Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. June.	Max.	Min.	8½ a.m.	9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	Max.	Min.	9½ a.m.	8½ p.m.	1 p.m.							
1.	30·187	30·153	29·66	30·10	30·11	30·14	30·13	79	46	62	76	45	56	54	e.	calm	sw.	sw.	......	......	......	......
☽ 2.	30·210	30·188	29·71	30·10	30·11	30·12	30·19	82	43	70	77½	50	68	61	e.	calm	sw.	sw.	......	......	......	......
3.	30·216	30·177	29·65	30·16	30·12	30·18	30·13	85	45	73	80½	51	60	59	e.	calm	sw.	e.	......	......	......	......
4.	30·178	30·159	29·60	30·10	30·09	30·14	30·09	83	49	74	80	55½	68	57	e.	calm	sw.	se.	......	......	......	......
5.	30·143	30·115	29·56	30·08	30·04	30·03	30·10	84	49	73	80	51	75	56	e.	calm	se.	w.	......	......	......	......
6.	30·084	30·035	29·48	30·00	29·98	30·04	30·06	89	53	76	77½	53	56	56	se.	calm	sw.	e.	......	......	......	·01
7.	29·978	29·883	29·33	29·95	29·73	30·07	30·00	90	56	78	80½	56½	55	53	sw.	calm	nne.	se.	......	......	......	·04
8.	29·871	29·806	29·29	29·80	29·71	29·91	29·80	79	50	63·5	65½	56	55½	53	sw.	calm	s.	se.	......	......	0·00	......
○ 9.	29·835	29·772	29·19	29·67	29·67	29·68	29·60	77	51	64	69	52½	56	56	sw.	calm	s.	s.	......	......	......	......
10.	29·995	29·912	29·34	29·70	29·68	29·65	29·64	77	56	67	67½	57	60	53	sw.	w.	s.	se.	......	......	......	·02
11.	30·195	30·149	29·53	29·97	30·03	29·83	30·00	81	53	72	66	53	54½	53	w.	w.	s.	w.	......	......	......	·30
12.	30·222	30·126	29·64	30·09	30·05	30·02	29·90	85	50	70	70	47½	60	56	w.	calm	ssw.	sw.	......	......	......	·03
13.	30·096	30·094	29·55	30·04	30·04	29·95	30·01	86	52	73	70	56	57	55	ne.	calm	w.	sw.	......	......	......	......
14.	30·106	30·074	29·47	30·07	30·04	30·04	30·07	82	50	77	75	55	56	52½	e.	calm	w.	sw.	......	......	......	......
15.	30·243	30·155	29·52	30·12	30·18	30·10	30·20	87	56	72	75	52	58	54	e.	calm	wnw.	w.	......	......	0·15	......
☾ 16.	30·320	30·289	29·64	30·25	30·25	30·26	30·29	87	50	77·5	79	50	56	54	e.	calm	se.	......	......	......	......	......
17.	30·314	30·243	29·65	30·24	30·20	30·26	30·23	88	53	76	83	51½	69	61	e.	calm	se.	w.	......	......	......	......
18.	30·210	30·145	29·59	30·16	30·09	30·21	30·12	88	54	76	83	58	70	62	se.	calm	sw.	se.	......	......	......	......
19.	30·106	30·054	29·45	30·04	30·04	30·03	30·22	93	53	75	82	59½	63½	50½	se.	calm	sw–n.	nnw.	......	......	......	·22
20.	30·181	30·087	29·52	30·18	30·22	30·32	30·30	87	62	68	69½	57½	54	49	e.	n.	ne.	e.	......	......	......	......
21.	30·190	30·061	29·65	30·16	30·02	30·20	30·05	81	56	68	77	48	57	46½	e.	calm	e.–s.	se.	......	......	......	......
22.	29·957	29·649	29·40	29·88	29·70	29·96	29·78	93	59	66	81	56	58½	56½	se.	calm	sw.	se.	·59	......	......	·18
23.	29·644	29·577	28·87	29·40	29·30	29·61	29·27	69	48	66	65	56	58	57	sw.	calm	w.	e.	·05	·32	0·10	·17
24.	29·517	29·401	28·95	29·22	29·30	29·12	29·29	67	48	64	61	48	54	53	sw.	w.	w.	......	......	·13	......	·16
25.	29·643	29·497	28·99	29·39	29·48	29·48	29·65	70	45	62	64	43	59½	53½	w.	calm	ssw.	e.	......	......	......	......
26.	29·681	29·647	29·22	29·48	29·41	29·69	29·67	67	54	63	59	44½	58½	54	sw.	calm	sse.	ese.	·09	·63	......	......
27.	29·797	29·629	29·12	29·44	29·54	29·57	29·62	73	50	65·5	65½	51	55½	54	sw.	calm	ssw.	se.	·01	......	......	·12
28.	29·850	29·800	29·32	29·61	29·43	29·70	29·62	74	58	66	66½	55	60	55½	sw.	calm	ssw.	se.	·03	......	......	......
29.	29·814	29·786	29·21	29·47	29·40	29·46	29·34	76	57	68	64	55	62	56	sw.	w.	sw.	se.	......	......	......	·35
30.	29·978	29·888	29·27	29·49	29·65	29·33	29·36	75	48	67	62	54	62	55	sw.	w.	wsw.	s.	·03	......	4·60	......
Mean.	30·025	29·955	29·41	29·878	29·853	29·876	29·891	81·46	51·80	69·7	72·3	52·6	59·75	54·86					0·80	1·08	4·85	1·60

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

SEPTEMBER 1846.

