ON SOME REMARKABLE INSTANCES OF CROOKES'S LAYERS, OR COMPRESSED STRATA OF POLARIZED GAS, AT ORDINARY ATMOSPHERIC TENSIONS.

BY

GEORGE JOHNSTONE STONEY, M.A., F.R.S.

[Read November 19th, 1877.]

1. In a communication which I had the honour to lay before the Royal Dublin Society at its last scientific meeting, I gave some instances of Crookes's layers at ordinary atmospheric tensions,* and among them described one which accounts for the great mobility that may be imparted to a light powder by heating it in a metal capsule. It is shown that in this case the powder floats on a stratum of air which it compresses by its weight, at the same time that it maintains the requisite polarized condition of the layer by radiating away its own heat so freely as to keep itself cooler than the capsule.

2. In exactly the same way we may explain a very curious phenomenon which has been recorded by travellers in Arabia, and to which Professor Barrett has directed my attention. There is in Arabia a mountain called Jebel Nagus, or Gong Mountain, which produces sounds resembling the booming of the Nagus, or wooden gong, used in Eastern churches instead of bells. The mountain consists of a white friable sandstone, which produces to the south-westward a great slope of very fine drift sand, and another smaller one to the north. The large one is 115 metres high, 70 metres wide at the base, and tapers towards the top. It is so steep, being inclined to the horizon at an angle of nearly 30°, and consists of such fine sand, that its surface can be easily set in motion by scraping away a portion from its base or by disturbing it

* The theory of unequal stresses in polarized gas has thus fulfilled an anticipation which Mr. Crookes entertained so long ago as 1873, that whatever theory would account for the motion of radiometers would, probably, also explain the spheroidal state of liquids, and the mobility of finely divided precipitates in heated capsules; for he enumerates these among phenomena, probably due, at least in part, to the same "repulsive action of radiation" as is manifested in radiometers. (See Philosophical Transactions, vol. 164, p. 526). I have only become acquainted with this passage since writing the present paper, or, if I had seen it before, I had forgotten it, otherwise I should have referred to it in my last paper.

elsewhere. If this is done after the surface has been for a long time exposed to the sun—

"The sand rolls down with a sluggish viscous motion and the sound begins, at first a low vibrating moan but gradually swelling out into a roar like thunder, and as gradually dying away." (Palmer's "Desert of the Exodus," vol. 1, p. 218).

That heat contributes largely to the effect was proved by the valuable observations made by Captain Palmer, for it was found—

"That the heated surface was much more sensitive to sound than the cooler layers beneath, and that those parts of the slope which had lain long undisturbed produced a much louder and more lasting sound than those which had recently been set in motion."

Moreover, when the experiments were repeated on the other talus, which faced towards the North, and part of which was in perpetual shade, it was found—

"That the sand on the cool shaded portion, at a temperature of 17° C, produced but a very faint sound when set in motion, while that on the more exposed parts, at a temperature of 40°, gave forth a loud and even startling noise."

These observations were made in winter. They clearly indicate that heat renders the surface of the slope more mobile by polarizing the air between the hotter and cooler particles of the sand.

The more intense the sunshine, the more powerful must the Crookes's layers be, and the more widespread will be the effect of any accidental disturbance. And if under the fierce glare of the tropical sun the strength of the Crookes's layers becomes sufficient to lift the uppermost grains of sand, the sliding motion, with its humming, booming, and thundering noise, will spring up without visible cause—a phenomenon that sometimes occurs and has naturally occasioned much speculation.

Mr. Howard Grubb has directed my attention to another natural phenomenon which admits of being explained by the mechanical properties of polarized layers of gas. In certain states of the weather *large* grains of sand, flat pieces of shell, and even flakes of stone of quite a considerable size may be seen floating on the tide as it flows in. I saw this phenomenon myself when a boy, but unfortunately did not make a careful examination of the attendant circumstances. It is, however, easy to see the conditions which would be most favourable to its production. They are—a very powerful sun to heat the stones and to maintain their temperature sufficiently high after they are set floating; calm air that no breeze may cool them; a cold sea to increase as much as possible the difference in temperature between the flakes of stone and the water, and the absence of waves that the heavy little barges may escape shipwreck.

I think it fortunate that I had written out the foregoing statement of the conditions indicated by the theory, before I saw the following record of observations upon this phenomenon made by Professor Hennessy. (See Proceedings of the Royal Irish Academy, Vol. I., Series 2):—

"On the 26th July, 1868, when approaching the strand at the river below the village of Newport, county Mayo, I noticed what appeared to be extensive streaks of scum floating on the surface of the water * * * until I stood on the edge of the strand, and I then perceived that what was apparently scum seen from a distance, consisted of innumerable particles of sand, flat flakes of broken shells, and the other small *débris* which formed the surface of the gently sloping shore of the river. The sand varied from the smallest size visible to the eye, up to little pebbles nearly as broad and a little thicker than a fourpenny piece. Hundreds of such little pebbles were afloat around me. The air during the whole morning was perfectly calm, and the sky cloudless, so that although it was only half-past nine, the sun had been shining brightly on the exposed beach. The upper surface of each of the little pebbles was perfectly dry, and the groups which they formed were slightly depressed in curved hollows of the liquid. The tide was rapidly rising, and owing to the narrowness of the channel at the point where I made my observations the sheets of floating sand were swiftly drifting farther up the river into brackish and fresh water. On closely watching the rising tide at the edge of the strand, I noticed that the particles of sand, shells, and small flat pebbles, which had become perfectly dry and sensibly warm under the rays of the sun, were gently uplifted by the calm steadily rising water, and then floated as readily as chips or straws."

The calm air, tranquil water, hot sun, and warm stones, predicted from the theory, are all recorded in these observations.

This rare phenomenon must not be confounded with the familiar one in which patches of *fine* sand float upon water in consequence of its surface tension. The surface tension of water in contact with air will not support flakes of stone of above a certain size, and those described by Professor Hennessy are at or beyond the limit of size* that could even if separate be floated by surface

* Taking the surface tension of water in contact with air as 8·25 grammes per metre as determined at 20° C. by M. Quincke, and assuming 2·5 as the specific gravity of the

tension. Hence they could not be supported by that agency *in the groups* which he describes. We are therefore forced to look elsewhere for the cause of the support of these groups; the thermal and mechanical properties of Crookes's layers show that they will suffice: and we have seen that all the conditions were present which would call Crookes's layers into existence.

Mr. George F. Fitzgerald has pointed out another very striking example. A piece of cold iron may be made to float on melted cast iron, and will even float high like cork on water. Here the difference between the temperature of the glowing mass of molten metal and the cold piece of iron is so considerable that the stresses that are developed are able to support the weight of the piece of iron while it is still at such a distance from the fiery liquid that it seems to float high upon it. What it floats on is in reality a bath of polarized air, the stresses within which both support its weight and force down the surface of the molten metal. This air-bath keeps it out of contact with the glowing mass; and, accordingly, it receives heat from below only by diffusion and radiation, in quantities far short of what it would receive from actual contact, and as it loses much heat by radiation upwards, it may be able for a considerable time to maintain a sufficiently low temperature to continue floating.

On the same principles we are to explain the safety of exploits that are occasionally performed, viz.—The licking of a white hot poker, the dipping of the fingers into molten metal, and the plunging of the hand into boiling water. In all these cases the Crookes's layers that intervene prevent that contact which would cause a dangerous scald or burn.

It is usual before performing these two latter experiments, to moisten the hand with soapy water, ether, turpentine, or liquid ammonia. All of these would have the useful effect of lowering the surface-tension of the hot liquid, and thus diminishing the extent to which it would compress the Crookes's layer.

But the most splendid example I have yet seen of a Crookes's layer is one which was first noticed by M. Boutigny, and which

stone, it follows that a circular disk, 16 m.m. in diameter and 0·85 of a m.m. in thickness would be the extreme theoretic limit that could be supported by surface tension. This is about the size of a fourpenny bit.

was shown by Professor Barrett, at the Brighton meeting of the British Association, with the improvement of adding soap to the water, an addition which seems essential to the full success of the experiment. A copper ball, some six cm. in diameter, furnished with a staple by which it can be lifted, was brought to a bright red heat, and while glowing was lowered into a large beaker of soapy water. As the ball approaches the cold surface of the water heat passes from the ball to the water by conduction or penetration* as well as by radiation; accordingly the intervening air becomes intensely polarzied, and the Crookes's stress that accompanies the polarization makes a hollow in the surface of the water. Let the ball be lowered till it is half submerged, the depression in the water is now nearly hemispherical, but not quite so, since the interposed layer of polarized gas will be thinnest at the bottom, where, to withstand the pressure of the water, it must exert most force. The stresses at any point of this polarized layer consist of a constant stress P nearly equal to the tension of the open atmosphere, acting equally in all directions, along with a variable Crookes's stress p, acting for the most part nearly in the direction of a radius of the ball; the most marked deviation from this direction being close to the horizontal surface of the water, where the action of the upper hemisphere of the ball gives an inclined direction to the Crookes's stresses, and helps to round off the surface of the water. The amount of the Crookes's pressure acting on the water will vary with the depth, being such at each point that it gives a component equal and opposite to the resultant of the pressure of the water at that depth, and of the surface tensions round the point. Whenever the Crookes's force is not quite in the direction of this resultant, there will be a free tangential component, and this must produce surface currents in the water. These, however, cannot be observed in the present experiment because they are of small amount, and too much mixed up with convection currents arising from the heat that reaches the water by radiation and diffusion.

When the ball is lowered until it is quite submerged it will be surrounded on all sides by its envelope of polarized air, thinnest at

* The heat which diffuses across a layer of gas passes under what are known as the laws of conduction if the number of gaseous molecules present is sufficiently large. If fewer molecules are present the heat passes under other laws, which may be distinguished from the laws of conduction by calling them the laws of penetration.

the bottom, where the pressure of the water is greatest, thickest above. So long as there is any communication between the polarized layer and the atmosphere, the lateral stresses within the layer will be equal to P, while those in the direction in which the heat penetrates will be $P + p$; but both of these will suffer an increase if the ball is plunged deeper after the communication with the atmosphere has been cut off. No one can see this splendid experiment for the first time without a feeling of astonishment.

A Crookes's layer formed in the same way, but without the exquisite beauty which it has in this experiment, may be seen any day in a smith's forge, whenever the smith has occasion to quench white-hot iron in water.

A phenomenon closely resembling the experiment with the glowing ball was witnessed lately by my brother and two other friends while out walking. There was a shower when they reached some rather deep water. The afternoon had become chilly, and the phenomenon that presented itself shows that the water must have retained a temperature higher than that of the air. As the rain-drops fell into the water, some of them (estimated at one in twenty) became spheroidal drops floating on the water, and of these some (estimated at one in six) were visibly submerged before floating about as spheroidal drops. They sank, perhaps about half a centimetre before they rose to the surface, and while under water looked like silvered pills, owing to the total reflection from the boundary between the water and the film of polarized air which enveloped each drop.

Several times, in the course of this communication, I have had occasion to speak of the feebleness of conduction or penetration, compared with the rapid outpour of heat which takes place on direct contact between a very hot and a cold body. This is well illustrated by an experiment of M. Boutigny, in which a spheroidal drop of water is formed inside a hot copper bottle, and the neck of the bottle partially stopped by a cork through which a thin tube passes. So long as the drop continues in the spheroidal state, a mixture of air and vapour slowly escapes through the tube in the cork, but the instant the spheroidal state ceases, and the water comes into contact with the copper, a sufficient portion of the water flashes off so suddenly into steam that the cork is driven out with explosive violence.

A still more instructive illustration of these facts is afforded by the familiar experiment, known to every smith, that an explosion will occur if a little water is dropped on an anvil, if a white-hot strap of iron is laid over the drop, and if the iron is then given a tap with the sledge-hammer. In this experiment the hot iron, when laid on the anvil, does not fit it accurately, but comes into contact only at a few points, and leaves a chink elsewhere. While the iron is descending towards the drop of water, a Crookes's layer of polarized air is formed between it and the cold water, which exerts a sufficient pressure upon the drop, both to flatten it out, and to keep it from coming into contact with the glowing iron. At this stage of the experiment the lower portion of the chink is occupied by water, and the upper portion by polarized air. The stratum of air moderates the flow of heat towards the water, so that the water is able to continue liquid by parting with as much heat downwards to the cold anvil as it receives from above, before it is itself warmed beyond the boiling point. But when the sledge-hammer descends, the soft iron yields, the chink is obliterated with a force greater than that which the Crookes's layer can support, and the glowing mass comes, in many places, into direct contact with the water. The vastly augmented flow of heat which is consequent upon this direct contact, rushes across the film of water with a speed equal to the velocity of sound in water, which will carry it across a film the seventh of a millimetre in thickness in the ten-millionth of a second. Within this brief period of time the greater part of the water is raised to a very high temperature, and its sudden conversion into red-hot steam causes the explosion.

Before concluding this communication I wish to take the opportunity of publicly thanking my scientific friends for their kindness in bringing such remarkable instances of Crookes's layers at ordinary atmospheric tensions to my notice and giving me permission to publish an account of them.

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ON THE CHEMICAL COMPOSITION OF THE COAL DISCOVERED BY THE ARCTIC EXPEDITION OF 1875–6.

BY

RICHARD J. MOSS, F.C.S.,

Keeper of the Minerals, Museum of Science and Art.

[Read November 19th, 1877.]

DURING the late Arctic Expedition an extensive seam of coal was discovered in Grinnell Land, close to the winter quarters of H.M.S. *Discovery,* 81° 43′ N.L., 64° 4′ W.L. Dr. Moss, late of H.M.S. *Alert,* presented a large specimen of the coal to this Society. It is now deposited in the Museum of Science and Art. The specimen was taken by Dr. Moss from the seam at about fifteen feet from its upper surface, the estimated thickness of the seam being about twenty-five feet. The coal possesses the lustre, fracture, and other external characters of bituminous coal of good quality, notwithstanding that the shale which overlies it is rich in fossil remains of a flora of the Miocene period. The coal cakes when heated, and leaves sixty-one per cent. of a coherent coke. Its specific gravity is 1·3. The following are the results of my analysis, every precaution having been taken to insure that the sample analyzed was a fair average of the entire specimen :—

Carbon,	75·49
Hydrogen,	5·60
Oxygen and Nitrogen,	9·89
Sulphur,*	0·52
Ash,	6·49
Water,	2·01
	100·00

The composition of the coal, excluding water, sulphur, and ash, is :—

Carbon,	82·97
Hydrogen,	6·16
Oxygen and Nitrogen,	10·87
	100·00

* Including Sulphur in the form of iron pyrites, 0·36 per cent.

I have analyzed the ash and found that it consists of:—

Silica,	34·69
Alumina,	27·30
Iron Sesquioxide,	6·72
Lime,	7·79
Magnesia,	1·83
Potash,	7·58
Soda,	0·10
Sulphuric Anhydride,	13·79
Phosphoric Anhydride,	trace
Chlorine,	trace
	99·80

The quantity of potash in the ash is unusually large. On comparing the composition of the coal with that of other coals of various geological ages it will be found that the Arctic coal most closely resembles those of the true carboniferous period. Coal from the great seam in the Bay of Fundy, Nova Scotia,* possesses a chemical composition almost identical with that of the Arctic coal. On the other hand some lignites of the Miocene period bear a close chemical resemblance to the Arctic coal, if the composition, exclusive of water, sulphur, and ash, be compared. For example, a lignite from the Island of Sardinia possesses the following composition† :—

Carbon,	82·26
Hydrogen,	6·52
Oxygen and Nitrogen,	11·22
	100·00

It has been shown by Zincken‡ that it is impossible to determine the geological age of coal from its chemical composition; of this fact the Arctic coal affords a good illustration.

* Percy's Metallurgy, Fuel, &c., p. 336.

† *Ibid.* 313.

