rollers driven by ratchets and pinions moved by a crank on the fly wheel shaft under the box. The fly wheel is of iron, two and a half feet across, and with a rim one inch and a quarter in diameter, and weighing about forty pounds, and is driven by a spur wheel about eight inches across on the end of the shaft, to which the driving handle is secured; the latter is thirteen inches long and is on the right hand side of the box, the fly wheel being on the left. From the fly wheel crank a rod eight inches long extends down to a lever sixteen inches long and hung to the lower part of the frame. From the other end of this lever another rod extends up to the knife frame, which is of cast iron, and supports a knife of the shape illustrated in the annexed cut.

As the handle is turned, the straw or corn stalks are drawn forward by the rollers and held over a square iron edge, when the knife comes down with a shearing cut and with power enough to cut the thickest corn stalk in two.

The point in the middle of the knife is the great feature of the improvement. The straw becoming crowded in a compact mass, by using a common inclined knife without this point is apt to cut hard. By using a knife of the above shape, however, the point begins to enter and cut with but little effort at the precise spot where, without it, the machine would require the greatest power to make the cut. There is another advantage in this knife. The point is so strong and the momentum so great, that a seasoned root or stock would be split and cut in two without checking the machine.

The retail price of this efficient instrument is twelve dollars.

(To be Continued.)

TWENTY-FIFTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

An Account of some Experiments with a Large Electro-Magnet. By Mr. JOULE.—Prof. W. THOMSON, in Mr. Joule's absence, brought the subject before the Section.—The relation of the exciting force to the sustaining power of a magnet was the subject which it was the author's desire to examine, the laws arrived at being very divergent from those usually received. The soft iron made use of in this magnet, was of such a nature that it always—probably on account of intense magnetization on some former occasion—retained a residual polarity which was always in the same direction. The magnet might be excited by a current which

*From the Lond. Athenæum, September, 1855.

developed a polarity opposed to the residual one; but, on the interruption of the current, the latter re-appeared. With high power, the lifting power fell short of being proportional to the square of the current; but with feeble excitation, Mr. Joule found the sustaining force to vary as the fourth or fifth power of the current strength employed.

Dr. ROBINSON gave an account of some of his own experiments on this subject, which confirmed those of Mr. Joule. The magnet made use of in his experiments consisted of two upright pillars of soft iron, with a movable crosspiece of the same metal, which enabled him to vary the length of the limbs of the magnet. To determine the lifting power, a keeper, or sub-magnet, accurately planed, was placed across from pole to pole of the magnet, and, by a suitable mechanical arrangement, the force necessary to separate it from the excited magnet was determined. An approach to the point of magnetic saturation soon manifested itself, and, in this respect, the experiments of Prof. ROBINSON were quite confirmatory of those of Mr. Joule.—Prof. TYNDALL remarked that he had sometimes been surprised to observe the comparatively low power of excitement in Mr. Joule's experiments at which the approach to magnetic saturation was exhibited. In Müller's experiments, where thick bars of iron were used, it required very strong excitation to produce the falling off, from the law that the magnetic attraction is proportional to the square of the exciting current. In Mr. Joule's experiments masses of soft iron were made use of, of far greater size than those used by Müller, but nevertheless the falling off from the law alluded to soon exhibited itself. In the remarks which had been brought before the Section, the shape of the magnet was omitted as an element in the question; but in all probability, it would be found that if one of the limbs of Dr. Robinson's magnet were employed alone as a straight bar, its magnetism being measured by its action upon a freely suspended magnetic needle, instead of by its lifting power, the magnetic saturation of the bar would be much more difficult of attainment. Or even preserving the form of experiment made use of by Dr. Robinson, and introducing a plate of non-magnetic matter one-tenth or one-hundredth of an inch in thickness between the keeper and the magnet, a totally different law of lifting power would be obtained; the magnetic attraction in the last case would preserve its proportionality with the square of the current for a much longer period. A current which would appear to saturate the magnet in Dr. Robinson's experiments would not saturate in the latter case; and there does not appear to be any sufficient reason for accepting the latter result in preference to the former as expressive of the absolute capacity of the magnet for magnetization.—Dr. Robinson observed in reply, that he believed his method of experiment preferable to that suggested by Prof. Tyndall for the special object in view. He regarded the disruption of direct contact better calculated to throw light on the true state of the magnet than the separation of the keeper where an interval existed between it and the magnet. He had actually introduced such an interval as that spoken of by Prof. Tyndall, and it was true that he had found a totally different law from that arrived at when magnet and sub-magnet were in contact.

