our diagrams. The chief difference is in the skewness of the London distribution, but as we have seen in the case of Llandudno, Stonyhurst and Margate, it is the skewness which is most influenced by local conditions. The constants and the observed and theoretical frequencies for the three telegraph stations London, Cambridge and Dover-Dungeness, are given in the form of supplements to our tables. They are of very considerable interest, but they have not been included in the general returns based on the selected distribution of twenty stations of the second order, as they would weight too much the eastern side of the British Isles unless an additional series of western stations had also been included.*

II.—On the Correlation of the Heights of the Barometer at Different Stations.

10. So far as we are aware, no tabulations have hitherto been made of the barometric heights at pairs of stations, and yet the degree of correlation between stations in different situations is one of extreme interest and importance. We have shown that the constants of barometric frequency vary continuously and gradually from one end to another of the British Isles. We should accordingly expect a close degree of correlation between the heights at different stations. This degree will probably be found to vary with the distance at a different rate along and perpendicular to the generalised isobars. It may also be greater when a certain interval is allowed between the observations at the two stations.† Our present object, however, being only to illustrate the general treatment of barometric correlation, we have dealt only with three pairs of stations and with contemporaneous observations.

The stations are the following:—

(1.) Babbacombe and Churchstoke for the eight years 1878 to 1885.

(2.) Southampton and Laudale for the eight years 1880 to 1887.

(3.) Hillington and Churchstoke for the eight years 1878 to 1885.

The results are exhibited in the following three tables:—

* We have to cordially thank Mr. R. H. Scott for allowing us to copy the manuscript records of the Meteorological Office for these three telegraph stations. We believe that the discussion of the frequency of twenty contemporaneous years of the whole system of telegraph stations would give most interesting results, but the labour of copying and reducing only thirteen years for but three stations has convinced us that it could hardly be undertaken by private individuals largely occupied with other work.

† A most interesting investigation would be the degree of correlation between suitable North-American and British stations, when the interval between the observations was varied from one up to six or seven days. Similar investigations for British and Continental stations might easily give important results bearing directly on the prediction of barometric changes.

TABLE VIII.

Babbacombe.

Churchstoke. 2922 days	31·0	30·9	30·8	30·7	30·6	30·5	30·4	30·3	30·2	30·1	30·0	29·9	29·8	29·7	29·6	29·5	29·4	29·3	29·2	29·1	29·0	28·9	28·8	28·7	28·6
31·0	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·9	..	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·8	..	2	3	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·7	..	..	2	17	17	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·6	..	..	..	7	33·5	25·5	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·5	..	..	..	..	17	53·5	27·5	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·4	..	..	..	..	2·5	25	61	54	10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·3	..	..	..	..	..	6	27·5	112	62	6	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·2	..	..	..	..	..	..	5	55·5	126·5	83	9	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·1	..	..	..	..	..	..	..	11	86·5	161	78	14·5	2	..	..	..	..	..	..	..	..	..	..	..	..
30·0	..	..	..	..	..	..	..	..	13·5	92	157	58	7	2	..	..	..	..	..	..	..	..	..	..	..
29·9	..	..	..	..	..	..	..	..	..	26	101·5	113	54·5	11·5	1	..	..	..	..	..	..	..	..	..	..
29·8	..	..	..	..	..	..	..	..	..	3	23·5	112·5	105·5	22	5	1	..	..	..	1	..	..	..	..	..
29·7	..	..	..	..	..	..	..	..	..	..	3·5	27·5	100·5	79·5	31	9	2	..	..	..	..	..	..	..	..
29·6	..	..	..	..	..	..	..	..	..	..	1	1	33	70	52	23	1·5	..	..	..	..	..	..	..	..
29·5	..	..	..	..	..	..	..	..	..	..	..	..	7	26·5	41	46	15	2	..	..	..	..	..	..	..
29·4	..	..	..	..	..	..	..	..	..	..	..	..	..	3	19·5	33·5	21	8	2	1	..	..	1	..	..
29·3	..	..	..	..	..	..	..	..	..	..	..	1	..	..	1	14·5	21·5	11	5·5	1	..	..	..	..	..
29·2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2·5	8	12·5	7·5	2	0·5	..	..	..	..
29·1	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	1	3	8·5	6	4	0·5	..	..	..	..
29·0	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	..	..	..	2·5	3·5	3	1·5	..	..	..	..
28·9	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	..	..	..	1	..	2·5	2	0·5	1	..	..
28·8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·5	3	..	..
28·7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	0·5
28·6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	0·5

TABLE IX.

Southampton.

2922 days / *Laudale.*	31·0	30·9	30·8	30·7	30·6	30·5	30·4	30·3	30·2	30·1	30·0	29·9	29·8	29·7	29·6	29·5	29·4	29·3	29·2	29·1	29·0	28·9	28·8	28·7	28·6	28·5	28·4	28·3	28·2	28·1	28·0	27·9
31·0	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·8	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·7	..	1	..	4·5	4	3	1·5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·6	..	1	3	7·75	9·75	8·5	3·5	2·25	0·25	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·5	..	1	0·5	6·25	9·25	17	15·75	6·5	5·5	1·75	0·5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·4	..	..	0·5	5·75	11·75	22	24·75	39·75	19·75	12·25	4·5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·3	..	..	..	5·25	11·5	15·5	24·75	55	50	25·5	6	5	1·25	0·25	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·2	..	..	..	1	4·25	21·75	24	38·5	55·5	66	32	12·5	4·75	2·75	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·1	..	..	..	..	2	10·75	21·75	23·75	55·75	55·5	46·75	26	15	3·25	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·0	..	..	..	..	..	5	11	26·25	48·25	63	58	27·5	24·5	10	1·5	1·5	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
29·9	..	..	..	..	..	4	4	19	31·75	54·25	63·25	57	27·5	11·5	8·75	1	0·5	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
29·8	..	..	..	..	..	..	7	13	24·25	41·75	59·75	48	36·75	18	17·5	6·5	3·5	0·5	..	1	..	..	..	..	..	..	..	..	..	..	..	..
29·7	..	..	..	..	..	..	2	8	12	36·5	41·25	47	42·25	19·5	16	7·5	5	5	2·5	0·5	..	..	..	..	..	..	..	..	..	..	..	..
29·6	..	..	..	..	..	..	0·5	4	8·5	16	26·5	47	32·75	25·5	23·75	12·5	8	3	1	3	..	..	..	..	..	..	..	..	..	..	..	..
29·5	..	..	..	..	..	..	..	1	3	13	18	31	45	39·5	20	10·5	8	2·5	..	0·5	..	..	..	..	..	..	..	..	..	..	..	..
29·4	..	..	..	..	..	..	..	..	0·5	5	13·5	17·25	24·5	23·75	18	13·5	7·25	5·25	3	0·5	1	1	1	..	..	..	..	..	..	..	..	..
29·3	..	..	..	..	..	..	..	..	..	2	6	13·25	17·25	18·5	11·5	10·5	7·75	5·75	1	2	1	1	..	..	..	..	..	..	..	..	..	..
29·2	..	..	..	..	..	..	..	..	..	2	5·25	5·25	6	14	13	8	7	3	2	..	1	..	..	..	1	..	..	..	..	..	..	..
29·1	..	..	..	..	..	..	..	..	..	1	0·25	2·75	5·5	10·5	10	12·5	6	9·5	3	..	1	1	..	..	..	..	..	..	..	..	..	..
29·0	..	..	..	..	..	..	..	..	..	..	1	..	4	3	5·75	3·75	6·5	6	4·5	2·5	1·5	..	..	..	..	..	..	..	..	..	..	..
28·9	..	..	..	..	..	..	..	..	..	..	..	..	..	1	2·75	5·75	2·5	4·5	4	..	2	1	1	..	..	..	..	..	..	..	..	..
28·8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	2·5	2	1·5	0·5	0·5	..	3	..	..	..	..	..	..	..	..	..	..
28·7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	1·5	..	1·5	1	3	..	..	..	..	..	..	..	..	..	..	..	..
28·6	..	..	..	..	..	..	..	..	..	..	..	..	1	..	0·5	1	..	1	..	1	..	..	..	..	..	..	..	..	..	..	..	..
28·5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
28·4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	..	..	..	1	..	..	1	..	..	..	..	..	..
28·3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	..	..	..	..	..	..	..	..	..	..	..	..	..
28·2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
28·1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
28·0	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	..
27·9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..

TABLE X.

Hillington.

2922 days	31·0	30·9	30·8	30·7	30·6	30·5	30·4	30·3	30·2	30·1	30·0	29·9	29·8	29·7	29·6	29·5	29·4	29·3	29·2	29·1	29·0	28·9	28·8	28·7	28·6
31·0	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·9	..	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·8	..	..	4	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·7	..	..	1	12	18·5	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·6	..	..	..	5·5	32·75	25·75	3·5	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·5	..	..	..	..	3·25	39·75	45	8·5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·4	..	..	..	..	..	9.5	72·25	62·75	8·5	0·5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·3	..	..	..	..	..	..	20·75	107·75	73·75	11·75	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..
30·2	..	..	..	..	..	..	1	17·25	142·75	99·25	13·25	1·5	..	..	..	..	..	..	..	..	..	..	..	..	..
30·1	..	..	..	..	..	..	..	2·75	49	172·5	108·75	18·5	2	..	..	..	..	..	..	..	..	..	..	..	..
30·0	..	..	..	..	..	..	..	1	3	50·5	164·75	88·25	17	3	..	..	..	..	..	..	..	..	..	..	..
29·9	..	..	..	..	..	..	..	..	0·5	6·5	54·75	130·5	88·25	22·5	3	..	..	..	..	..	..	..	..	..	..
29·8	..	..	..	..	..	..	..	..	..	..	16·5	64	113	69·5	9·75	2·25	1	..	..	..	..	..	..	..	..
29·7	..	..	..	..	..	..	..	..	..	..	1	14·25	66·75	98·25	58	11·75	2·5	..	..	..	..	..	..	..	..
29·6	..	..	..	..	..	..	..	..	..	..	..	1	13·5	43·25	69·5	43·75	11·5	1	1	..	..	..	..	..	..
29·5	..	..	..	..	..	..	..	..	..	..	..	..	1	11·75	39·5	52·25	20	6	4·5	..	..	..	..	..	..
29·4	..	..	..	..	..	..	..	..	..	..	..	..	..	1·25	10·25	29·5	30	15	6·5	2	..	..	..	..	..
29·3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2·5	8·5	14·5	16	9	2·5	..	..	..	..	..
29·2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	0·5	3·5	9·5	7·5	11	2	0·5	0·5	..	..
29·1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2	1·5	10	5·5	4	1	..	..	..
29·0	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·5	2	4	1·5	1	1	..
28·9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·5	0·5	3·5	..	2	..	..
28·8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	2	1	1	..	..
28·7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·5	..	..	..
28·6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	0·5	1	..	..

Churchstoke.

Now the distribution of frequency at each station being skew, the correlation surfaces as based upon these tables will also be skew, and the curves giving the mean height at one station for a given height at the other are no longer straight lines. We do not propose on the present occasion to discuss at length the properties of this type of skew correlation, but to refer the reader to a paper by Mr. G. U. YULE, published in the 'Roy. Soc. Proceedings,' vol. 60, pp. 477 *et seq.*, 1897. In that paper it is shown that the coefficient of regression is still significant in the case of skew correlation, it gives the slope of the line of closest fit to the curve of regression, or the locus of the mean heights of one station for successive heights at the other. Since the locus is not very far removed from a straight line in any of the cases dealt with, it follows that the line of closest fit will very approximately represent it. Calculating the coefficients of correlation and the regressions for the three pairs of stations by the usual formulæ (see "Mathematical Contributions to the Theory of Evolution, III.," 'Phil. Trans.,' A, vol. 189, pp. 265–6, 275–7), we have the following results:—

TABLE XI.—Barometric Correlation.

Pairs of stations.	Coefficient of Correlation.	Coefficient of Regression.	Probable deviation of array.
Babbacombe . . . Churchstoke . . .	0·9824	0·8901 1·0818	0″·0401 0·0441
Southampton . . . Laudale	0·7572	0·6260 0·9159	0″·1449 0·1752
Hillington . . . Churchstoke . . .	0·9576	0·9267 0·9895	0″·0663 0·0685

The application of this table to predict the height of the barometer at one station from a knowledge of the contemporaneous height at a second will be clear to readers familiar with the mathematical theory of correlation. For example, if the height of the barometer at Babbacombe be x'' above the mean Babbacombe height, then the height to be predicted at Churchstoke is $1·0818\ x''$ above the Churchstoke mean, with a probable error of $0''·0441$. We may give a numerical illustration. The barometer at Churchstoke stands at $30''·0176$; what are its probable heights at Babbacombe and Hillington?

The mean height at Churchstoke $= 29''·9545$ (see Table IV.); hence the observed height at Churchstoke is $0''·0631$ above the mean. The most probable heights at Babbacombe and Hillington will accordingly be $0·8901 \times 0''·0631$ and $0·9267 \times 0''·0631$, or $0''·0562$ and $0''·0585$ above the respective means of those places. Extracting these means from Table IV., we find: $30''·0349$ and $30''·0014$ for the probable heights at Babbacombe and Hillington. The probable deviations in the two cases are 0″·0401 and 0″·0663. Thus, the prediction would probably be correct to within $\frac{1}{20}$ of an inch.

We should not propose, however, to base the prediction of the barometric height at one station on its correlation with the height at a single other station. We should think it desirable to apply the principles of multiple correlation, and endeavour by a suitable selection of stations to decrease the probable deviation of the array at the given station which corresponds to observed heights at the selected stations. We shall cite here the general formulæ for the prediction of the height of the barometer from a knowledge of its heights at correlated stations.

Let x_m be the probable height of the barometer at the m^{th} station above its mean value for that station. Let $r_{mm'}$ be the coefficient of correlation between the m^{th} and m'^{th} stations, where in finding $r_{mm'}$ the correlated heights may be taken at different times, or if it seems desirable on different days. Let $h_{m'}$ be the observed height at the m'^{th} station and $\sigma_{m'}$ the standard deviation, both measured from the mean of that station.

11. Case (i.).—*Prediction from one Correlated Station only.*

$$x_1 = r_{12}\frac{\sigma_1}{\sigma_2}h_2, \text{ with a probable deviation of } 0·6745\sigma_1\sqrt{1 - r^2_{12}}.$$

It is clear that unless r_{12} be very nearly unity, *i.e.*, the stations very close, the predicted height will be subject to a large probable deviation. For very close stations, such as London, Cambridge, Dover-Dungeness, where a rough investigation leads me to the conclusion that the correlation is as high as 0·998, we have the probable deviation about $0·04487 \times \sigma_1$, or about 0·015″. In such cases the above formula will give fairly closely the height to be predicted at the first station. It may be used for purposes of interpolation.

The approximate linearity of the "regression" leads us to an interesting property of barometric correlation. There is a certain height of the barometer which, if it occurs at one station, will itself be the most probable height at the correlated station. This may be termed the *balance height.* This height may be easily found from the equation :

$$m_1 + x_1 = m_2 + h_2,$$

whence, if

$$\rho_{12} = r_{12}\sigma_1/\sigma_2, \qquad h_2 = (m_1 - m_2)/(1 - \rho_{12}),$$

and the balance height

$$= (m_1 - \rho_{12}m_2)/(1 - \rho_{12}).$$

Above and below the balance height the relative heights of the two stations are reversed. If above the balance height the first station has a probable height of its barometer invariably higher than the observed height at the second station, then below the balance height the probable height at the first station will be invariably lower than the observed height at the second station, and *vice versâ.*

The following are the balance heights of the three pairs of stations dealt with in Table XI. :—

Babbacombe . Churchstoke .	30″·2745	Above this value Churchstoke, below Babbacombe, has usually the higher barometer.
Southampton . Laudale . . .	28″·5022	Above this value Southampton, below Laudale, has most probably the higher barometer.
Hillington . . Churchstoke .	31″·0477	Above this value Churchstoke, below Hillington, has most probably the higher barometer.

In the case of close and highly correlated stations, the balance value, since the probable deviation is small, may be roughly found from a mere inspection of the barometer records for the two stations. Thus, we expect, it is about 30″·5 for London and Cambridge. Perhaps a better approximation might be obtained by estimating,

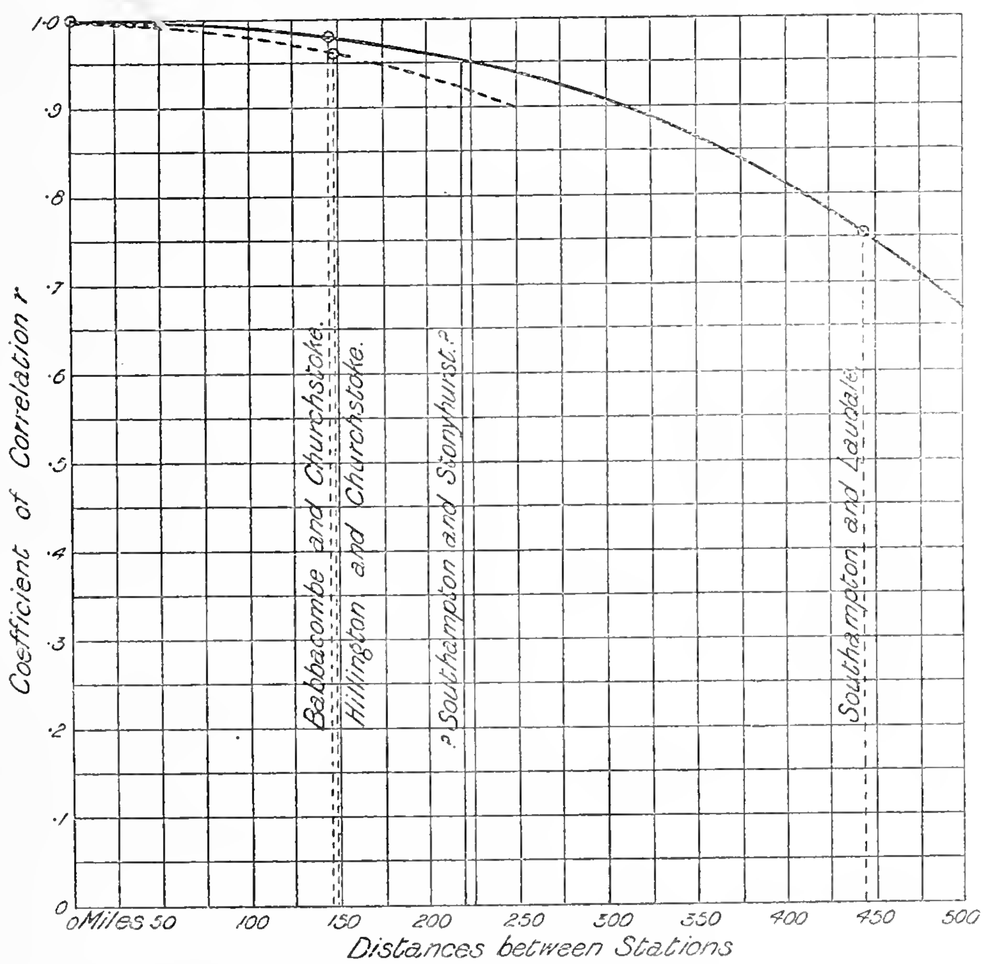


Diagram illustrating application of Theory of Correlation to predict Barometric Heights.
——— Stations approximately orthogonal to isobars.
- - - - - - - - - Stations approximately along isobars.

from their known distance, the coefficient of correlation for the two stations by means of the diagram above, and then calculating ρ_{12}, since σ_1 and σ_2 are given in Table IV., or can be found by interpolation from that table.

The diagram indicates graphically, what is fairly clear from Table XI., that correlation is not a single valued function of the distance between the two stations. It suggests—we wish especially to note that it does not prove—that correlation differs in its variation with the distance according as the two stations have their distance practically along or orthogonal to the generalised isobars. Unfortunately, without crossing the Irish Channel, it was impossible to find two stations along a generalised isobar so far apart as Southampton and Laudale. There can be little doubt, we think, that such stations would, however, give a sensibly less correlation than the north and south stations. Although the diagram (owing to the very considerable labour of calculating the correlation for a pair of stations even for eight years) is based upon a very inadequate number of measurements, yet we should expect, with continuity in the correlation coefficient, which can hardly fail to be the case, that it would give fairly approximate values. For example, we should anticipate that the correlation between Southampton and Stonyhurst would be about 0·94 to 0·95. Since Stonyhurst is, roughly, about equally distant from Southampton and Laudale, the correlation between Stonyhurst and Laudale may with somewhat smaller probability be also put at 0·94 to 0·95. Clearly the general relationship between correlation, distance, and direction of distance will only be determined when a very great number of pairs of stations have been worked out, and these stations ought to be distributed, not over a small area like the British Isles, but over a large continental area.

12. Case (ii.).—*Prediction from two Correlated Stations.*

Here :

$$x_1 = \frac{r_{12} - r_{23}r_{13}}{1 - r_{23}^2}\frac{\sigma_1}{\sigma_2}h_2 + \frac{r_{13} - r_{23}r_{12}}{1 - r_{23}^2}\frac{\sigma_1}{\sigma_3}h_3$$

with a probable deviation

$$p_1 = 0·6745\sigma_1\sqrt{\frac{1 - r_{23}^2 - r_{13}^2 - r_{12}^2 + 2r_{23}r_{13}r_{12}}{1 - r_{23}^2}}.$$

Hence if we wish to predict the height of the barometer as closely as possible at one station from either an earlier or contemporary observation of the heights at two other stations, we ought to choose out intervals of time and the distribution of the three stations, so that p_1 may be as small as possible. It would thus seem, considering the great variety of times and places available, within our power to predict almost exactly the height at any selected station from a knowledge of the heights at two other selected stations at selected intervals of time. The importance of testing this principle seems to us very great ; it might lead to quite novel methods of predicting barometric change. Unfortunately the needful knowledge of the correlation coefficients of widely separated pairs of stations for divers intervals of observation is still wholly wanting. The following example is merely illustrative. Suppose the second and third stations, so selected that they have equal correlation $r = r_{12} = r_{13}$ with the first, and a correlation $\rho = r_{23}$ with each other. Then p_1 would be absolutely zero if

$$\frac{1 - \rho^2 - 2r^2 + 2\rho r^2}{1 - \rho^2} = 0,$$

or if

$$1 = \frac{2r^2}{1+\rho},$$

that is, if

$$r = \sqrt{\tfrac{1}{2}(1+\rho)}.$$

For example, if the correlation between the second and third stations be 0·7572,

$$r = \sqrt{0 \cdot 8786} = 0 \cdot 9373.$$

In other words, if three stations could be found with coefficients $r_{12} = 0 \cdot 9373 = r_{13}$ and $r_{23} = 0 \cdot 7572$; then the barometric height at the first would be exactly a linear function of the contemporaneous heights at the other two stations. There seems no reason why stations should not be found with these correlations, or similarly related correlations. The correlation between Southampton and Laudale is 0·7572; the correlation between Stonyhurst and Laudale and between Stonyhurst and Southampton must be about 0·94 to 0·95. We increase the distance and make it less perpendicular to the generalised isobars by moving across towards the East coast. Probably somewhere near Whitby the required correlation of 0·9372 would be reached, and at such a station we should expect the barometric height to be very nearly a linear function of the heights at Southampton and Laudale.*

Supposing the relation $r = \sqrt{\frac{1}{2}(1+\rho)}$ to hold, then it is easy to deduce from the expression for x_1 above, that if H_1, H_2 and H_3 be the absolute heights of the barometer at the three stations,

$$H_1 = m_1 - \frac{\sigma_1}{2r}\left(\frac{m_2}{\sigma_2} + \frac{m_3}{\sigma_3}\right) + \frac{\sigma_1}{2r}\frac{H_2}{\sigma_2} + \frac{\sigma_1}{2r}\frac{H_3}{\sigma_2}.$$

This passage of correlation into causal relationship† is of such extreme importance, that it is worth while to see what approach we can find to it, even within the somewhat narrow limits of the British Isles. We have no details available for the barometer

* Let the reader imagine the heights at Southampton and Laudale replaced by the heights at two (or if necessary more) stations on the American continent, say, along the East coast, and the height at the Whitby station replaced by the height, after an interval of time, at a British station, say Valencia, and then he will grasp the sort of possibility—not the proven feasibility—of prediction, which the authors wish to lay stress upon.