XXVII. *On the Magnetic Affection of Light, and on the Distinction between the Ferromagnetic and Diamagnetic Conditions of Matter.* By MICHAEL FARADAY, *F.R.S., Foreign Associate of the Academy of Sciences, &c.**

WHEN a ray of polarized light and lines of magnetic force pass simultaneously and parallel to each other through a transparent solid or liquid medium not possessing forces of double refraction, the ray is rotated according to a simple law of action, which I have expressed in the last part of the Philosophical Transactions†. When such a ray passes through certain specimens of rock crystal, oil of turpentine, &c., it is also rotated according to a natural law well-known, without any reference to magnetic force. A very striking distinction exists between these two cases of rotation, though they at first appear to be the same; for the former rotation is dependent in its direction upon the lines of magnetic force, and not upon the position of the observer or the course of the ray of light, whereas the latter is dependent upon the position of the observer or the course of the ray.

Upon consideration it appeared that the peculiar character of the magnetic rotation might be made available in exalting the final effect of the magnetic force upon the ray, and also in demonstrating many important points in a more marked manner and higher degree than had yet been possible; and upon referring the idea to experiment, it was found to be true. The following pages contain some of the results.

A parallelopiped of heavy glass 0·7 of an inch square and 2·5 inches long, had the two ends polished and silvered. The silvering was then removed from a space about 0·1 of an inch wide along one of the edges of one end, and also from a corre-

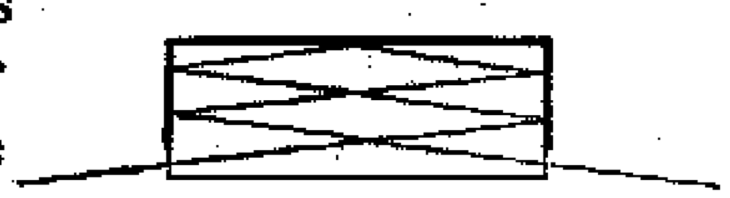
* Communicated by the Author.

† 1846, part i. pp. 4, 5. [Phil. Mag. vol. xxviii. pp. 298, 299.]

sponding space on the other end, except that the parts cleared were on the contrary sides of the parallelopiped; so that each end was furnished with a good plane reflector, but these overlapped each other (fig. 2). In consequence of this arrangement, a ray of light could be transmitted diagonally across the length of the piece of glass; or the ray, after entering at one end, could be reflected two or more times within the glass and then passed out.

A similar piece of heavy glass was silvered at the two ends and one side of the prism; and the silvering was then removed at the ends for the space of 0·1 of an inch from those edges which were the furthest from the silvered side (fig. 1). A ray of light passing in at the unsilvered part of one end with a certain degree of obliquity, could be reflected at the other end, then at the side, and again at the first end, passing thus three times along the glass and finally out at the second end. At other inclinations the ray would pass five, seven, nine, eleven, or a greater number of times along the glass before it issued forth on its course through the air to the eye of the observer.

Fig. 1.

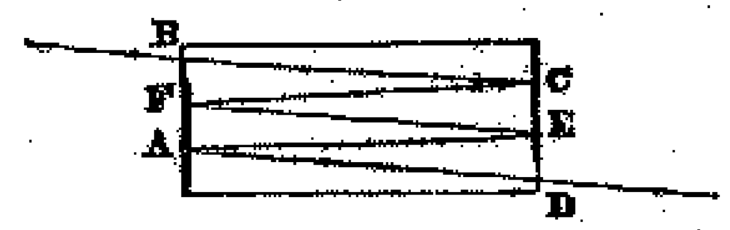


Either of these pieces of glass could produce the desired result of repeated reflexions within, but the first form was found most convenient in use. When a strong light was employed, it was not difficult to follow the series of images produced by successive reflexion up to the ninth or tenth image, these corresponding of course to a transit of the ray seventeen or nineteen times along the substance of the glass. A little change of position of the silvered glass between the Nichol's prisms used as the polarising and analysing apparatus, was sufficient to bring any one of these images into view, the glass being at the same time under the full influence of the electro-magnet, or the helix, employed to generate lines of magnetic force. A further advantage is gained if the ends of the piece of glass are not quite parallel to each other, the sides proceeding from the edges where the ray enters and issues forth being in a very slight degree different in length. This arrangement causes the series of reflected images to open out if seen at one end and to close up if seen at the other, and thus the observation of a particular image or the simultaneous comparison of two or more images, is favoured.

On considering the effect of this arrangement, it is evident that if ABCD represent a trough of solution of sugar, or any other body having the ordinary rotating influence over a polarized ray, then a ray sent in at D and passing out at A

would be rotated to a certain amount. But if, instead of proceeding onwards at A, it were reflected by the surface A F to E, and were there observed, it would be found to have received no rotation, for the effect produced in going from D to A would be exactly compensated by its return from A to E. Or if the reflexions were made more numerous and recurred at E F and C, so that the ray should traverse the body five times, still an amount of rotation equal only to that which its passage once along the substance could effect would be finally produced.

Fig. 2.



Such would not be the case if ABCD were a diamagnetic, rotating the ray by means of magnetic force; for then, whichever way the ray was passing, it would still be rotated in the *same* direction in relation to the lines of force. So if observed issuing forth at A, it would have an amount of rotation (which we may call right-handed) equal to what one transit across the diamagnetic could produce; if observed at E, it would have an amount of left-handed rotation double the amount of the first or unit quantity; if observed at F, it would have three times the first amount of right-handed rotation; if observed at C, four times the amount of left-handed rotation; and at B would possess five times the original amount of right-handed rotation.

This was confirmed by the result of an experiment. The great magnet described in the Philosophical Transactions* was employed, and the parallelopiped of glass (fig. 2) submitted to its powers; the direct ray, or that producing the first image of the luminous object, acquired a right-handed rotation equal to 12°. Moving the glass a little the second image was brought into view, or that produced by the ray which had traversed the glass three times, and its rotation was 36°. The third image was then observed, and the rotation of the ray producing it was 60°, as nearly as my rough apparatus could measure angular quantities. The same general results were obtained with the second piece of glass described.