On Peristaltic Induction of Electric Currents in Submarine Telegraph Wires. By Prof. THOMSON.—Recent examinations of the propagation of electricity through wires in subaqueous and subterranean telegraph cables, have led to the observation of phenomena of induced electric currents, which are essentially different from the phenomena (discovered by Faraday many years ago) of what has hitherto been called electro-dynamic, or electro-magnetic induction, but which, for the future, it will be convenient to designate exclusively by the term electro-magnetic. The new phenomena present a very perfect analogy with the mutual influences of a number of elastic tubes, bound together laterally throughout their lengths, and surrounded and filled with a liquid which is forced through one or more of them, while the others are left with their ends open or closed. The hydrostatic pressure applied to force the liquid through any of the tubes will cause them to swell, and to press against the others, which will thus, by peristaltic action, compel the liquid contained in them to move in different parts of them in one direction or the other. A long solid cylinder of India-rubber, bored symmetrically in four, six, or more circular passages parallel to its length, will correspond to an ordinary telegraphic cable containing the same number of copper wires, separated from one another only by gutta percha; and the hydraulic motion will follow rigorously the same laws as the electrical conduction, and will be expressed by identical language in mathematics, provided the lateral dimensions of the bores are so small, in comparison with their lengths, or the viscosity of the fluid so great, that the motions are not sensibly affected by inertia, and are consequently dependent altogether on hydrostatic pressure and fluid friction. Hence the author considers himself justified in calling the kind of electric action now alluded to, *peristaltic induction*, to distinguish it from the electro-magnetic kind of electro-dynamic induction. The mathematical treatment of the problem of mutual peristaltic induction is contained in the paper brought before the Section; but the author confined himself in the meeting to mentioning some of the results. Among others, he mentioned, as being of practical importance, that the experiments which have been made on the transmission of currents backwards and forwards by the different wires of a multiple cable, do not indicate correctly the degree of retardation that is to be expected when signals are to be transmitted through the same amount of wire laid out in a cable of the full length. It follows, that expectations as to the working of a submarine telegraph between Britain and America, founded on such experiments, may prove fallacious; and to avoid the chance of prodigious losses in such an undertaking, the author suggested that the working of the Varna and Balaklava wire should be examined. He remarked that a part of the theory communicated by himself to the Royal Society last May, and published in the Proceedings, shows that a wire of six times the length of the Varna and Balaklava wire, if of the same lateral dimensions, would give thirty-six times the retardation, and thirty-six times the slowness of action. If the distinctness of utterance and rapidity of action practicable with the Varna and Balaklava wire are only such as to be not inconvenient, it would be necessary to have a wire of six times the diameter; or better, thirty-six wires of the same dimensions; or a larger number of still

smaller wires twisted together, under a gutta percha covering, to give tolerably convenient action by a submarine cable of six times the length. The theory shows how, from careful observations on such a wire as that between Varna and Balaklava, an exact estimate of the lateral dimensions required for greater distances, or sufficient for smaller distances, may be made. Immense economy may be practised in attending to these indications of theory in all submarine cables constructed in future for short distances; and the non-failure of great undertakings can alone be insured by using them in a preliminary estimate.