† [This expression is used advisedly to draw attention to the importance of the limit when a correlation passes into a causal relationship. If the unit A be always preceded, accompanied or followed by B. and without A, B does not take place, then we are accustomed to speak of a *causal relationship* between A and B. On the other hand, if when A occurs amounts, $b_1, b_2, b_3 \ldots b_n$ of B are found with frequencies $p_1, p_2, p_3 \ldots p_n$ per cent. of the occurrences of A, we speak of a correlation between A and B. Now, if we approach the limiting case of $p_1 = p_2 = p_3 = \ldots = p_{r-1} = p_{r+1} = \ldots = p_n = 0$, and $p_r = 100$ per cent., it is clear that more and more nearly b_r of B will occur whenever A occurs, *i.e.*, a fixed amount of B on every occurrence of A. This is the transition of correlation into causal relationship. It is the construction of a q-dimensioned correlation surface, of which a particular $q-1$ dimensioned section approaches indefinitely close to a frequency curve of zero standard deviation.]

at Whitby, and if we had them and worked out the Whitby-Southampton and Whitby-Laudale correlations, it is unlikely that they would both be exactly equal, and equal to 0·9373. In fact the correlation for a number of Yorkshire stations, with both Laudale and Southampton, would have first to be worked out, and then the true station having an exact causal relationship with Laudale and Southampton would require to be found by interpolation.

But we should expect stations not so far removed from the right position to have their barometric height given in terms of those of Laudale and Southampton by an approximately linear relation. Hence, to indicate to the reader that our conclusion —namely, that a barometric correlation may pass into a causal relationship—is not so paradoxical as may appear at first sight, we have endeavoured to test how far the Stonyhurst height is a linear function of the heights at Laudale and Southampton. In order to do this we have neither assumed nor worked out the Laudale-Stonyhurst and the Southampton-Stonyhurst correlations. The former was too risky,* the latter too laborious for the end to be desired. We have simply assumed a linear relationship between the heights at the three stations, *i.e.*,

$$H_{St} = xH_{So} + yH_L + z,$$

where x and y are numerical constants, and z is a number of inches. To determine x, y, and z we chose twelve observations, taking the 15th day of each month for one year, and working by the method of least squares. Unfortunately the resulting equations for x, y, and z, throw back their determination on decimal figures, which are the limit of what is usually tabulated in barometric observations. The resulting equations were

$$30{\cdot}159x + 30{\cdot}020y + z - 30{\cdot}130 = 0,$$
$$30{\cdot}161x + 30{\cdot}022y + z - 30{\cdot}132 = 0,$$
$$30{\cdot}161x + 30{\cdot}024y + z - 30{\cdot}133 = 0.$$

The solution of these equations is

$$x = 0{\cdot}50, \qquad y = 0{\cdot}50, \qquad z = 0{\cdot}04'',$$

where the values of x and y are certainly not correct to the second place of decimals.

The resulting formula gives

$$H_{St} = 0{\cdot}5H_{So} + 0{\cdot}5H_L + 0{\cdot}04''.$$

An attempt to approximate to the coefficients of correlation, gave the Southampton factor a somewhat higher value than the Laudale factor, and this is probably the case. But with the data available we shall hardly do better than the above formula. To test its degree of accuracy 50 values were taken out of the returns for Southampton, Laudale and Stonyhurst at fortnightly intervals, and the observed and calculated values at Stonyhurst are given in Table XII. The differences are distributed fairly evenly, positively and negatively, and their mean value is about 1/40″. We consider

* We have already noted that the Stonyhurst data are not, in our opinion, very satisfactory: see p. 446.

them sufficiently satisfactory to justify the view that a station could be found in Yorkshire for which correlation would pass into a causal relationship.

TABLE XII.—Illustrating Approach from Correlation to Causal Relationship.

Stonyhurst.		Difference.
Observation.	Calculation.	
30·58	30·61	+0·03
30·17	30·14	−0·03
30·03	29·98	−0·05
30·06	30·11	+0·05
29·92	29·94	+0·02
30·35	30·38	+0·03
29·85	29·84	−0·01
30·12	30·11	−0·01
30·13	30·13	0·00
30·19	30·20	+0·01
30·51	30·48	−0·03
30·65	30·62	−0·03
30·17	30·21	+0·04
29·52	29·55	+0·03
30·51	30·50	−0·01
29·91	29·90	−0·01
29·94	29·94	0·00
30·17	30·18	+0·01
30·13	30·10	−0·03
30·34	30·33	−0·01
30·74	30·73	−0·01
30·18	30·25	+0·07
30·16	30·18	+0·02
30·41	30·38	−0·03
30·01	30·00	−0·01
29·15	29·18	+0·03
30·28	30·26	−0·02
29·79	29·85	+0·06
29·93	29·90	−0·03
29·91	29·93	+0·02
30·11	30·11	0·00
29·99	29·92	−0·07
29·43	29·45	+0·02
30·15	30·15	0·00
30·22	30·22	0·00
30·16	30·13	−0·03
30·27	30·29	+0·02
29·56	29·61	+0·05
29·66	29·85	+0·19
30·23	30·25	+0·02
29·02	28·99	−0·03
30·35	30·30	−0·05
29·92	29·92	0·00
29·99	29·95	−0·04
29·95	29·96	+0·01
29·58	29·62	+0·04
30·11	30·11	+0·00
30·18	30·22	+0·06
29·92	29·89	−0·03
29·49	29·46	−0·03

As a rule there will exist for most triplets of stations a certain height, which may be termed the *balance height*, by which we are to understand that if the barometer stand at the balance height at two of the stations, its most probable value at the third correlated station is the balance height also. The balance height is at once found by putting $x_1 = H_b - m_1$, $h_2 = H_b - m_2$, $h_3 = H_b - m_3$ in the formula for regression and finding H_b. If we put $H_{So} = H_L = H_{St}$, in the formula on p. 460, we are led to $H_b = \infty$ for the balance height; this is probably very far from being the true balance height, and for the simple reason that the factors, 0·5 of H_{So} and H_L on p. 460, are only approximate.

If we found a Yorkshire station which had the correlation 0·9373 with both Laudale and Southampton, its balance height would be given by the formula, on p. 459; and supposing it to lie on, or nearly on, the same generalised isobar as Stonyhurst, *i.e.*, to have nearly the same mean and standard-deviation, then its balance height with Laudale and Southampton would be 29″·868. An error of between 0·001 and 0·002 in one of the factors, 0·5 of the Stonyhurst-Laudale-Southampton linear relationship, on p. 460, would thus have reduced the balance height of those stations from ∞ to about 29″·9.

The general expression for the balance height of a station 1, with regard to stations 2 and 3, is given by

$$H_1\,(\text{balance}) = \frac{(r_{12} - r_{23}r_{13})\dfrac{m_2}{\sigma_2} + (r_{13} - r_{23}r_{12})\dfrac{m_3}{\sigma_3} - (1 - r_{23}{}^2)\dfrac{m_1}{\sigma_1}}{(r_{12} - r_{23}r_{13})\dfrac{1}{\sigma_2} + (r_{13} - r_{23}r_{12})\dfrac{1}{\sigma_3} - (1 - r_{23}{}^2)\dfrac{1}{\sigma_1}}.$$

Hence if the three stations lie on the same generalised isobar, since m_1, m_2, m_3 and σ_1, σ_2, σ_3 have very approximately the same value, the balance height will be the mean height, and the same for every station with regard to the other pair.

One further general proposition may be noted before we leave the special case of prediction from two correlated stations. Suppose the barometer constant at one station, then the coefficient of correlation between the heights at the other two is given by*

$$\mathrm{r}_{23} = \frac{r_{23} - r_{13}r_{12}}{\sqrt{1 - r_{13}{}^2}\sqrt{1 - r_{12}{}^2}}, \text{ for a selected height at the first station.}$$

$$\mathrm{r}_{13} = \frac{r_{13} - r_{23}r_{12}}{\sqrt{1 - r_{12}{}^2}\sqrt{1 - r_{23}{}^2}}, \text{ for a selected height at the second station.}$$

$$\mathrm{r}_{12} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{1 - r_{13}{}^2}\sqrt{1 - r_{23}{}^2}}, \text{ for a selected height at the third station.}$$

* These values have been termed by Mr. G. U. YULE *nett* coefficients of correlation, to distinguish them from r_{23}, r_{13}, and r_{12}, which he terms *gross* coefficients. The difference would, perhaps, be best expressed mathematically by the use of such terms as *partial* correlation coefficient and *total* correlation coefficient, the former being the value of the coefficient when one variable is not allowed to vary, and the latter when it is. They are at once obtainable from the general expression on p. 287 of the Memoir, 'Phil. Trans.,' A, vol. 187, by putting, say x = const. and remembering that the coefficient of correlation for y and z in $P = P_0 e^{-\frac{1}{2}(a_2 y^2 - 2a_{23}yz + a_3 z^2)}$ is $a_{23}/\sqrt{a_2 a_3}$.

The values of these expressions are peculiarly interesting, for they lead us to the general theorem that whenever the total correlation between two stations is less than the product of the total correlations at the other two pairs of stations, then the partial correlation between the latter two stations will be negative, *i.e.*, for a given value of the barometer at the first station, a rising barometer at the second station will on the average mark a falling barometer at the third station, and a falling barometer at the second station a rising barometer at the third.

Owing to the generally high values of barometric correlation it is comparatively easy for $r_{13}r_{12}$, for example, to be greater than r_{23}. Thus in the case where two of the stations have equal correlation with a third we have, if $r_{23} = \rho$, $r_{12} = r_{13} = r$,

$$\mathrm{r}_{23} = \frac{\rho - r^2}{1 - r^2}, \quad \mathrm{r}_{13} = \mathrm{r}_{12} = \frac{r}{\sqrt{1 - r^2}} \sqrt{\frac{1 - \rho}{1 + \rho}},$$

and r_{23} will always be negative if r^2 be $> \rho$. For example, we have Laudale and Southampton with a correlation 0·7572, while Stonyhurst and both these stations must have a correlation of about 0·94 to 0·95. It follows, therefore, that the partial correlation of Laudale and Southampton with regard to Stonyhurst is negative, or for a constant value of the barometer at Stonyhurst rising at Laudale would in general denote falling at Southampton, and *vice versâ*. This is best illustrated by noting that x_2 being the mean height at Southampton for heights h_1 and h_3 at Stonyhurst and Laudale (x_2, h_1 and h_3 being measured from the respective mean heights of the stations),

$$x_2 = \frac{r_{23} - r_{13}r_{12}}{1 - r_{13}^2} \frac{\sigma_2}{\sigma_3} h_3 + \frac{r_{12} - r_{23}r_{13}}{1 - r_{13}^2} \frac{\sigma_2}{\sigma_1} h_1,$$

or, in the special case approximately,

$$= \frac{\rho - r^2}{1 - r^2} \frac{\sigma_2}{\sigma_3} h^3 + \frac{r(1 - \rho)}{1 - r^2} \frac{\sigma_2}{\sigma_1} h_1.$$

Hence, if $r_{13}r_{12}$ be $> r_{23}$ or $r^2 > \rho$, an increase of h_3 for a constant h_1 means a decrease of x_2.

In the particular instance of correlation passing into causation referred to by us on p. 459, *i.e.*, when $1 + \rho = 2r^2$ we have

$$\mathrm{r}_{23} = -1, \qquad \mathrm{r}_{13} = \mathrm{r}_{12} = 1.$$

$$x_2 = -\frac{\sigma_2}{\sigma_3} h_3 + 2r \frac{\sigma_2}{\sigma_1} h_1.$$

Thus the partial correlation between the three stations is "perfect," but between the second and third it is negative. The ratio of the *fall* at the second to the *rise* at the third, for stationary barometer at the first, is σ_2/σ_3.

This principle, which at first sight appears rather paradoxical, namely, that at three stations, A, B, C, a rise at A will, on the average, be accompanied by a rise at C, but that a rise at A for a constant barometric height at B may, on the average be accompanied by a fall at C, appears capable, when extended, developed, and illustrated by actual numerical examples, of throwing considerable light on the nature of barometric variation.*

To show the importance and truth of the principle, a table has been formed for the deviations from the means at Southampton and Laudale, when the barometer at Stonyhurst does not differ by more than $\frac{1}{100}$ inch from its mean value, 29″·94.

In the course of two years 31 values were found, *and with only one exception, possibly a misreading, the barometer at Laudale was always above or below the mean, according as it was below or above the mean at Southampton:* see Table XIII.

TABLE XIII.—Deviations of Barometer from the Means at Southampton (29″·98) and Laudale (29″·86) when the Barometer does not differ more than $\frac{1}{100}$ of an inch at Stonyhurst from its Mean.

Stonyhurst height.	Southampton deviation.	Laudale deviation.
29·95	+·15	−·20
29·94	+·11	−·27
29·94	−·12	+·07
29·93	−·07	+·12
29·94	+·03	−·11
29·94	−·07	+·00
29·94	+·09	−·13
29·95	+·07	−·05
29·93	−·03	+·03
29·94	+·04	−·07
29·93	−·00	+·04
29·95	+·07	−·06
29·94	+·12	−·13
29·94	+·09	−·07
29·95	+·25	−·18
29·94	−·04	+·01
29·93	−·19	+·30
29·95	+·04	−·08
29·93	−·06	+·06
29·94	+·03	−·08
29·94	−·04	+·17
29·95†	+·04	+·01
29·95	+·08	−·00
29·95	+·11	−·18
29·95	−·14	+·04
29·93	+·02	−·10
29·93	−·08	+·12
29·95	+·13	−·22
29·94	−·06	+·24
29·93	+·06	−·18
29·95	−·19	+·14

* The corresponding apparent paradox in the theory of heredity is referred to in 'Phil. Trans.,' A, vol. 187, p. 289.

† This is an exception to the general rule, that for the mean at Stonyhurst a high barometer at

13. Case (iii.).—*Prediction from three Correlated Stations.*

The general formulæ are given on p. 294 of the memoir previously cited. We do not reproduce them here, as we have no numerical data available at present by which to illustrate them. We will, however, consider a special application similar to that dealt with under Case (ii.).

Suppose it possible to select three stations so situated round a fourth that the three stations have equal correlation r with each other, and each a correlation ρ with the fourth. The latter being marked with the subscript 1 in the formulæ, the following are the proper relations which may be obtained after some algebraical reductions from the general case above referred to.

$$x_1 = \frac{\rho\sigma_1}{1 + 2r}\left(\frac{h_2}{\sigma_2} + \frac{h_3}{\sigma_3} + \frac{h_4}{\sigma_4}\right),$$

with a probable deviation $0{\cdot}6745\sqrt{1 - \frac{3\rho^2}{1 + 2r}}$,

$$x_2 = \frac{\rho\sigma_2}{1 + 2r}\frac{h_1}{\sigma_1} + \frac{(r - \rho^2)\,\sigma_2}{1 + r - 2\rho^2}\left(\frac{h_3}{\sigma_3} + \frac{h_4}{\sigma_4}\right),$$

with a probable deviation $0{\cdot}6745\sqrt{\frac{(1 - r)(1 + 2r - 3\rho^2)}{1 + r - 2\rho^2}}$.

Here the closest prediction will be obtained, if we select stations, if possible, such that $\rho = \sqrt{\frac{1}{3}(1 + 2r)}$, which is the point at which correlation passes into causation. In this case we find

$$x_1 = \frac{\sigma_1}{3\rho}\left(\frac{h_2}{\sigma_2} + \frac{h_3}{\sigma_3} + \frac{h_4}{\sigma_4}\right),$$

$$x_2 = \frac{\sigma_2}{3\rho}\frac{h_1}{\sigma_1} - \sigma_2\left(\frac{h_3}{\sigma_3} + \frac{h_4}{\sigma_4}\right),$$

with vanishing probable deviations. In the first case, if $\rho^2 > r$, which can easily be true for correlated stations within 300 miles of each other; in the second case always, a rise of the barometer at two of the "outer" stations for a steady barometer at the "inner" or first station marks a fall at the fourth station and *vice versâ*. Now, it is not contended that four stations can be found for which exactly $r_{12} = r_{13} = r_{14}$ and $r_{23} = r_{34} = r_{42}$, still less that it is possible to make $1 + 2r = 3\rho^2$. But it is suggested that with the values of the barometeric correlation coefficients such as we have found in the British Isles approximations to these relations can be found for selected stations, and that such stations are what we require for close prediction or interpolation. Further, such principles as we have noted with regard to the relation

Laudale is a low barometer at Southampton, and *vice versâ*. Or, the rule (by differentiation) may be stated for steady barometer at Stonyhurst a rise at Laudale indicates a fall at Southampton, and *vice versâ*.

of rise and fall at correlated stations are independent of the special relations between the coefficients, which we have selected to illustrate them, they are really a deduction from the sign of the regression coefficient, or of the coefficient of partial correlation, which has the same sign.

14. Case (iv.)—*Prediction from any Number of Correlated Stations.*

The general formulæ are given, p. 302 of the memoir above cited, namely :

$$x_1 = \left(\frac{R_{12}}{R}\frac{\sigma_1 h_2}{\sigma_2} + \frac{R_{13}}{R}\frac{\sigma_1 h_3}{\sigma_3} + \frac{R_{14}}{R}\frac{\sigma_1 h_4}{\sigma_4} + \ldots\right)$$

with a probable deviation of $0{\cdot}6745\ \sigma_1\sqrt{R/R_{11}}$ where R is the determinant below and R_{pq} is the minor formed by leaving out the p^{th} column and q^{th} row.

$$\begin{vmatrix} 1, & r_{12}, & r_{13}, & r_{14} & . & . & . \\ r_{21}, & 1, & r_{23}, & r_{24} & . & . & . \\ r_{31}, & r_{32}, & 1, & r_{34} & . & . & . \\ r_{41}, & r_{42}, & r_{43}, & 1 & . & . & . \\ . & . & . & . & . & . & . \\ . & . & . & . & . & . & . \end{vmatrix}$$

In order to obtain close prediction, it might be supposed that all that is necessary is to take a sufficiency of correlated stations. This is very far from being the case. The true test of closeness of prediction is the smallness of $\sqrt{(R/R_{11})}$, and this can often be obtained by a few well-selected stations better than a great number. In order to roughly illustrate this, suppose the correlation coefficients of the stations to be all of the same order of magnitude, *i.e.*, about ϵ, then the order of $\sqrt{(R/R_{11})}$ for

1, 2, 3, 4 . . . n stations

is given by

$$\sqrt{(1+\epsilon)(1-\epsilon)},\ \sqrt{\left(1+\frac{\epsilon}{1+\epsilon}\right)(1-\epsilon)},\ \sqrt{\left(1+\frac{\epsilon}{1+2\epsilon}\right)(1-\epsilon)},$$
$$\sqrt{\left(1+\frac{\epsilon}{1+3\epsilon}\right)(1-\epsilon)}\ \ldots\ldots\ \sqrt{\left(1+\frac{\epsilon}{1+(n-1)\epsilon}\right)(1-\epsilon)},$$

or the prediction is only increased in certitude in the ratio of $\frac{1}{\sqrt{1+\epsilon}}$ to 1 by taking an indefinitely great number of stations, or, since ϵ cannot be greater than unity, it can only be increased in the ratio of $\frac{1}{\sqrt{2}}$ to 1. Our object should accordingly be to make $\sqrt{(R/R_{11})}$ as small as possible* by a fit selection of comparatively few stations

* It is easy to illustrate its vanishing by taking one station equally correlated with $(n-1)$ others (ρ), which are equally correlated among themselves (r). In this case we have

ERRATUM.

Page 466, line 7 from top, the formula should be :—

$$x_1 = -\left(\frac{R_{12}}{R_{11}}\frac{\sigma_1 h_2}{\sigma_2} + \frac{R_{13}}{R_{11}}\frac{\sigma_1 h_3}{\sigma_3} + \frac{R_{14}}{R_{11}}\frac{\sigma_1 h_4}{\sigma_4} + \ldots\right)$$

at different intervals of time. Nor should it be forgotten that theoretically we have much power of varying the magnitude of our correlation coefficients. They decrease from about 1 at the distance zero to zero as the distance increases. We have thus a zone round every station of zero-correlated stations; beyond this it is highly probable that the correlation becomes negative as we reach places which have cyclones corresponding to anticyclones at the given station. This zone is of course not reached in such a small area as the British Isles. Passing through an area of negative correlation, we should in all probability ultimately reach an extended area of zero correlation. In the next place the time interval is under our control, and the coefficient of correlation can be reduced by increasing this. Lastly, it varies also, although probably to a much less extent, by varying the direction in which the distance between two stations is taken. We can imagine no more useful piece of work than the determination of the correlation for a period of 10 or 20 years of a series of stations taken so far as possible round, say, a parallel of latitude. We believe that the future of barometric prediction, *i.e.*, the accurate foretelling of the arrival of depressions, &c., lies in an extended knowledge of the correlation between a system of barometric stations widely diffused over the surface of the earth; special attention being paid to the changes of the correlation with intervals of time.

The object of the present writers has not been to make an elaborate investigation of the numerical values of barometric variation or correlation, but rather to indicate to those more directly occupied with meteorological investigations how the mathematical theory of statistics may be applied to barometry with novel and, they believe, valuable results.

Appendix.

On a Frequency-registering Barometer by G. U. Yule.

In all ordinary forms of registering barometer the resulting diagram shows the height of the barometer at each instant of time. To construct a frequency curve from such a diagram, the heights must be read off for all the times desired, corrected if necessary, grouped, and replotted in the manner described in the preceding paper. This procedure is somewhat tedious, and it may be obviated by so constructing the barometer that it shall give the frequency record automatically.

$$x_1 = \frac{\rho\sigma_1}{1+(n-1)\,r}\left(\frac{h_2}{\sigma_2}+\frac{h_3}{\sigma_3}+\frac{h_4}{\sigma_4}+\dots\right),$$

with a probable deviation $0{\cdot}6745\ \sqrt{\left(1-\frac{n\rho^2}{1+(n-1)\,r}\right)^{\frac{1}{2}}}$. Hence we should have absolute prediction if $\rho=\left(\frac{1}{n}\left(1+\overline{n-1}\,r\right)\right)^{\frac{1}{2}}$. Similar propositions follow for partial correlation and for rise corresponding to fall. It seems doubtful, however, whether such a system of correlation could possibly be arranged for more than four stations.

The barometer devised for this purpose is illustrated in figs. A and B. Fig. A is a diagrammatic plan of the instrument; fig. B is from a photograph of a rough model

Fig. A.