The experiment proves in a very striking manner, that whichever way the ray of light is passing through the diamagnetic, the direction of its rotation depends essentially and alone upon the direction of the lines of magnetic force.

It also proves and manifests in a manner not to be mistaken, the difference in this respect between the magnetic rotation of

* 1846, p. 22, par. 2247. Phil. Mag. vol. xxviii. p. 398.

the ray and that produced by quartz, sugar, oil of turpentine, and such bodies.

Either by independent or by conjoint observations of the different images, it proves that the effect is proportionate to the length of ray submitted to the magnetic force (Experimental Researches, 2163); for the unit length and multiples of the unit length may be observed at once, the intensity of the magnetic force and other circumstances remaining unchanged.

It permits the attainment of a far greater degree of accuracy in the measurement of the amount of rotation of a given ray, or in the estimation of the comparative degrees of rotation of the different coloured rays.

The form of the arrangement makes a short piece of any given diamagnetic, as a crystal, &c., sufficient for an experiment, which would not suffice if the ray were passed but once through it.

It allows of the concentration of the magnetic force by an approximation of the poles, when a magnet is used, so as to exalt the effect; or to render a weak magnet equivalent to a stronger one, so that even good ordinary magnets may now be made available. Or if a helix be used, a much shorter and weaker one than that which before was necessary, may now be employed.

[*Note.*—A heavy domestic affliction having suddenly taken the author's attention away from this paper, the remaining part must be deferred to the next number of the Phil. Mag.]

XXVIII. *On the Blue Compounds of Cyanogen and Iron.* *By* Alexander W. Williamson, *Ph.D.**

It is a well-known fact that the different substances which pass by the general name of prussian blue, when prepared in the usual way, are not pure combinations of iron, but invariably contain potassium, of which the quantity varies according to the circumstances under which they are formed. It has been the subject of frequent experiment to decide whether this potassium should be considered as an admixture or as an essential constituent, and in the latter case to discover what part it plays in the constitution of the compound. Among those various researches we may confine ourselves to the consideration of those of Berzelius† and Gay-Lussac‡.

Gay-Lussac found that prussian blue retains potassium so

* Communicated by the Chemical Society; having been read March 16, 1846.

† Poggendorff's *Annalen*, vol. xxi. p. 490. ‡ Ibid. vol. xxv. p. 385.

tenaciously that the latter cannot by mere washing be removed from it; for as soon as the precipitate had been freed from all potassium, it was found to consist no longer of prussian blue, but hydrated oxide of iron. From this fact he concludes that the purest prussian blue contains potassium. Berzelius, on the contrary, by washing continuously for three weeks a precipitate of ferrocyanide of potassium with a persalt of iron, succeeded in obtaining a substance, which, though retaining the colour of the original compound, was perfectly free from potassium. The water passing through in the later periods of the operation contained this potassium in the form of ferridcyanide, the formation of which salt he attributes to a process of oxidation resulting from the action of the air; thus admitting that a decomposition of the original precipitate accompanied the removal of the potassium. Without the proof afforded by an analysis, or some characteristic reaction, we cannot venture to conclude that the residue after this decomposing reaction retained the composition of prussian blue. Berzelius thus agrees with Gay-Lussac in the fact that a decomposition of the prussian blue is a necessary condition for the removal of the potassium. The difference consisted only in the kind of decomposition which took place: in Berzelius's experiments the residue after decomposition still maintained a blue colour, while in Gay-Lussac's, in which by the action of a water which he himself concluded must have been alkaline, the residue consisted of peroxide of iron. Both lead however to the same conclusion.

Berzelius analysed two kinds of prussian blue containing potassium. The first, which was soluble in water, he found, after separation by means of alcohol from the red and yellow cyanides, which were simultaneously formed, to contain for every 12 atoms of iron 1 of potassium. The other, which was insoluble in water as it remained on the filter, from which the soluble salt had been washed away, contained iron and potassium in the proportion of 8 equivalents to 1. The former of these substances he considers as a combination of 2 atoms of the yellow prussiate with 3 atoms of prussian blue,—

$$2(\mathrm{Fe\,K_2\,Cy_3}) + 3(\mathrm{Fe_7\,Cy_9}).$$

The second is a combination of 1 atom yellow prussiate with 2 prussian blue,—

$$\mathrm{Fe\,K_2\,Cy_3} + 2(\mathrm{Fe_7\,Cy_9}).$$

This great chemist admits however, what indeed the complex nature of these formulæ sufficiently indicates, that further light is needed on the nature of these compounds than these analyses afford.

I shall now describe the results of some experiments which I have performed in the laboratory of Professor Liebig, for