‡ Die Physiographie der Braunkohle.

NOTES ON THE SKELETON OF AN ABORIGINAL AUSTRALIAN.

BY

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[Read November 19th, 1877.]

THE Museum of the Dublin University has recently received a fine skeleton of an aboriginal Australian, which is a valuable addition to its ethnological department. I am indebted to Dr. R. Tuthill Massy, of Brighton, for this interesting donation, and have made a careful series of measurements and observations on it, the results of which I have embodied in this paper.

The stature was small, 5′ 1½″, which is about the average for Australian natives; though I am informed by Dr. Johnston, of this city, who has had extensive experiences of these races, that in some tribes, much taller individuals are common; and that on one occasion he met with an encampment of natives on the Murray river, many of whom were six feet high. Such are, however, exceptional, for the general consensus of opinion is that "very few could be said to be tall and still fewer to be well made." (Collins' Account, p. 356.)

The skeleton is that of a male, known and employed as a messenger, and who had been known to travel seventy miles in a day. The proportions are as follows compared with the standard in Professor Humphry's Work :—

TABLE I.

	Height.	Spine.	Humerus.	Radius.	Femur.	Tibia.
Australian, .	1·00	–	·203	·151	·286	·226
Irishmen, .	1·00	·340	·194	·154	·270	·225
Negroes, .	1·00	·311	·195	·151	·274	·232
Bushmen, .	1·00	·314	·20	·153	·277	·2389
Bushwoman,	1·00	·333	·182	·131	·264	·2108

To represent more accurately the relationships of the *intermembral* lengths, I append the following three tables:—

TABLE II.

RELATIVE PROPORTION of FOREARM to HUMERUS in length.

	Humerus.	Forearm.
Australian, . .	1·00	·74
Bushman, . .	1·00	·80
Negro, . . .	1·00	·76
European, . .	1·00	·79

The decimal obtained in this table or *antebrachial index* is interesting, as it shows that this individual had a greater proportional length of forearm than the average of Europeans, a pithecoid character.

TABLE III.

RELATIVE PROPORTION of FEMUR to TIBIA.

	Femur.	Tibia.
Australian, . .	1·00	·80
European, . .	1·00	·85
Negro, . . .	1·00	·89
Bushman, . .	1·00	·78

From this table it appears that the *crural index* is rather shorter than usual.

TABLE IV.

RELATIVE PROPORTION of FOREARM and ARM to THIGH and LEG.

	Lower.	Upper.
Australian, . .	1·00	·69
Bushman, . .	1·00	·87
European, . .	1·00	·70

This *intermembral index* shows that in this Australian the proportion of the arm to the leg is smaller even than the European.

The three methods of measurement adopted above are, I think, calculated to give us more definite results as to the relative regional developments of extremities than any other plans hitherto used. With many of the most interesting aboriginal tribes it is impossible to get whole skeletons, and when got, the spinal unit, so important a factor in Professor Humphry's whole number, is rather a vague one, on account of the varying thicknesses of intervertebral substance, unless the observer has obtained the skeleton while fresh. Limb bones, on the other hand, are easily obtained, and by a series of measurements like those given above and carried out on an extensive scale, we can easily formulate in the simplest possible manner, the relative developments of the portions of the limbs. One set, like that given here, tells us comparatively little of ethnological importance, as the original may have been a Rob Roy among his tribe, or else *brachybrachial*, but an extensive series of such measurements would, doubtless, be of value.

The vertebral column showed no appearances of note. The third cervical had a trifid spine, as is not uncommonly the case. The spine of the sixth cervical was bifid, and the transverse process of the seventh perforated at each side. The sacral laminæ were mesially ununited, and the spinal curvatures were smaller than usual. The sacrum measured $3\frac{3}{4}$ inches in length, and 4·6 inches in breadth, the average male European sacrum being 3·9 inches by 5 inches.

The os innominatum is small, not unduly elongated, with no pre-auricular groove (Zaaijer), but with well-marked muscular impressions. The thigh bone has a small carina, and its neck forms a large angle (123°) with the shaft. The anterior intertrochanteric and the ectogluteal ridges are strong and rough.

The tibia has a tuberous external condyloid eminence for the attachment of the ligamentum iliotibiale (Maissiat and Merkel), a structure which I have always found to be of proportional strength in all persons, with great powers of endurance in their legs.

The skull presents the usual Australian dolichocephaly, and is of the usual small size, with narrow frontal region. The sutures are open and are comparatively simple, the lachrymo-planal being reduced to a very short space 0·2″ in length. There is a distinct double temporal crest (Hyrtl.), a double supra-orbital hole on the right side, the inner opening being for the supra-trochlear nerve. The spheno-parietal suture measures 0·4″ on the left, 0·35″ on the right. The occiput has two rough splenial ridges and a short fissure exists behind each. The tympanic is separated from the periotic by a distinct deficiency, and the post-glenoid process is separated from the tympanic bone by a fissure continuous with the glaserian. A strong supra-mastoid ridge exists, and strong stylohyals; a narrow transverse glenoid cavity, and a vertical anterior edge of the squamosal. The spine of the sphenoid has a double styloid process, one external and one internal to the foramen spinosum. The left lachrymal bone has an inferior hamulus, and the chin process is small.

A FRAGMENT OF HUMAN SKELETON FROM NORTH LATITUDE 81° 42′.

BY

DR. EDWARD L. MOSS.
Late Surgeon H.M.S. *Alert*.

[Read November 19, 1877.]

At the time the Arctic Expedition of 1875, left England, all that was known of the migrations of the Eskimo appeared to warrant the hope expressed in the manual supplied to the expedition by the Royal Geographical Society that Ethnological results of interest might be obtained. That hope was based upon the consideration that the route chosen for the expedition lay through an altogether exceptional region, exceptional in that it afforded the only ascertained gap in the northern frontier line bounding the geographical distribution of man.

In every other part of the circumpolar regions expeditions had penetrated either to lands, such as Spitzbergen or Franz Joseph Land, that bore no trace of an indigenous people, or to a barrier of eastward drifting perennial floes, impassable alike by Eskimo or European, and which if their full import had been appreciated might have saved much speculation as to the possibility of an inhabited Polynia in the middle of the Polar ice-cap.

But on the eastern side of the Parry group, and along both shores of Greenland, land spread continuously to the northward, and though each successive explorer had forced his way to a latitude never before reached on land, all had been obliged to confess that at their turning point the foot-prints of their uncivilized predecessors still lead Poleward.

When our ships started, the most northern known traces of man, were those found by Dr. Bessels, of the U.S.S. *Polaris*, at Cape Lupton. There, within a day's march of the furthest point on land reached by his expedition, rings of stones that had been used to fasten down the edges of tents, marked the temporary camp of some travelling Eskimo.

It was from this latitude that our continuous search along the coast-lines began. South of this point our visits to the shore on both the outward and homeward voyages were of the most flying character. Every movement of the ice had to be taken advantage of, and our naturalists had often less than twenty minutes to bundle together their specimens, Botanical, Zoological, and Geological; and yet abundant evidences of man were found on every beach, till Lady Franklin Sound intersected the coast-line. The *Discovery* wintered in a bay inside Bellot Island, on the north shore of Lady Franklin Sound, and in the same latitude as Cape Lupton, and the shores to the north instead of being merely visited at intervals, were traversed over and over again by our sledge parties.

Lady Franklin Sound had not interrupted the onward passage of the Eskimo. Tempted doubtless by the reindeer and musk-oxen, on Bellot Island and the neighbouring main-land, their hunters had crossed the sound, and several hearths, where splinters of burnt drift wood and bits of scorched bone lay amongst the blackened stones, where found on a long low spit of Bellot Island. On a little rocky island within a stone's throw of the main-land similar marks of summer hunting parties were discovered. For seventeen miles to the north-eastward the shore still affords a practicable path; but at Cape Beechey, the steep cliffs of Robeson Channel rise abruptly from the tumbling stream of Polar ice, that pours through the strait, under these cliffs and at the very end of the practicable coast, Captain Feilden discovered a broken sledge and a broken lamp. From this point northward our sledges followed the coast for 100 miles to the north-east, and for 250 miles to the north-west and found no further trace.

All the likely parts of the coast were traversed dozens of times both before and after the disappearance of the snow, and no spot where Eskimo had ever camped or cooked could possibly have escaped us. I feel quite confident that all who have travelled over the respective coasts will indorse the opinion already published by Captain Feilden,* that "the men whose tracks we followed to the 82° parallel never got round Cape Union, and that it is impossible for any Eskimo to have rounded the northern shores of Greenland."

* "Naturalist," Aug. and Sept., 1877.

The frontier line of known migration is therefore complete. Henceforward any hypothesis that inhabited lands exist in the unknown North must be based only on the vague traditions of the tribes on both sides of Behring Strait that emigrants from their shores have reached Kellett's Land.

But the traces we have hitherto spoken of do not include any vestige of man himself.

Even at Norman Lockyer Island, where we found the remains of a whole city of not only tent circles but "yourts," meant for winter habitations, we failed after hours of careful search to find anything like a burial place. When Captain Feilden discovered the lamp and sledge, the way in which the lamp was broken—so like the broken vessels left on the Indian graves of the far west—and the valuable pieces of wood that had been left lying beside it suggested the possibility at least that they had been left for the future use of their buried owner, but I could find no heap of stones or anything else like a tomb in the neighbourhood.

But seventeen miles backward, along the coast on the northern shore of Lady Franklin Strait, and at the most southern point overlooking Kennedy Channel, the fragment of human femur which I exhibit this evening was picked up. I am not aware that any human bone has been found in a higher latitude than the Etah burial-ground at Port Foulke, and this fragment lay 200 miles beyond that spot. The spot where it was found was eighty feet above the beach, and 100 yards inland, opposite the spit of Bellot Island, and about three-quarters of a mile eastward from the marks of camps on "Dutch" Island—the rock already spoken of. It was embedded in the side of one of those little polygonal hillocks into which the frost splits clayey ground. Both ends of the bone are broken off, the one through the neck and trochanters, the other about two inches and a quarter from the lowest point of the articulating surface. It has been gnawed by either wolf or fox, though I think the jaws of the little arctic fox are hardly strong enough to break the ends off so strong a bone. Like every other trace of man found at high latitudes in Smith's Sound it is very old; the tongue adheres to its surface, it is spotted with lichen, and a moss has found root in the cancellous structure of its upper end. The fragment measures eleven and a half inches in length, and by comparing it with femora from old Eskimo graves on an island near Egedesminde, I estimate its restored length at sixteen inches. When its front is placed on a level surface the antero-posterior curve deviates a quarter of an inch from the horizontal at either end. As may be seen from the annexed section it is as carinate as most platycnemic femora. There are two nutritious foramina three and three-quarters and three inches below the lesser trochanter. Its minimum circumference is three inches below the lesser trochanter, and measures 3·43 inches. This with the estimated length gives a perimetral index of ·214.

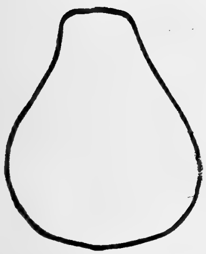
If its index were taken at the English average, *i.e.*, ·194, its restored length would be 17·68 inches, but the bone is evidently shorter than an average English femur, and the former estimate is probably the more accurate.

If Aeby's statement that races do not materially differ in the relative proportions of their limbs is correct, we may roughly estimate the stature of the man who owned this bone. The proportionate measurements of the skeleton given by Pruner Bey make the femur 27·29 in 100 of total height. Humphry's average is slightly greater, but calculating by either of them the man when living must have stood a little over 5 feet.

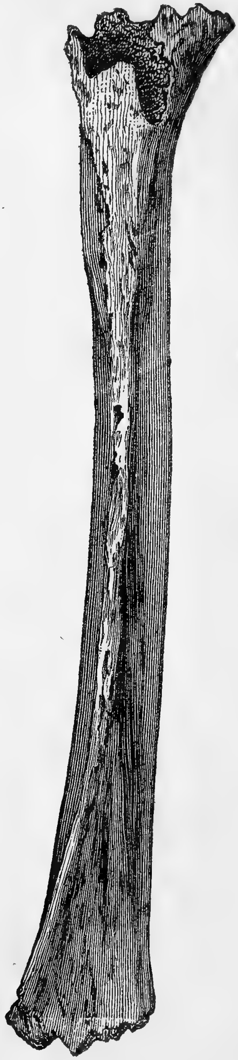
A careful search was made round the spot where this bone lay. The ground was very uneven. Some vertebræ and the skull of a young musk-ox were found within 200 yards.

It seemed probable that a longer search would have led to the discovery of some dilapidated cairn or cyst, for if the bone had been recent when first exposed to the attacks of carnvora, the medullary cavity would certainly have been broken into.

But no other vestige of man or trace of anything like a burial-place could be discovered before movements of the ice between our boat and the ship put a peremptory stop to our proceedings. The *Alert* and *Discovery* were then only waiting for an opening across the ice of Lady Franklin Strait to begin their return voyage, and our visit to the spot was never repeated.



Section through the centre of the Bone, natural size.



ON THE ELECTRIC TELEPHONE.

BY

W. F. BARRETT, F.R.S.E

[Read November 19th, 1877.]

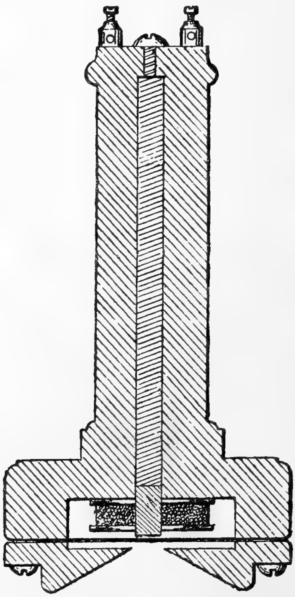
The following paper does not lay claim to any originality. It is simply a brief description of an instrument which will probably play an important part in the future of the human race; together with an historical note of what had been accomplished by Reis fifteen years ago.

The various attempts to communicate audible speech by means of electricity have culminated in the recent discovery by Professor Graham Bell of the articulating telephone. The discovery was not the result of chance but of long and patient endeavour. Every sound of the human voice is communicated from the speaker to the listener by means of aerial vibrations of a definite character. For the same sound the same wave-form is always reproduced. Starting from this fundamental axiom, Professor Bell's first efforts were made with the view of visibly recording speech. A model of the human ear was constructed, to the tympanum of which a delicate style was attached, the movements of which recorded themselves on a moving slip of smoked glass. Thus the vowel sounds and a few simple words were readily recorded by this phonautograph; which, however, differed but little from similar instruments previously constructed. These experiments revealed to Professor Bell the important point that the transmission of speech by electricity could onlybe accomplished by using what may be termed an undulatory current: that is to say, one that merely varied in strength without the occurrence of any actual interruptions which would give rise to a discontinuous or intermittent current. It is this principle of an unbroken current which distinguishes Bell's telephone from all preceding efforts. The electric currents in this telephone are in simple proportion to the motions of the air produced by the voice, and further the electric waves sent to the distant extremity are (by a receiving arrangement precisely similar to the transmitting instrument), caused to reproduce motions of the air identically the same in character as those that gave birth to the currents. Thus not only is articulation heard perfectly, but moreover, the different qualities of different voices are heard, so that at 50 or 100 miles distance the individuality of the speaker is transmitted as well as his ideas.

A section of the present form of this important instrument is shown in figure 1. A diagrammatic representation of the essential parts is given in figure 2: a permanent bar magnet (N.S.) (about four inches long), has a coil of fine wire (*b*) wound at one extremity, in front of the coil a disc (*a*) of thin sheet iron is fixed. The transmitting and receiving instruments are precisely alike, and no further source of electricity is required than that produced by

FIG. 1.

Section.