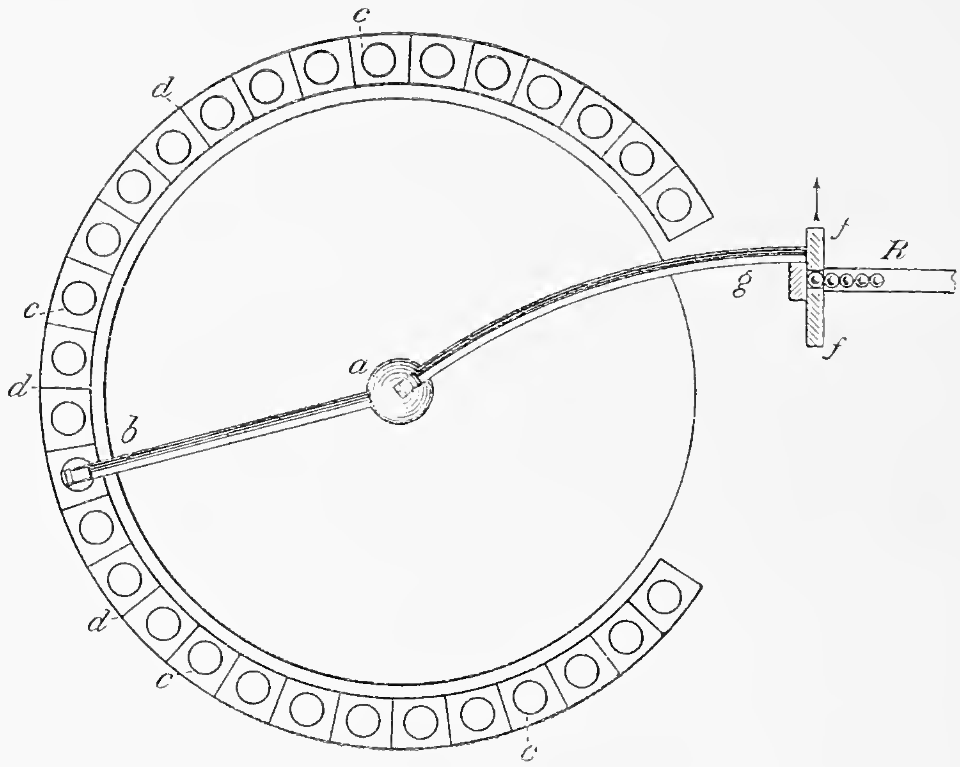
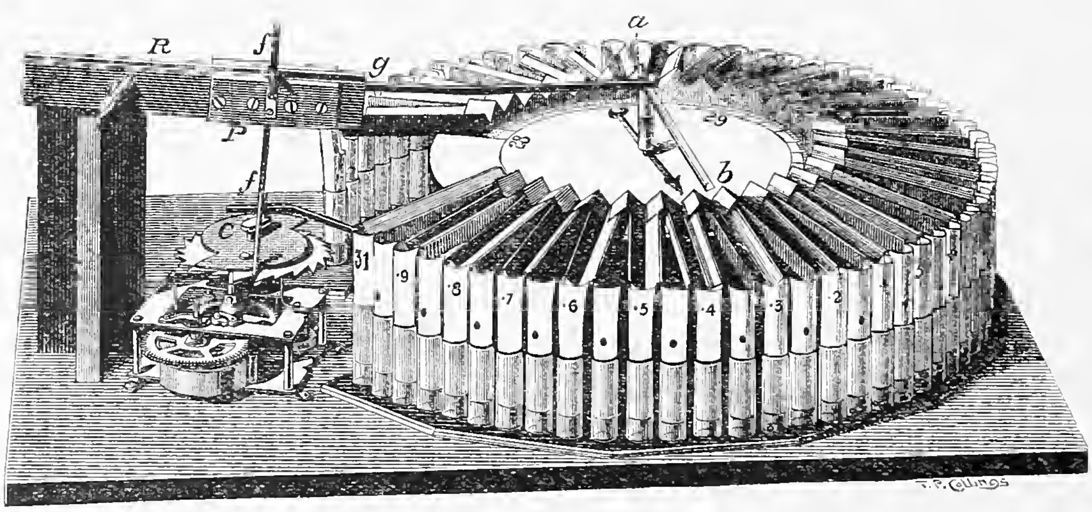


Fig. B.



made by the Cambridge Instrument Company. The barometer must be an aneroid, or some instrument working with a needle over a horizontal dial. This needle is removed, and replaced by a light V-shaped gutter of paper or metal-foil, sloping downwards from the centre to the periphery of the dial. This gutter needle is indicated by a, b in fig. A. Its outer extremity projects slightly beyond the dial over the tops of a series of small vertical tubes, c, c, c, which are equally spaced and separated by small wedge-shaped divisions, running along the lines d, d, d. If we suppose a ball to be dropped into the gutter at a, it will roll down to b, drop over, and fall either straight into one of the tubes or on to one of the wedges, which will guide it into the corresponding tube. The shot will then be a record that the barometer needle stood once over the section corresponding to the tube. If balls be dropped in at regular intervals, a frequency record is obtained by simply counting up the number of balls in each tube. The record, of course, suffers from the disadvantage that the usual corrections cannot be made.*

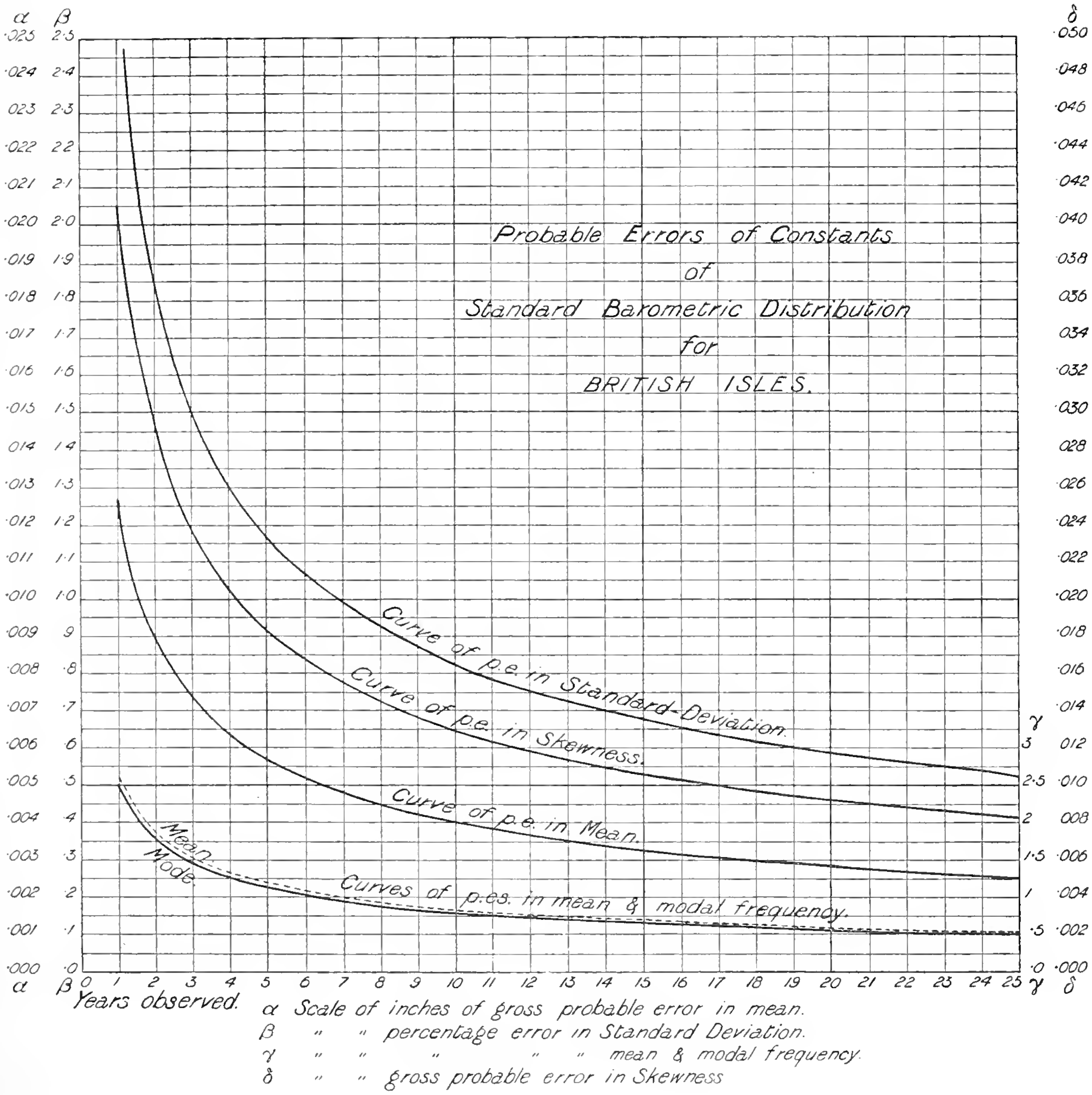
In practice, of course, the wedge edges *d, d, d* must be arranged so as to correspond exactly to the divisions between, say, the tenth inches. Any one tube will then record the number of times that the needle stood over a particular tenth when the record was taken. In the model itself a fixed gutter, *ag*, is brought over the centre of the needle gutter, just above its axis; it stands clear of it, and runs back to a reserve of balls, R. A trigger arrangement, *f, f*, is fired by a clock-driven cam as often as desired. The trigger gear can be seen in the second figure. The large cam, C, turns round once in the twenty-four hours, one of the saw teeth being pushed on each hour by the hand of the clock. The channel R, containing the reserve of balls† in single file, is normally closed by the forked brass lever, *ff*, which is pivoted near P, and bears at its lower end against the cam, being held forward by a spring. Only when the lever is fully cocked, *i.e.*, pushed over to its furthest reach by the cam, does the fork of the lever stand in line with the reserve channel. One ball then drops into the fork, which is just of the thickness of a ball. When the lever is freed again, the fork comes opposite the fixed gutter (*a, g* of fig. A), and the ball drops first into this, then falls into the needle gutter, and ultimately into a vertical tube.

It may be desirable to take two or more frequency records for different hours of the day, and keep these records separate, so as to admit of the study of systematic differences. Mr. HORACE DARWIN, to whom the details of the working model are due, devised a very simple arrangement for doing this, which can be seen in the general view of fig. B. The balls do not drop straight from the needle-gutter into the collecting tubes, but run into radial gutters fixed to the dial-face of the barometer. At their outer terminals these fixed radial gutters stand so far apart that they only deliver balls into alternate tubes, *e.g.*, to the set marked on the figure with numbers not with black spots. Before the alternate records are made, the whole barometer, with the radial gutters attached, is given a slight turn by the same clock that serves to release the trigger. The radial gutters now stand opposite the tubes marked with black spots, and the record is made in them.

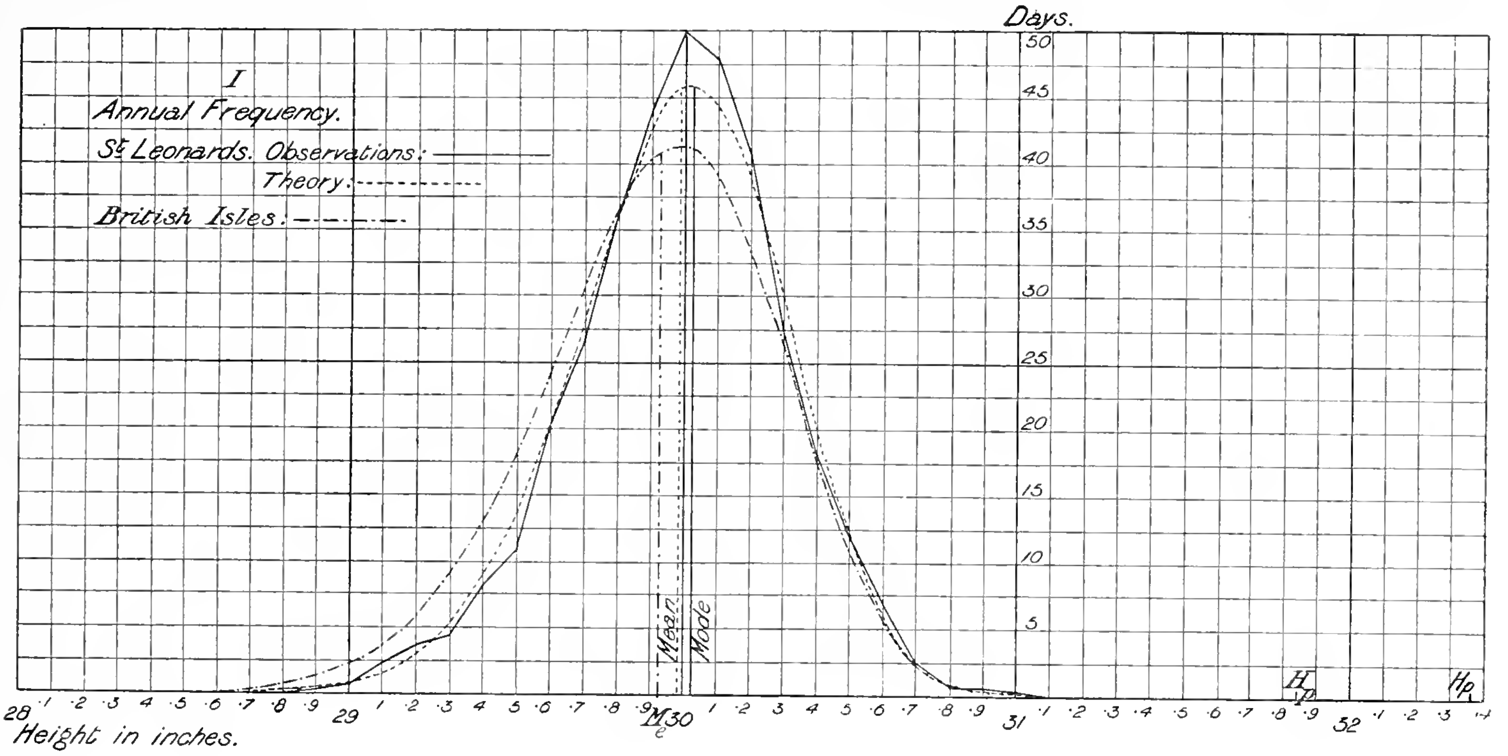
The working model was kept running for rather more than a month, and worked very satisfactorily, considering its somewhat rough construction. There were one or two "misfires," due to the balls hanging up, but such accidents could be easily remedied by a slight alteration.

* It must be remembered, however, that grouping in tenths of an inch gives quite sufficiently smooth and detailed returns for the purpose of calculating frequency curves.

† Bicycle bearing balls were actually used, as shot were found too irregular in shape and size.