Days of Month. 1846. July.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¾ a.m.	Barometer. Orkney, Sandwick. 8½ p.m.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Thermometer. Orkney, Sandwick. 9¾ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.
☽ 1.	30·014	29·973	29·43	29·70	29·60	29·60	29·66	70	58	65	60	51	56½	55	sw.	calm	sw.	......	·04	......	......	·14
2.	30·074	29·988	29·37	29·75	29·87	29·76	29·87	75	59	67	65	51	60	54½	w.	w.	sw.	......	·01	·09	......	......
3.	30·165	30·129	29·52	29·92	29·95	29·80	29·78	80	46	69	63	58	62	61	sw.	calm	sw.	ssw.	......	......	......	·10
4.	30·154	29·990	29·50	29·99	29·94	29·87	29·91	92	66	72·5	67½	57	63½	56	s.	calm	sw.	w.	......	......	......	·05
5.	29·768	29·608	29·14	29·70	29·40	29·88	29·65	95	50	80	81	50	60	52	s.	calm	sw.	e.	·24	......	......	·02
6.	29·630	29·448	28·97	29·24	29·36	29·45	29·50	71	55	65	59	53	54½	49	sw.	sw.	sw.	n.	·13	·37	......	·42
7.	29·876	29·744	29·16	29·56	29·68	29·60	29·73	72	49	62	63	50	50	50	w.	sw.	sw.	nw.	......	·22	......	·04
○ 8.	29·850	29·764	29·34	29·69	29·65	29·81	29·86	68	56	59	59	45	51	50	s.	calm	ne.	se.	·08	......	3·10	·10
9.	29·669	29·641	29·18	29·64	29·65	29·86	29·82	72	56	61	63	52½	56	53	sw.	calm	ne.	se.	·16	·10	......	......
10.	29·987	29·734	29·20	29·79	29·90	29·87	29·90	73	45	64	67	53	55½	52½	s.	calm	nw.	w.	·39	·45	......	......
11.	30·140	30·083	29·55	29·96	29·98	29·92	29·86	73	49	62	62	50	55	53	w.	calm	nw.	sw.	......	......	......	·07
12.	30·142	30·070	29·55	29·89	29·90	29·78	29·87	78	49	67	64½	53	57½	55	w.	w.	sw.	w.	......	......	......	·30
13.	30·063	29·717	29·47	29·90	29·81	29·87	29·75	87	58	69·5	65½	59½	59	55½	s.	calm	s.	sw.	......	......	......	·06
14.	29·771	29·664	29·12	29·68	29·66	29·67	29·65	84	54	73	71	55	60	54	s.	calm	w.	w.	......	......	......	·22
☾ 15.	29·907	29·812	29·25	29·73	29·70	29·68	29·70	76	55	69	68	55½	55	53½	sw.	nw.	sw.	s.	......	......	0·27	·05
16.	29·763	29·504	29·10	29·50	29·25	29·46	29·34	70	55	68	63	58	58½	55	sw.	sw.	sw.	......	·02	......	......	·04
17.	29·511	29·406	28·80	29·13	29·18	29·21	29·13	71	52	65	65½	56	56	53	sw.	w.	w-sw.	wsw.	·06	·07	......	·02
18.	29·450	29·294	28·82	29·10	29·02	29·18	29·12	71	51	58	60	48	59	53	sw.	sw.	se-sw.	s.	·22	......	......	·01
19.	29·828	29·574	29·80	29·32	29·52	29·16	29·54	74	52	63	64	46	55	53½	sw.	w.	w.	sw.	·05	·14	......	·05
20.	29·995	29·828	29·30	29·68	29·72	29·64	29·72	78	51	65	64½	54	61	55½	sw.	calm	w.	......	......	......	......	......
21.	29·905	29·833	29·24	29·58	29·51	29·66	29·50	73	55	68	63½	54½	59	55½	sw.	w.	w.	se.	·04	......	......	·05
22.	29·923	29·873	29·25	29·61	29·68	29·55	29·69	76	54	63	65	53	57	54	sw.	w.	w.	......	......	·07	......	·06
○ 23.	29·888	29·825	29·26	29·55	29·43	29·56	29·33	80	58	67·5	61½	56	59	56	sw.	sw.	sw.	s.	......	......	0·90	·03
24.	29·793	29·425	29·20	29·58	29·61	29·46	29·56	71	47	66	55	55	61½	57	w.	calm	sw.	s.	·32	......	......	......
25.	30·067	29·957	29·35	29·70	29·80	29·60	29·77	76	45	65	65½	52½	64½	56	sw.	nw.	sw.	sw.	......	·40	......	......
26.	30·152	30·123	29·53	29·97	29·97	29·93	29·85	76	56	66·5	67	53	59½	56	w.	calm	sw.	sse.	·02	......	......	·04
27.	30·202	30·157	29·54	29·83	29·93	29·72	29·83	75	63	65	66	56½	59	58	sw.	w.	sw.	se.	......	·07	......	·19
28.	30·218	30·126	29·58	29·98	29·98	29·90	29·95	83	53	74	68	59	59½	56½	w.	calm	sw.	w.	......	......	......	·07
29.	30·092	29·981	29·40	30·01	30·01	30·04	30·16	88	60	75·5	62	58	60	57	se.	calm	sw.	sw.	......	......	......	......
30.	29·975	29·937	29·34	30·01	30·08	30·25	30·26	89	62	74	74½	66	61	54	se.	ne.	sw.	s.	......	......	1·52	......
☽ 31.	29·956	29·870	29·39	30·07	29·94	30·18	30·09	89	64	70	74	59	63	58	e.	ne.	e.	se.	......	......	......	......
Mean.	29·933	29·809	29·31	29·701	29·699	29·708	29·720	76·64	54·29	67·0	65·0	54·1	58·32	54·58					1·78	1·98	5·79	2·13

THE

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[THIRD SERIES.]

OCTOBER 1846.

XLI. *On the Magnetic Affection of Light, and on the Distinction between the Ferromagnetic and Diamagnetic Conditions of Matter.* By MICHAEL FARADAY, *F.R.S., Foreign Associate of the Academy of Sciences, &c.*

[Continued from p. 156.]

HAVING ascertained the great advantage which this form of apparatus possessed for the examination of many substances which would give no sensible results by the process I formerly described, I proceeded to apply it to the cases of air and some doubly refracting bodies (Experimental Researches, 2237). For this purpose I made the faces of the magnetic poles reflectors, by applying to each a polished plate of steel; and as the poles were moveable, their reflecting surfaces could be placed at any distance and in any position required, the substance experimented on being between them.