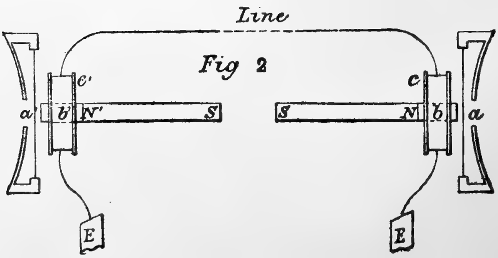


Fig 2

the to and fro motion of the iron disc. On speaking into the instrument vibrations of the disc are set up which give rise to magneto-electric currents in the coil of wire, one end of which is attached to the line wire. These electric pulsations produce corresponding changes in the intensity of the magnetic field at the receiving instrument, and hence give rise to motions of the iron disc analogous to those at the other end.

The extraordinary feature of this instrument is its extreme sensitiveness. The amplitude of the vibrations of the transmitting disc must be wonderfully small, and yet every inflexion of the voice is faithfully transmitted. At the receiving end the amplitude of the vibrations of the iron disc is so small as to be immeasurable, nevertheless not a syllable is lost, and in the larger instruments the voice is heard at a distance of some feet from the instrument.

The question arises, is the sound due to a molar or a molecular motion of the disc? Iron when magnetized and demagnetized gives rise to a peculiar click due to molecular changes, to which reference will be made in the sequel. No motion of the iron would be apparent in this case, but if the sound sprung from any motion of the disc as a whole, such as would arise from currents of sensible strength fluctuating through the coil beneath, then the motion of the disc might be capable of detection. Attaching a mirror to the disc and reflecting therefrom a ray of light, no motion of this reflected ray was visible on speaking through the instrument. Other arrangements were tried by the author of the present paper to test this question. A sensitive flame is an extremely delicate acoustic re-agent. Removing the wooden mouthpiece of the telephone, a small metal chamber was substituted, into this chamber coal gas at high pressure was sent, and burnt from an orifice furnished with a suitable jet. The gas thus traversed the iron disc within two inches of which it issued as a sensitive flame, so that any sensible motion of the disc would be detected by a disturbance of the flame. Although a flame of extreme sensitiveness was obtained, no effect was produced on the flame by any sounds shouted or hissed into the distant and electrically-joined telephone. These results confirm an experiment of Professor Bell's, who glued the iron disc to a thick block of wood, and in this way spoke through and heard by the telephone. Here any molar motion would seem impossible. But if it be a molecular motion one would expect the sound to be of a peculiar quality, the molecular motions of solids having a crepitating character.

The solution of the difficulty seems to be this: Lord Rayleigh has shown (*Nature*, vol. 16, p. 114) that sonorous vibrations may be heard although the amplitude of the waves generating them be of transcendent smallness, an amplitude of one ten-millionth of a centimetre, Lord Rayleigh states is perfectly audible. Hence it is probable that the varied motions set up in the iron disc are motions of the disc as a whole, but of surpassing minuteness, and if so the telephone has directly confirmed Lord Rayleigh's deductions, and added to the wonders of our organs of hearing.

The greatest obstacle to the practical introduction of the telephone is at present the disturbing noises produced by induction from powerful battery currents traversing neighbouring wires. Apart from this it would appear that conversation can easily be carried on two or three hundred miles apart, and breathing has been heard by Professor Bell at 150 miles distance. By employing a return wire instead of an earth connexion much of the inductive disturbance can be neutralized. In the trials of the telephone at the Royal Dublin Society, a loud ticking was heard every second which much interfered with telephonic conversation. This ticking was not due to any neighbouring clocks, but was found to arise from the general electric clock system of the town. Instead of a return wire connexion was made with the gas-pipes of the building; this "earth" happened to be the same as is employed in the electric clocks, and hence the disturbance. Substituting a second wire the disturbance was removed.

Another obstacle at present before the telephone is the difficulty of its use in long submarine lines. This arises from the well-known cause termed "inductive embarrassment," the line with its insulating sheath and the sea around acting as a condenser, and thus preventing the rapid delivery of electric pulsations. Nevertheless, conversation has very successfully been carried on between Dover and Calais and Holyhead and Dublin. The mere *resistance* of a long line but little impedes telephonic conversation. The author of this paper has spoken with great ease through an actual line of some thirty miles, with an added resistance exceeding that of the Atlantic cable. The addition of the artificial resistance simply renders the sound of the voice of the distant speaker fainter, as if he had gone further off, but in no way alters the quality of the sounds heard. Professor Bell has even spoken through a resistance of 60,000 ohms (the resistance of the Atlantic cable is equal to 7,000 ohms).

Various practical applications of the telephone at once suggest themselves. It is already largely in use in the United States for commercial purposes. It has been successfully tried in diving and mining operations in this country. In physical research it promises to be the starting point of new investigations, and as a delicate phonoscope, or sound test, it will doubtless be most useful both in the lecture-room and physical laboratory. The telephone also reveals the existence of very feeble electric currents by the audible vibration of its iron disc. So prompt and sensitive is it to the slightest fluctuation in the strength of the current traversing its coil that it is not unlikely it may be of use in searching out rapid and feeble variations in a current that may escape detection by a galvanometer, owing to the inertia of even a light magnetic needle. Information as to the duration and character of rapidly intermittent currents is needed in medical science and not improbably the telephone may be able to furnish this information, when associated with a chronograph.

The first attempt to transmit sounds by electricity is due to Philip Reis, teacher of natural history in a grammar school at Freidrichsdorf near Homburg. A brief reference to what Reis accomplished may here be of interest. I am indebted to Dr. Messel, a name well-known to chemists, who was a former pupil of Reis and eye-witness of his early experiments, for the following interesting letter on this subject:—

"Reis' first experiments date as far back as about 1852. But at that time ended in failure, and were not resumed as far as I know till 1860. The first publication about Reis' telephone appeared in a daily paper of Frankfort-on-Main, which however I have not succeeded in procuring. Reis gave his first public lecture on October 26th, 1861, when he showed his telephone before the Physikalische Verein (Physical Society) of Frankfort-on-Main, and I send you herewith a copy of his paper.

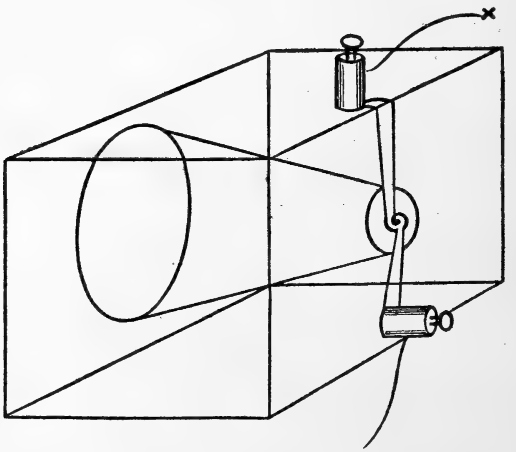
"The original telephone was of a most primitive nature. The transmitting instrument was a bung of a beer barrel hollowed out, and a cone formed in this way was closed with the skin of a German sausage which did service as a membrane. To this was fixed with a drop of sealing wax a little strip of platinum, corresponding to the hammer of the ear, and which closed or opened the electric circuit, precisely as in the instruments of a later date. The receiving instrument was a knitting needle surrounded with a coil of wire and placed on a violin to serve as a sounding board. It astonished everyone quite as much as the more perfect instruments of Bell now do.

"The instrument I have described has now passed into the hands of the Telegraph Department of the German Government."

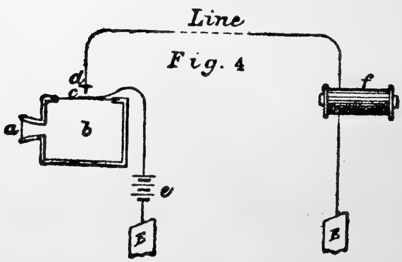
The paper referred to in the foregoing letter is contained in the annual report of the Physical Society of Frankfort-on-Main for the year 1861. It is entitled "Telephony by means of electric currents" for the word *telephone* is first suggested by Reis in this paper as the name of his instrument. The transmitter is shown in fig. 3, instead of a cork a cubical wooden block is used with a conical orifice closed at the smaller end by a membrane.

The accompanying diagram, fig. 4, shows the principle of Reis's

FIG. 3.



Reis's early form of Transmitter.

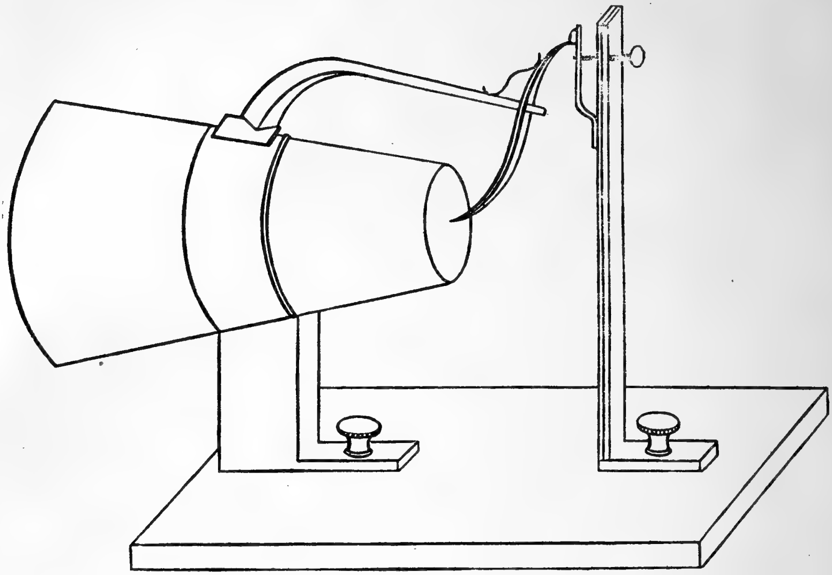


telephone; *b* is a box or small resonant cavity into which the operator sings through the mouthpiece *a*; a diaphragm of bladder or paper *c* covers the upper part of the box. This is thrown into vibration by the voice, and comes into contact with the platinum point *d*. The centre of the diaphragm is furnished with a fragment of platinum foil whereby contact is made and broken with the battery *e*. The *intermittent* currents thus transmitted through the line arrive at the receiver *f*, which is simply a straight iron wire, surrounded by the coil through which the current passes. The rapid magnetizations and demagnetizations of the iron wire by the current give rise to a musical note emitted by the iron, the pitch of the note corresponding to that sung into the receiver.

The discovery that a sound was produced in iron by magnetization is due to an American page in 1837. It was explained by De la Rive, of Geneva, in 1843, who showed that it was caused by the slight elongation of the iron which accompanies the act of magnetization, a fact discovered by Joule in 1842. The author of this paper has found that the magnetic metals nickel and cobalt also yield a corresponding sound on magnetization: with cobalt the note is clearer and more metallic. The author has also corroborated the fact noticed by De la Rive, that stretching the iron wire diminishes the sound, because it diminishes the elongation by magnetization, and further at a certain tension the sound ceases, the elongation here ceasing. At a still greater tension iron *shortens* by magnetization, and the author has found that here too, as we might expect, the sound again is produced, and shortly before the breaking strain of the wire is reached the loudness of the "magnetic tick" is almost as great as with the unstretched wire. By attaching the iron wire, surrounded by its coil, to a monochord, and using a rapidly interrupted current, the rise and fall and extinction of the sounds by varying the tension of the wire can be easily heard throughout a very large theatre.

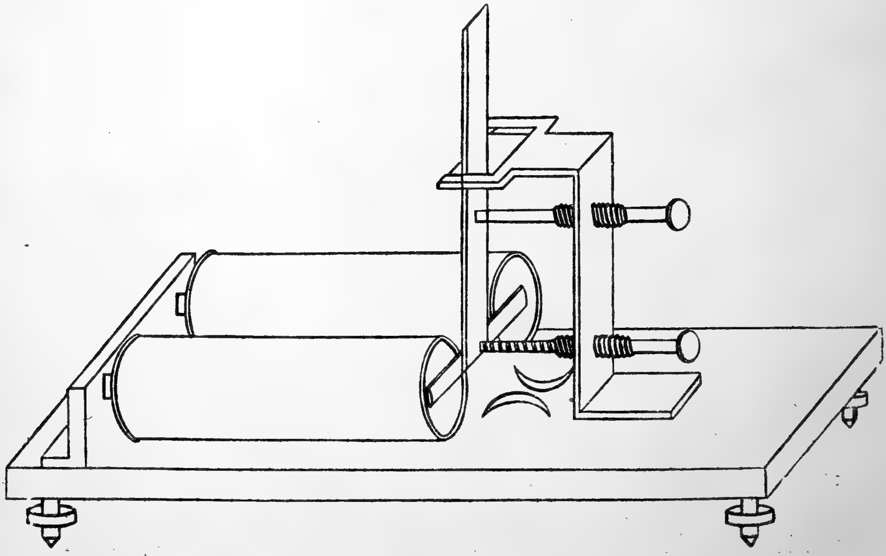
After his first success Reis improved both his transmitting and receiving instruments. In a report on Reis's telephone by Legat, Inspector of Telegraphs in Cassel, &c., published in 1862 in the journal of the East-German Telegraph Company and reprinted

FIG. 5.



Reis's improved Transmitter.

FIG. 6.



Reis's Electro-magnet Receiver.

in 1863 in Dingler's Polytechnisches journal Vol. 169, p. 29, the following sentence occurs. "Melodies can be reproduced with astonishing certainty, whilst single words, in reading speaking, &c., were less distinct, although the peculiar modulations of the voice in speaking, calling, interrogation, surprise or command were clearly marked."

The instrument described in this report is somewhat different from the earlier form. The diaphragm was a collodion film and the contact-breaker behind it was lighter and constructed in form of an S shaped lever, the longer arm of which was in contact with the membrane while the shorter made and broke the circuit (Fig 5.) There was no metal disc on the membrane but the circuit was completed by means of the arm on which the lever delicately moved. The receiver, moreover, was a small horse shoe electro-magnet, fixed horizontally to a sounding-board. (Fig. 6.) Here the movement of a light keeper adjusted by a spring before the poles of the magnet reproduced the original sounds.

In a paper on Reis's improved telephone published in *Böttgers Polytechnisches Notizblatt* No. 15, 1863, it is stated "Particularly distinct was the reproduction of the scale. The experimenters could even communicate to each other words; only such however as they had already heard frequently." In confirmation of this may be added the following extract from a recent letter of Dr. Messel to the present writer. "There is not a shadow of doubt about Reis having achieved imperfect articulation, I personally, remember this very distinctly and could find you many other ear-witnesses of the same fact."

In 1865 a modification in Reis' transmitter was made by Mr. Yeates of Dublin which might have led to important results had it been followed up at the time. A drop of water was introduced between the contact breaker of the transmitter. By this means the current was to some extent rendered a *continuous* one, the essential feature in a perfect articulating telephone, where gradual variations of the current strength are necessary and not sudden interruptions. Mr. Yeates also independently adopted the electro-magnet form of receiver that Reis had introduced in his later form of telephone. The instrument as modified by Mr. Yeates was shown at a meeting of the Philosophical Society in Dublin in 1865 and the articulation of several words was distinctly heard. But even in this and in

Reis's latest form of telephone there is no comparison between the feeble attempts at articulation and the almost perfect articulation that is obtainable in Professor Graham Bell's simple and beautiful instrument: *almost* perfect, but not quite, for there is a peculiarity in the sibilants which render them extremely difficult of transmission, the letter S sounds as F to the listener at the receiving instrument and M sounds as P; so that *whim* becomes *whip* or even *hip*: *strength* turns into something like *creap* or *creace*, &c.

In Professor Bell's telephone the voice itself generates magneto-electric currents and hence the reproduced sounds are very faint and slight electric disturbances are frequently fatal to the effective working of this instrument. The telephone of the future will doubtless employ the voice of the speaker to modulate the strength of an electric current generated by independent means. Hence the discovery of a more perfect and pliable means of varying the resistance in a circuit by the act of speaking is one of the chief objects to be attained at present in the new art of electric telephony.