Probable Errors of Constants of Standard Barometric Distribution for BRITISH ISLES.
Curve of p.e. in Standard-Deviation.
Curve of p.e. in Skewness.
Curve of p.e. in Mean.
Curves of p.es. in mean & modal frequency.
Mean.
Mode.
Years observed.
α Scale of inches of gross probable error in mean.
β " " percentage error in Standard Deviation.
γ " " " " " mean & modal frequency.
δ " " gross probable error in Skewness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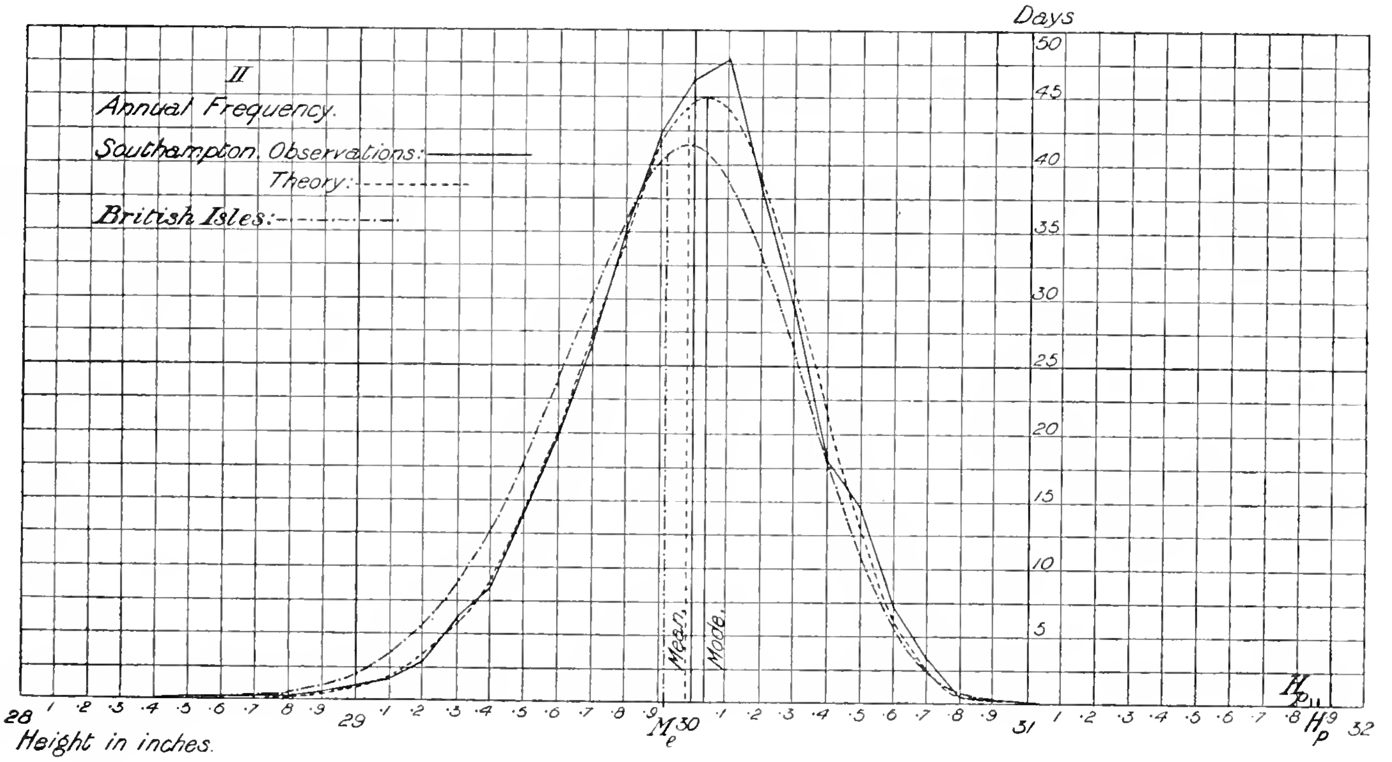
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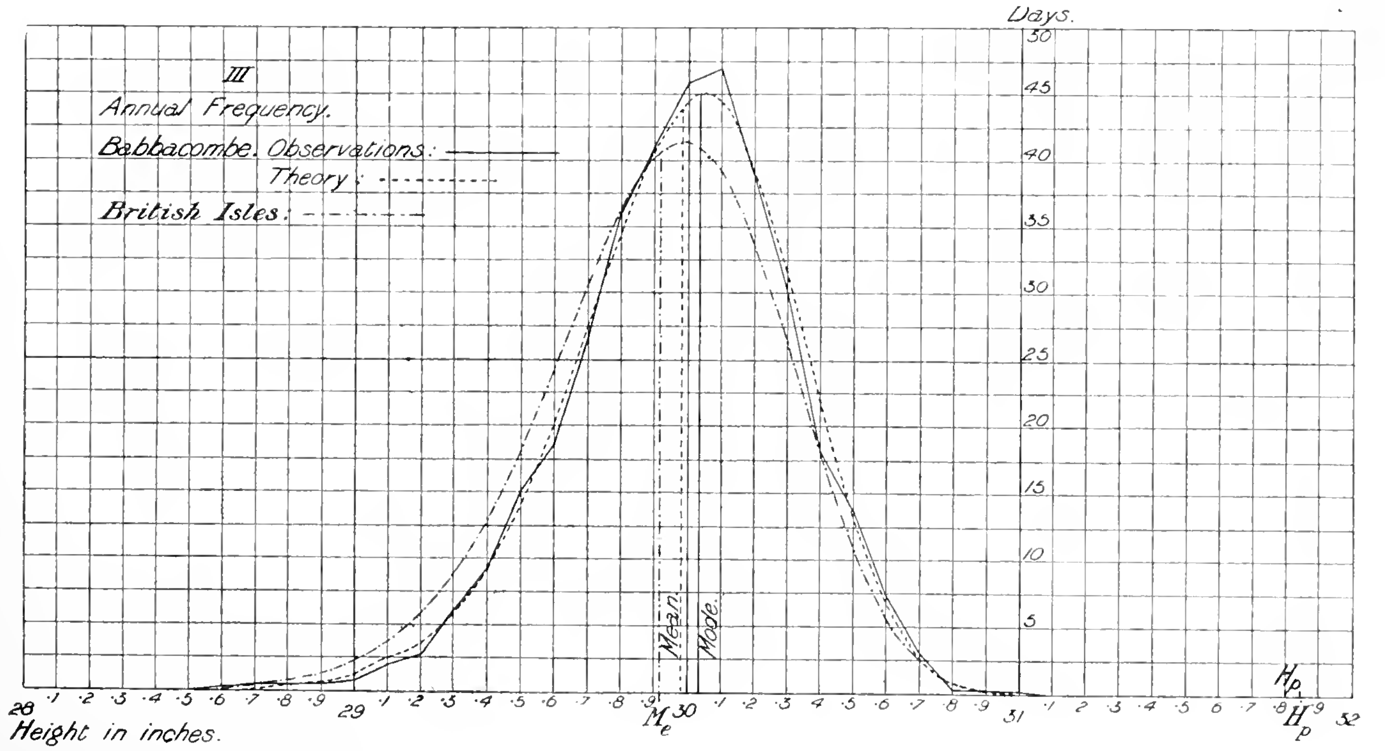
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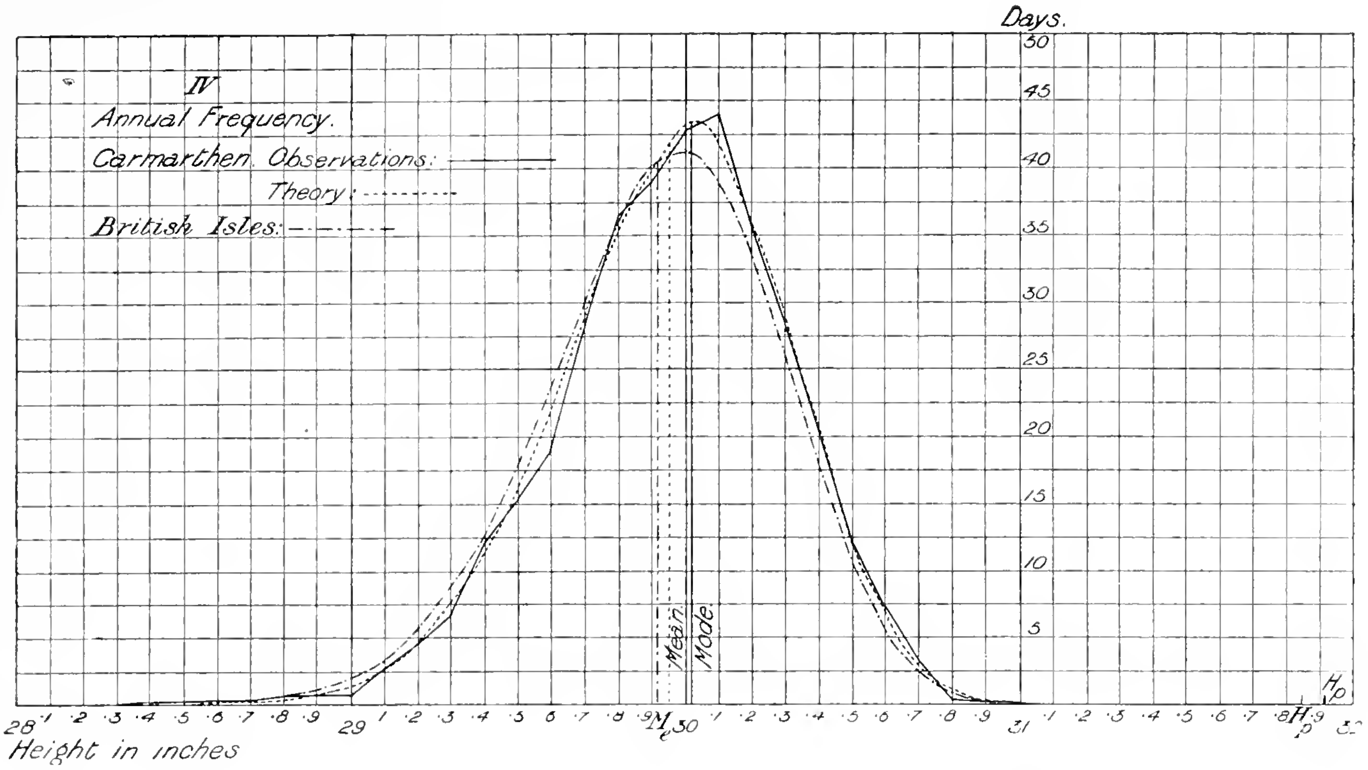
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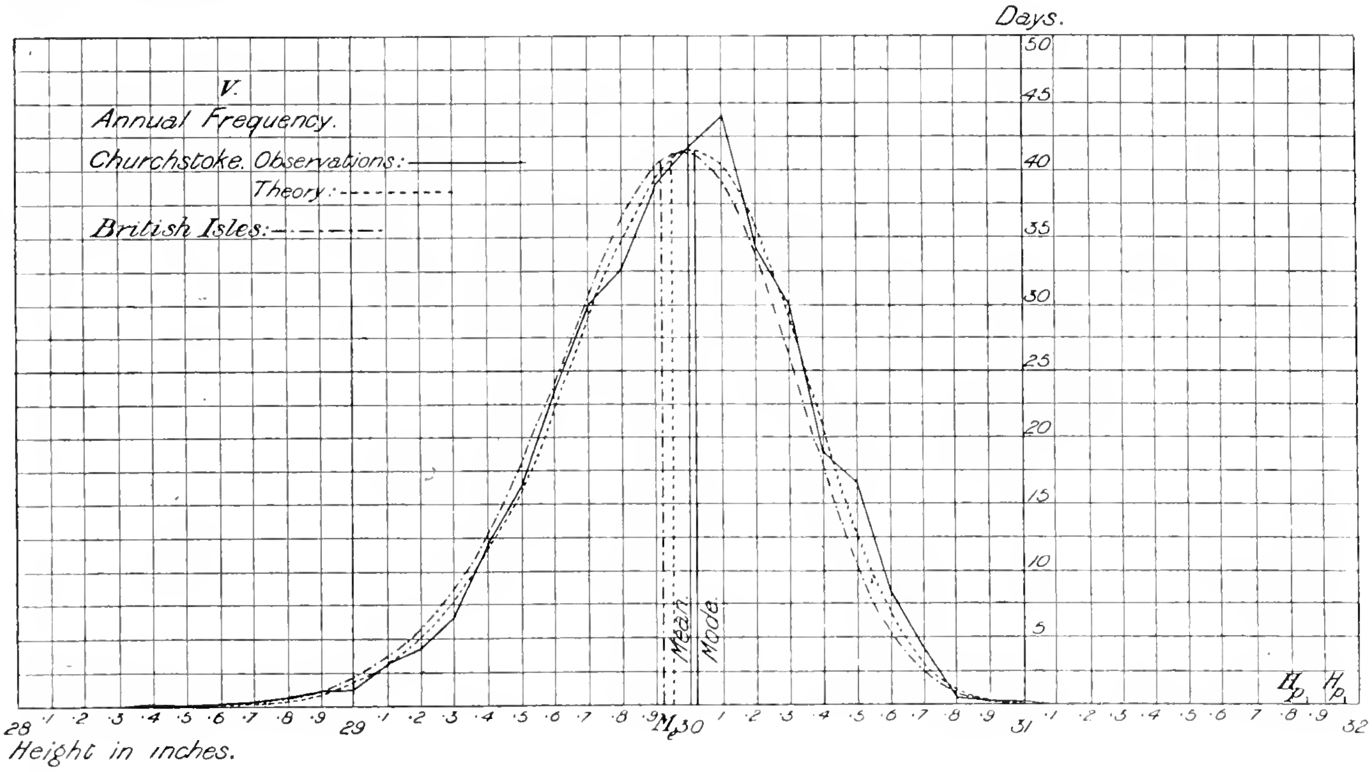
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Mode.
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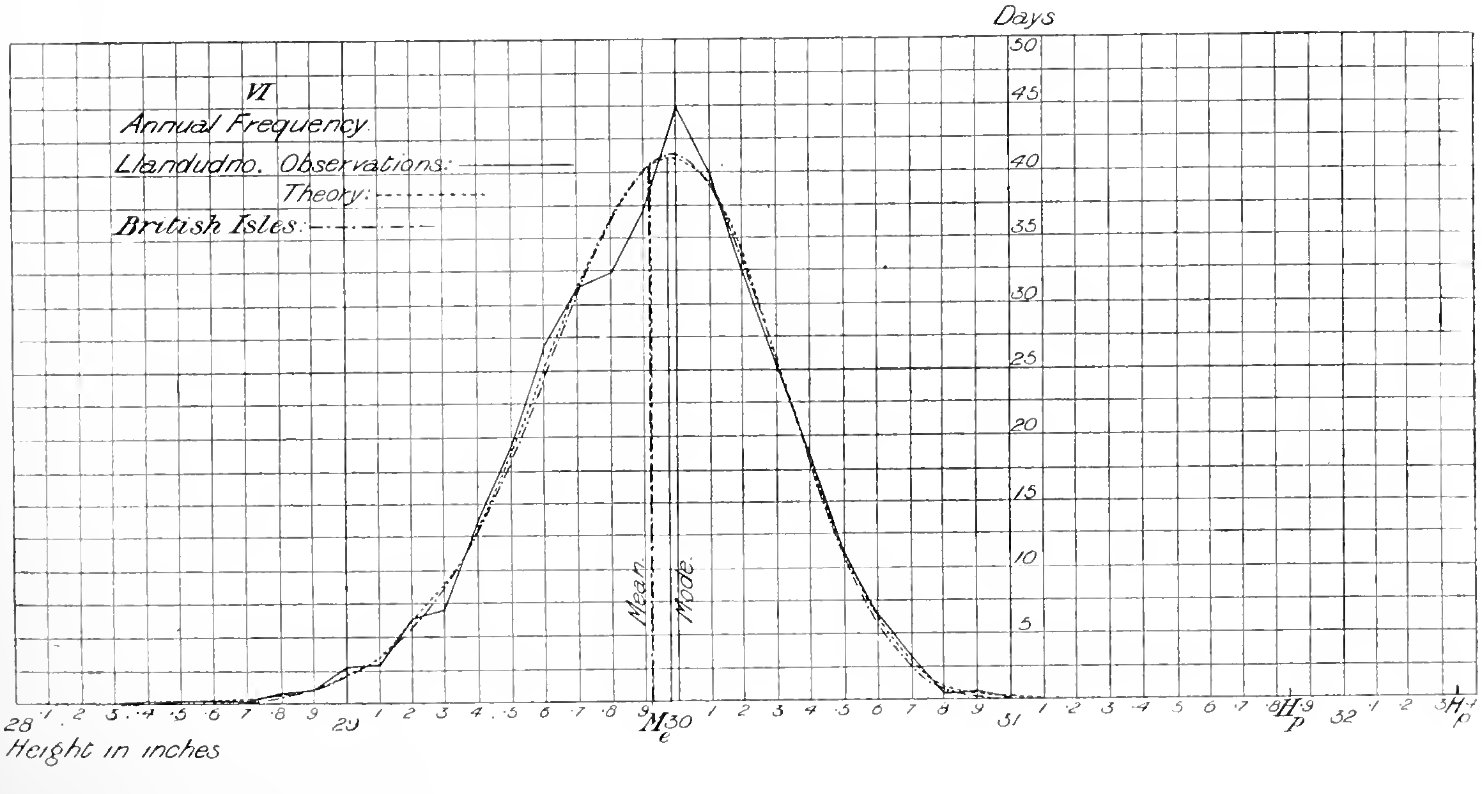
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Days
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Mode.
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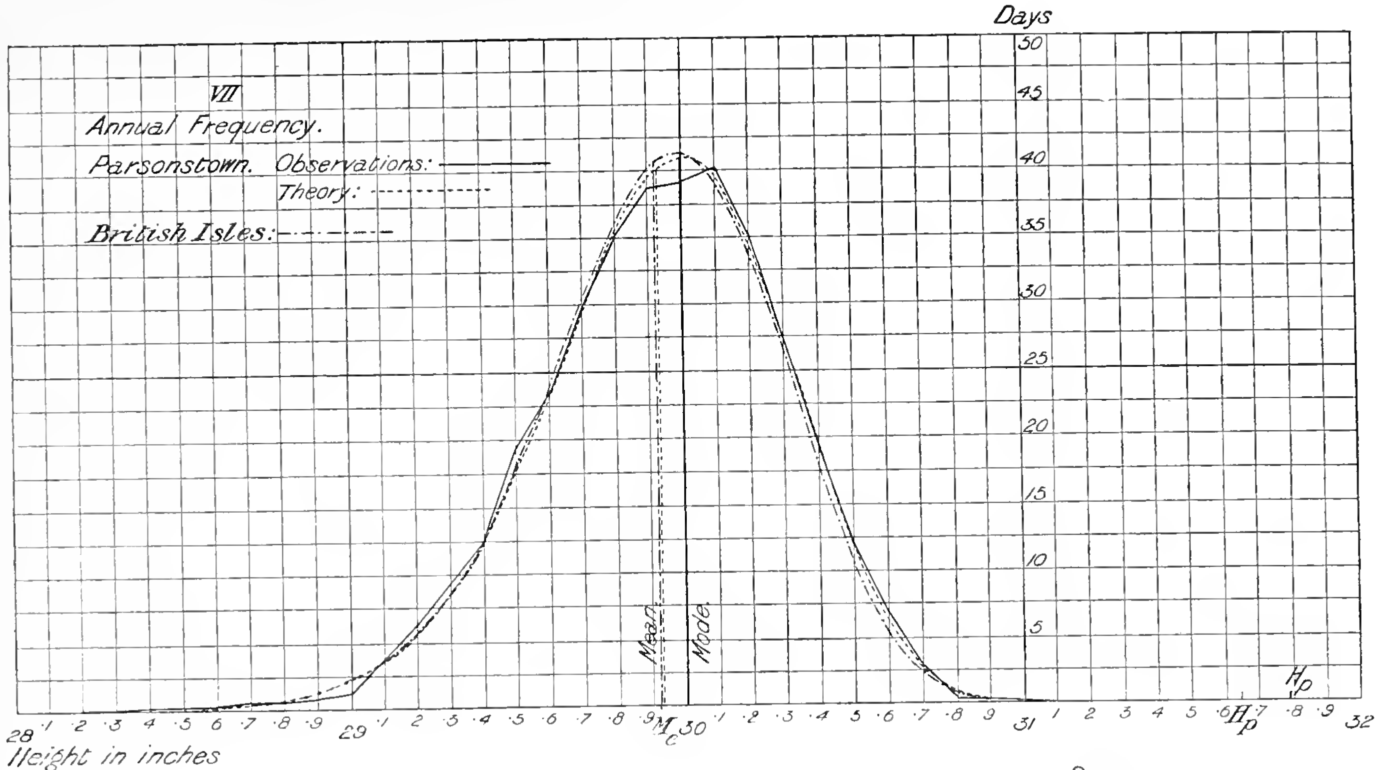
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Mode
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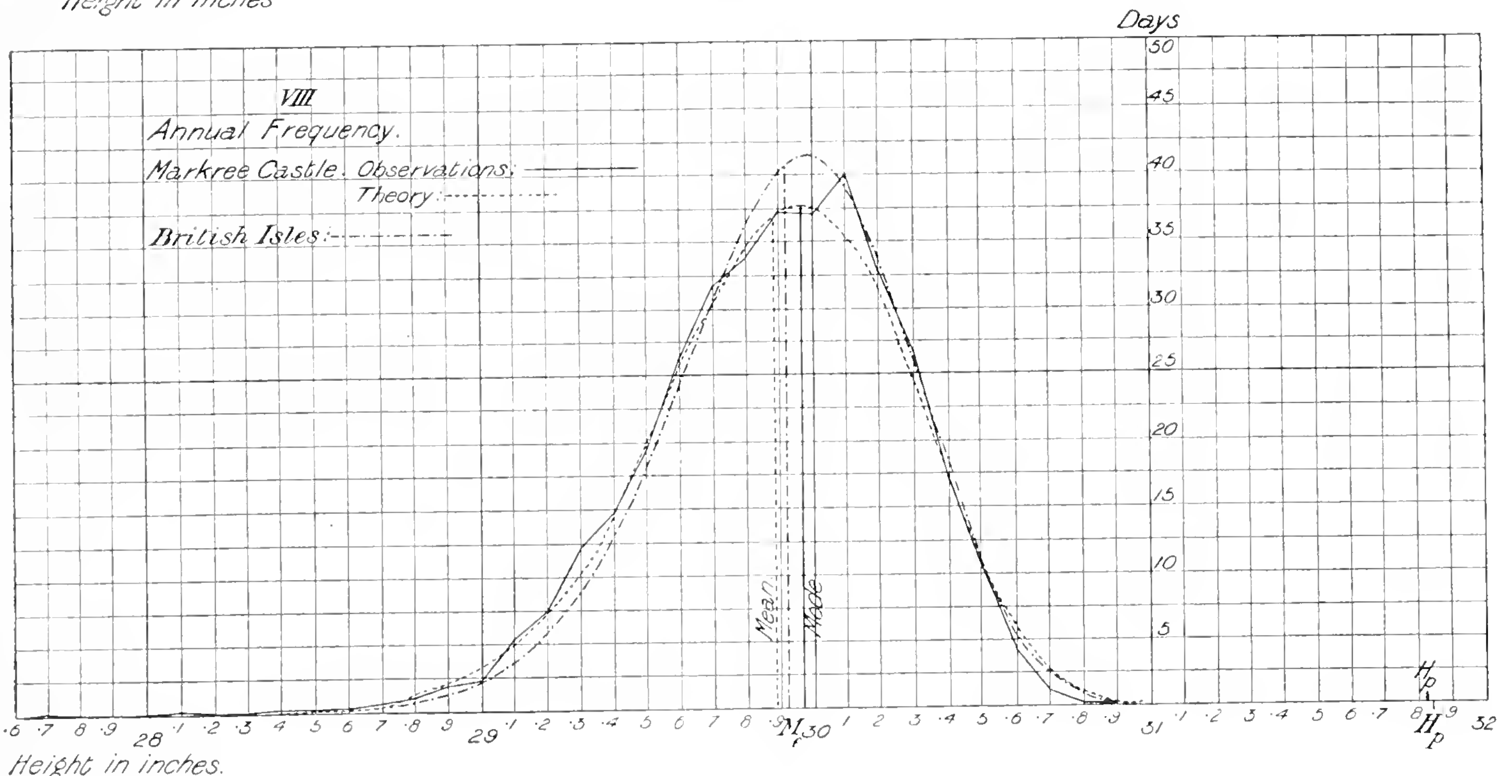
H_p
Height in inches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H_p
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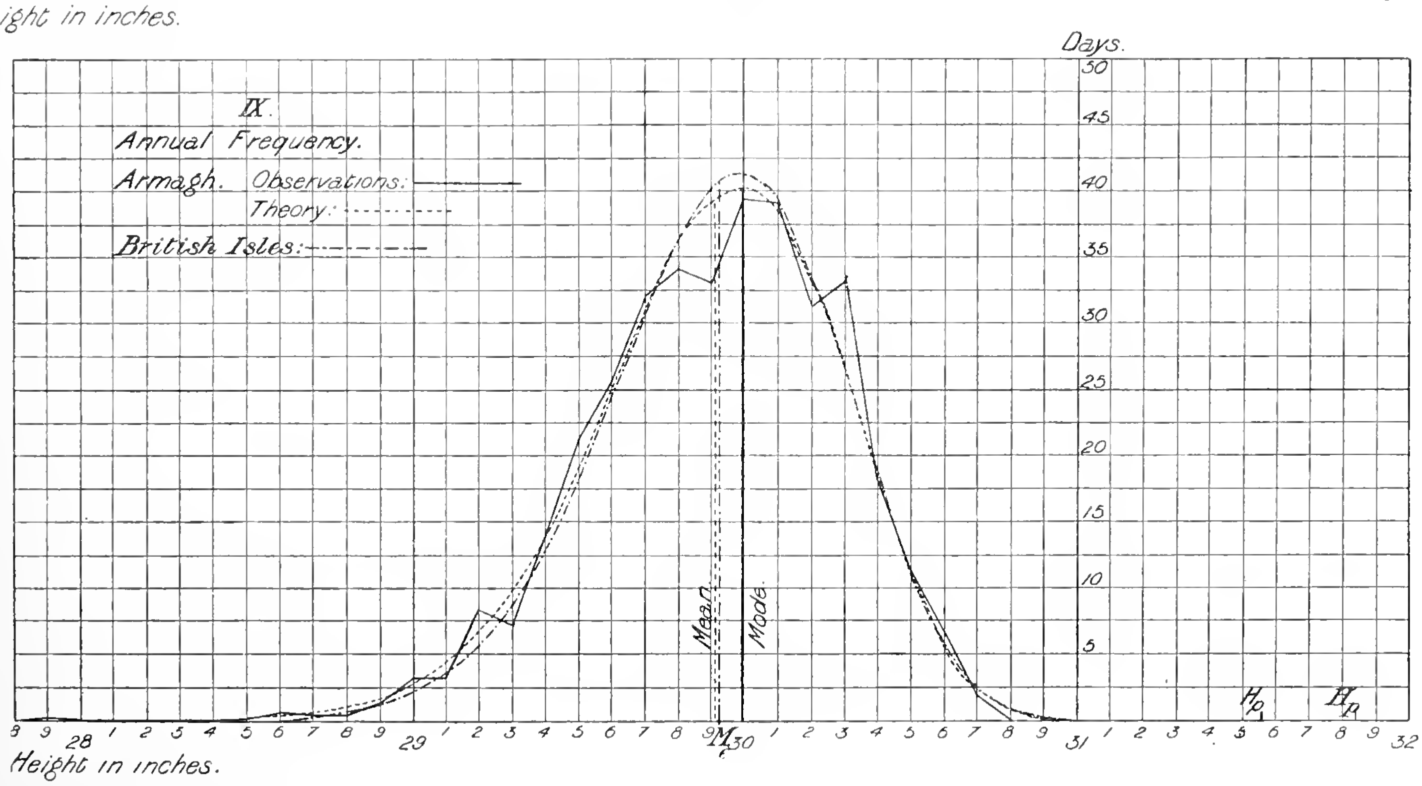