Air.—I could obtain no signs of action upon the ray when air was between the magnetic poles, even with the fourth, fifth and sixth images.

Rock-crystal.—The cubes of this substance formerly described (Experimental Researches, 2178) were submitted to examination; but I could detect no trace of action on the ray of light when passing through them, although they were 0·75 of an inch in the side, and the ray was observed after passing seven, and even nine times across them. The cubes were examined in all directions.

Iceland Spar.—A cube of this substance (Experimental Researches, 2179) was examined in the same manner, but I obtained no effect.

Heavy glass presented the expected phænomena easily and well.

Failing to procure any positive result in these trials, either with air or with doubly refracting crystals, I silvered the latter in the manner that had been employed for the heavy glass, that the magnetic poles might be brought as close as possible; still no evidence of any magnetic action on the ray could be observed.

A natural six-sided prism of rock-crystal, 2·3 inches in length, was polished and silvered at the ends: no magnetic effect upon the light could be observed with this crystal with either the first, second or third image.

M. E. Becquerel thinks that he has observed an effect produced in doubly refracting crystalline bodies; and it is probable that his apparatus is far more delicate for the observance of optical changes than mine. In that case, if combined with the procedure founded on repeated transits of the ray, it perhaps would produce very distinct results: but the latter process alone has not as yet given any evidence of the action sought after.

Certain indications led me to look with interest for any possible effect which the crossing of the reflected rays might produce in the arrangement of reflectors and glass represented in fig. 1; but I could find no difference of action between it and the other arrangement, fig. 2, in which no such crossing occurred.

Near the close of last year I sent to the Royal Society two papers On the Magnetic Condition of all Matter*, in which I believed that I had established the existence of a magnetic action new to our knowledge; antithetical in its nature to the magnetism manifested by iron in any of its forms or conditions, strong or weak, or to that magnetism which iron could, in any quantity or under any circumstances, produce. Further, that all bodies not magnetic as iron, were magnetic according to this new mode of action; and that as *attraction* by the magnet marked the magnetic condition of iron, however small its quantity, or whatever its state might be, so *repulsion* was the distinctive characteristic of all those bodies which were naturally fitted to acquire the new state, and develope this new form of power.

M. Becquerel has sent a note to the Academy of Sciences†, in which he states certain results of his own much anterior to mine, due to ordinary magnetic action, and in which the position of the substances was *across* the magnetic axis. I need not quote the whole, but will select the following words at the end:—"From these facts it results that the magnetic effects

* Philosophical Transactions, 1846, p. 21, or Phil. Mag., vol. xxviii. 1846.

† *Comptes Rendus*, 1846, p. 147.

produced in steel or in soft iron by the influence of a magnet, differ from those which occur in all bodies, in this; that in the first the distribution of magnetism is always in the direction of the length, whilst in tritoxide of iron, wood, gum-lac, &c., it occurs generally in a transverse direction, especially when they are formed into needles. This difference of effect is due to the circumstance, that the magnetism being very weak in these bodies, we may neglect the reaction of the particles of the bodies on themselves." These words, and the time of their publication (January 1846), sufficiently show that M. Becquerel does not admit the new form or condition of magnetic action, which I supposed I had previously demonstrated.

M. E. Becquerel, in a communication to the Academy of the date of June 1846*, after confirming the action on light which I had announced, touches the question of magnetic action on all bodies; and at this late period is still of the same opinion as his father. I need quote only a few brief lines here and there to show this state of his mind. After speaking of Coulomb's results, and of his father's in 1827, he says, "These experiments have been made nearly twenty years, and nevertheless M. Faraday has announced anew the phænomenon of transverse direction, has made of the bodies which place themselves thus a new class of substances, and has named them diamagnetic substances. . . . . I ask myself how, under these circumstances, one can push the spirit of classification thus far, when one may give to the same substance all the various positions of longitudinal, transverse or oblique. In fact, place the two extremities of two strong magnets opposite each other, at 0·08 or 0·12 of an inch apart, and at about 0·04 from their surface, suspend by means of cocoon silk, a small needle of wood or of copper, about 0·04 in diameter and 1·9 or 2·3 inches long, it will place itself transversely. If the needle be cut in two and again divided, &c., we shall end by having a fragment, which will place itself in the line of the poles. It is simply a phænomenon of the resultant forces; for we may give to one and the same substance these different positions, according to its form, by modifying the distance of the poles." Then, speaking of the small quantity of iron which he found in certain specimens, and of his former statement, that these substances behaved as a mixture of inert matter and magnetic particles, or as ferruginous mixtures, he says, "all these conclusions still subsist at this day in all their generality."

These conclusions from two profound philosophers, so well

* *Comptes Rendus*, 1846, p. 595.

able in every respect to judge any question arising on such a subject as magnetism, made me anxious on two accounts; for first, I thought it possible I might really be in error respecting the broad and general principle of magnetic action, which I supposed I had discovered; and next, that if right on that point, I must have been sadly deficient in describing my results not to have conveyed a better impression to the minds of those so competent to receive and understand. I therefore, for my own sake, entered into an examination of this point; for though I am nearly indifferent to the fate of any speculative or hypothetical view I may venture to send forth, I am far from being so as regards the correctness of any announcement I may make of a law of action or a new fact; and having carefully experimented on one or two of the cases of transverse position assumed by certain bodies magnetic, as iron, I now give the general result.

Some good uniform peroxide of iron (being one of the substances which M. Becquerel experimented with) was prepared and introduced regularly into a thin glass tube, 0·25 of an inch in diameter and 1·4 inch long; it was then suspended by a long filament of cocoon silk, and could be brought into any position relative to one of the poles of a strong electro-magnet, which could be made to assume various forms by the use of terminal pieces of iron. As peroxide of iron can occasionally receive and retain a feeble magnetic state, it is necessary the experimenter should be aware of the possibility, and guard against its effect in producing irregular results.