NOTE ON THE SPHEROIDAL STATE.

BY

W. F. BARRETT, F.R.S.E.

[Read December 17th, 1877.]

At the last meeting of this Society, Mr G. Johnstone Stoney gave a new and beautiful explanation of the so-called spheroidal state of liquids, wherein he showed that the force detected by Mr. Crookes, and which is the cause of the motion of radiometers, was also competent to explain the phenomena of the spheroidal state. A liquid drop is said to be in the spheroidal state when falling upon a hot body it does not come into contact with the surface but rolls over it as a flattened spheroid. A mobile elastic spring evidently buoys up the drop until such times as the hot body cools, when, with a sudden rise of temperature and generation of steam, the drop comes into contact with the surface below it, spreads out into a film, and rapidly disappears into vapour.

Hitherto this phenomenon has been regarded as due to the fact that the proximity of the hot surface converts a portion of the liquid into vapour, the elastic force of which sustains the drop. There are, however, several phenomena, allied to the spheroidal condition, to which this generally received explanation gives no solution. Such, for example, as the mobility of light powders in a hot crucible, or the formation of globules on the surface of water and other liquids. Mr. Stoney's explanation, on the other hand, embraces the whole of these outstanding and hitherto enigmatical phenomena. Briefly stated this theory is based on the fact that whenever two bodies at *different temperatures* are brought sufficiently near each other a modification takes place in the molecular structure of the layer of gas or vapour between them, giving rise to the so-called 'Crookes' force,' wherein there is an excess of pressure in the direction joining the hot and cold surfaces over the pressure in transverse directions. Now this excess of pressure depends partly on the quantity of heat making its way across the intervening layer of gas or vapour, and partly on the proximity of the two surfaces. A proximity not to be estimated absolutely, but with reference to the length to which a molecule of the gas will travel in the intervals between its encounters with other molecules. Hence there are obviously three modes whereby the excess of pressure, this Crookes' force, may be developed or augmented :—

1st. By lengthening the paths of the molecules between the warm and cool surfaces, accomplished by attenuating the gas.

2nd. By bringing the hot and cold surfaces very near together.

3rd. By increasing the difference of temperature between the two surfaces.

Now if the support of the spheroidal drop be due to this Crookes' force a difference of temperature must exist between the drop and the surface over which it stands, and the greater this difference of temperature the larger the drop that ought to be supported, and the more persistent the phenomenon. Mr. Moss has shown (Proc. R. D. S., Dec. 1877) that by securing a continual difference of temperature a globule of ether may be supported on the surface of its own liquid for upwards of an hour, until in fact some accidental derangement occurs. The conditions of the two theories being thus defined, it is easy to see that several crucial experiments might be devised which should help to decide the question at issue.

The following experiment the author has made with this object in view. Upon the surface of the ordinary petroleum of commerce, liquid globules of transient duration can readily be formed, simply by removing a small quantity of the liquid in a pipette and carefully depositing a drop on the surface of the liquid. These drops are clearly in the spheroidal condition, and they are easily and abundantly formed by dipping a vibrating tuning fork into the liquid, or by drawing a fiddle bow over the edge of the vessel containing the liquid. According to the ordinary explanation the drops are supported by the elastic force of the vapour of the liquid, which would, of course, be greater the higher the temperature of both liquid and drops. According to Mr. Stoney's theory the drops are supported by the Crookes' force, generated by the proximity of the drop and liquid, and by the fact that they are at different temperatures. Evaporation rapidly cools the drops jerked up from the liquid, and thus a slight difference of temperature instantly comes into play. If, however, Mr. Stoney's theory be true, then a drop of cool petroleum would be more easily and longer sustained on a surface of warm petroleum, or *vice versâ*, than a drop taken from the mass of liquid below it, where only a slight temperature difference is created.

Two beakers were filled with petroleum from a common source, one (A) at the temperature of the air, the other (B) at a temperature of 100° F. With a pipette some liquid was taken up from A and a drop carefully deposited on its own surface, a globule was formed, floated for a fraction of a second and then disappeared. The same occurred with a drop from B placed upon B. A drop of B was now removed and deposited on A, a large globule was easily formed on the surface, floated about from 10 to 20 seconds and then disappeared. A drop of A was now placed on B, the same thing occurred, but the duration of the drop was not quite so great, owing to the greater density of the cool drop tending to sink it below the surface of the warm liquid, thus rupturing the Crookes' layer and destroying the difference of temperature.

There is no doubt or uncertainty whatever about this experiment, and it shows that, if the ordinary explanation be correct the second case, where B rests on B, should give the best result, whereas the reverse is the case. Further, the experiment wherein the best result is obtained, is such as best fulfils the condition of Mr. Stoney's theory.

The limit of formation of these spheroids, when the liquid is uniformly dropped through a gradually increasing height, may be employed to test the relative degrees of force which sustain the globule, and careful experiments made by the author in this direction still further corroborated the truth of Mr. Stoney's views.

ON THE SPHEROIDAL STATE.

BY

RICHARD J. MOSS, F.C.S.

(Read December 17, 1877.)

In a paper on "The Penetration of Heat across Layers of Gas" read at a recent meeting of the Society, Mr. Stoney* includes Leidenfrost's phenomenon, or as M. Boutigny† calls it, the spheroidal state of volatile liquids, as an instance of the action of the peculiar form of pressure which is exerted between hot and cold surfaces when they are within a certain distance from one another, depending on the length of the free path of the molecules of the gas enclosed between the hot and the cold surfaces, and on the difference of temperature of these latter. This explanation of the spheroidal state places the phenomenon in an entirely new light. It has hitherto been supposed that a volatile liquid in this condition was sustained, and prevented from touching the adjacent hot body by a rapid disengagement of vapour from its surface. The explanation that was generally accepted is very clearly expressed by Dr. Tyndall in his "Heat as a mode of Motion," p. 154, where, referring to a spheroid of water in a hot metallic basin, he says :—"The drop rolls about on its own vapour—that is to say, it is sustained by the recoil of the molecular projectiles discharged from its under surface. I withdraw the lamp, and allow the basin to cool, until it is no longer able to produce vapour strong enough to support the drop. The liquid then touches the metal; the instant it does so violent ebullition sets in."

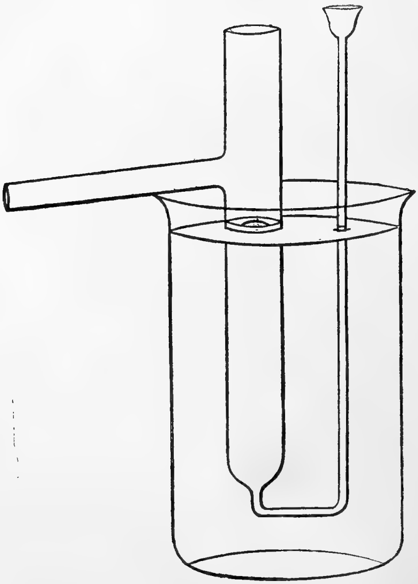
It has, however, been regarded as a remarkable circumstance that a quantity of vapour sufficient to produce the effect could be given off by the liquid under the circumstances, for, as M. Boutigny has shown, the spheroidal drop is always at a temperature below its boiling point, a condition that is not favourable

* Transactions of the Royal Dublin Society, vol. 1 (new series), p. 13.

† Proceedings of the Royal Institution of Great Britain, vol. 1, p. 179.

to the maximum production of vapour. Mr. Stoney's explanation not only removes this anomaly, but demands the comparatively low temperature of the spheroid as an essential condition of the phenomenon, since the existence of the layer of polarized air or vapour by which the spheroid is supported, requires the proximity of a cool surface, in order that the velocity of the gaseous molcules moving from this surface may be much less than that of the molcules which move towards it from the adjacent hot surface.

Mr. Stoney suggested to me the possibility of maintaining spheroids on liquid surfaces of the same substance by ensuring that the floating drop shall continue cooler than the liquid underneath, and by otherwise removing the causes which occasion its shipwreck. Commercial ether readily adapts itself to the required conditions. After a series of experiments I have adopted the arrangement shown in the figure as one which successfully accomplishes the desired objects.



A thin glass tube about 15 mm. in diameter and 12 cm. long having a spout-like limb about 7 mm. in diameter and 7 cm. in length attached 4 cm. from one end, is contracted at the other end, and attached to a narrow tube bent twice at right angles, so that the two tubes are parallel. The narrow tube terminates in a small funnel. This apparatus, held vertically in a clip, is immersed in a beaker of water, so that the surface of the water is about a centimetre below the attached end of the spout-like limb, the other end of which extends beyond the edge of the beaker. By means of the funnel ether is poured into the narrow tube until it rises in the larger tube to the level of the water in the beaker, or a millimetre or two above it. The beaker stands on an iron plate which can be heated in any convenient way until the temperature of the water in which a thermometer is placed reaches 30°-32° C. If a narrow pippette with a very small opening be now immersed to the depth of 1 or 2 cm. in the ether in the large tube and quickly withdrawn to a short distance, the drops which fall from it being rapidly cooled by evaporation, readily assume the spheroidal state, and often continue to float on the warm ether for a long time. The evaporation from the spheroid is so very slight that some time must elapse before any alteration in its size can be detected. On the other hand, the warm ether on which the spheroid floats evaporates very quickly, and its heavy vapour flows out through the spout at the side mixed with air which is drawn in at the mouth of the large tube. This cool air passing over the surface of the spheroid enables it to part with the heat which it is constantly receiving from the warm ether by radiation, by the passage of heat through the Crookes's layer on which it is supported, and by the precipitation of ether vapour. The ether lost by evaporation is replaced from time to time by pouring ether in at the funnel. By means of this arrangement I have kept a spheroid about 5 mm. in its longest diameter, floating for more than an hour and a half.

In the progress of this experiment the sustaining medium becomes ether vapour alone; it may, however, be experimentally demonstrated that the presence of a gaseous medium other than that derived from evaporation produces the same effect. For this purpose it is desirable that the substance employed should not be very volatile. It is also important that it should be specifically light, as the heavier the spheroid the more force has to be exerted to keep it afloat, or in other words, the greater must be the disparity of the velocities of the molecules of the intervening medium; and to produce this disparity there must be a considerable difference of temperature between the spheroid and the liquid on which it floats. In the case of light liquids there is less force to be exerted, and therefore a slight difference of temperature suffices. I find melted paraffin supplies all the requirements of the case. It is light, its sp. gr. at 15° c, being 0·86, while in the liquid state it is much lighter. On placing ordinary paraffin in a shallow silver basin and melting it I found that drops in the spheroidal state were easily obtained at temperatures not greatly exceeding the melting point of the paraffin. They are very easily obtained at 80°–90° C, and by directing a current of cool air over the surface of the liquid they may be kept in existence for a considerable time. I have kept them afloat for more than twenty minutes without any special attention to the temperature of the paraffin or the strength of the current of air employed to keep the drops cool, and I have no doubt that with care their existence might be indefinitely prolonged. I have devised a method for performing the experiment in a closed flask in which any gas may be contained, and at any desired tension; my experiments with this apparatus are, however, not yet completed.

For the purpose of ascertaining whether there was any appreciable evaporation from melted paraffin at a temperature favourable to the existence of spheroids, I placed three porcelain capsules containing paraffin on an iron plate, and heated them until a thermometer placed in one of them rose to 90° C. One of the capsules was then allowed to cool, and weighed, after which it was replaced on the hot plate and kept there for two hours, during which time I occasionally produced spheroids on the paraffin in the third capsule, some of these lasting a considerable time. When the weighed capsule was allowed to cool and again weighed I found a very slight increase had taken place. On repeating the experiment twice similar results were obtained. I therefore decided upon performing the experiment *in vacuo* for which purpose 5·5 grammes of the paraffin that had been repeatedly heated in one of the capsules was transferred to a small flask which was attached to an air pump and exhausted.

The paraffin was melted by immersing the flask in boiling water. After half an hour the flask was allowed to cool *in vacuo* and weighed. This operation was now repeated, the paraffin being heated for an hour by enclosing the entire flask and part of the tube communicating with the pump in a metallic vessel which was immersed in boiling water. The paraffin having been allowed to cool *in vacuo* was found on again weighing the flask to be precisely the same weight as it was originally. I could have detected a loss of 0·00005 gramme or $\frac{1}{110000}$ of the weight of the paraffin if it had taken place, and considering that the temperature employed was 10–20° higher than that at which spheroids of paraffin are readily produced, I think one may reasonably conclude that paraffin is not appreciably volatilized at a temperature which admits of the existence of a spheroid on its surface. And since the spheroid must in this case be at a lower temperature than the liquid on which it floats, it is even less likely to produce vapour sufficient to keep it floating. In confirmation of this conclusion I may mention that I have been unable to detect the slightest diminution in the size of paraffin spheroids, though I was occasionally much puzzled by the appearance of small spheroids in the place of large ones which I had left floating a short time before, until I detected one of these large drops disappearing with a slight splash which called into existence a new spheroid much smaller than its predecessor. These experiments in all their details are in accordance with Mr. Stoney's explanation of the phenomenon, and they demonstrate that the previously accepted theory is untenable.

It is noteworthy that in 1874 Mr. Crookes* suggested that "The phenomenon of the spheroidal state is probably due in some measure to a repulsive force exerted between closely approximated bodies, one of which is at a very high temperature." And although the true nature of "the repulsive action of radiation," to which Mr. Crookes considered the phenomenon attributable was at this time unknown, he ventured to anticipate "that a condition similar to the spheroidal state will be found to obtain between non-volatile bodies."

* Philosophical Transactions of the Royal Society of London, vol. clxiv., part 2.

NOTE ON THE MICROSCOPIC STRUCTURE OF THE SCALE OF *AMIA CALVA.*

BY

H. W. MACKINTOSH, B.A.,
Senior Moderator in Natural Science, Trinity College, Dublin.

[Read December 17th, 1877.]

ABOUT four years ago Professor Macalister, Director of the Museum of Comparative Anatomy and Zoology in the University of Dublin, obtained a fine specimen of the North American Ganoid *Amia calva,* and before sealing it up and placing it in the Museum, kindly gave me one of the scales for microscopic examination. Pressure of other matters prevented me at the time from bestowing upon it more than a cursory glance, and it has lain mounted in my cabinet almost forgotten till a few months ago, when, having occasion to demonstrate some points in the structure of the scales of fishes, I was led to give it a more careful study, which brought to my notice a peculiar form of lacunæ which does not seem to have been hitherto described, and which may be of interest to the members of this Society.