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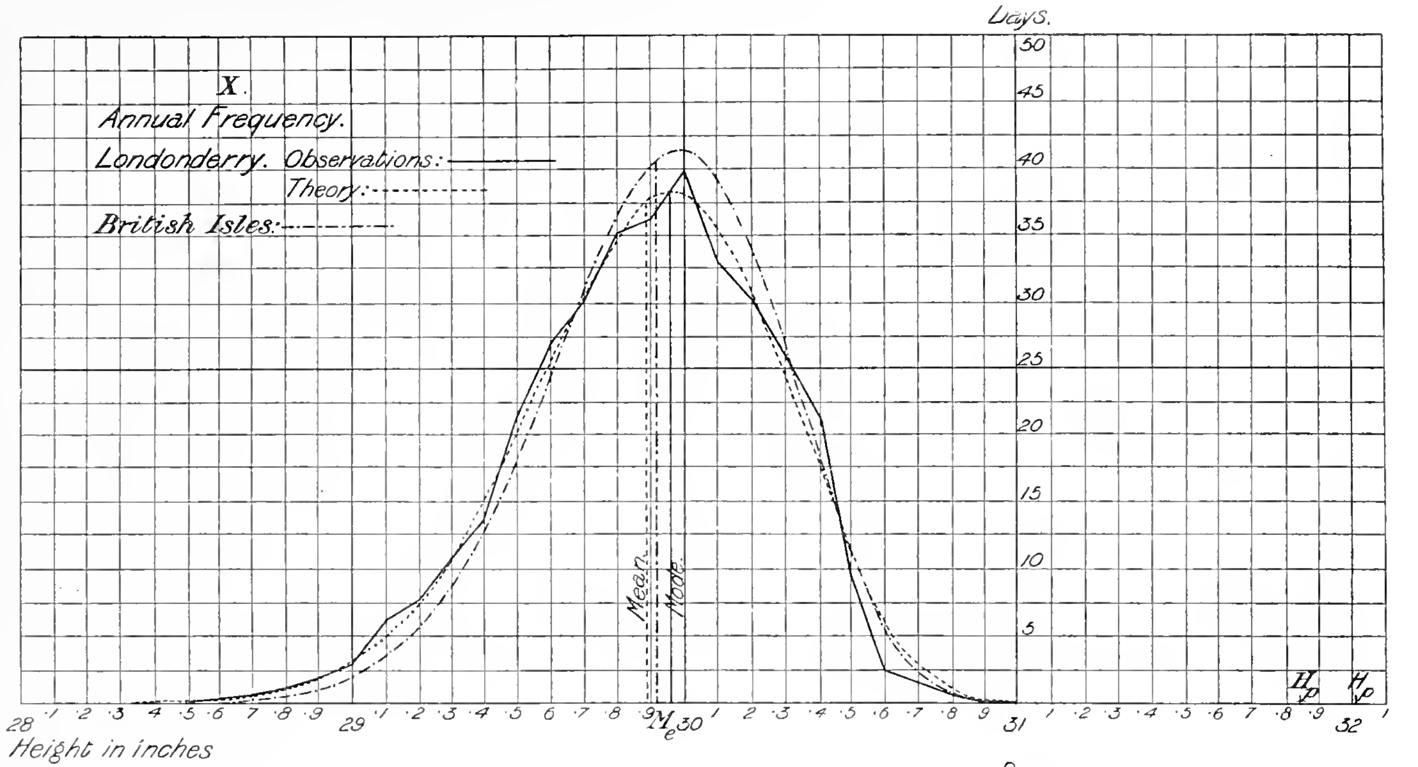
H_p
Height in inches.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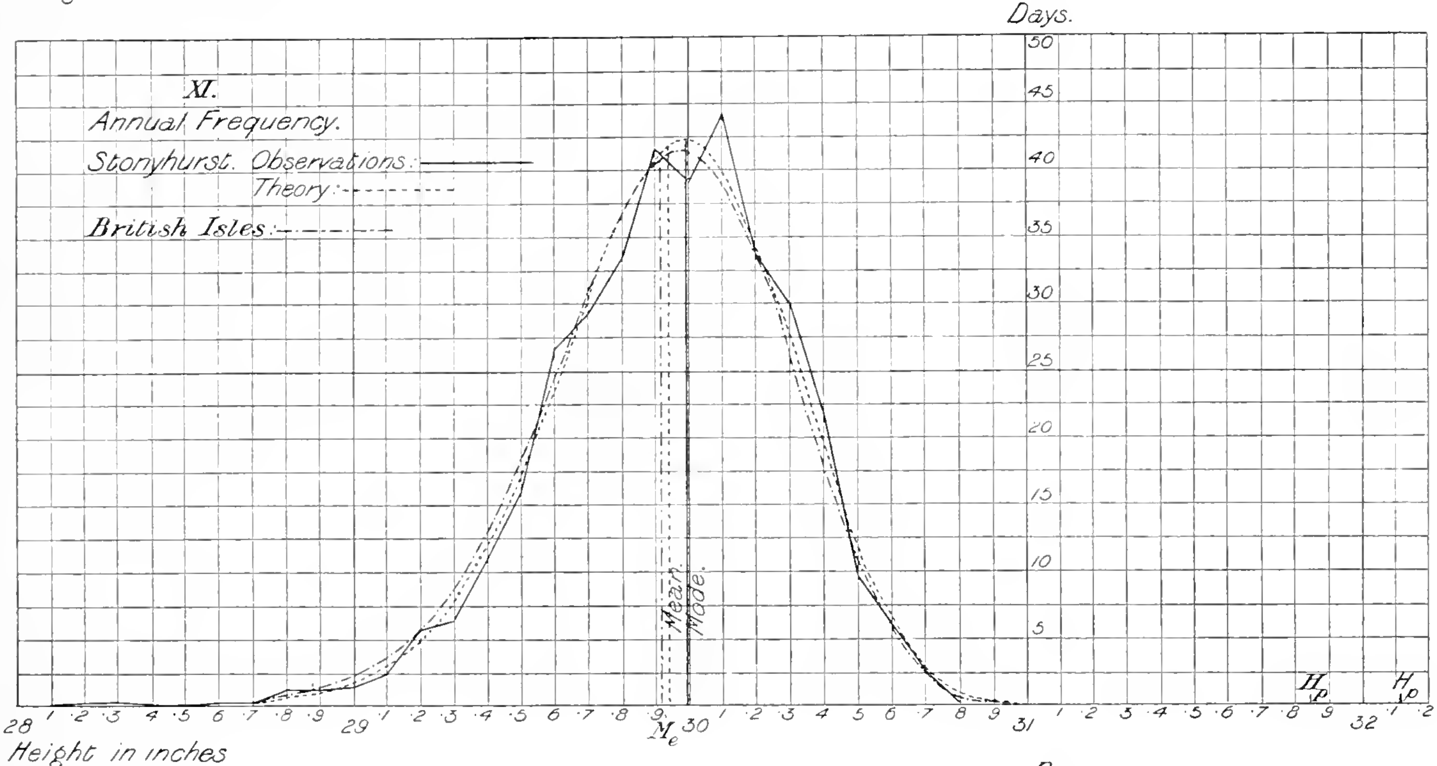
Mode.
M_e
H_p
Height in inches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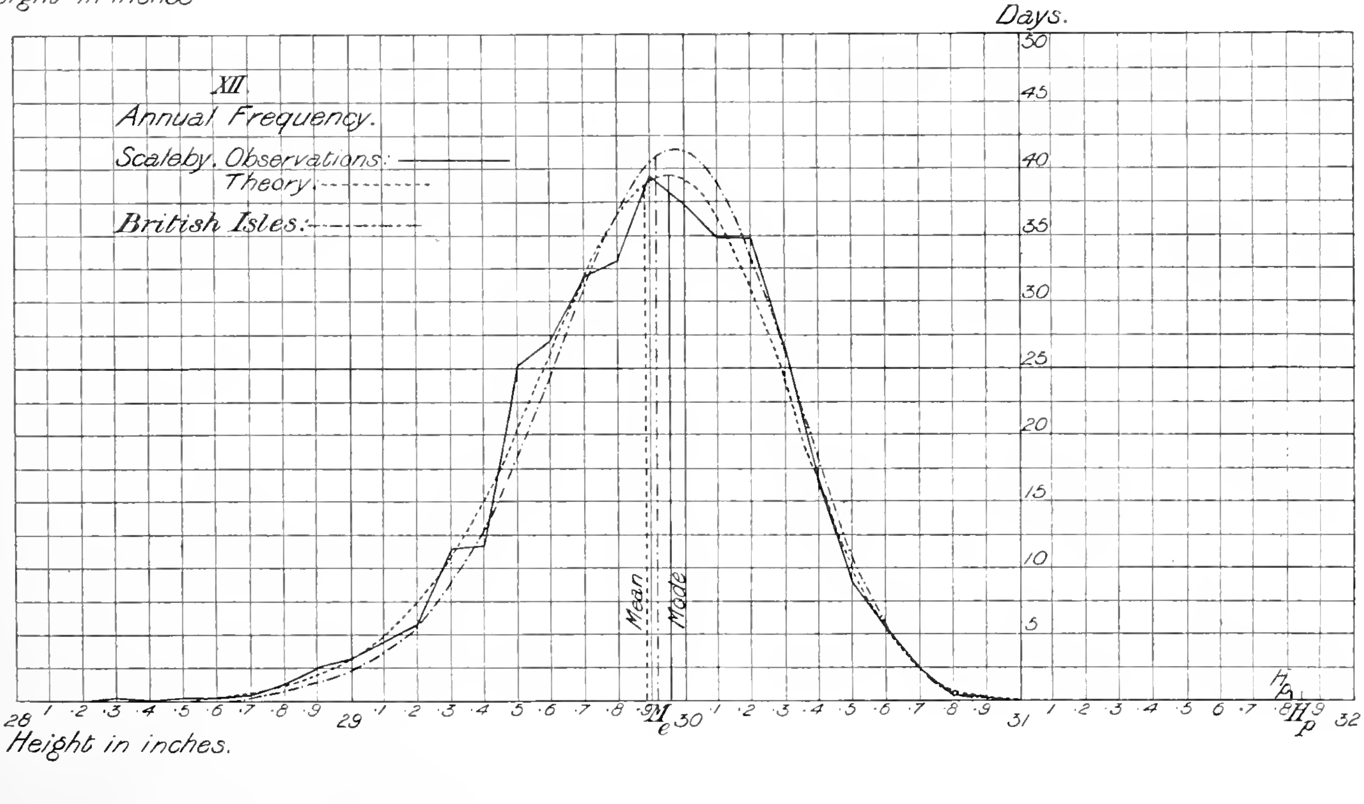
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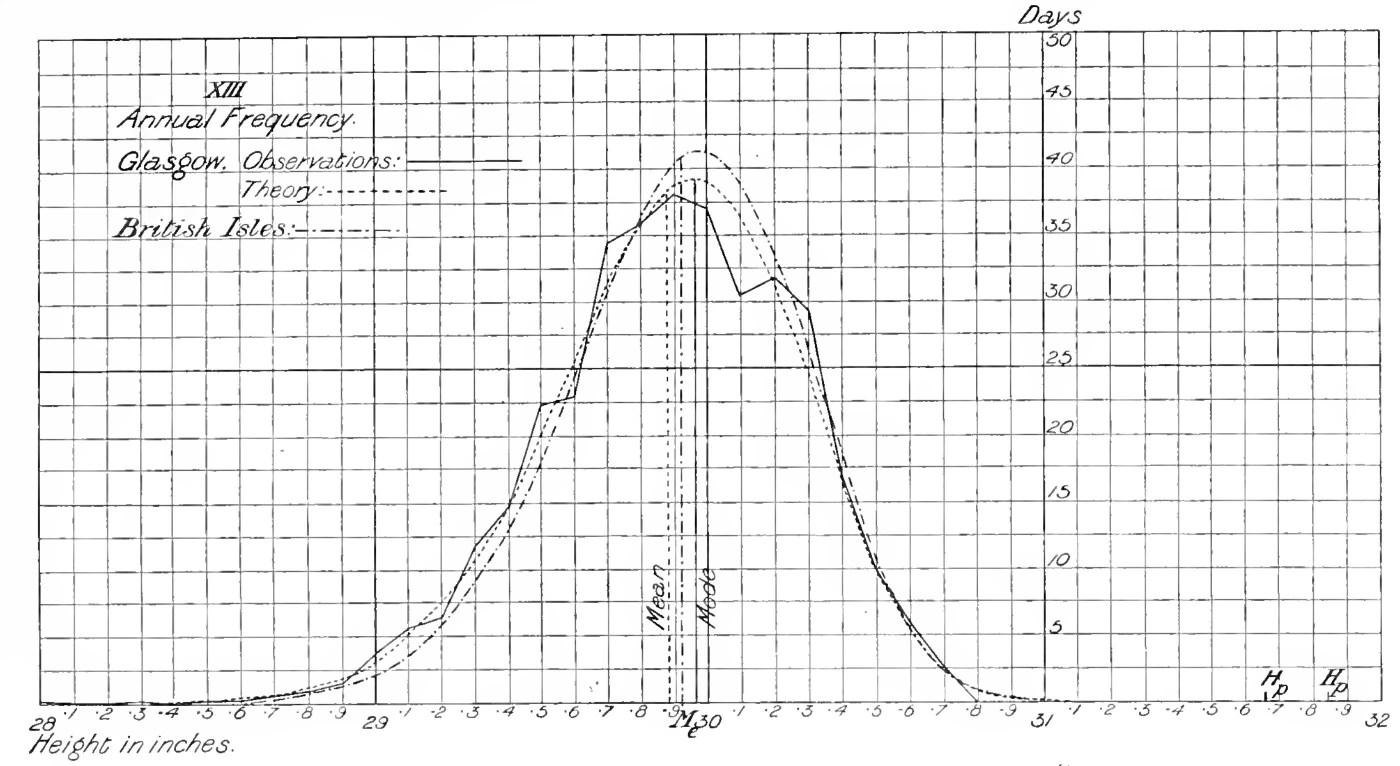
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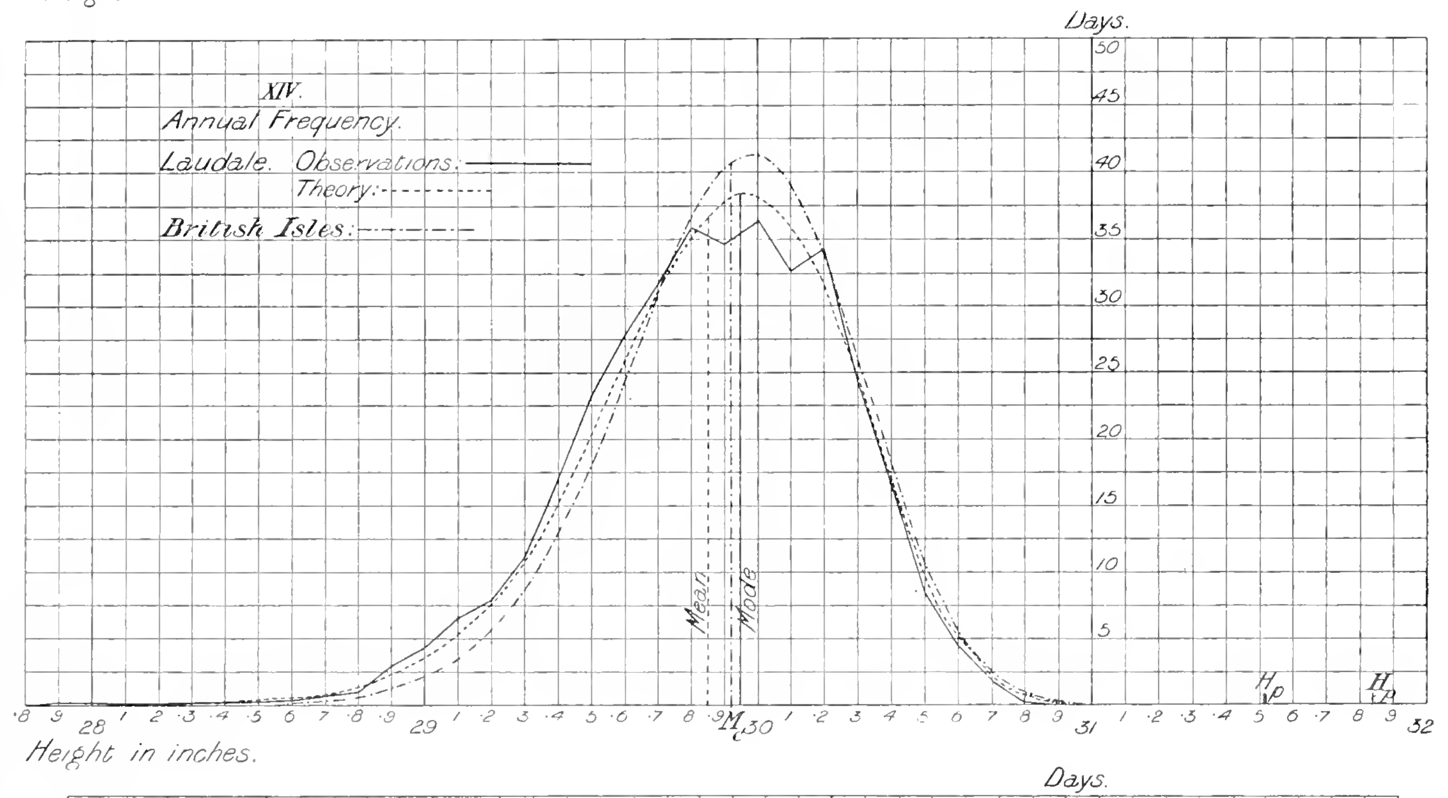
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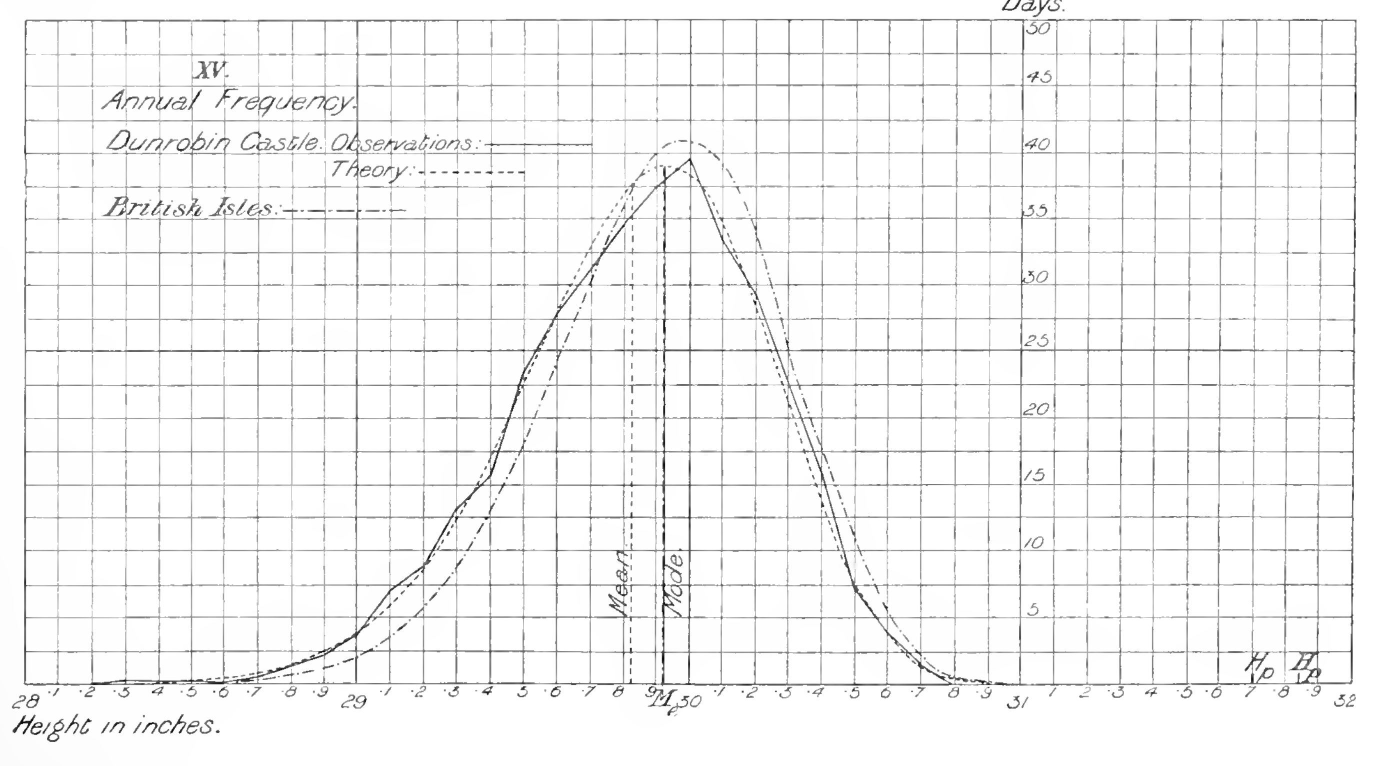
Height in inches.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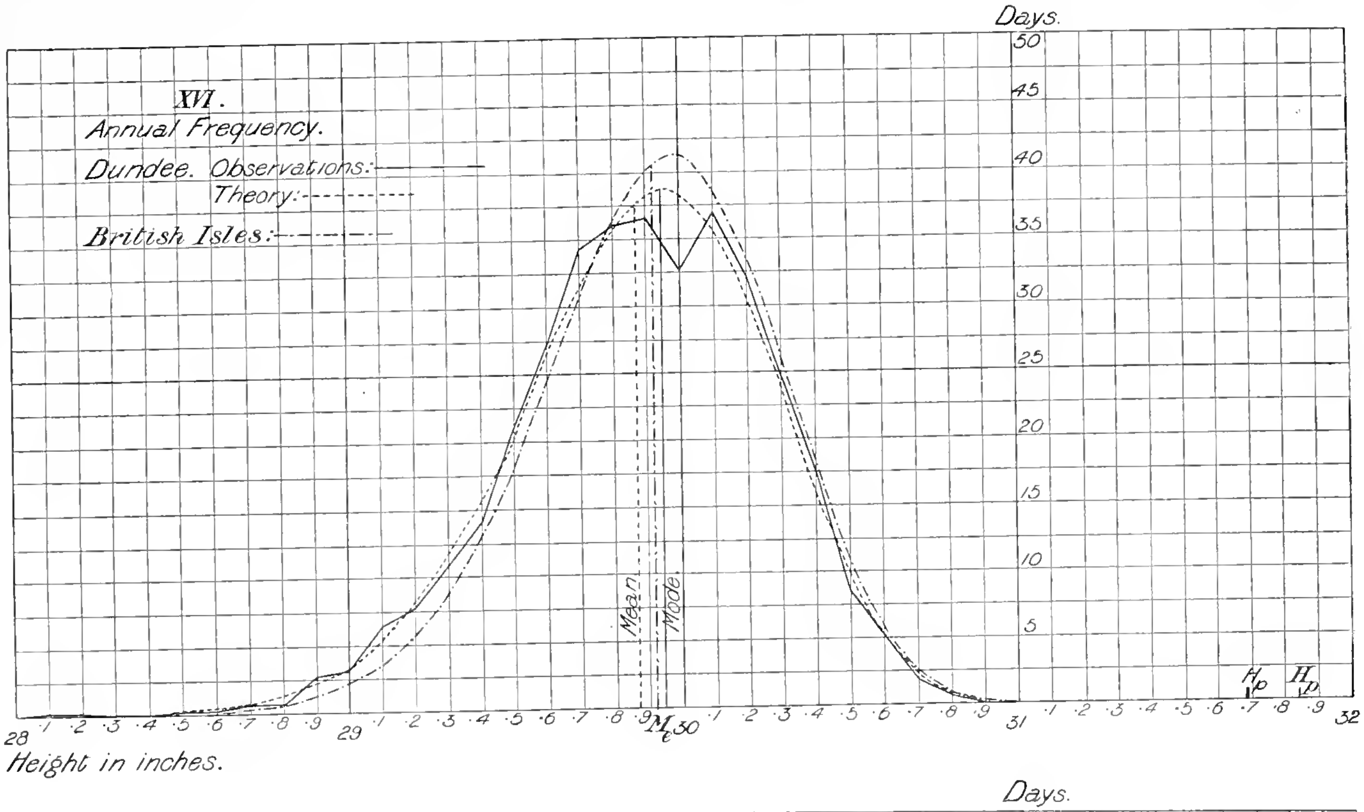
Height in inches.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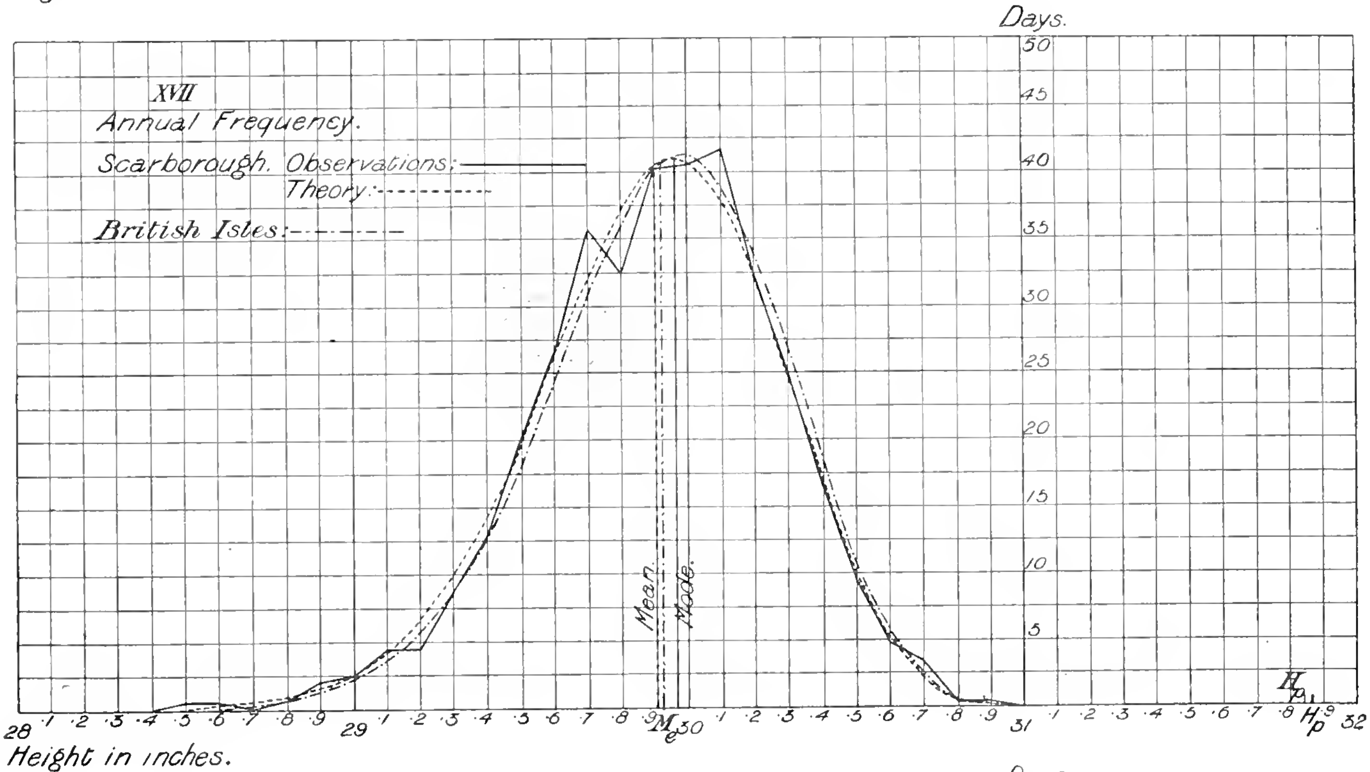
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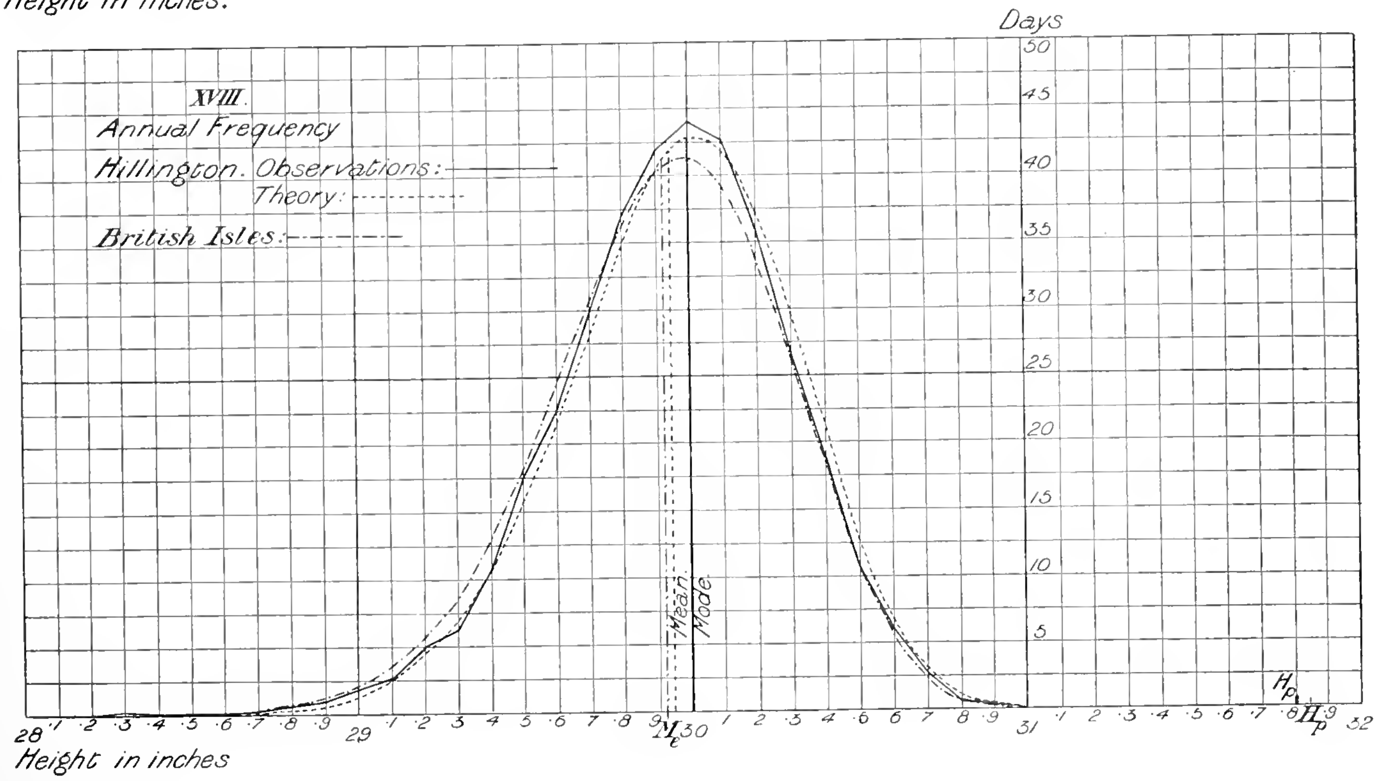
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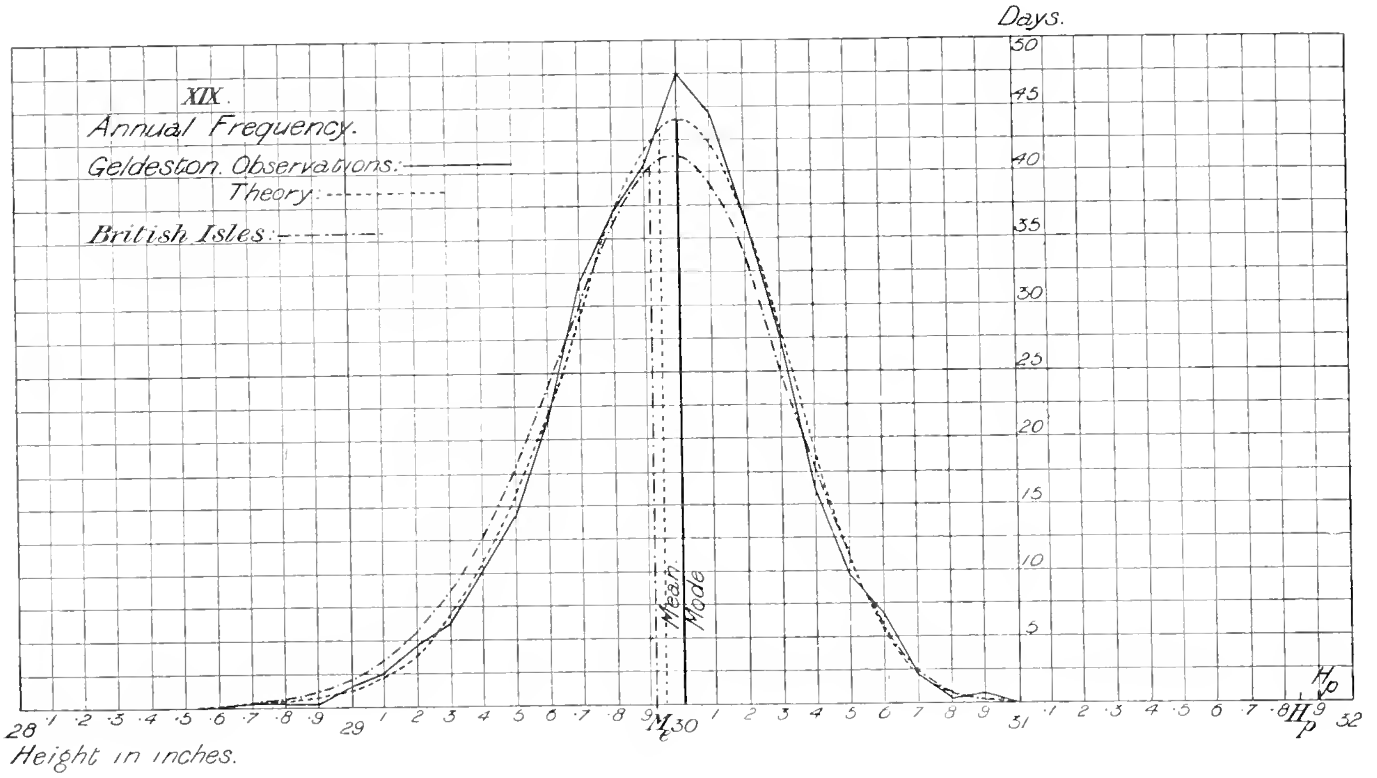
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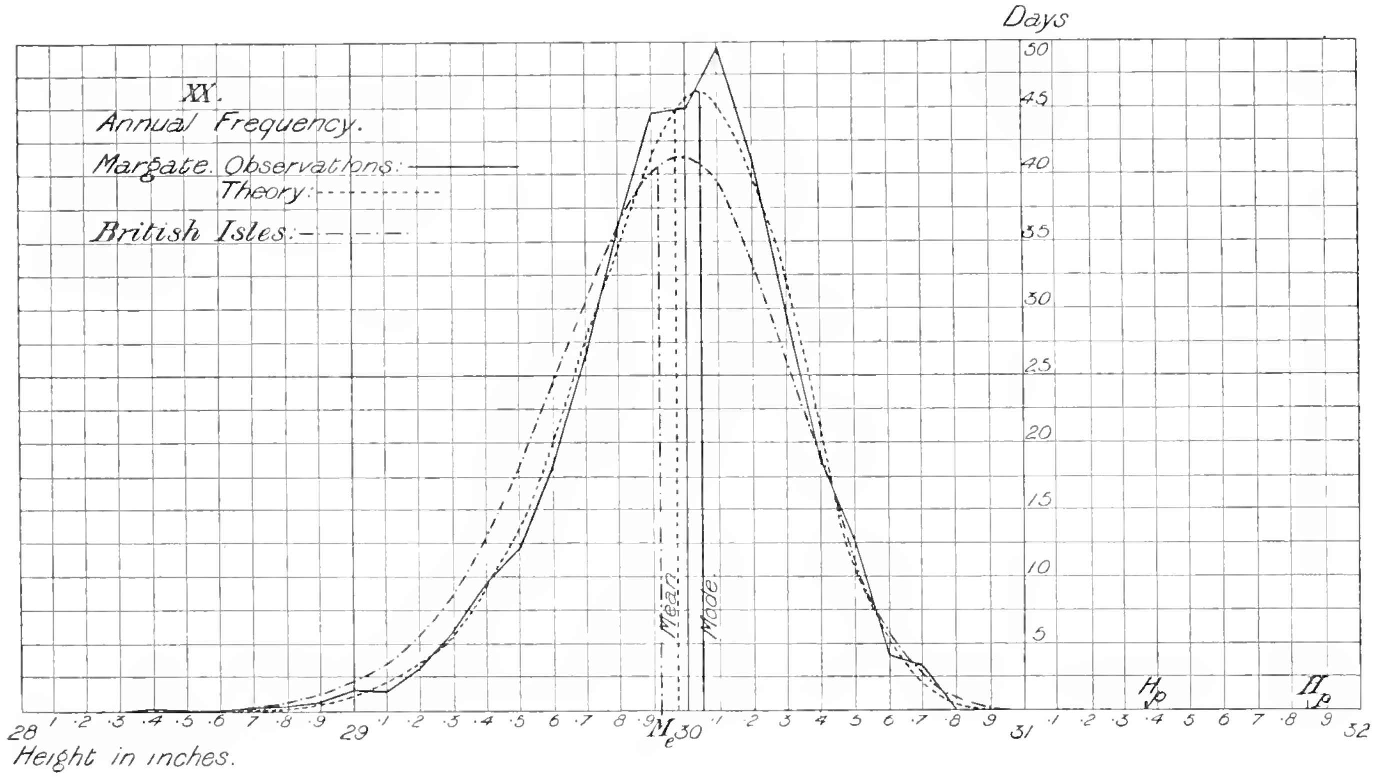
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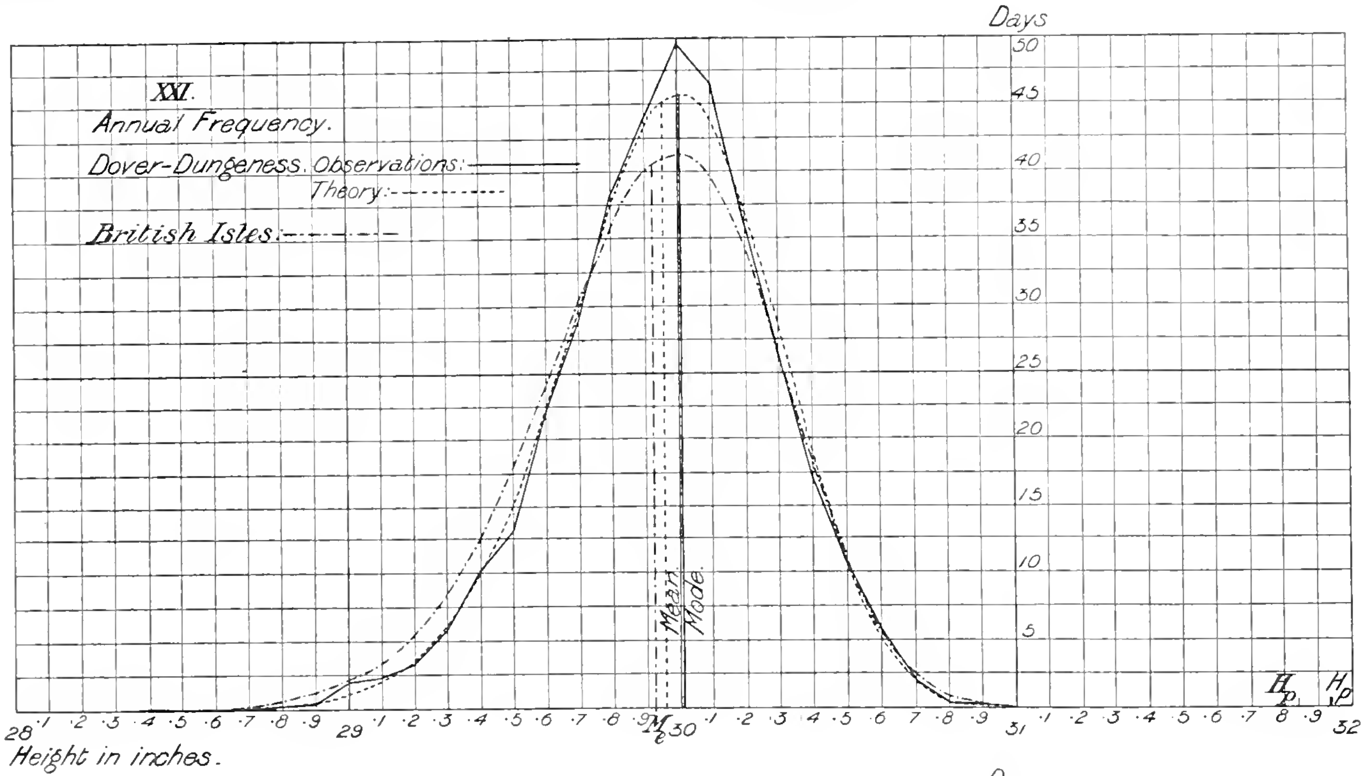
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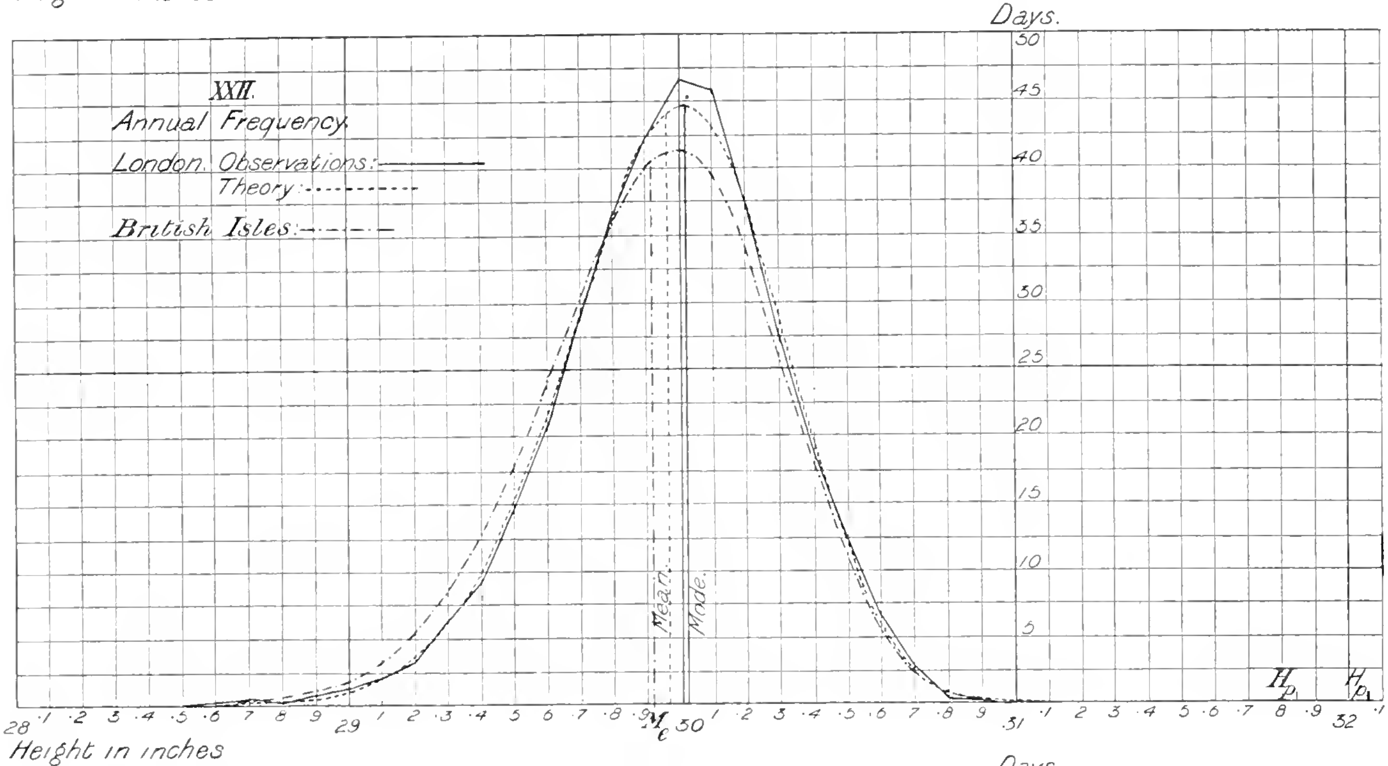
Height in inches.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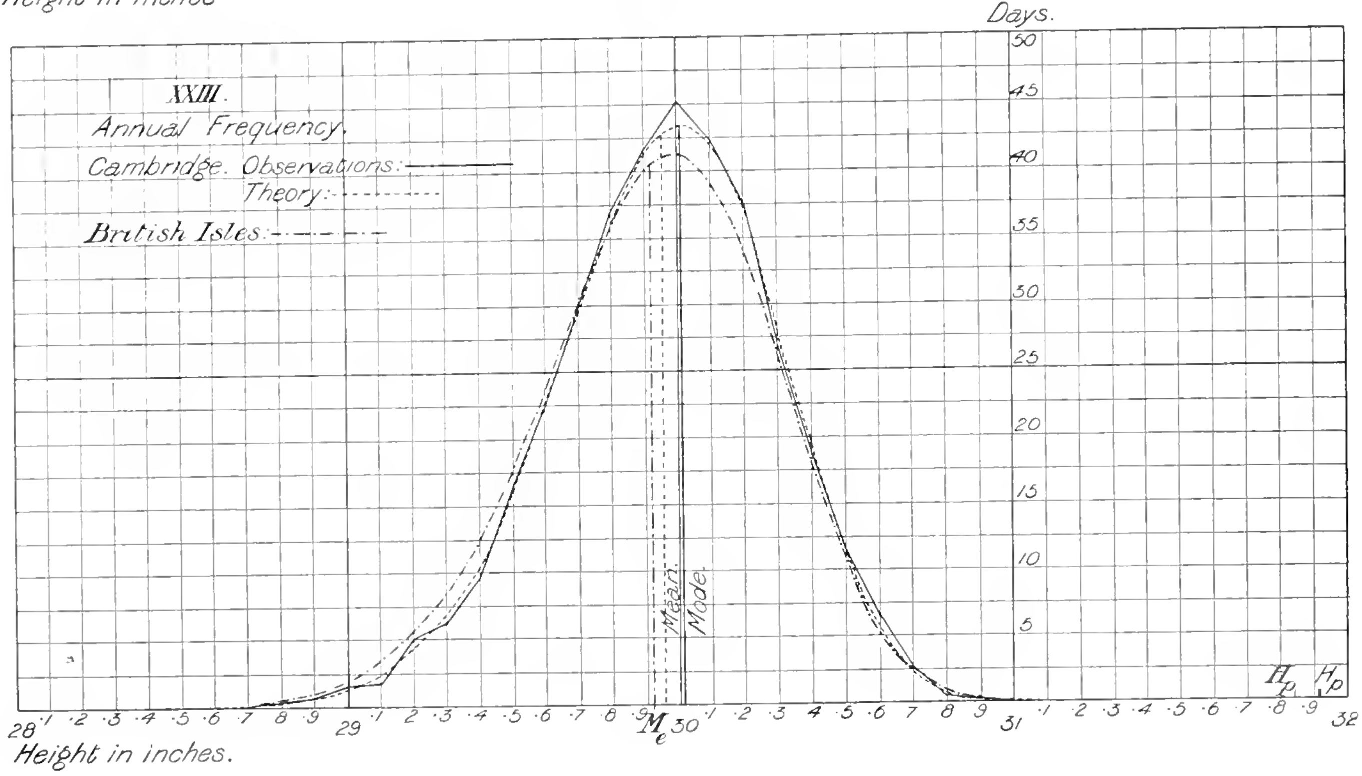
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Height in inches.