The pole of the magnet was in the first place a cone, of which the base was 1·5 inch in diameter, its axis being in a horizontal line. The cylinder of peroxide of iron was advanced towards the cone with its centre of suspension in a line with the axis. When within the influence of the magnet, and transverse to the axial line, it retained that position: but this was a position of unstable equilibrium; for if the cylinder became oblique to the axial line on either side, then the end nearest to the cone approached towards it, being attracted, and at last went up to and remained against it. But whether directly across the axial line, and so in the position of unstable equilibrium, or in any other position, the centre of gravity of the whole was always *attracted*; an effect easily appreciated with an electro-magnet by interrupting and renewing the exciting current.

As a contrast with diamagnetic bodies, I will state, that if a similar cylinder of phosphorus, bismuth, or heavy glass be placed in precisely the same circumstances, then the transverse position is a position of *stable* equilibrium: if the cylinder be

moved from it, it returns by vibrations into it; and during the whole time the centre of gravity of the mass is *repelled*.

A square end was now given to the pole of the magnet, the face opposed to the cylinder of peroxide being 1·75 inch wide and of an equal height. The axial line is that which passed horizontally from the middle of this face, and took its course through the centre of gravity of the cylinder, which was also its centre of rotation. When the cylinder was at any distance less than 0·3 of an inch from the face of the pole, it stood parallel to the face, and therefore transverse to the axial line: being moved out of this position, it resumed it, so that the position was one of stable equilibrium. At distances a little greater this became a position of unstable equilibrium, and two positions of stable equilibrium were found equally inclined on the opposite sides of the transverse position, becoming more and more oblique to it as the distance was increased. Both the transverse positions and the oblique positions were easily referable to the concentration of the lines of magnetic force at the edges of the square end of the magnet. Effects due to the same cause have already been pointed out in the Experimental Researches (2298, 2299, 2384)*.

In every position of the cylinder of peroxide before this magnetic pole, the peroxide, as a mass, was *attracted*.

By using another termination of iron, the end of the pole opposite the peroxide was enlarged in its horizontal dimensions to 2·5 inches. All the former phænomena recurred; but the distance between the face of the pole and the tube of peroxide could be increased to one inch or more, before the tube began to assume the oblique positions.

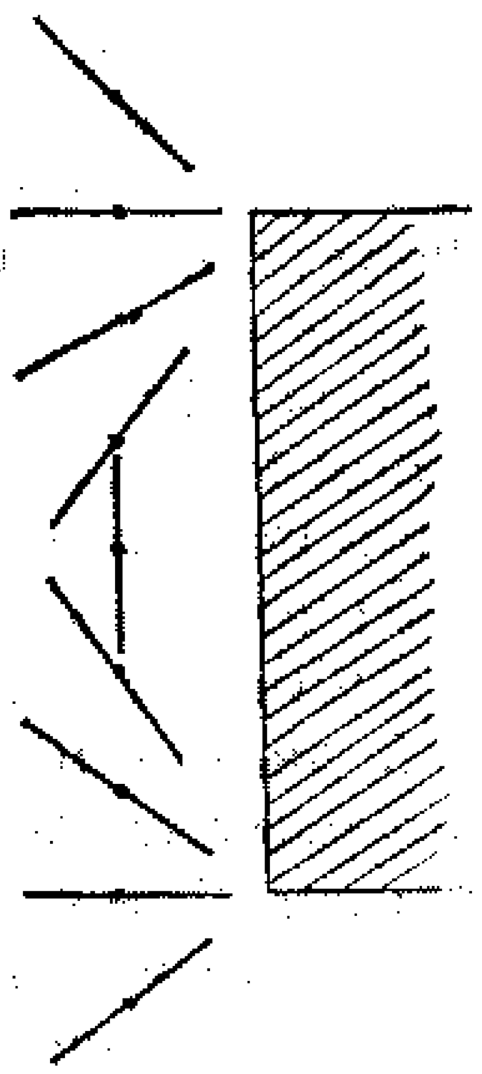
A third termination presented a face having 3·5 inches of horizontal extension: the phænomena were here precisely the same, but the distance could be increased to 1·75 inch before the cylinder ceased to be parallel to the face, and began to acquire an oblique position.

For the complete comprehension of this and other effects due to the form of the face of the pole, and the concentration of the lines of magnetic force passing through the air near the edges, I will describe the positions assumed by the cylinder of peroxide, when its centre of suspension was preserved at a constant distance from the face of the pole, but was carried into different positions on one side or the other of the axial line. These are represented in the figure; by which it will be seen, that as the peroxide was carried to one side or the other of the axial line it became inclined to that line, in

* Philosophical Transactions, 1846, pp. 32, 48.

a manner and to a degree easily comprehensible by those who consider the concentration of magnetic force at the edges of the face. The same was the case with the former face of 2·5 inches. Either end of the cylinder of peroxide might be the end nearest to the face of the pole; but the centre of gravity of the cylinder was in every case *attracted* by the magnet.

Fig. 3.



Other cylinders of peroxide of iron of different diameters and lengths were employed; and when they were smaller in length than the opposed faces of the magnetic poles, the results were precisely the same.

A cylinder of paper, magnetic through the presence of iron, was used and produced the same results.