Amia is commonly described as a ganoid with overlapping cycloid scales. I submit that in the face of figs. 1 and 2, the term cycloid must be abandoned and replaced by ctenoid. The mistake has probably arisen from the fact that the teeth on the free edge are too fine to be noticed by the unaided eye, whilst the ridges of which the teeth are the terminations give a circularly striated appearance to the whole scale, a deception which is aided by the deposit of thickening layers on the proximal parts of the scale, leaving thinner and therefore more transparent spaces between them. In reality the ridges run in a sinuously longitudinal direction, with a certain tendency to confluence along a line (*a*, *b*, fig. 1) transverse to the scale, and placed at a short distance from its fixed (anterior) edge. They are apparently compound, separated from each other by a very narrow groove (*a, a, a,* fig. 2), and bearing on their upper surface a broad flat elevation, the middle of which is again raised up into a sharp narrow ridge. The lacunæ whose form I wish specially to call attention to are grouped together in the middle line near the anterior edge of the scale, and appear as minute black dots visible to the unassisted eye on careful scrutiny. They are placed immediately beneath the superficial layer of ganoin which clothes the whole surface of the scale, and apparently makes up the entire of the posterior thin part, which is altogether devoid of lacunæ of any sort. When examined under a low power (180 diams.) they are seen (fig. 3) to consist of a central axis which for the most part has a decidedly fusiform shape, but which sometimes (*a,* fig. 3) is simply linear. From this central axis arise a number of very short canaliculi, which are mostly linear and end abruptly, but sometimes taper rapidly to a very fine point. Starting from these, in some cases, and superposed on them in others, are other canaliculi, which lying thickly together often give the lacuna to which they belong a wonderfully confused appearance (*b,* fig. 3), and may even render it a matter of some difficulty to trace the central axis. It must be understood, however, that in many cases like *b,* fig. 3, the apparent canaliculi are really small independent lacunæ, not connected at all with the central lacuna but merely overlying it. As a rule there is not much tendency to communication amongst the different lacunæ; sometimes (*c, d,* fig. 3) we find a small lacuna or a system of them in slight connexion with a larger one, though I have not been able to satisfy myself that this is always a true anastomotic union. Employing a higher power (260 diams., fig. 4) we bring out more clearly the fusiform shape of the main lacuna, and the usually abrupt termination of the canaliculi. Very often (*d,* fig. 3; *a, b,* fig. 4) these are placed with singular regularity, forming a series of crosses with the points of intersection at the lacuna, giving the entire system an appearance which is unique so far as I can ascertain. These are the only lacunæ found at the centre of the scale, but as we go out towards the margin we find others making their appearance, more in accordance with the usual form. Thus in fig. 4, which represents a group occurring in the same field of view, whilst *a, b,* and *c* are eminently characteristic and unlike ordinary lacunæ, *d* with its elongate and more tapering form and fewer canaliculi is assuming a more normal appearance, and *e* is still more normal, having altogether lost its short abrupt lateral branches, and only presenting very fine and tapering terminal ones. At the margin of the scale we find lacunæ of this kind alone (fig. 5), differing from each other only in respect of their length, number and extent of canaliculi, &c., and reminding us of the well known lacunæ of the allied *Lepidosteus osseus* (fig. 6) which are figured in most books on microscopy. They differ from them, however, in being much larger, less globular, and having much wider canaliculi which do not anastomose to the same extent; whilst the scale itself does not exhibit any of the canals running in from the surface which seem to be constant in the scales of Lepidosteus. There do not appear to be any lacunæ in the posterior part of the scale from about *c d* backwards, in front of this line they begin as small simple forms like *e*, fig. 4, and *a*, fig. 5.

Explanation of the Plates.

Fig. 1. Entire scale of *Amia calva* magnified 5 diameters, showing the finely toothed posterior edge, the general direction of the ridges, and the position of the characteristic lacunæ. The line *a b* indicates the place where the ridges tend to run into each other, *c d* the level behind which no lacunæ are found.

Fig. 2. A portion of the posterior part of the scale showing the nature of the ridges and the toothed border, ×90. The arrows show the direction in which the light came, oblique illumination having been used.

Fig. 3. Lacunæ from the central part of the scale, ×180. At *a* is seen one in which the central lacuna is linear; *b* is one of the highly complex forms; *c* and *d* have smaller lacunar systems connected with them.

Fig. 4. Lacunæ occurring in the same field of view, ×260. Taken from a part of the scale about half way between the centre and the margin; *a*, *b*, *c*, are of the characteristic complex form, *d* and *e* are simple.

Fig. 5. Lacunæ from the marginal part of the scale, ×260. At *a* is seen one of the simplest forms.

Fig. 6. Lacunæ from the scale of *Lepidosteus osseus*, ×260 showing the small size of the lacunæ, their globular shape and very fine canaliculi.

The drawings were all made under a Wollaston camera lucida.

Fig. 1.

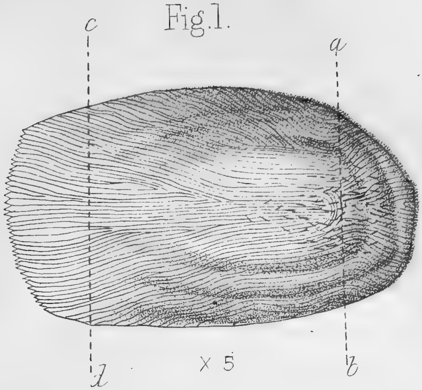


Fig. 2.

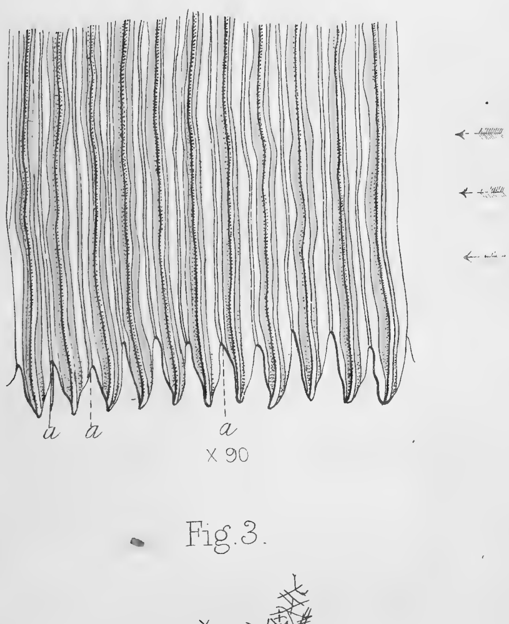


Fig. 3.

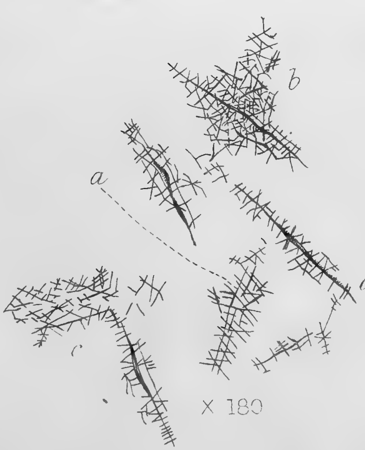


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Fig. 4.

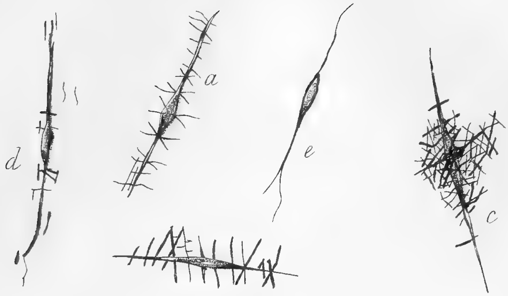

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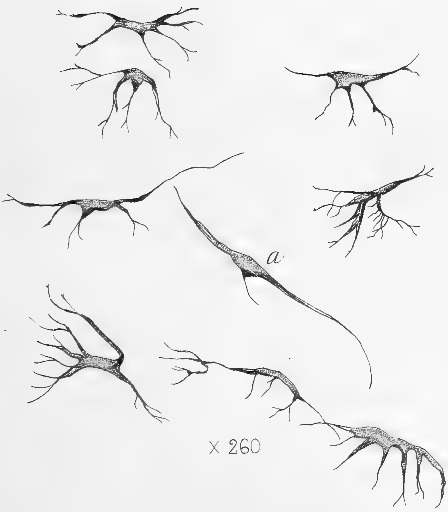
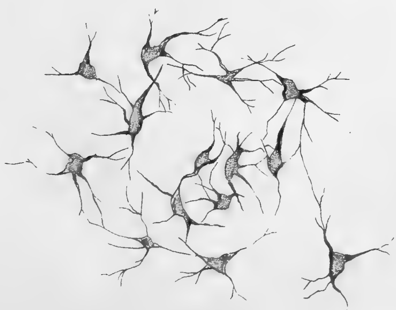
Fig. 5.



X 260

Fig. 6.



X 260

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PUBLICATIONS OF THE ROYAL DUBLIN SOCIETY.

TRANSACTIONS: *Quarto, in parts, stitched.*

Vol. I. (new series). [Already published.]

Part 1.—On Great Telescopes of the Future. By HOWARD GRUBB, F.R.A.S. (November, 1877.)

Part 2.—On the Penetration of Heat across Layers of Gas. By G. J. STONEY, M.A., F.R.S. (November, 1877.)

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Part 1.—Pages 1 to 52. (November, 1877.)

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" 3.—Concluding Volume I. With Title Page and Table of Contents. (November, 1878.)

Vol. II. (new series.)

Part 1.—Pages 1 to 120. (October, 1878.)

Part 2 will be published in January, 1879.

ON THE VARIOUS FORMS OF APPARATUS USED FOR POLISHING SPECULA FOR REFLECTING TELESCOPES.

BY

SAMUEL HUNTER, F.R.A.S.

[Read 17th December, 1877.]

We owe the reflecting telescope to Sir Isaac Newton,* and the idea was presented to his mind through a remarkable mistake he made in believing that refracting telescopes as then made were incapable of further improvement. In a paper presented to the Royal Society in February, 1672, he writes :—"In the beginning of 1666 (at which time I applied myself to the grinding of optical glasses of other figures than spherical) I procured me a triangular prisme to try therewith the celebrated phenomenon of colours," which "was at first a very pleasing divertissement." Starting from this "divertissement" he found that the colours observed in the telescopes, as then constructed with object-glasses consisting of a single lens, were due to the nature of light. He writes—"When I understood this I left off my aforesaid glass works, for I saw that the perfection of telescopes was hitherto limited—not so much for want of glasses truly figured—as because that light itself is a heterogeneous mixture of differently refrangible rays; so that were a glass so exactly figured as to collect any one sort of rays into one point, it could not collect those also into the same point which, having the same incidence upon the same medium, are apt to suffer a different refraction." That is, he discovered that rays of light falling on a single lens at AA′ become decomposed into the colours of the spectrum on leaving it at B B′, the red rays coming to a focus at r, the violet at v, and the other colours at intermediate points. Sir Isaac failed† to discover that with different kinds of glass the distance vr, varies (*i.e.* their dispersion) and hence did not see that by combining glasses of

* Gregory described his form of reflecting telescope in 1663, and had one constructed in 1664, but failed in obtaining a satisfactory result.

† Query—Did this arise from his having used only one kind of glass in his prisms?

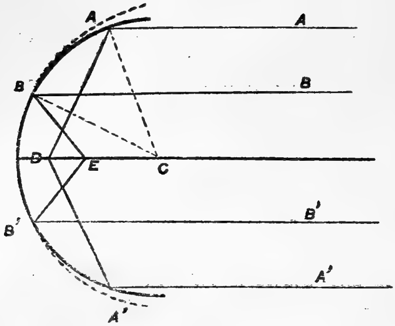


Fig. 1.

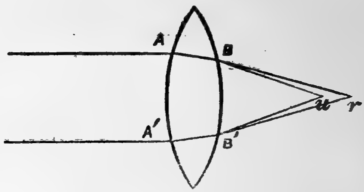


Fig. 2.

different dispersive powers an achromatic telescope could be formed.

"This made me," he writes, "take reflexions into consideration, and finding them regular, so that the angle of reflexion of all sorts of rays was equal to their angle of incidence, I understood that by their mediation optick instruments might be brought to any degree of perfection imaginable, provided [1] a reflecting substance which would polish as finely as glass, and [2] reflect as much light as glass transmits, and [3] the art of communicating to it a parabolic figure be also attained." Here Sir Isaac gives us the three requisites to a good reflecting telescope, but it is with the last alone we have to do at present.

Let us first see why the figure must be parabolic. Suppose the rays A and A′, B and B′ fall on a segment of a sphere, A and A′ being more remote from the axis than B and B′, C being the centre of curvature, Sir Isaac found that the angle of incidence AAC was equal the angle of reflection DAC. So also the angle BBC=EBC. Hence parallel rays falling on a true spherical surface come to a focus at different points, according to their distance from the axis of curvature DC. Now if the curve at A could be flattened a little, as indicated by the dotted line, it is clear that the angle of incidence, AAC, could be made=the angle EAC and all the rays would come to a focus at the same point E. The curve of which that holds good is called a parabola. The difference between these two curves is so small that at the margin A of a telescope 4 feet in diameter and 40 feet focus, their distance apart is less than the $\frac{1}{21000}$ of an inch ($\frac{1}{213333}$). You will observe that the parabolic figure is best suited for parallel rays, such as come from the heavenly bodies; for viewing terrestrial objects the spherical form is just as good, and for near objects, much better.

After two years' interruption, caused by the Great Plague, Sir Isaac Newton, to use his own words, had then "thought on a tender way of polishing proper for metal," and set to work. After a time he succeeded in making the first reflecting telescope of thirteen inches radius, by which he was able to see Jupiter's four concomitants, as he calls them. And strange to say, since then we have simply been following in his track; he indicated the metals still used in the construction of the speculum, and the mode of operation we owe to him in a great measure; we have only improved in the details.

The production of a spherical surface is comparatively easy, for the mutual rubbing of two bodies naturally tends to produce that form, but it is otherwise with the parabolic.

We will first describe in detail the hand process of polishing specula used up to the time of Sir Wm. Herschel, who first constructed a machine for this purpose.

A tool made of iron, pewter, or some such material, was cast and turned to a radius of twice the intended focal length. Some had this tool of a greater diameter than the speculum, some of the same diameter; this was fastened on an upright post, with the face upwards, emery and water was applied, and the operator, holding the speculum by a wooden handle cemented to its back, walked round the post, pushing the speculum to and fro in straight, elliptic, or circular strokes, supplying emery from time to time, until every part of the speculum was acted on equally by the emery. The tool was then examined to see if the curvature had altered, if so it was turned again to the proper radius, and the grinding proceeded as before. When the speculum was equally acted on by the emery, and the tool found to be of the proper curve, finer emery was applied, and finally using only the sediment obtained from water in which flour of emery had been stirred up and allowed to stand for ten seconds, thirty seconds, and up to four minutes before being poured off.

If proper care were taken, the surface was now free from scratches, capable of reflecting a considerable amount of light, and of a spherical form. The polishing now commenced, the object of which is not only to make the surface reflect the greatest amount of light, but also to give the parabolic figure, and in this consists the great difficulty.

Speculum metal is very porous; a moderate magnifying power will show the surface full of little holes, so that if we used an elastic cushion of any kind to carry the polishing powder, the result would be that the powder would be forced into these pores, whose edges would be worn away, and the entire surface would thus be full of little pits, so that a true figure or good reflecting surface would be impossible.

On the other hand, if we used a hard unyielding substance, we could only produce a spherical surface, as it is by the mutual adaptation of the two surfaces that we are enabled to obtain the parabolic.

Fortunately, in pitch we have an almost inelastic body capable of adapting itself to any surface with which it is in contact, and yet being a solid. Ice is the only other solid that I know of which possesses this latter property. The tool used in grinding, or another made to the same curvature, usually a little larger than the speculum, was now covered with pitch to a depth varying from the thickness of a half-crown to half an inch; grooves were cut in this to allow it to expand equally in a lateral direction, and thus become more quickly adapted to the speculum. Some made the polisher smaller than the speculum, and others used an oval polisher.

The Rev. Mr. Edwardes, writing in 1787, recommends that the longest diameter be to the shortest as 10 : 9, the latter being equal to that of the speculum, and seems to be the first who used the oval polisher. Sir John Herschel speaks highly of the oval form, as also Messrs. Delarue and Nasmyth.

The consistency of the pitch should be such that a sovereign resting on its edge for one minute should leave clear impressions of three of the millings on its edge (Lassell's test); if softer than this it will not produce a good figure, if harder an inferior polish will be the result. The pitch can be made harder by boiling or softer by the addition of oil of turpentine.

The polisher being now prepared the speculum was laid on it, being first gently warmed to about 80°, and moved about on the polisher gently until it was found that the pitch touched it at all points. Rouge and water was then applied and polishing commenced, the operator proceeding as in grinding, carefully observing that the polishing proceeded equally.

Tripoli was at first used, but is now entirely superseded by the sesquioxide of iron or Jewellers' rouge, by which name it is sold in the shops.