INDEX SLIP.

XII. *Cathode Rays and some Analogous Rays.*

By Silvanus P. Thompson, *D.Sc.*, *F.R.S.*

Received May 10,—Read June 17, 1897.

1. *On the Electrostatic Deflection of Cathode Rays and the Production of Negative Cathodic Shadows.*

In the experiments first to be described, the aim of the research was primarily to discover whether, and in what way, the shadow cast by the so-called cathode ray* was affected by the physical state of the object interposed between the cathode and the tube-wall, or other opposing surface capable of luminescing under the stimulation of the ray.

A pear-shaped Crookes tube, depicted in fig. 1, was made, having as an electrode at its smaller end a flat disk, A, of aluminium. At opposite sides of the bulb were introduced transversely two short cylindrical electrodes, B and C, of aluminium wire. These were mounted, as usual, on platinum wires, which were fused into the aluminium and sealed in through the glass wall of the tube. This tube was exhausted until the stage was reached at which all the pale internal nebuloid patches of luminous gas had disappeared, and the tube showed the yellow-green surface luminescence characteristic of soda-glass. When A was used as cathode, Röntgen rays were emitted from the glass at the opposite end of the tube, but the exhaustion was only just sufficient for this purpose, the emission ceasing when the tube became warmed with prolonged discharges. At this particular stage of exhaustion the tube was sealed off. It was in that stage of exhaustion in which it exhibited, in the luminous patch opposite the cathode, a singular unstable creeping luminosity, flickering in dendritic forms suggestive to the casual observer of splashes. The phenomenon has frequently been observed in Röntgen tubes, and is the subject of further notice in § 3 below. Throughout the entire research the electric source employed was an Apps induction coil capable of yielding sparks 25 centims. long, but with the break ordinarily adjusted so as to yield sparks up to 8 or 10 centims. only in length.

When the flat electrode, A, of this tube was made the cathode, shadows of B and

* The term ray is used here and throughout in the most general sense, not as in any way postulating a wave-propagation.

8.3.98

C were projected upon the tube-walls. As is usual in such tubes, these shadows were quite well defined and were surrounded by the usual margin of brighter luminescence. When B and C were made cathodes, while A was made anode, each cathode was observed to produce an oval luminescent patch upon the tube-wall around its own region. When B and C were both made anodes, being connected together by an external wire, their shadows were both quite small and narrow, considerably less than the geometric shadow, being only about 6 millims. long and 1 millim. broad. Nor were they rectangular in shape (fig. 2), but somewhat tapered to a point; and beyond the tip of each appeared a more luminous point in the splashing luminescence on the glass wall. If B was alone anode, while C was disconnected, the shadow of C was full-size, and of nearly rectangular shape; while the shadow of B (the anode) was somewhat smaller, and showed at its end a flickering luminous spot at *b* (fig. 3).

Fig. 1.

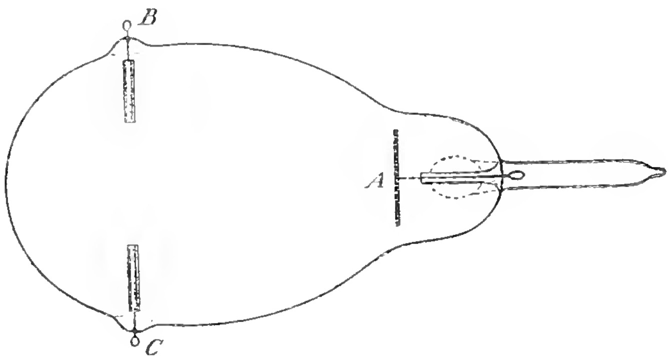


Fig. 2.

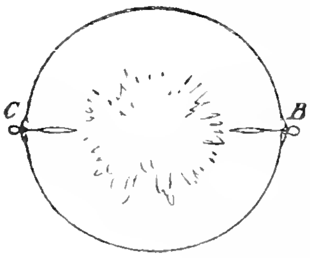
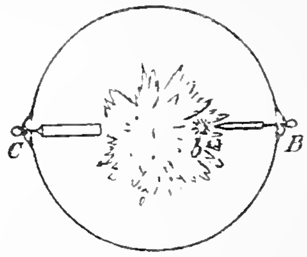


Fig. 3.



When B was connected to A so that both A and B were cathodes, C being anode, the shadow of B swelled out to an oval shape, some 22 millims. long and 18 millims. wide. The shadow of C was enlarged similarly if B was made anode, and C joined to A as cathode. The size of this oval shadow was found to depend on the nature of the connexion to the cathode. By introducing a bad conductor, such as a piece of damp wood or a highly exhausted vacuum tube into the conducting line, between the cathode and the electrode casting the shadow, the size of the shadow could be varied from normal dimensions up to the full oval. When either B or C was thus made cathode, the anode shadow shrank up altogether and disappeared, though the luminous spot at *c* or *b* remained.

When A was made anode, and B and C both cathodes, each was, as remarked above, surrounded by an oval luminous patch on the glass wall; which patch was brighter than the rest of the surface of the bulb, and was outlined with a still brighter marginal line. If B was disconnected, or its continuity with the cathode impaired by introducing a bad conductor, the patch surrounding it shrank, while that surrounding C expanded. If B was joined to the anode the patch surrounding it disappeared. When, as in fig. 4, B and C were joined through a rod of wood, to which the cathode wire from the coil was brought, the sizes of the two patches could be varied by sliding the wire along the wood. That patch expanded toward which the cathode wire was moved, while the other patch contracted.

The first conclusion to be drawn from these observations was that *the size of the cathodic shadow of an object depends upon its own electric state.** *If it is positively electrified the shadow contracts, if negatively the shadow expands.* The same result was found to occur when the electrification of the object was produced independently by use of an influence machine.

Fig. 4.

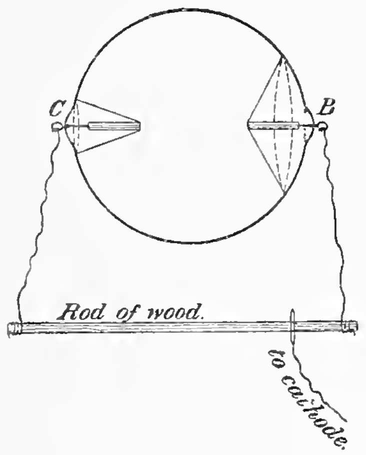


Fig. 5.

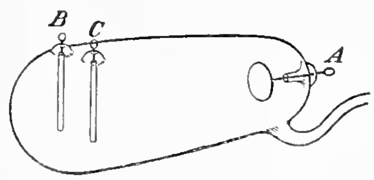
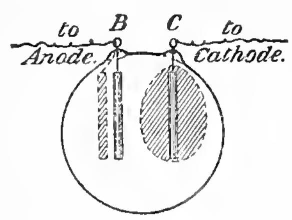


Fig. 6.



Tube [No. G 4], (fig. 5), was constructed to verify the above results. It also had three electrodes, one a slightly convex disk, A, at one end, the other two, B and C, short cylindrical wires inserted transversely to the tube, near together, and parallel to one another. The degree of exhaustion of this tube was made rather higher than that of the preceding. When electrode A was made cathode, shadows of B and C were cast on the broad end of the bulb. If B and C were both made anodes their shadows were slightly narrower than the geometric shadow, and became extremely well defined. When B was left as anode and C connected to the cathode through a rod of wood or through another vacuum-tube of high resistance, the shadow of C at once swelled out to an oval shape (fig. 6), while that of B was shifted a little sideways, as if repelled from B. If A was made anode and C cathode, a shadow of B was thrown upon the side wall of the tube. If then B was made anodic, by connecting it to A through a rod of wood, its shadow thinned down and became more brightly marginate. If B was made cathodic, by similarly joining it to C, its shadow widened out to an oval patch, while at the same time an oval shadow of C was cast on the opposite wall. By varying the resistance in the connexions, in the way described above, for the first-mentioned tube, the sizes of these two oval shadows could be varied, one enlarging when the other diminished. While the disk A was thus serving as anode, it also cast a shadow of itself upon the smaller end of the tube. This shadow shifted slightly sideways according as B or C was cathode.