Having been led to think that the disaggregation of the peroxide of iron had considerable influence over these phænomena, by obstructing and preventing the communication of magnetic induction from particle to particle, and was far more influential than the mere weakness of magnetic force, I took some substances, even more weak in magnetic power than the peroxide, and formed them into cylinders. The substances were solution of proto-sulphate of iron, muriate of cobalt and muriate of nickel, which were introduced into thin glass tubes 0·25 of an inch in diameter and 1·4 inch long. These, when suspended before the poles with their centres of suspension in the axial line, did not act as the peroxide of iron or the paper. They could indeed be kept in a position parallel to the face of the pole, but this was a position of unstable equilibrium; and when the least removed out of that position, the end nearest the magnet continued to approach until it came in contact, and then the whole remained unchanged. The action was precisely like that of a piece of iron, but very inferior in energy.

A saturated solution of proto-sulphate of iron was diluted with five times its volume of water, and still showed precisely the same phænomena as the stronger solution: yet its magnetic power was very far inferior to the magnetic force of the peroxide of iron, as was fully manifested by the extent of the attraction of the centre of gravity of the two portions of the substances. When the peroxide was under experiment, the suspension-thread was drawn twice or even three times further from the perpendicular than when the solution was used.

If a piece of iron wire be substituted for the cylinder of

peroxide or the magnetic solutions, it will not remain parallel to the face of the pole, or oblique and *not* touching it; for one end will always go up to the face of the pole; or if it be very short, and then by loading, or otherwise be prevented from coming in contact with the pole, one end will point towards the part of the pole face nearest to it. In this respect it is as the magnetic solution, and not as the peroxide: however weak the magnetic pole may be, if it have power to affect the iron wire at all, it will produce the *same* effect. Further, if the iron wire be rendered perfectly free from magnetism, by making it red-hot, either end may be made that which is nearest to the face of the pole.

A piece of hæmatite, separated, not by an iron tool but by an agate, or otherwise in a careful manner, pointed in the manner of iron, though of course not with the same power, *i. e.* it did not take up a stable position either parallel to the face of the magnet or inclined, but not touching it; for one end or the other always went up to and remained in contact with the metal of the pole. The hæmatite, being powdered and put into a small tube, acted in the same way as when whole.

A piece of bottle-glass tube, which was magnetic from the iron it contained, acted as the hæmatite, either when whole or powdered up and put into a flint-glass tube: it therefore was unlike the peroxide of iron.

Pure peroxide of manganese appeared to take aplace between these bodies and the peroxide of iron. Generally speaking, the end nearest to the flat face of the pole went up to it and remained there; but when one end was opposite the edge of the face and the other end nearly opposite the middle, at the time the magnet was made active, the latter end, though nearer than the former, would recede, and the former end come up and remain in contact. If the latter end was still nearer, it would approach from the first; and, there was no place of stable equilibrium for the tube in which it remained parallel to the face, or nearly so, and neither end touching it.

A piece of thick platinum wire acted as the hæmatite or green glass, and not as the peroxide, in Becquerel's experiment. Spongy platina pressed into a tube acted as the peroxide of manganese, giving like it in certain situations, the beginnings of an action like that of the peroxide of iron.

The centre of gravity of all these bodies was attracted by the magnet, whatever part of the face they were placed in juxtaposition with. In no case was anything like a repulsion shown.

Now I do not see how any difficulty can arise in referring all these results of position to the ordinary action of magnets upon matter ordinarily magnetic, as iron is. All depends upon the shape and size of the poles, and upon the state of unity or disintegration, more or less complete, of the magnetic matter; for matter which is much weaker in magnetic power than peroxide of iron, as the solution of nickel, or dilute solution of iron or the metal platina, does not act as the peroxide, but as metallic iron. Still, in every case, the phænomena are phænomena of *attraction*; for not only is the centre of gravity of the whole mass attracted, but the parts of the cylinder of peroxide of iron, as well as of the other magnetic substances, are in those positions which the resultants of attractive force would necessarily give them. This is precisely the reverse of what takes place with diamagnetic bodies, for there the centre of gravity of the whole mass is *repelled*; and whatever form the mass may have, its parts take those positions which are most consistent with the direction and degree of the *repulsive* force.

All ambiguity and doubt may be removed in such cases as the above by the use of a single pole, either conical, wedge-shaped or round. It is true, that if a wire of iron, two or three inches long, be placed with its middle part near the apex of a conical or wedge-shaped pole, it will stand at right angles across the axis of the cone or wedge, vibrating like a balance-beam; and also that if a cylinder of bismuth, phosphorus or heavy glass be placed in the same position, it will take a similar position. But no magnetician who looks at the effect can for a moment confound the phænomena; for he will see at once that the iron as a mass is attracted, and the diamagnetic is, as a mass, repelled: and then, if for this observation of the latter phænomena he will take small spheres of iron, peroxide of iron, or solution of muriate of iron on the one hand, and of bismuth or phosphorus, copper or wood on the other, he will have the phænomena in the simplest state, and the fundamental fact will be before him; for the magnetics will be attracted, the diamagnetics will be repelled.

I cannot find any case of transverse position which does not enter into one or other of the two kinds referred to above; that is to say, which is not either a magnetic or a diamagnetic result. Even as regards the effect of ordinary magnetism in peroxide of iron and such like bodies, I see no reason to accept the statement of M. Becquerel, that the distribution of magnetism tends to take place in a transverse direction. It appears to me that the destruction of the continuity of the mass in these cases, combined with the degree of magnetic force in the substance, prevents the transmission of the force by induction or conduction from particle to particle, to the same extent as

when the continuity exists, and so consecutive poles at short distances and in different directions are produced; and hence the reason why the solution of iron or nickel, or the platina, does not behave as the peroxide of iron, though weaker in magnetic force than it, but acts as metallic iron.