Mr. Mudge polished the speculum to a true spherical surface and then ended by a few large circular strokes upon the round polisher so as to increase the radius of curvature near the margin. I hope you will not consider that I have gone too much into detail, but as the machines used only change the mode of using the power, I shall not have to recur to these processes again.

The disadvantages of hand polishing are, the unequal pressure, the inability to exactly control the length and direction of the stroke, the irregular increase of temperature from friction, and finally the inability to work specula larger than eight or nine inches in diameter.

As already stated, Sir William Herschel was the first who used a machine in polishing, although it is not many years since a description of it was first published.

We shall now proceed to describe it.

Sir William Herschel's Machine.

The ring R S G surrounded the speculum which rested on the polisher face downwards; within this ring loosely fitting, and held in position by three pins above and below, there was a thin flat ring T K L, on which was screwed a ratchet ring; this annulus also carried three cocks at T, K, and L, which rest upon the speculum, with flanges projecting downwards, covered with felt, and capable of being adjusted so as to hold the speculum concentric, yet without being pinched; R C S is a claw attached to the ring by two pivots at R and S, and with an eye at C, to connect it by a pin to the lever A B, which pivots on B, the power being applied at A. D K and D L are two arms fastened

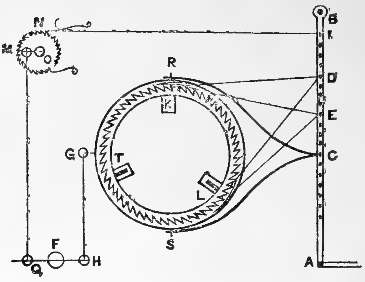


Fig. 3.

to the lever at D, and kept pressed by springs at K and L against the ratchet ring, D K ending in a hook, so that when the lever A B is pushed towards the speculum this hook seizes the ratchet ring and causes the speculum to turn a little, because the motion at D is less than at C; for the same reason, when the motion is reversed D L pushes against the ratchet ring and causes another little turn.

I N is a similar arm, causing at each pull the ratchet wheel M N to turn; this wheel carries an arm, O M, attached to the lever Q H, H being attached to the projection G; Q H pivots on F. By regulating the length of O M or F H or both, the amount of side motion is adjusted. The arms attached to the lever at E caused the polisher to revolve when a circular one was used. But Sir John Herschel states, that with a speculum of 18¾ inches diameter by using a fixed oval polisher of 1·12 and 0·97 diameters (the speculum being 1), with grooves at an angle of 45° to the stroke, he obtained most satisfactory results without using any side motion, H being then fixed in position and the arm I N detached. When the speculum and polisher are caused to revolve, with the side motion in action, the speculum will describe curves somewhat similar to those of Lord Rosse's polisher, but the shock given to the speculum at the commencement of each push and pull must be injurious. In Lord Rosse's machine this shock is not felt, the stroke being given by a crank motion. The length of stroke used on a round polisher without side motion was 0·47, and with side motion 0·29, the total side motion being 0·19, the speculum as before being 1.

Lord Rosse's Machine.

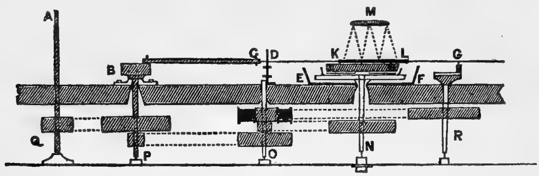


Fig. 4.

In this machine the speculum is worked face upwards, both grinder and polisher being grooved so that an even distribution of the emery used in grinding is obtained as well as of the rouge in polishing. The polisher fits loosely but accurately in the ring, so that it revolves with the speculum, but at a different rate, partly from being carried round with it by friction at the end of each stroke, and partly from the change of direction given to the stroke each time by the revolution of the eccentric.

The power is applied by means of the spindle A, which drives the spindle P, carrying the eccentric B ; this eccentric gives the length of stroke, equal about one-third the diameter of speculum. P again drives O, which latter drives N and R ; on N rests the speculum H I, while R carries the back eccentric G, which controls the side motion, usually about one-fifth of the diameter (measured on the side of the speculum). The diameters of the various pulleys, as now used for polishing the three feet speculum, are approximately as follows :—Those on the spindle P, 30 and 7 (inches) ; O, 18, 9, and 18 ; N 36, and R 30 ; G performs a revolution in about five strokes of the eccentric B, and the speculum once in about eleven ; D is a fixed guide, D G being rigidly attached to the ring K L, carrying the polisher. This bar and polishing ring is supported at D, and by the fork at G, which latter is free to revolve in its socket. The polisher is counterpoised, leaving a weight of about 10 lbs. pressing on the speculum, M being a circular disc attached to one end of the lever by its centre, but by six hooks in its circumference to the polisher.

The curve actually described by the centre of the polisher in the model of Lord Rosse's machine which you see before you, of one-third the dimensions, and on which I have frequently worked to an excellent figure a 9-inch speculum, is given in figure 5, where you have the result of $9\frac{1}{2}$ strokes, $2\frac{1}{4}$ revolutions of eccentric G, and 1 revolution of the speculum, the stroke, &c., being adjusted to a 9-inch speculum.

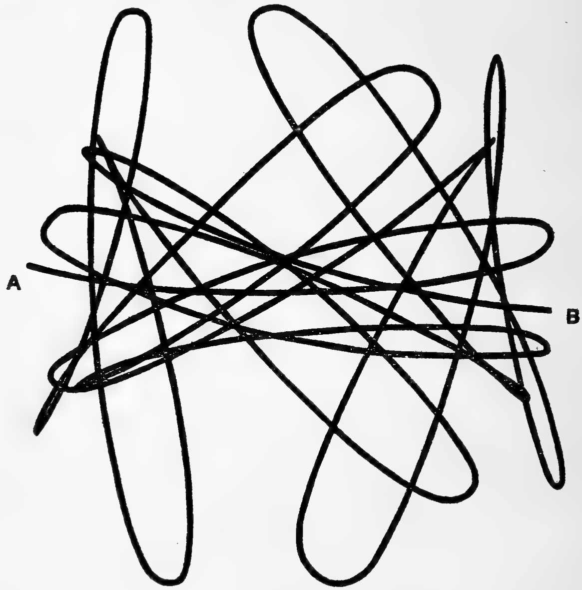


Fig. 5.

Mr. Lassell's Machine.

The power is applied in this machine to the pully I I, which drives the worm spindle H K, this drives the wheel L of 77 teeth, and by means of the band G, a similar wheel C, on which the speculum rests. The spindle M turns with L, carrying the arm S P; the wheel O is fixed to the frame N, so that as the arm S P is carried round the pinion Q of 16 teeth works in O of 72; R of 72 revolves with Q, working in the pinion T of 16; T carries with it the eccentric V; T and V can be adjusted by means of the slots as shown in the figure. A pin at V carries round the polisher J.

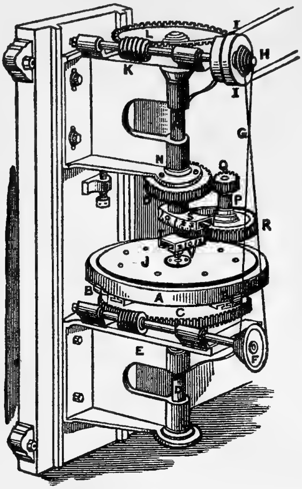


Fig. 6.

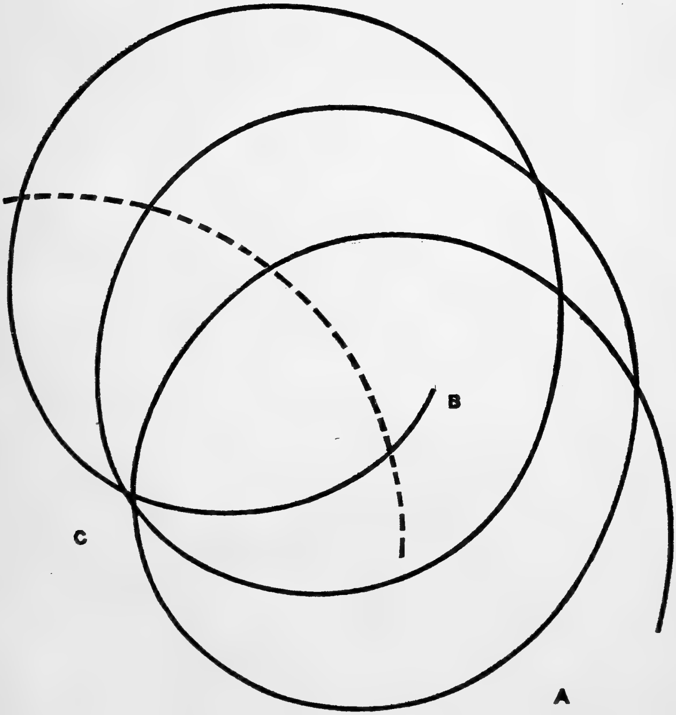


Fig. 7.

For a 2-feet speculum H K revolves about 132 times per minute; the eccentric S 1·7 times; V 34·7. The speculum A, 0·46, T being 1·7 inches from its centre to centre of spindles, M, and V being 1·4 inches from centre of T. The curve described by the centre of the polisher in these conditions is shown in figure 7. An exactly similar curve would be obtained if S did not revolve at all (the revolutions of T being unaltered), and the speculum at a speed of 1·7+0·46, the sum of their velocities, equivalent to 1 turn of speculum to about 16 of V. The dotted line in figure 7 shows the path of the centre of T, the centre of the speculum being at C. It was found by Mr. Lassell that the polishing was apt to proceed in rings to the detriment of the figure, he caused the speculum to rest on a sliding bed instead of directly on the wheel C, which being acted on by a roller fastened to the wall plate, received a thrust of about one and a-half inches on two opposite sides. Delarue made a further improvement by causing the speculum slowly to revolve on this, so that the thrust did not always occur across the same diameter.

Mr. Grubb's First Machine.

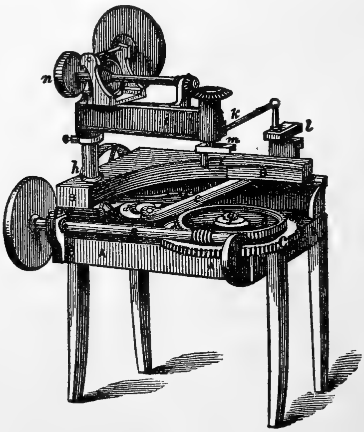


Fig. 8.

This machine gives Lassell's curves, and is capable also of approximating closely to Lord Rosse's.

f is a worm-shaft driving the wheel *b* of 90 teeth; *c* also of 90 revolves with *b* and works a wheel of 52 attached to the lower surface of the cam *d*; in the groove of this works a pin fastened to the cam-lever *e*, which is thus made to oscillate; this is linked to the sliding-plate B, which thus by the rotation of the cam is made to vibrate with a uniform motion. The plate B carries a spindle *h*, on which vibrates the arm *i*. The link *k* will either hold this arm in position or cause it to vibrate with the eccentric *l*, which is driven by a shaft at the back. The variable crank *m* can be driven either quickly by the pully *n*, or slowly by the worm-wheel *o*. The pin in *m* drives the polisher.

To give Lord Rosse's motion the pin of *m* is set central, and *l* is set in motion, this gives the stroke, the side motion being given by the sliding plate B. To give Mr. Lassell's, *l* is stopped and fixed in position, so that the pivot of *m* is equal the distance of the centre of the pinion T (fig. 6) from the centre of the speculum, the arm *i* being held firm by the link *k*; the pin of *m* is then set to the distance V T (fig. 6), and by the vibration of B, Mr. Lassell's eccentric motion can be given.

This machine Mr. Grubb has superseded by one of great simplicity of construction.

Mr. Grubb's Second Machine.

In figure 9 you have an isometric perspective view of the machine with which the great Melbourne telescope was ground and polished.

A is the speculum in its box revolving on a vertical spindle, B the polisher, a portion of the weight of which is counterpoised by a lever attached to the bar *a*. The horizontal bars *b*, *b′* are attached to *a* at *c*; these are moved by the cranks at *d d′*, which receive their motion (by means of bevelled wheels) from a horizontal bar connected with the driving pully D. By the adjustment of the length of the arms *b b′*, or of the cranks at *d d′*, a great variety of curves can be given to the bar *a*, carrying the polisher.

By means of the handle at *e* the speculum was made to turn on its edge so as to view a distant artificial star, and thus to test the figure—thus a great saving of time was effected and the risk of accident diminished.

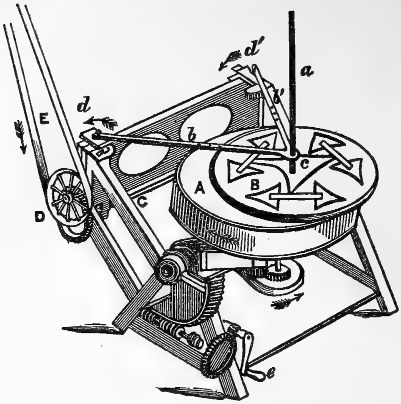


Fig. 9.

A curve produced from a similarly constructed machine set at random is given in fig. 10.

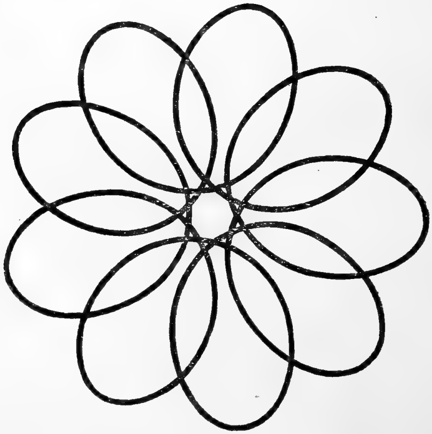


Fig. 10

It is unnecessary to speak of Foucault's process of hand abrading or the American system of local polishers, as these require highly skilled artificers to be successfully used.

The choice of machines evidently lies between that of Lord Rosse's and that of Mr. Grubb's No. 2. The latter is so compact and admits of such a variety in the curves which the polisher may be caused to describe, that it probably deserves the preference. On the other hand, on looking at figure No. 5, and observing the variety of continuously varying curves described by the polisher in Lord Rosse's machine, one has little doubt that with it there will be less liability to polish the surface in rings than with a machine which gives a uniform curve of whatever description.

The objection that the polisher is kept too long over the edge by the eccentric (G, figure 4), is obviated by making the pully by which it is driven oval as in the machine for Lord Rosse's 6-feet speculum.

It is certain, however, that both machines have produced good results in the hands of their inventors.

NOTE.—In Mr. Grubb's Machine No. 2, one revolution of the speculum coincides with 14 strokes of the eccentrics, of which there were 33 per minute in the rough grinding, and 24 in fine grinding and polishing.

ON BABBAGE'S SYSTEM OF MECHANICAL NOTATION AS APPLIED TO AUTOMATIC MACHINERY,

BY

HOWARD GRUBB, C.E., F.R.A.S.

Read December 17th, 1877.

In the year 1826, the celebrated mathematician, Mr. Charles Babbage, presented a paper to the Royal Society of London, on a method of expressing by signs the action of machinery.

The ingenious and elegant system Mr. Babbage describes in this paper appears to have been paid but little attention to by engineers, and I can only find one mechanical author, and that of old date, treating of Mr. Babbage's system.

The fact of this (as it seems to me), most useful system of notation, having been apparently buried in oblivion, has induced me to bring the matter under the notice of this Society in the hope that it may prove as useful to many others as it has been to me. I may also say that I am further induced to notice this matter, from the fact that my father, who made considerable use of this system in planning his automatic printing, and other machinery, found the system capable of extension in directions, certainly not recorded, and perhaps never contemplated, by Mr. Babbage.