* Crookes, in 'Phil. Trans.,' 1879, Part II., p. 648, has alluded to the widening of a cathode shadow, and to the production of a penumbra under an unsteady electrification of the object casting the shadow.

When B and C acted jointly as cathodes the shadow of A occupied a mean position; but under no circumstances were two shadows of A seen, or any appearance of overlapping shadows. These effects were repeated, using an influence machine to electrify the wires casting the shadows. A dry pile, ordinarily used to charge gold-leaf electroscopes, proved inoperative. It acted merely as a high resistance.

Tube [No. G 8], (fig. 7), has three small lateral disk electrodes, A, B, and C, and a central electrode D of aluminium wire, about 2 millims. thick. Electrodes A and B being connected as cathodes, and C as anode, the shadows of D were about of the geometrical size. The exhaustion was at this stage such that a spark would just pass in an alternative path between blunt points about 7 millims. apart.

When the wire D was made anodic its shadows at once became narrower. When is was made cathodic its shadow widened enormously, being about 16 millims. wide, with an ovate end.

When A and C were made cathodes, and B and the wire D anodes, there were produced two narrow shadows at *a* and *c* respectively, opposite the two cathodes. As the pump was worked, and the exhaustion increased, these shadows grew narrower, becoming mere lines, with a brighter spot of luminescence above the tip of each. Then the upper parts of these linear shadows closed up entirely, so that instead of presenting each, as at first, a dark line about 1 millim. broad, emarginate with a luminous edge, each now presented the appearance, except at the base, of a narrow bright luminescent line. At this stage the exhaustion was such that a spark would just not pass at a gap of 18 or 19 millims. in the alternative circuit. As exhaustion proceeded further, the luminescence on each edge began to overlap; and finally the shadows became two *bright* linear strips each about 1·5 millim. broad. The spark-gap at this stage, when the tube was sealed off, was 30 or 32 millims.

In this tube, then, the effect of making anodic the object which casts the shadow was to produce a deflexion of the cathode rays into the geometric shadow, even to overlapping; the result being to produce *negative shadows*, that is to say, shadows which appear bright upon a less bright background. It also appeared that *the enlargement of a shadow when the object is made cathodic, and the diminution of the shadow when the object is made anodic, both depend upon the degree of exhaustion of the tube; and both are augmented by raising the degree of exhaustion.*

These two effects are, however, unequal. The enlargement when cathodic exceeds by many times the diminution when anodic, under identical conditions of exhaustion and excitation of the tube.

These observations furnished an explanation of the luminous spot observed in tube (fig. 3) at the ends of the shadows of the lateral electrodes, and of the tapering form of those shadows when the electrodes were anodic. The surface when anodic deflects the cathode rays into the geometrical shadow; and, having been deflected, they continue in a new direction. As the surface of the bulb where the shadow falls is curved, the rays that cast the tip of the shadow have to travel a longer distance than those near the base of the shadow, which consequently becomes distorted to the extent of producing at the tip an overlap, with a luminous or negative shadow at that part, whilst the remainder of the shadow is of the usual dark or positive sort.

The preceding observations, and particularly the observation of the lateral shifting of the shadow of the wire B in tube [No. G 4], (fig. 6), when the parallel wire C was made cathodic, establish the following point :—*Cathode rays are capable of being deflected electrostatically; being apparently strongly repelled from a neighbouring cathodic surface, and less strongly deflected towards a neighbouring anodic surface.* Though no precise experiments were made to determine the shape of the path of a deflected cathode ray which passes near an anodic or a cathodic point, the observa-

Fig. 7.

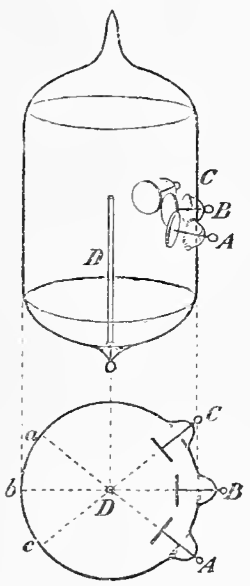


Fig. 8.

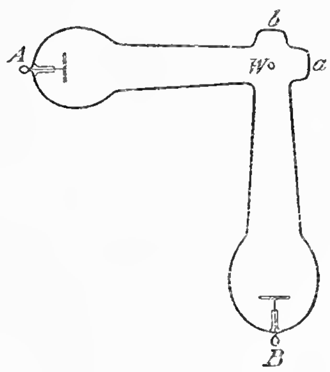
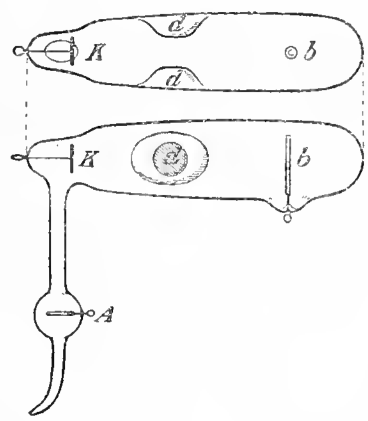


Fig. 9.



tions appeared to indicate a hyperbolic path. Incidentally, these experiments in which two shadows of one object were simultaneously produced from two cathodes, as in the case of tube [No. G 4], (fig. 7). described above, prove that two cathode beams are capable of passing through or penetrating one another, just as two beams of light will. This was further demonstrated by a special tube [No. G 6], (fig. 8), which has two arms at right angles, each ending in a bulb containing a small disk cathode. The object, an aluminium wire inserted at the point where the axes of the two arms intersect one another, cast two shadows, one on each of the tube-walls respectively opposite to the two cathodes. If one of these shadows was first produced alone no shifting of its position was seen when the second cathode was connected up to cast the second shadow.

2. *Electrostatic Deflexion of Cathode Rays by Conductors protected by Glass.*

In the preceding experiments the objects used for giving shadows were of metal, their electrified surfaces being exposed directly to the residual gases in the tube, and to the cathode rays. It was desirable to ascertain whether any such effects were produced when the conducting surface was shielded from direct action by the interposition of a non-conducting layer of glass. Attempts to deflect the cathodic shadows by holding charged bodies outside the tubes led to no definite result. In some cases it was possible by laying the finger on the outside of the tube to produce slight displacements, particularly if the cathode pole of the coil was earthed at the time, so that the finger acted cathodically. Tube [No. G 17], (fig. 9), was constructed to test this point. It is of a pear shape, with a small disk cathode, *k*, at one end, and an anode, *a*, in the side tube by which the bulb was connected to the pump. A wire, *b*, to cast a shadow, was inserted near the broad end, and two dimples or depressions, *d*, were impressed into the sides, leaving an internal distance of about 5 millims. between their faces. These depressions were covered externally with tinfoil. The wire *b* gave a shadow the size of which varied, as in previous experiments, according as the state of the wire was neutral, anodic, or cathodic. On each side of this shadow appeared a shadow of the dimple *d*. When the metal coatings of the two dimples were made cathodic, the space between their shadows decreased slightly. When made anodic, there was no measurable increase in the space between their shadows. When one was made anodic and the other cathodic, their shadows appeared to shift slightly as from the cathode side, and the shadow of the wire *b* was very slightly shifted in the same sense.

Tube [No. G 16], (fig. 10), was of a vertical pear shape,* with a small disk cathode of about 6 millims. diameter inserted at the side, and a similar disk anode at the bottom. Through the top was inserted a narrow glass tube, closed at the bottom, open at the top to receive mercury. This tube cast the usual shadow on the opposite wall, and gave identical shadows when empty and when filled. A wire was then inserted into the mercury to enable it to be electrified. At low degrees of exhaustion the shadow remained unchanged in size, whether the mercury thread within it was neutral, anodic, or cathodic. But, as the exhaustion was increased, a point was reached at which, almost suddenly, sensitiveness set in, and the size of the shadow became variable, contracting very slightly when the mercury was made anodic, expanding enormously when made cathodic. It was noticed that this change from the non-sensitive to the sensitive state occurred at the stage of exhaustion at which the "splash" phenomenon appeared on the bulb-wall opposite the cathode. It was also noticed that the sensitiveness depended upon the conditions of excitation, being greater when the break of the coil was lightly adjusted, so as to operate the coil with the spark-gap at 3 millims., than when tightened up to operate with the spark-gap at 15 or 20 millims. or more.

Several tubes were made of the general form of [No. C 2], (fig. 11), having a small disk cathode at one end, an anode in a side tube, and, as object to cast shadows, a tube containing mercury. These mercury tubes were made of several different

* This tube, and the succeeding one, were exhibited at the evening gathering of the Royal Society, October, 1896.

sizes and of different thicknesses of glass. All showed the same general set of phenomena. At low exhaustions there was little or no electrostatic deflexion by a glass-protected electrode, whether cathodic or anodic. But, at the stage of exhaustion where splashing sets in, the electrostatic deflexion of the shadow made its appearance, with resulting enlargement if the object were made cathodic. In several cases the mercury tube pierced, the mercury slowly oozing in minute drops into the bulb. When this had occurred, the shadow of the drop was electrostatically sensitive at exhaustions lower than that which was necessary to render sensitive the shadow of the glass-protected thread of mercury; the shadow assuming in consequence a grotesque nodular form.

It appears, then, that *the electrostatic deflexion of cathode rays by an electrified object is dependent upon the surface of that object, as to whether that surface is or is not conductive;* and that *for objects protected by a non-conducting layer there is a certain minimum stage of exhaustion below which they cause little or no electrostatic deflexion of the rays.*

3. *The "Splash" Phenomenon.*

Many CROOKES tubes show the phenomenon already twice alluded to, in which the glass surface opposite the cathode appears to be "splashed" by the cathodic discharge; creeping dendritic forms of an unstable kind appearing in the luminescence of the glass. This "splash" phenomenon is independent of the kind of glass used. It occurs with soda-glass, lead-glass, and uranium-glass tubes. It does not occur

Fig. 10.

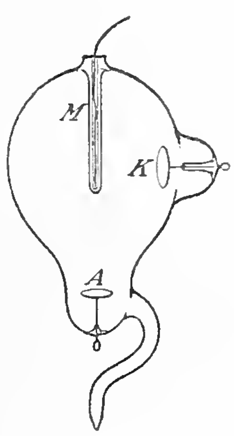


Fig. 11.

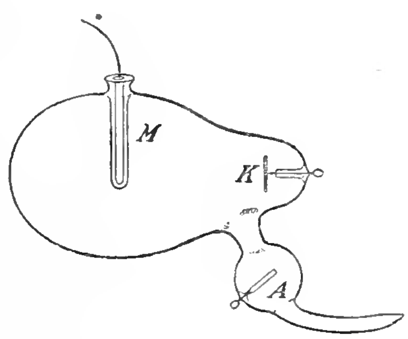
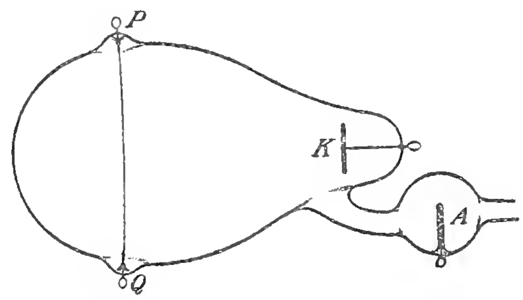


Fig. 12.



on an anticathodal surface of metal, nor, apparently, on an anticathodal glass surface, coated internally with plaster of Paris, or with powdered scheelite. It occurs at a particular stage of exhaustion a little below that needed for the production of RÖNTGEN rays, and at the stage, previously considered, at which there is a rapid increase in the electrostatic sensitiveness of the cathode rays. The dendritic forms assumed by the splash strikingly resemble the LICHTENBERG's figures (of the positive kind), but are in general less fine-grained. The spreading of the "splashes" is affected by electrostatic influences; they spread on the inner surface of the tube toward an anodic point, whether that point be internal or external to the glass wall. If a single "splash" is produced by a solitary discharge from the coil, the external surface of the tube at the part splashed shows an electrostatic state of charge evidenced by the adherence of dust.* The explanation of the phenomenon appears to be the following:—At this stage of exhaustion, the first portion of a cathodic discharge electrifies the inner surface of the glass where it strikes, giving it a negative charge, or making it temporarily cathodic. The presence of this cathodic charge electrostatically affects the next-advancing portion of the discharge, and causes it to strike the glass a little on one side, so further distorting the ray. This may occur in any direction from the central point and will obviously present an instability; the "splash" creeping outward from the centre, first in one direction, then in some other, ramifying as it spreads, and fading out almost as fast as it is formed.

4. *Cathode Shadows of Hot Wires.*

After the experiments described in § 2, in which threads of mercury in fine closed glass tubes were used to cast shadows, others were made to test the effects of electric currents passing in the object which casts the shadow. A tube was constructed resembling fig. 11, but having the bulb traversed by a narrow glass tube which opened to the air at both ends. This was filled with mercury, and connected to a small battery to pass currents through it. No effect was noticed that depended either on the direction or the strength of the current.

Another tube [No. C 4], (fig. 12), was made, having a thin bare platinum wire stretched across the bulb between inserted terminals, P, Q. This wire gave as its shadow in the cathode rays a fine black line, bordered by the usual margin of brighter luminescence. On sending through the wire a current from a small insulated battery of accumulators no effect was observed until the current had been so far increased as to make the wire red hot, when its shadow was observed to be rather wider at the end by which the current left than at the end by which it entered. On reversing the direction of the current, this effect also changed direction. If, under these circumstances, the wire was made anodic as a whole by connecting the insulated battery, or any part of its circuit, to the anode pole of the coil, the shadow of the wire at once changed to a luminous line (a negative shadow, in fact, as described in § 1 above), which showed no change on reversing the battery, and which was unaltered whether the current is on or off. On similarly making the wire cathodic, its shadow expanded to some 6 millims. wide; and again, no effect was perceptible on reversing the current, though, apparently, the wire when hot

* A similar observation has been made by VILLARI ('Rendiconti della R. Accademia dei Lincei,' vol. 5, May 17, 1896), who has investigated the external electrostatic state of RÖNTGEN tubes and GEISSLER tubes by the use of electroscopic powders.

yielded a slightly larger shadow than when cold. But this could not be determined with certainty.

5. *Attempts to Concentrate Cathode Rays.*

The concentration of cathode rays by the use of concave cathodes to focus the beam dates from the classic researches of CROOKES.* In the experiments now made it was sought to concentrate the rays by other means. The first of these means was reflexion from the surface of a non-conductor—glass—at a small grazing angle. A tube [No. G 20], (fig. 13), was prepared, within which was suspended by platinum links an inner funnel of glass, about 47 millims. long, having an internal diameter of 12 millims. at the larger, and 8 millims. at the smaller end. The suspension

Fig. 13.

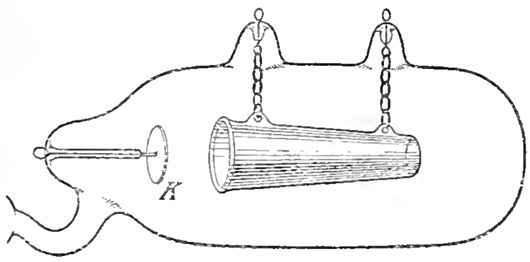


Fig. 14.

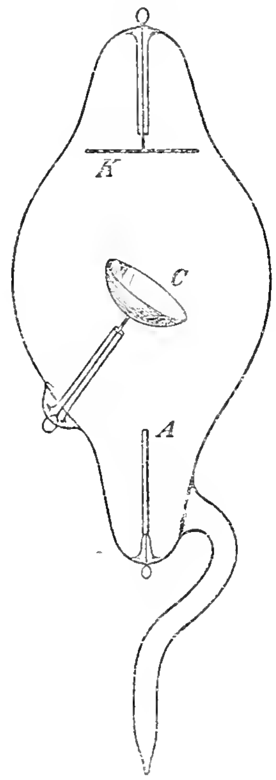
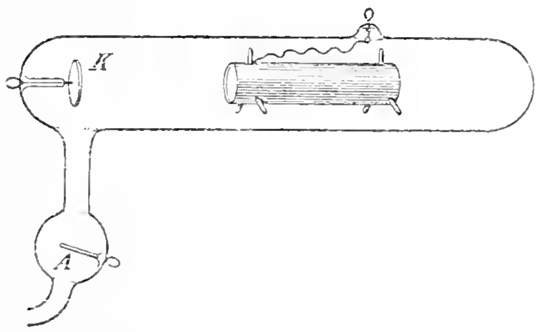


Fig. 15.



permitted the funnel to be swung aside by tilting the tube. This tube was exhausted to the point at which RÖNTGEN rays are but just emitted, the emission ceasing when the tube was warmed. To the eye there was no apparent difference in the brightness of the yellow-green luminescence of the anticathodal end of the tube whether the funnel were present or absent. The funnel itself cast a broad annular shadow, but it produced no difference in the brightness of the central patch within. On examining, by the aid of luminescible screens of platinocyanide of barium or of scheelite, the emission of RÖNTGEN rays from this central patch, no difference was perceptible in the luminosity, whether the funnel was absent or present. It did not concentrate the cathode rays.

* 'Phil. Trans.,' Part I., 1879, p. 142.

In the experiments made with tube [No. G 17], (fig. 9), described above, when the two external coatings of foil were made cathodic, no increase of brightness had been observed in the patch of luminosity on the end of the bulb, in spite of the narrowing of the luminous space between the two lateral shadows.

A tube [No. C 24], (fig. 14), was constructed, having a flat cathode, K, opposite to which was inserted obliquely, as an anticathode, a concave cup, C, of aluminium. A third electrode, A—an aluminium wire—was inserted at the further end of the bulb as anode. The cathode discharge was directed against this concave anticathode, at various degrees of exhaustion up to the highest when no spark could be sent through the tube. At no stage, however, was there any appearance, either by internal cones of rays, or by any special spot of luminescence on the glass wall, or by the evidence of a luminescible screen applied outside, of any concentration by the concave anticathode of rays of any kind.

Another tube [No. C 9], (fig. 15), was constructed, having an internal cylindrical tube of silver supported at each end by three projecting feet. This was not found to concentrate cathode rays that were passed along its axis. When it was itself made anodic its shadow was more sharply defined. But, when it was made cathodic, so far from any concentration being produced on the cathode rays directed along its axis, there appeared a new set of phenomena which are described below in § 8.

Another attempt to concentrate the rays by passing them along the axis of a helix of iron wire within the bulb, while a current traversing the wire produced a longitudinal magnetic field, is also narrated below. It also failed to produce any concentration of the cathode rays.

6. *Comparison of Cathode Shadows with* RÖNTGEN *Shadows. Production of Internal or Paracathodic Rays.*

In order to be able to compare together ordinary cathode shadows and the shadows produced on external luminescent screens by RÖNTGEN rays a number of tubes were constructed, having wires or other objects introduced for the purpose of casting shadows. One of these tubes was described at the meeting of the British Association at Liverpool.* A somewhat simpler tube [No. M 12] is depicted in fig. 16. It consists of a pear-shaped bulb having a concave cathode, C, focussing upon an oblique anticathode,† A, at its upper end. A lateral tube into which the upper end of the pear-shaped bulb is united has an aluminium wire, B, inserted as an object to cast shadows. This aluminium wire, about 3 millims. in diameter and 17 millims. long, is mounted upon a platinum wire fused in through the tube-wall.

At fairly low degrees of exhaustion the wire B casts a shadow upon the tube-end

* 'British Association Report,' 1896. See also 'Electrical Review,' p. 417, September 25, and p. 432, October 2, 1896.

† The term *anticathode* signifies an object upon which cathode rays are directed, as against a target. See 'Nature,' March 12, 1896, p. 437.

p, when C is made cathode. It is immaterial whether B or A serves as anode, or whether an anode in a bulb in the exhausting-tube is used. The shape and position of this shadow, which is dark against the yellow-green luminescence of the tube-wall, indicate distinctly that it is cast by rays proceeding from A. This quasi-cathodic shadow is cast by rays proceeding from A, even when A is anode, and under conditions which preclude the possibility of an oscillatory discharge. No shadow is produced if the cone of cathode rays proceeding from C is diverted by the influence of an external magnet from falling on the anticathode A. It is therefore clearly due either to cathode rays reflected at A, or to some other rays, resembling cathode rays, which are originated at A under the impact of the cathode rays from C.* Specular reflexion of cathode rays is not known to exist, and has not been observed in any of these tubes. No trace is seen of any blue cone or beam that might be a geometrical prolongation of the reflected cathode cone or beam. If reflexion is here operative, it is diffuse, not specular. But if specular, it differs from ordinary specular reflexion in two respects : (1) the shadows have no penumbra but are sharply defined, even though the anticathode surface is relatively large ; (2) the distribution of the rays differs from that of ordinary specular reflexion.

Fig. 16.

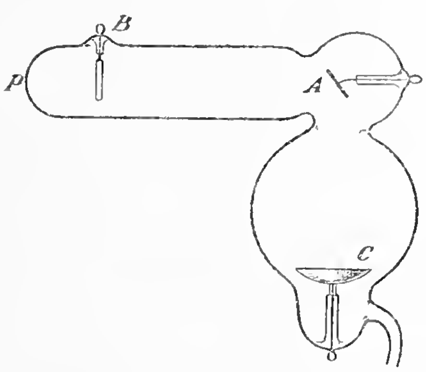
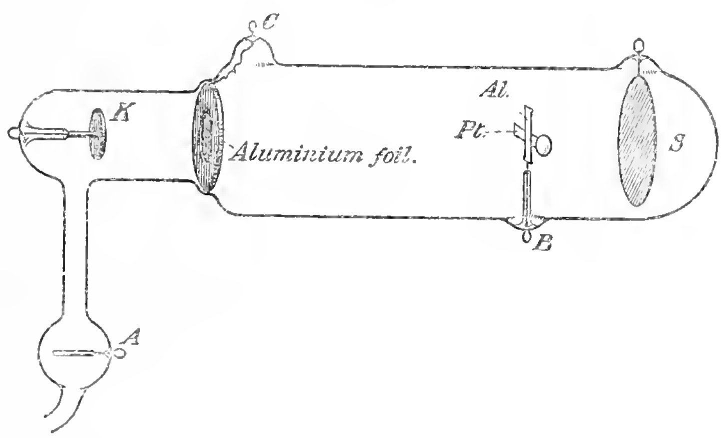


Fig. 17.



The shadow of B thus thrown on the tube-wall at p can be observed at a degree of exhaustion quite insufficient to excite Röntgen rays ; it can also be observed up to the highest exhaustion. Like the shadow of ordinary cathode rays, it can be deflected by a magnet placed over the tube between the object and the tube-end. It is also susceptible to electrostatic deflexion ; the shadow expanding when B is made cathodic, contracting slightly when made anodic. The colour of the luminescence of the glass under the impact of these rays is identical with that produced by ordinary cathode rays.

If, now, a luminescible screen of platino-cyanide of barium, or one of scheelite, is

* This conclusion was reached by Wiedemann and Ebert as the result of experiments yet unpublished. See their paper "Ueber elektrische Entladungen," in the 'Sitzungsberichte der Physikal.-med. Societät zu Erlangen,' December 14, 1891.

held outside the tube near p, there will appear upon it, if the exhaustion is sufficiently high, a second shadow cast by the RÖNTGEN rays; the size and position of this shadow being such as to indicate that it is cast by rays which have their point of origin also at A, and which have traversed the glass wall of the tube. If no perturbing magnetic or electrostatic influences are present, these two shadows lie geometrically on the same lines projected from A as origin. Both are sharply defined; the internal one more so than the external one. But the external shadow due to RÖNTGEN rays does not shift when a magnet is placed between B and p. Neither does the external RÖNTGEN shadow change its shape or size when B is made anodic or cathodic. If the shadows are simultaneously observed it will be seen that one and the same object, illuminated from one and the same point of origin, is capable of casting two shadows of different shape in different directions at the same time. It is clear that there are present two kinds of rays, which differ in their deflectibility by magnetic and by electrostatic forces, and in their power of penetrating glass.