If it had not been for the remarkable relation of a vacuum, and with it of those attenuated forms of matter, air, gases and vapours, which I have for this very reason amongst others insisted on in the Experimental Researches (2432, &c.), it might have become questionable whether those bodies which I have called diamagnetics, were not acted upon strictly in the same manner as magnetic bodies; and the result, whether of attraction or repulsion, a consequence of a difference of degree only between the body observed and the medium surrounding it (2438, &c.). But I cannot help looking upon a vacuum as presenting a zero point in the phænomena of attraction and repulsion: and as magnetic bodies are attracted, and diamagnetic bodies repelled (2406, 2436) by a magnet, when surrounded by and in relation to it, so I believe that these two conditions represent two antithetical forms of magnetic force. This is the conclusion I have set forth in my original papers, and notwithstanding my very great respect for the judgement of MM. Becquerel, it is that which by the facts I am encouraged still to maintain*.

When heavy glass is submitted to the action of a powerful electro-magnet, the maximum degree of rotation of the ray is not obtained at once, but requires a sensible interval of time (Experimental Researches, 2170); this I have attributed to the gradual rising in intensity of the force of the magnet, and

* I take the liberty in this note to refer to a similar point in the philosophy of static electricity. I have often been asked for the proof of an absolute natural zero between positive and negative electricity; and in reference to M. Peltier's views, that the earth is negative to the space around it, which in its turn is positive, have been told that if all parts of a portion of its plane surface were equally negative, we on that surface could not tell that it was not in a zero state. But such is not the case. A surface which is truly negative may appear, in comparison, to be positive to one still more highly negative; or a negative surface may seem to be in a zero state in relation to two other surfaces, one of which is more negative and the other less so, or even positive; but if referred to a true standard its real state is shown at once, and this standard is given by the inside of any metallic vessel, from which, by its shape or depth, external influence is excluded. Such a vessel always presents the same normal condition within, whatever charge its external surface may have; and by comparing the surface of the earth with the inside of such a vessel, which is easily done by the use of carriers, such as Coulomb employed, any one may tell for himself whether that surface is in a negative or a zero state.

the corresponding rising of the effect of that force in the glass. M. E. Becquerel does not agree with me in the above explanation, but thinks it is due to this; that the particles of the diamagnetic itself require time to assume their new state. That they may require time is, I think, very probable. I do not know any state the acquiring of which does not need time. This time is however most probably exceedingly small in the instance of diamagnetic bodies; and that the phænomena referred to by me are not due to such a cause, is, I think, shown by two considerations. The first is, that the electro-magnet is well known by other kinds of proof, as the induction of currents, &c., to require time to develope its maximum force due to a given electric current. I have shown that the rotation of the ray must increase during the time the magnet is thus rising; and I found that the power to induce currents exists simultaneously with the increasing rotation. The next consideration is, that when the diamagnetic is submitted, not to the action of an electro-magnet, but of a helix, the rotation does not increase in the same gradual manner as before, but is instantly at a maximum (Experimental Researches, 2195): hence my reasons for adhering to the explanation I gave in the papers themselves last year.

But on subjects so new as these, differences of opinion must inevitably arise upon many points; and it is better for the inquiries themselves that it should be so, for the facts in consequence receive a more close investigation. I therefore leave many points of difference between myself and others unnoticed for the present; believing that new and connected facts will rapidly accumulate, and that time, with his powerful aid, will in a very few years give both facts and opinions their right places.

Royal Institution, August 11, 1846.

XLII. *On the Composition and Resolution of Forces. By* G. W. HEARN, *Esq., B.A. of Cambridge, and Professor of Mathematics in the Royal Military College, Sandhurst* *.

1. EQUAL forces are such as being applied to the same point in contrary directions make equilibrium.

2. The resultant of any number of forces acting on a point is a single force, statically equivalent to the whole of them, that is a force such as would produce the same pressure in the same direction.

3. If two equal forces inclined to each other act on a point, their resultant is in the plane of the two forces, and bisects

* Communicated by the Author.

the angle formed by their directions; and half that resultant is defined to be the 'resolvent' of either of the equal forces.

4. When any number of forces act on a point, if any straight line be drawn through that point, and planes be drawn through this line and the respective directions of the forces; and if in each plane a force equal to the force in that plane be applied to the point, making an angle with the straight line equal to that which the other force makes, but on the contrary side of the line, such a system of forces is called 'supplementary' to the former.

5. A system of forces acting on a point is in equilibrium when their resultant is zero.

Prop. I. When a system of forces is in equilibrium, the sum of the resolvents in the direction of any straight line through their point of application is zero.

The system being in equilibrium, the supplementary system will also be in equilibrium; for this latter system is nothing more than the former turned through 180° round the assumed line.

The two systems are therefore also in equilibrium, and consequently the resultant zero. But this resultant consists of the (algebraic) sum of the several resultants of the pairs of equal forces in the several planes through the assumed line, and half the resultant is therefore the sum of the resolvents of the given system. The resultant being zero, the sum of the resolvents is also zero.

Prop. II. Denoting the resolvent of a force P in the direction of a line making an angle θ with the direction of the force by $\mathrm{P}f\theta$, it is required to assign the form of $f\theta$.

It is an immediate consequence of our definition of resolvent that $f\theta=1$ when $\theta=0$, and that $f\theta=0$ when $\theta=\frac{\pi}{2}$. Moreover, that $f\theta$ cannot $=0$ for any value of $\theta<\frac{\pi}{2}$.

Also that $f\theta$ is a periodical function going through all its values, as θ increases from 0 to 2π; and that $f\theta$ can never be infinite, and must have some determinate *single* value for every value of θ, and is therefore a function which can be expanded in integral positive powers of θ.

Let two equal forces keep a point in equilibrium. Sum of resolvents $=0$ gives

$$\mathrm{P}f\theta+\mathrm{P}f(\pi+\theta)=0,$$

or

$$f\theta+f(\pi+\theta)=0.$$

This equation is easily resolved, but is too general for our present purpose.