I shall first endeavour to explain the object which Mr. Babbage had in planning this notation, then the principle of the system, and lastly the uses to which it may be applied.

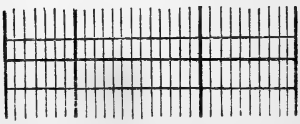
Firstly. The object Mr. Babbage aimed at was to supply a serious want which he felt existed in graphically representing an elaborate piece of machinery. He desired to devise such a method of graphical representation as would present to the mind of the mechanic a true representation, not so much of the general form and disposition of the small parts, for that can be done by ordinary draughtsmanship, but of the quantity and nature of the different movements, the time each movement occupies, and the sequence of such movements, &c. Such a representation could no doubt be made by a series of drawings of each part, showing the machine in every possible position, but this would require enormous labour, and numerous sets of drawings for each individual position of the machine, and such, even if accomplished, would not fulfil the required conditions, for it would be almost impossible for any person to properly follow the various motions through these elaborate drawings, and carry in his mind the true nature of the movements.

Mr. Babbage's system, however, enables a person who has a slight mechanical knowledge, and a very little practice, to perfectly understand the complicated movements of a piece of machinery like this automatic numbering machine,* from a few minutes study of a single chart, on which all its motions are laid down according to his system.

Secondly. The principle of this system of notation may be thus described :—The various movements of the machine are classified, and named, and placed, one under the other in the first column of the sheet. Opposite them is a portion ruled into small vertical columns, which in its total horizontal dimensions is supposed to represent a certain space of time, in fact the time occupied by the machine in completing one period or cycle of its duty. This may be divided into any convenient number of parts. In the present case as the machine completes a cycle in about six seconds, I have divided the space into six parts, and each of these again into ten, representing tenths of seconds.

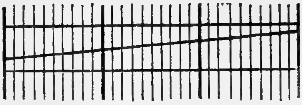
The vertical distances represent, on various empirical scales, "spaces" travelled over by that particular part of the machine specified in the first column.

As the horizontal distances represent "*time*," and the vertical space, any portion of a machine at rest for any particular number of seconds, or tenths of seconds, is represented by a horizontal line, thus :—

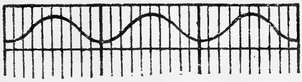


* An automatic numbering machine, the invention of Mr. Thomas Grubb, F.R.S., was exhibited as an illustration to this paper.

A uniform motion is represented by an inclined straight line, inasmuch as the spaces passed over in each unit of time are equal:—



A crank motion which begins gradually, attains its greatest velocity in the centre of its half-stroke, and ends also gradually, is represented thus:—



In fact the space passed over in any particular unit of time, is represented by the difference of the ordinates at beginning and ending of that unit.

A glance therefore at the accompanying chart will show the actual positions of all parts of the machine at any particular tenth of a second during the whole six seconds.

Thus, for example, it will be seen in the first line that the inking rollers roll backward and forward by a crank motion (or motion analogous to it) six times during a cycle. In the second line it will be seen that the inking rollers rise uniformly, and attain their highest level in three-tenths second; then they continue (rolling backward and forward, as the first line has previously informed us), for 1·1 seconds; they then uniformly descend and ascend again (to change position of rollers) in 0·6 second, continue at their highest level, inking the rollers for 0·6 second, and then uniformly descend and spend the remainder of this time, 3·0 seconds, in taking up fresh ink.

I might go through all the lines in the same way, but as the same principle applies, it is hardly necessary as a very little study will render the matter quite plain.

Thirdly. The uses to which this system is capable of being applied are numberless, and seem to continually increase as one gets more conversant with the principle and practical working of the system. They may, however, be classified into three heads:—

(*a*). The designing of the machine.
(*b*). The working out of the details of construction.
(*c*). The putting together of the machine.

(*a*). Let us first consider the designing of the machine. Whatever difficulties there are in understanding the working of a complicated machine from accurate drawings made after the machine is completed, or even from the machine itself, much greater are the difficulties to be encountered in designing the machine, for in this case the designer has neither machine nor drawings to guide him, and the only representation of the machine lies in his own brain; while, therefore, he mentally plans one part, he has to keep in his mind (if he can) the relative positions and actions of all the other parts, and frequently to go back in his work and modify and remodify various parts in his mind's eye, and all this must be thought out before he puts pen to paper; for after all, *mechanical drawings represent, not the actions and motions of the machine but the appliances (levers, wheels, cams, &c.), by which these motions are produced.* We cannot plan these appliances before we know what is required for each part to do.

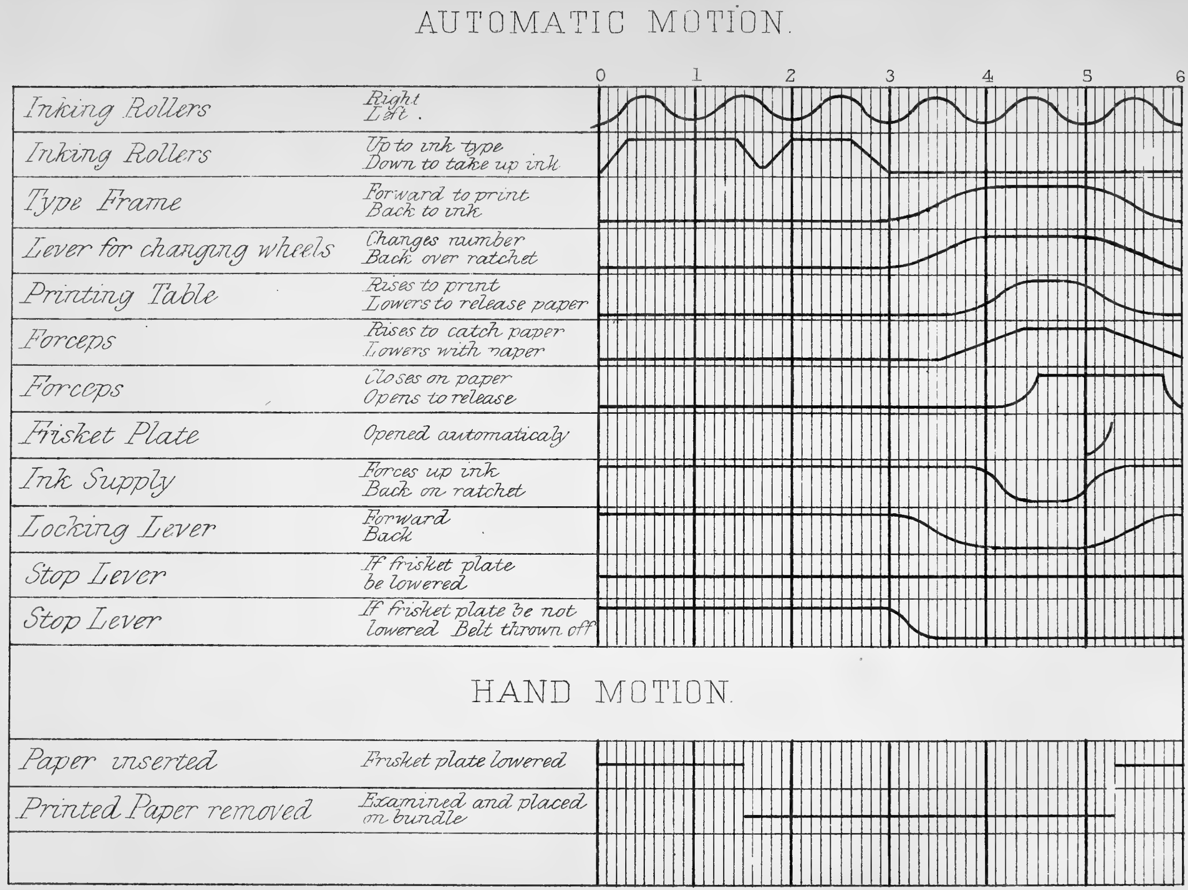
What we do want, therefore, is a means of graphically representing the various motions and actions of the machine without reference, in the first place, to the nature of the mechanical appliances by which these various motions and actions are effected. I have said enough I think to show that this part is most admirably fulfilled by this system of notation, for as the designer plans motion after motion, and action after action, he represents the nature, direction, quantity, and quality of each by these various curved lines, and can go backwards and forwards touching up and modifying the various actions (without taxing his brain to carry the nature of all or any one of these actions), until he gets all to his satisfaction, and in proper sequence.

(*b*.) The next task of the designer is that of working out the details of construction of the machine, and here again the system assists him, for he can decide from the nature of the curve on his chart (which curve he has before decided on to be the best possible for that particular purpose), what class of motion, whether crank, link motion, wheels, or cam work, he should adopt, and supposing he is required to adopt the more complicated form of cam work, the curve on the chart will enable him to sketch directly, and without any calculation the shape of his *cam*.

(*c*.) Now as to the putting together of the machine. I do not mean by this, simply the reputting of the machine together after it has been once completed, but I mean the putting together for the first time of the various parts which have been separately and disjointedly completed to scale drawings, the ascertaining of the exact position in which to place each wheel, levers, and cams, so that all may work properly, and each part fulfil its proper duty, *and at the proper time.* In complicated machines, this is the one operation in which above all others, workmen are apt to make mistakes, for as the appliance for each motion has to be placed and "keyed" independently, it is very difficult indeed to say, except by a laborious tentative system of trial, whether that particular motion tallies perfectly with all the other motions, not only these already attached, but those to be attached.

Here again Mr. Babbage comes to our assistance, for by attaching to the main shaft of the machine (which makes one revolution for each cycle), a cardboard disc divided into the same number of parts as the chart, we have in keying on any particular motion, only to bring the shaft round till the divided disc reads such a division as corresponds to some certain action of that particular cam, or lever—say the commencement or end of some particular action, and then turn the cam or lever on the shaft until that action does actually take place, and there key it, and so on through all the motions: so that really the workman might put the whole machine together, find out the position for all the motions, key them on, and be perfectly satisfied in his own mind, that the machine will work perfectly correctly, *even though he never tried it once.*

It may be said that a mathematician could describe and note down the nature and quality of all these actions without reference to a graphical representation. No doubt; but in the first place the mathematics required even for very simple machinery would be far higher than what we can expect mechanics to be conversant with, and I think we need no argument to show that a mathematical explanation of the mechanical motions would not satisfy the requirements even of a highly educated mechanic, when we find Mr. Babbage himself, one of the greatest mathematicians of this century, actually engaged in devising this system of graphical representation.


AUTOMATIC MOTION.
0
1
2
3
4
5
6
Inking Rollers
Right
Left.
Inking Rollers
Up to ink type
Down to take up ink
Type Frame
Forward to print
Back to ink
Lever for changing wheels
Changes number
Back over ratchet
Printing Table
Rises to print
Lowers to release paper
Forceps
Rises to catch paper
Lowers with paper
Forceps
Closes on paper
Opens to release
Frisket Plate
Opened automaticaly
Ink Supply
Forces up ink
Back on ratchet
Locking Lever
Forward
Back
Stop Lever
If frisket plate
be lowered
Stop Lever
If frisket plate be not
lowered Belt thrown off
HAND MOTION.
Paper inserted
Frisket plate lowered
Printed Paper removed
Examined and placed
on bundle

SUGGESTIONS FOR AN EXPERIMENT TO DEMONSTRATE THE POLARIZED STATE OF THE GAS IN CROOKES'S LAYER.

BY

GEORGE FRANCIS FITZGERALD, M.A., F.T.C.D.

[Read 21st January, 1878.]

I DESIRE to apologise to the Society for bringing before them only a proposed instead of a performed experiment, but my excuse is that it will probably be some time before I am able to perform the experiment myself, and as I desire to get credit for having at least proposed it, I take this opportunity of publishing it, and of giving my reasons for supposing it likely to be successful, by showing that the quantities involved are quite within the reach of our present methods of observing them.

I would first notice that, according to both Clausius and Maxwell's theories of the conduction of heat in gases, the existence of a force like Crookes's depends essentially upon the distribution of the velocities among the molecules, it being easily seen from either of their investigations (as is also evident from many other obvious considerations) that it is quite possible to imagine such a distribution of velocities among the molecules as that, though heat be propagated through the medium, yet the pressure in all directions shall be the same. Hence, any independent method of demonstrating a polarized state of the gas is of considerable importance; and the experiment I am about to propose is for the purpose of doing so.

When any homogeneous transparent substance is in a state of stress, its refractive index for light, polarized in certain planes, is different from that for light polarized in other planes, and consequently a ray of plane polarized light, when passed though such a medium in certain directions, emerges elliptically polarized. If the gas in a Crookes's Layer be in the state of stress that theory indicates, it ought to behave similarly, and a plane polarized beam when transmitted along it should emerge elliptically polarized. Now, the amount of elliptic polarization, we may expect, can be calculated in the following manner:—I have estimated by a series of unfortunately rather rough experiments that when a red hot ball is plunged into cold water to a depth of half a centimetre, the thickness of the Crookes' Layer formed, is about a quarter of a millimetre. This is, of course, only a rough approximation, but it will give us results which determine with what order of quantity we are dealing. Hence, the excess of pressure in the vertical over that in the horizontal direction that I measured was half a gramme per sq. centimetre, which is about the ·0005 of the atmospheric pressure. Now, I will make an assumption which is, however, only partially true, but as one object of the experiment is to determine to what extent it is so it is legitimate provisionally, and it is that we may treat the strain in the gas as due entirely to a difference of density in different directions. That we may do so to some extent, at least, is manifest, for, according to theory, the number of molecules moving in the direction of the strain is greater than the number moving in other directions. To what extent this is true could only be determined either by elaborate theoretical investigations into the state of the gas or else by experiments such as I am proposing. Assuming then the strain to be wholly due to a difference of density we can proceed as follows:—The law connecting the refractive indices of a gas at different densities is $\frac{\mu-1}{d}=\frac{\mu'-1}{d'}$ so that in air when $\mu=1{\cdot}0002940$, and in the case we are considering where $\frac{d'}{d}=1{\cdot}0005$ we have $\mu'=1{\cdot}0002941$. As there are 100,000 vibrations of light per five centimetres *in vacuo* there will be 100029·40 when the density is μ, and 100029·41 when it is μ', and consequently a difference of phase of ·01 of a wave length will be introduced per five centimetres or a twentieth of a wave length in 25 centimetres. Now, as the intensity of light in the analyzer depends upon the square of the sine of the difference of phase, this will give the intensity as the square of the sine of 9°, which is ·02 or $\frac{1}{50}$th. Hence I conclude that if a ray of plane polarized light be transmitted through 25 centimetres of a Crookes's Layer between two surfaces, one of which is red hot, and the other below the temperature of boiling water, and with an interval between them of a quarter of a millimetre, then one-fiftieth of the light will be restored in an analyzer which was so turned as to extinguish the beam before transmission between the plates. Such an effect could be easily observed, but as it is very unlikely that the whole of the strain in a Crookes's Layer is due to a difference of density of the gas in different directions, it is very unlikely that so great an effect would be produced. One object, however, in trying the experiment would be to determine to what extent the strain is due to a difference of densities.

ON THE BARYTES MINES NEAR BANTRY.

BY

EDWARD T. HARDMAN, F.C.S., of the Geological Survey of Ireland.

[Read January 21st, 1878.]

Sulphate of Barytes or Heavy Spar is a mineral of not very common occurrence in Ireland, and is only met with in a few localities in sufficient quantity to be of commercial value. In various places in the county Cork it is found in some abundance; and near Bantry it has been, and I believe is at present, being extensively worked. Having visited these mines some time ago, I propose giving a brief description of them.