These internal rays which are deflectible are not, however, ordinary cathode rays. Cathode rays proper, at a sufficiently high exhaustion, possess the characteristic property of exciting RÖNTGEN rays wherever they impinge upon solid, or as ROITI has shown,* upon liquid matter. These internal rays fail to exhibit, either at high or low exhaustion, any trace of this property. There are produced, as is shown below, under certain conditions, some other internal rays, which also differ from ordinary cathode rays. Hence it becomes necessary to distinguish the different species by adopting an appropriate nomenclature. The ordinary cathode rays may be called *ortho-cathodic*; while the internal rays described above, which are emitted along with RÖNTGEN rays at the surface of the anticathode, may be termed *para-cathodic*.

The emission of these para-cathodic rays demands especial attention. They may be observed in any RÖNTGEN ray tube of the focus type. As pointed out by the author,† in April, 1896, the emission of RÖNTGEN rays from the surface of a plane anticathode follows a distribution entirely different from that of the emission of any known kind of light. It does not follow LAMBERT's law of the cosine, the intensity remaining nearly uniform right up to a grazing angle, where it abruptly ends. This is demonstrated by photometric measurements made on the luminosity of a barium platino-cyanide screen placed near to the bulb. As viewed in such a screen the emission of RÖNTGEN rays is confined to the region in front of the plane of the anticathode; the whole hemispherical space in front being filled with them, while the whole hemispherical space behind is devoid of them;‡ a sharp delimitation

* Mem. Accad. Lincei, s. V., vol. 2, July, 1896.

† 'Comptes Rendus,' *loc. cit.* See also 'Philosophical Magazine,' August, 1896, p. 156, and 'Proc. Royal Institution,' May 8, 1896.

‡ This is the state of things when the exhaustion is sufficient. But the author has many times observed, and has put on record ('Comptes Rendus,' *loc. cit.*) the circumstance that during exhaustion, and at the stage at which RÖNTGEN rays just first appear to be emitted, they are emitted from both

between brightness and darkness (as viewed in the screen) occurring in the plane of the anticathodal surface. As was pointed out in a communication to the Physical Society,* and often since noticed by other observers, a similar oblique delimitation is visible in the yellow-green luminescence upon the walls of the bulb. At the time it was supposed that this internal luminescence extending over the region traversed by the RÖNTGEN rays was caused by them on their way through the glass wall of the bulb. That they are not due to RÖNTGEN rays may be, however, readily shown. If a magnet-pole be brought close to the bulb near the delimiting edge of the patch of luminescence upon the glass, that edge is seen to be distorted. It is due therefore to rays that possess magnetic deflectibility. It is also possible to produce an electrostatic distortion of this delimiting edge. But if with a barium platino-cyanide screen one observes the corresponding delimiting edge of the RÖNTGEN ray emission in the same oblique plane, one finds that it undergoes neither electrostatic nor magnetic deflexion. A small displacement in some cases to be noticed is due to want of exact complaneity of the anticathode surface, and to a displacement by the magnet of the focus of the incident (ortho-)cathodic beam.

It therefore appears that from the anticathode surface there are emitted simultaneously with the RÖNTGEN *rays, and with a similar abnormal lateral distribution, para-cathodic rays which differ from the* RÖNTGEN *rays in respect of their power of penetration, as well in being electrostatically and magnetically deflectible. They also differ from the* RÖNTGEN *rays in being emitted at a lower degree of exhaustion than is necessary for the production of the former, and from ordinary (ortho-)cathodic rays in not exciting* RÖNTGEN *rays where they impinge on a solid surface.*

From the similarity in the abnormal distribution of the para-cathodic rays and of the RÖNTGEN rays, it may be inferred that the physical processes concerned in their emission at the anticathode are similar.

7. *Sifting of Cathode Rays.*

It has long been known that cathode rays at different stages of exhaustion of the tube, and excited under different electromotive forces, differ in character from one another. The changes in magnetic deflectibility during the process of exhaustion, noticed by CROOKES,† are familiar. The cathode rays observed outside the CROOKES tube, by LENARD,‡ differ from those within in several physical respects, and also apparently from one another under different conditions of production. WIEDEMANN and EBERT in particular have dwelt on the heterogeneity of cathode rays, and

front and back of the anticathode, which, as viewed edgeways in a luminescent screen, appears a a dark line between two dim luminous regions. See also a case recorded in § 7 below.

* 'Proc. Physical Society of London,' June 12, 1896, and 'Philosophical Magazine,' August, 1896.

† 'Philosophical Transactions,' 1879, Part I., p. 160.

‡ WIEDEMANN'S 'Annalen,' vol. 51, p. 225, 1894.

on the operation* of the deflecting magnet in separating the different elements of a bundle emitted simultaneously at a cathode. The remarkable success of LENARD in transmitting cathode rays through a window of aluminium leaf into the open air naturally suggested the possibility of partially separating within the tube itself the constituents of a heterogeneous cathode beam, by interposing screens or films to sift out the less penetrating from the more penetrating parts.

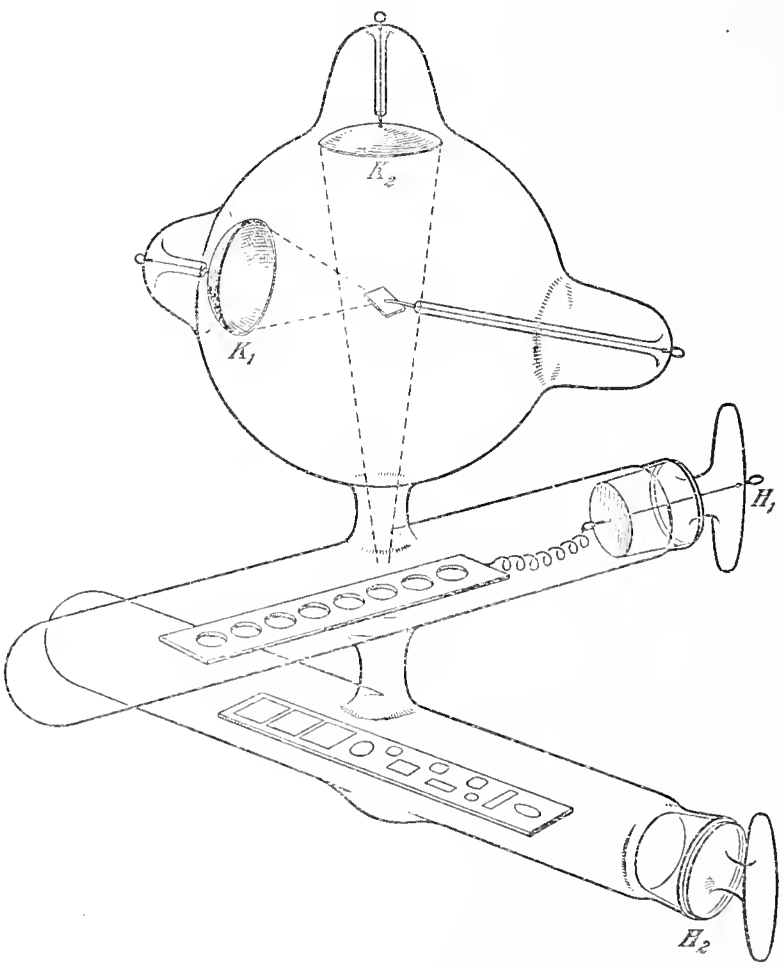
Accordingly, a tube [No. G 19], (fig. 17), was prepared, having at one end a small disk cathode, K, of aluminium, and an aluminium wire anode, A, in a bulb in the lateral exhausting tube. An interior luminescible screen, S, of mica coated with scheelite was inserted near the far end of the tube, and in front of this was erected, upon a conducting support, as an object to cast shadows, a cross, one arm of which was of aluminium foil, another of platinum foil, whilst the third was of platinum upon which a globule of glass was fused. Near the cathode the tube was fashioned with a shoulder, into which loosely dropped a ring of lead, carrying a diaphragm of aluminium foil. A platinum wire, C, inserted through the glass, enabled this ring and diaphragm, when in place, to be connected up to anode or cathode if desired. The ring and diaphragm being removable by tilting the tube, ordinary cathodic shadows could be produced for comparison. The tube was exhausted in the usual way to a high degree, and sealed off the pump. When the diaphragm was shaken out of its seat to allow a clear passage of the cathode rays, the shadows of the object upon the screen, S, were very sharp and distinct, and all parts of the shadow were equally dark. When the diaphragm was replaced, there was a shadow also, provided either B or S was earthed, or connected to the anode through a resistance, or made itself the anode. If C (the diaphragm) was connected to the cathode, the shadow became more brilliant, but with so rapid volatilization of lead, or of occluded gases, that this connexion was only possible for a second or two. The shadow was in all cases deflectible by the magnet, and appeared to be more readily deflected if the magnet was applied over the region between the diaphragm and the object than if applied between the cathode and the diaphragm. Although the diaphragm fitted fairly well in its seat, which was lined with aluminium foil, and permitted no direct cathode discharge around its edges, there was a distinct luminescence of the glass at the far end of the bulb around the edges of the mica screen, which cast a well-defined, dim shadow. The colour of this luminescence was the usual yellow-green, but there was a singular dark-orange luminescence over the end of the bulb where screened by the mica disk. When the aluminium diaphragm was connected to the cathode, and the platinum support of the mica screen was connected to the anode, a similar orange luminescence appeared in the neighbourhood of the cathode, K, during the brief moments that the operation could be continued. Further reference is made below to this second species of luminescence.

* *Op. cit.* This dispersion was also observed by the author in May, 1896, see 'Proc. Oxford University Junior Scientific Club,' May 26, 1896.

The tube was opened and resealed to the pump in a vertical position, so that the diaphragm remained constantly in its seat. The electrode K was used as cathode, and the support S as anode, during the exhaustion, which was continued until the spark-gap in the alternative circuit could be increased to about 25 millims. There were now clear shadows of aluminium, platinum, and glass, all equally dark. But on allowing the discharge to pass for 8 or 10 seconds, during which time the exhaustion diminished, the shadow of the aluminium arm faded out, leaving those of the arms of platinum and glass as dark as before. At this stage of things a barium platino-cyanide screen, placed directly over the bulbous end of the tube, showed a faint luminosity, but no shadow of the cross could be distinguished. Exhaustion was carried gradually higher, until the spark-gap was 4 inches; but at no stage could the shadow of the platinum upon the mica screen be made darker than that of the aluminium without first letting the discharge pass for a few seconds. The glass walls of the tube showed yellow-green luminescence only on the lowest region between the cathode and the diaphragm, save a slight trace on the upper end of the tube, which also showed yellow-green. Yet, on examining the side of the tube with a barium platino-cyanide screen, it was found that Röntgen rays were being emitted both above and below the diaphragm, the luminescence being almost as bright above as below. The place where the diaphragm lay across the tube was marked by a strong clean black line between two regions of almost equal luminosity. Hence it would seem that in this case the rays which produce the yellow-green luminescence are stopped by the aluminium diaphragm, whilst other rays pass through, which are competent both to cast an internal deflectible shadow upon a scheelite screen, and to produce Röntgen rays, which cause luminescence outside the tube on a platino-cyanide screen.

To test the sifting action of aluminium foil more thoroughly, the tube [No. M 14], (fig. 18), was designed. The bulb is provided with two concave cathodes, one, K_1, at the side, to focus upon a small oblique anticathode of platinum at the centre, the other, K_2, at the summit, being larger and shallower, intended to focus past the anti-cathode to a point in the narrow aperture at the bottom of the bulb. The object of this design was to allow either ordinary (ortho-)cathodic rays to be thrown directly down from K_2, or by the use of the lateral cathode K_1 and the oblique target, to throw down Röntgen rays, accompanied by para-cathodic rays. Below the aperture at the bottom of the bulb a horizontal glass tube, H_1, was fixed, and below this, with a short vertical neck intervening, a second horizontal tube, H_2, at right angles to the first. Each of these tubes was fitted at one end with a ground glass stopper, their other ends being closed. A platinum wire was sealed in through the stopper of H_1. Nitrate of silver was used as a cement to make the stoppers vacuum-tight. In the upper of the two horizontal tubes was laid a short slip of sheet-lead, having eight holes punched in it. These holes were covered with pieces of aluminium foil in various thicknesses, to act as screens or filters to sift the rays. In the experiments to be described only the thinnest of these filters was used; it was a leaf about 0·07 millim. thick. The leaden slip was itself connected by a thin spiral of platinum wire to the wire leading out through the stopper. Into the lower horizontal tube was introduced a similar loose slip of sheet-lead, carrying upon its upper surface a number of fluorescible materials: scheelite, ruby, jacinth, diamond, Iceland-spar, fluor-spar, calcium sulphide, zinc sulphide (SIDOT's blende), and a preparation of calcium carbonate, containing in solid solution about 1½ per cent. of manganese carbonate. The disposition of the two horizontal tubes at right angles to one another enabled the two lead carriers to be moved, by tapping, independently of one another, while the

Fig. 18.



tube was connected to the pump. This tube was used for sixteen consecutive days upon the pump. It was several times pumped out to the point at which RÖNTGEN rays are emitted. When left to itself, the vacuum slowly deteriorated, the tube relapsing in two days to the stage below that at which the yellow-green luminescence appears. Various observations were made on the luminescibility of the materials enumerated above; but these had little direct bearing on the point now in question. When K_2 was used to produce ortho-cathodic rays, all the materials named luminesced brightly, most of them doing so even when the state of exhaustion was too low for the production of the yellow-green luminescence of the glass. They all, also, luminesced, though less brightly, when K_1 was used to produce para-cathodic rays. A horse-shoe magnet, placed with its poles on either side of the neck joining the two horizontal tubes, deflected the rays. When the screen of aluminium foil was shaken into its place to intercept the rays, the luminescence of almost all the above-mentioned materials was stopped. The scheelite proved to be the most luminescible of them, but no effect was produced upon it at any exhaustion up to the highest, unless the screen was itself also made cathodic by joining the lead slide in H_1 through a resistance to the cathode pole. The luminescence of the scheelite under these conditions was faint, and the tint, instead of the usual bluish-green, was of a lavender colour. The magnet did not seem to affect the production of this luminescence. Under no circumstances did the ruby luminesce when the aluminium screen was present.

A narrow aperture was punctured with a penknife through one of the thicker screens in the lead slide, and the effect was observed of throwing ortho-cathodic and para-cathodic rays through this slit upon the luminescible materials below. If the screen was itself neutral or anodic the luminescent effects of the ray that traversed the screen were exactly the same in kind as those produced on the same materials when the screen was absent. With para-cathodic rays the effects were the same as those with ortho-cathodic, but fainter, and in the case of several materials, particularly the ruby, so faint as to be practically invisible. When, however, the perforated screen was made cathodic, somewhat different results followed. The cathode ray, which was thus filtered through a slit in a negatively electrified or cathodic screen, no longer produced an effect identical in kind; the luminescence was, in general, weaker, but the tints differed, being in general duller.

The action of the magnet on rays filtered through a narrow hole in the diaphragm was carefully observed, a patch of the leaden surface of the slide, H_2, coated with powdered scheelite, being used as a luminescent screen to watch the effects. The beam of cathode rays streaming downward from K_2, and falling upon the perforated screen, was intercepted save the small part which passed through the aperture. When this small beam entered the magnetic field between the magnet-poles it was spread out, as described by WIEDEMANN and EBERT,* and by the author,† several patches of luminescence appearing on the scheelite surface, where it was struck by rays of different degrees of deflectibility. The exhaustion was varied while the magnet remained in place, when it appeared that the deflexion of any given ray did not depend on the degree of exhaustion. But as the vacuum was carried to a higher point the more-deflected patches of luminosity died out, while the less-deflected patches persisted. This is not inconsistent with the observation of CROOKES,‡ who

* *Op. cit.*

† See *supra*. The above apparatus was made early in June, 1896, before the publication of the researches of M. BIRKELAND on the magnetic spectrum. The results were briefly described by the author at the British Association meeting in September, 1896.

‡ 'Phil. Trans.,' 1879, Part I., p. 160 ('The Bakerian Lecture,' Art. 577).

found the curved cathode beam across a tube to become straighter as the exhaustion proceeded. From these experiments it appeared :—

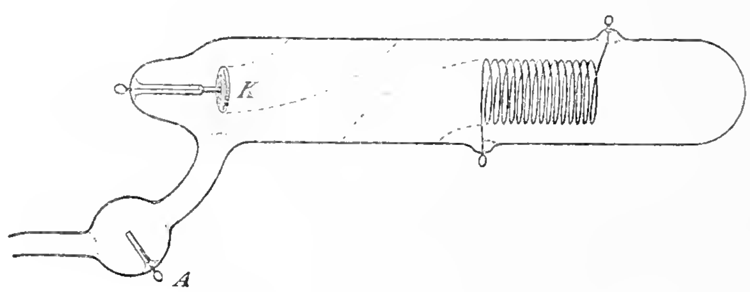
(1.) *That the various constituents of a heterogeneous cathode beam are emitted in various proportions at different degrees of exhaustion ;* (2) *That in the cathode rays emitted at higher degrees of exhaustion there is a greater proportion of the less-deflectible rays ;* (3) *That the least-deflecitble rays are those which most readily penetrate through a perforated screen when that screen is itself made cathodic.*

8 *Effect of a Negatively-electrified Screen on Cathode Rays, Double Fluorescence. Dia-cathodic Rays.*

Attention having been directed by previous experiments to the particular effects produced—notably in tube [No. M_{14}], (fig. 18)—by the interposition of a screen, which was itself made cathodic, other tubes were prepared to aid in examining these effects.

In tube [No. C 10], (fig. 19), there was inserted a helix, made of a dozen turns of bare iron wire, the ends of which were welded to platinum terminals passing through the glass. The cathode, K, was a flat disk ; and there was an anode in a lateral bulb in the exhaust tube. The terminal turn of the helix, at the end nearest the cathode,

Fig. 19.

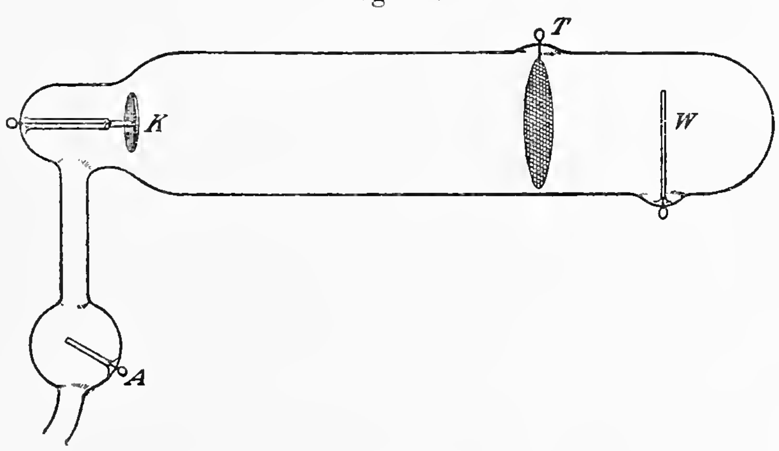


was brought down across the end face of the spiral, so that its cathodic shadow on the end wall of the tube appeared as a vertical diametral line across the circular outline of the helix. One purpose in constructing the tube thus was to test the effect of a longitudinal magnetic field upon the cathode rays. When a current was sent through the helix, the ampere-turns being about 140, and the intensity of the field nearly 60 C.G.S. units, the shadow of the diametral wire was rotated through about 20°. When the wire helix was made cathodic, the yellow-green luminescence of the glass was (as in the case also of tube [No. G 19], described above) confined to the portion of the tube between the cathode and the nearest end of the helix, no yellow-green luminescence being now produced on the tube end, and almost none on that part of the tube which covered the helix. But there now appeared a blue cone of rays proceeding from the inside of the further end of the helix, and extending thence to the far end of the tube. The tube-wall at this end now showed the dark orange luminescence which has been more than once alluded to in the preceding paragraphs as casually occurring. This phenomenon is a second species of fluorescence of the glass, and is always produced, provided the exhaustion be sufficiently high, when a cathode beam is directed against glass through a thin screen or through an aperture in a metallic screen which is itself made cathodic. This second fluorescence was examined with the spectroscope, when its light was found to consist simply of the D-lines of sodium.

The effect of an external magnet upon this tube was remarkable. When the horse-shoe was placed vertically, with its poles on either side, over the tube between the cathode and the helix, the yellow-green luminescence on the glass was driven into two portions ; one, apparently due to rays proceeding from the cathode proper, being deflected up to the upper side of the tube ; the other, apparently due to rays proceeding from the near end of the helix, being deflected down to the lower side of the tube. Or, on reversing the poles of the magnet, these deflexions were reversed. But in each case the blue cone proceeding from the far end of the helix appeared to be slightly deflected downwards. When the magnet was, however, brought down over the further part of the tube, so that its poles were one on each side of the tube, with the blue cone of rays between, the cone of rays was absolutely unaffected. That these rays were really rays of some kind was proved by the circumstance that they could cast shadows. A scrap of glass which accidentally remained inside the tube was observed to cast a shadow behind itself, none of the tawny fluorescence appearing in the shadowed portion.

To investigate the phenomenon further, tube [No. C 11], (fig. 20), was then made. Opposite the cathode there was introduced a screen of iron wire gauze, mounted upon

Fig. 20.



a platinum terminal, T, which passed through the glass. Between this screen and the tube-end was inserted a vertical wire, W, as an object to cast a shadow. If the gauze screen was made anode, or was left neutral, the usual sharp shadow of it was cast on the tube-wall by the (ortho-)cathodic rays, the whole tube being lit up with yellow-green fluorescence except where the lines of shadow fell. When the gauze screen was made cathodic that part only of the tube which was between the cathode and the screen showed the yellow-green tint, but, beyond the screen, there was the blue cone of rays, producing on the remainder of the tube the second, or dark-orange, fluorescence, as before, and in this second fluorescence the shadow of the wire could be seen upon the glass. This shadow was not displaced by a magnet held over the blue cone; it was, however, displaced if the magnet was held over the tube at the part where the yellow-green fluorescence showed itself between the cathode and the screen. The rays of this blue cone did not appear to be electrostatically sensitive, since the size of the shadow of the wire was not affected by electrifying the wire. Another feature which had been noticed with the preceding tube, but was exceedingly striking in the present one, was the difference in the situation of the two kinds of fluorescence. In the present tube, with its thin gauze screen, the two kinds of fluorescence met in the plane of the screen, and it was evident that while the fluorescence of the second, or dark-orange, kind was exactly confined to the inner surface of the glass, the fluorescence of the first, or yellow-green, kind was not so confined, but extended right through the glass. This seemed at first to be an optical illusion, but careful scrutiny proved it to be really so. It is most suggestive to find from spectroscopic evidence that both kinds of fluorescence are referable to the same element—the sodium of the glass employed. The circumstance that the rays last described should excite the emission of light giving a spectrum of so totally different a character, is itself sufficient to justify their being considered as different from the ordinary cathode rays. It is, therefore, proposed to distinguish them by the name *dia-cathodic rays*.

These observations may be summed up as follows :—

(1.) *When cathode rays fall upon a perforated metallic screen, which is itself made cathodic, or upon a tubular cathode, there emerge beyond the latter some rays, here termed dia-cathodic, which are incapable of exciting the ordinary cathodo-luminescence.*

(2.) *These dia-cathodic rays are not themselves directly deflected by a magnet.*

(3.) *They are capable of exciting a different kind of luminescence, the luminescent surface emitting light which, in the case of sodium glass, shows a gas spectrum.*

(4.) *They can cast shadows of intervening objects.*

[Note added November 7, 1897.—Recent further examination of the rays, here termed diacathodic, has shown me that they are very similar in their properties to, if not identical with, the "Kanalstrahlen," which GOLDSTEIN has found to be projected backward from cathodes. I have not yet been able to determine whether these diacathodic rays always accompany the ortho-cathodic rays or not. Observations on the yellow-green fluorescence of the first kind, made through revolving slits or in a rotating mirror, show that its colour changes before it dies away, becoming orange. This may be due to fatigue, however, and not to the greater persistence of emission of a different kind of ray.—S.P.T.]

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ERRATUM.

P. 226, line 16, *for* $Ar \cos\theta$ *read* $Ar^2(3\cos^2\theta - 1)$, *and for* $Br^{-2}\cos\theta$ *read* $Br^{-3}(3\cos^2\theta - 1)$, *and replace the remainde of the sentence by the words* where the values of A, B, A , B are determined by the surface conditions

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