The most extensive lode is met with in the townland of Derry-ginah, Middle, about two miles east of Bantry. It bears nearly due east and west, N. 80° E. & S. 80° W.; cutting the strike of the Old Red Sandstone slates, at an angle of about ten to fifteen degrees. The lode is ten to fifteen feet thick, and has been followed for some 200 or 300 yards, the workings extending to a depth of about fourteen fathoms. About one-third of the lode in the centre consists of extremely pure Barytes, but the sides of it consist of an impure variety called cawk, which contains a quantity of quartz, carbonate of lime, green carbonate of copper, Peacock copper ore, and micaceous or specular iron. The last is found in considerable quantity—so much so, that the manager of the works was of opinion it might prove commercially valuable could it be smelted.

Besides the difference in purity of the Barytes, two varieties occur in this lode. One a crystalline glassy-looking specimen; the other a granular saccharoidal variety, and the last is the kind most valued, as it is the most easily ground in the process of preparation.

From this mine a considerable quantity of mineral has been obtained and exported by the Bantry Barytes Mining Company, who are now working it. When in full work they can easily turn out twenty tons per day. But the mine is capable of yielding a much larger quantity, being in fact so far only limited by the amount of labour obtainable, and the state of the market. Owing to the cost of carriage also, the price of the mineral is necessarily rather high.

The next locality for Barytes is a little more than a mile south-east of Bantry, in the townlands of Ardargh and Darreengreanagh, and so far as I know of, has not been mentioned in any list of localities of that mineral; although the occurrence of the lode is marked in the field maps of the Geological Survey. And the mode in which it occurs is sufficiently curious to deserve a passing notice.

This deposit occurs in similar grits and slates to those enclosing the first named, but instead of forming a lode it consists of a thick pipe-like mass of nearly pure Barytes. This pipe is about thirty feet long, and fifteen wide, and it has been proved to extend downwards for at least ninety feet, having been entirely excavated to that depth. At the corners it throws off small branches or veins, from two to five feet thick, and some of these have been found at the surface some distance from the main body, but appear to thin away on every side.

This great mass is almost entirely composed of the very purest sulphate of Baryta. An analysis of it showed it to contain over ninety-five per cent. of sulphate. The "seconds" or "cawk" (which forms but a very small proportion of the lode, being principally confined to the walls), contain various copper ores, the green carbonate, Peacock ore, and copper pyrites, as well as galena, all in very small quantity. The walls of the lode are coated in some places with steatite, or chlorite. The rocks enclosing it strike N. 80° E., with a high dip of 75° to 80°.

This deposit has been worked for a considerable time by the Scart Barytes Mining Company, and the principal mass of ore has been removed to the depth above stated—ninety feet.

There is always an amount of mystery kept up as to the uses for which Barytes is intended, arising from the fact that it is chiefly in demand for purposes of adulteration, its high specific gravity being taken advantage of. Thus it is principally in request for the adulteration of white lead and other paints; and some even say that it is employed as a commercial substitute for sugar. Such at least is the Bantry native opinion. It is besides occasionally useful for the manufacture of glazes for porcelain*.

The mineral is worth about £1 per ton, delivered free on board at Bantry, but when ground and prepared it fetches £4 per ton.

A few words on the probable mode of formation of this mineral may not be out of place. And first I may mention that many of the Irish localities for veins of sulphate of Barytes appear to lie in the Old Red Sandstone. Thus Portlock records its occurrence in the Old Red Sandstone of Ballynascreen and Desertlyn, county Derry, and Clogher, county Tyrone. But this is merely a coincidence, because is it found here as in other counties in many other rocks, crystalline and sedimentary, and in England it occurs largely in the Carboniferous limestone. Now it is tolerably easy to account for the presence of veins of this mineral in limestone, which is easily soluble and quickly worn into fissures or pipes by ordinary atmospheric water, in which, under some circumstances, Barytes might be deposited, but the first difficulty with which we have to contend in this case is the solution of such rocks as sandstone and slate. The Derryginagh deposit can be accounted for by a simple fissure, but this will not account for the other case, in which the original material has been removed in the form of a nearly square pipe, which could never have been produced solely by fissuring. There can be no doubt but this receptacle has receivedits present form through the action of water. Doubtless such pipes are due to fissures in the first instance which, allowing the water to percolate freely, are eaten away bit by bit into their present form.

Age of these Veins.—As these veins run partly along and partly across the strike of the strata, which lie in flexures dipping at high angles, it follows that they must be of more recent date than that of the upheaving and flexuring of the Old Red Sandstone and Carboniferous rocks of the south of Ireland. Now, as Professor Hull has shown, these flexures are due to forces acting at the close of Carboniferous and previous to the Permian Periods.† It

* It appears to me that the granular varieties might, with advantage, be substituted for alabaster, for statuary and ornamental purposes. The mineral can be obtained in large blocks.

† Jour. Roy. Geol. Soc., Ireland, iv., pt. iii., p. 114.

is certain therefore that these lodes, as well as the copper lodes of the other parts of Cork as well as Kerry, are younger than the Carboniferous period, and may be therefore about the same age as those of Cornwall. It is, of course, impossible to determine at what period past the Carboniferous they have been deposited, since there are no newer strata in this part of Ireland; but however this may be, we may suppose that the original fissures were most likely opened during the disturbances which produced the flexures of the Old Red Sandstone in the south-west of Ireland.

Deposition of the Barytes and associated Minerals.—Barytes being one of the most insoluble substances known, it is unlikely that it could have been deposited from solution in cold water; on the other hand it is so very infusible that the heat necessary to reduce it to a plastic condition would be more than sufficient to melt the surrounding rocks. Its deposition is therefore to be ascribed with most probability to the action of thermal springs, the waters of which were forced upwards into these fissures, while the strata at present exposed were still buried under a great mass of superincumbent rocks. The waters at first warm enough to hold small quantities of such difficultly soluble minerals in solution would, as it came nearer the surface, become somewhat cooler, and these minerals would be then deposited along the sides of the fissure. This point, which is insisted on by Delesse, is demurred to by Bischof, who considers that the waters of ascending hot springs cannot produce these deposits, but it is evident he left out of consideration the cooling of the water as it rose.

Source of the Sulphate of Barytes.—This is to be sought for either in the immediately outlying or surrounding rocks, or in masses of rock at some distance, from which some compound of barium may be carried down into springs. Carbonate of barium is by no means an uncommon mineral, and barium in some form is of common occurrence in minute proportion in limestone. Silicate of barium is also found occasionally in igneous rocks, and might, therefore, also occur in parts of the Old Red Sandstone which are derived from the debris of such rocks*. Those com-

* The very small quantity of Barium compounds disseminated through rocks, is of little moment in this consideration. As Bischof well remarks, the minimum quantities in rocks may become the maximum quantities in lodes.

pounds of barium are to a small extent soluble in water, and would be brought down through the strata to rise again from deep-seated springs. Meeting now with soluble sulphates, these salts of barium would be converted into sulphate, and as the water cooled in rising to the surface, this would be deposited. As a matter of fact crystals of sulphate of barium have been found on the granite of Carlsbad, where a hot spring, containing in solution traces of that substance, burst out. Chloride of barium is sometimes noticed in spring waters, and this would also give rise to sulphate in the manner pointed out.

In fact it is only through the medium of hot water that the sulphate of Barytes of Bantry, and the very insoluble minerals associated with it, can be supposed to have been deposited.

ON THE ARTIFICIAL PRODUCTION OF MINERALS AND PRECIOUS STONES.

BY

MM. FEIL AND FRÉMY.

[Read January 21st, 1878.]

THE artificial production of minerals presents, in a scientific point of view, an interest which everybody understands. It has long afforded a wide field of research to many scientific men, among whom may be mentioned Ebelman, Senarmont, Deville, &c.

We thought it might be interesting, even after the works of these eminent scientific men, to publish the result of our researches on the crystallization of alumina and of divers silicates.

If we place in a furnace, heated to a high temperature, a mixture of alumina and oxide of lead, we obtain white crystals alumina, and, by adding a metallic oxide as colouring matter, —either chromium for obtaining a red colour, or cobalt for blue,— we can produce rubies in the first case, and sapphires in the second. It is necessary, however, in order to procure chemically pure crystals, from the mass obtained to separate the lead which may yet remain in combination with the vitrifiable earth proceeding from the crucible, and which may itself have been combined during the fusion. We may separate these bodies by different processes, either by the action of hydrofluoric acid or by potash in fusion, or by prolonged calcination in hydrogen, and subsequent treatment with alkalies and acids.

The following are the properties of the rubies we obtained:— They scratch quartz and topaz. Their density is 4·0041.

They lose, like natural rubies, their pink coloration when they are strongly heated, and regain colour on cooling. They are as hard as natural rubies.

When submitted to optical examination—that is to say, to the microscope of polarization of Amici—our rubies, which have the form of hexagonal prisms, present the characteristic black cross and coloured rings.

The crystals we obtained, and which we had cut by a lapidary, have not yet the requisite limpidity for commercial purposes neither do they present to the lapidary favourable facets for cleaving; but they have the colour and the hardness, &c., and we have prepared large masses in which crystals have been found which leave absolutely nothing to be desired. Besides, we are still studying this interesting question, and without doubt we shall soon arrive at a perfect result in every point of view.

The second part of our investigation, *i.e.*, that on the crystallized silicates, will serve to demonstrate the influence of the fluorides as crystallizing agents. By submitting to heat, during a determined time, a mixture of fluoride of aluminum and vitrifiable earth, we noticed that, by the mutual reaction of the two bodies, fluoride of silicium is evolved, and we obtain a crystallized body which seems to be disthene—that is to say, a silicate of alumina. It appears under the form of acicular double refracting crystals, which extinguish the light obliquely towards their edges. These crystals afforded on analysis the following results:—

Vitrifiable earth,*	47·65
Alumina,	51·85
Loss,	0·50

It is about the same as for disthene, or its varieties, trebolite, bucholzite and bamlite, and sillimanite.

By operating in a certain manner, and by heating to a high temperature a mixture of alumina and fluoride of barium, we obtained prismatic needles several centimetres in length. Their analysis afforded:—

Vitrifiable earth,	34·32
Baryta,	35·04
Alumina,	30·37

M. Jannettez has ascertained that these long prisms are often composed of four blades with parallel faces forming the surfaces of a hollow prism; they extinguish the light under the microscope, or rather they let the obscurity persist between two crossed prisms. They may be cut at angles of 60° 42′ and 119°.

In the course of the reaction which generates the crystallized double silicate just described, some corundum is formed. These bodies are the results of the following changes:—

By heating the mixture of alumina and fluoride of barium, fluoride of aluminum and barium is formed.

* This term is doubtless used as a synonym of silica.—*Eds.*

Having once first produced fluoride of aluminum and decomposed it again under the influence of steam, hydrofluoric acid is formed and corundum is crystallized.

By acting at the same time on the vitrifiable earth of the crucible, there is first produced some silicate of alumina, which by uniting itself with baryta, has produced the beautiful crystals of double silicate of aluminum and of barium which we have presented.

Such is the theory we think rational; and it seems to result from our experiments that fluorides are not only excellent mineralizers in the mass, but that they can carry along with them the most fixed bodies. As a proof of this assertion one can instance that remarkable formation of orthose felspar produced artificially in the upper part of a copper furnace at Mansfeld. The use of the fluoride of calcium in the bed of fusion, which produced that felspar, permits us to suppose that the fluorine served here as a transporting agent or vehicle.

Thus, we are justified in expecting that, by prosecuting our studies, we may be able to produce crystals capable of application in jewellery and watchmaking. The last experiments have shown us that we are making good progress. We have already obtained cut crystals of remarkable value. We hope they will shortly be presented for inspection.

THE FOLLOWING PAPERS were also read and ordered to be printed in the *Transactions of the Royal Dublin Society* :—

1877.

Nov. 19th.—WENTWORTH ERCK, LL.D., F.R.A.S., "On the Satellites of Mars."

Dec. 17th.—EDWARD HULL, F.R.S., and E. T. HARDMAN, F.C.S., "On the Nature and Origin of the Chert-beds in the Upper Carboniferous Limestone of Ireland."

1878.

Jan. 21st.—T. ROMNEY ROBINSON, D.D., F.R.S., "On the places of 1,000 stars observed at the Armagh Observatory."

VERBAL COMMUNICATIONS were also made to the Society on the following subjects :—

1877.

Nov. 19th.—On Phenol-phthalein as a test of Alkalinity by Professor J. EMERSON REYNOLDS, M.D.

" " On Specimens of Crystallized Phosphorus, exhibited by Mr. R. J. MOSS, F.C.S.

" " On the limits of Geological Time by Rev. S. HAUGHTON, M.D., F.R.S.

" " On the relation of Science to Practical Engineering by ROBERT MANNING, C.E.

Dec. 17th.—On a simple form of Telephone exhibited by Professor W. F. BARRETT, F.R.S.E.

" " On a new form of Spectroscope, exhibited by Mr. HOWARD GRUBB, M.E., F.R.A.S.

" " On the discovery of Brine in the Valley of the Mersey at Warrington by Professor HULL, F.R.S.

" " On Specimens of ornamental and other stones from Jypore, India, exhibited by Professor E. Hull, F.R.S.

" " On Specimens of Idocrase rock from Cumberland, exhibited by Professor HARKNESS, F.R.S.

" " On Nests of the oven-bird (*Furnariidæ*), exhibited by Professor MACALISTER, M.D.

" " On an Automatic Numbering Machine, exhibited by Mr. HOWARD GRUBB, M.E., F.R.A.S.

1878.

Jan. 21st.—On M. Cailletet's experiments on the liquefaction of Oxygen and other gases, by Professor J. EMERSON REYNOLDS, M.D.

" " On the Phonograph, by Professor W. F. BARRETT, F.R.S.E.

Jan. 21st.—On the relation between the electric and capillary properties of a surface of mercury in contact with different liquids, by Mr. G. F. FITZGERALD, F.T.C.D.

" " On new forms of Laboratory apparatus for obtaining heat and light, exhibited by Mr. R. J. MOSS, F.C.S.

" " On the Glacial phenomena of upper and lower Lough Bray, by the Rev. M. H. CLOSE, M.A.

" " On *Totanus Haughtoni*, (Armstrong) a new species of Greenshank from India, exhibited by Professor A. MACALISTER, M.D.

" " On Specimens of Barytes and associated Minerals from Bantry, exhibited by Mr. E. T. HARDMAN, F.C.S.

" " On Nests of Mason-bees from Rayal Pindi, Ladakh, exhibited by Mr. J. W. HAUGHTON, junr.

" " On a series of Human Vertebræ with anomalous processes, exhibited by Professor A. MACALISTER, M.D.

" " On Nests of Trapdoor Spiders from Jamaica, exhibited by Professor A. MACALISTER, M.D.

" " On the skull of a Fanti from West Africa, exhibited by Professor A. MACALISTER, M.D.

" " On a new form of Tell-tale clock, exhibited by Mr. W. HANCOCK.

" " On a Gas-light improver; also on a new form of gas engine, used with the Gramme Magneto-electric machine, exhibited by Mr. JOHN R. WIGHAM.

Evening Scientific Meetings.

The Evening Scientific Meetings of the Society and of the associated bodies (the Royal Geological Society of Ireland and the Dublin Scientific Club) are held in Leinster House on the third Monday in each month during the Session. The hour of meeting is 8 o'clock, P.M. The business is conducted in the undermentioned sections.

Section I.—PHYSICAL AND EXPERIMENTAL SCIENCES.
Secretary to the Section, R. J. MOSS, F.C.S.

Section II.—NATURAL SCIENCES (including Geology and Physical Geography).
Secretary to the Section, R. M'NAB, M.D.

Section III.—SCIENCE APPLIED TO THE USEFUL ARTS AND INDUSTRIES.
Secretary to the Section, HOWARD GRUBB, M.E., T.C.D.

Authors desiring to read papers before any of the sections of the Society are requested to forward their communications to the Registrar of the Royal Dublin Society (Mr. R. J. Moss), or to one of the Sectional Secretaries, *at least* ten days prior to each evening meeting, as no paper can be set down for reading until examined and approved by the Science Committee.

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