Experimental Observations on an Electric Cable. By Mr. WILDMAN WHITEHOUSE.—After referring to the rapid progress in submarine telegraphy which the last four years have witnessed, Mr. Whitehouse said that he regarded it as an established fact that the nautical and engineering difficulties which at first existed had been already overcome, and that the experience gained in submerging the shorter lengths, had enabled the projectors to provide for all contingencies affecting the greater. The author then drew the attention of the Section to a series of experimental observations which he had recently made upon the Mediterranean and Newfoundland cables, before they sailed for their respective destinations. These cables contained an aggregate of 1125 miles of insulated electric wire,—and the experiments were conducted chiefly with reference to the problem of the practicability of establishing electric communications with India, Australia, and America. The results of all the experiments were recorded by a steel style upon electro-chemical paper by the action of the current itself, while the paper was, at the same time, divided into seconds and fractional parts of a second by the use of a pendulum. This mode of operating admits of great delicacy in the determination of the results, as the seconds can afterwards be divided into hundreds by the use of a “vernier,” and the result read off with the same facility as a barometric observation. Enlarged fac-similes of the electric autographs, as the author calls them, were exhibited as diagrams, and the actual slips of electro-chemical paper were laid upon the table. The well known effects of induction upon the current were accurately displayed; and contrasted with these were other autographs, showing the effect of forcibly discharging the wire by giving it an adequate charge of the opposite electricity in the mode proposed by the author. No less than eight currents—four positive and four negative—were in this way transmitted in a single second of time through the same length of wire (1125 miles) through which a single current required a second and a half to discharge itself *spontaneously* upon the paper. Having stated the precautions adopted to guard against error in the observations, the details of the experiments were then concisely given, including those for “velocity,” which showed a much higher rate attainable by the magneto-electric than by the voltaic current. The author then recapitulated the facts, to which he specially invited attention:—First, the mode of testing velocity by the use of a voltaic current divided into two parts (a split current), one of which shall pass through a graduated resistance tube of distilled water, and a few feet only of wire, while the other part shall be sent through the long circuit, both being made to record themselves by adjacent styles upon the same slip of electro-chemical paper. Second, the use of mag-

neto-electric "twin currents," synchronous in their origin, but wholly distinct in their metallic circuits, for the same purpose, whether they be made to record themselves direct upon the paper, or to actuate relays or receiving instruments which shall give contacts for a local printing battery. Third, the effects of induction, retardation of the current, and charging of the wire, as shown autographically; and contrasted with this—fourth, the rapid and forcible discharging of the wire by the use of an opposite current; and hence—fifth, the use of this as a means of maintaining, or restoring at pleasure, the electric equilibrium of the wire. Sixth, absolute neutralization of currents by too rapid reversal. Seventh, comparison of working speed attainable in a given length of wire by the use of repetitions of similar voltaic currents as contrasted with alternating magneto-electric currents, and which, at the lowest estimate, seemed to be seven or eight to one in favor of the latter. Eighth, proof of the co-existence of several waves of electric force of opposite character in a wire of given length, of which each respectively will arrive at its destination without interference. Ninth, the velocity, or rather amount of retardation, greatly influenced by the energy of the current employed, other conditions remaining the same. Tenth, no adequate advantages obtained in a 300-mile length by doubling or trebling the mass of conducting metals. The author, in conclusion, stated his conviction that it appeared from these experiments, as well as from trials which he had made with an instrument of the simplest form, actuated by magneto-electric currents, that the working speed attainable in a submarine wire of 1125 miles was ample for commercial success. And may we not, he added, fairly conclude also that India, Australia, and America, are accessible by telegraph without the use of wires larger than those commonly employed in submarine cables?

Note on Solar Refraction. By Prof. PIAZZI SMYTH.—Amongst other interesting and important consequences of the dynamical theory of heat, Prof. W. Thomson having deduced the necessity of a resisting medium, the condensation of this about the sun, and a consequent refraction of the stars seen in that neighbourhood. Prof. Piazzì Smyth had endeavored to ascertain by direct astronomical observation, whether any such effect was sensible to our best instruments. Owing to atmospheric obstructions, only three observations, yielding two results, had been yet obtained; but both these indicated a sensible amount of solar refraction. Should this effect be confirmed by more numerous observations, it must have important bearings on every branch of astronomy; and as the atmosphere at all ordinary observatories presents almost insuperable obstacles, the author pointed out the advantage of stationing a telescope for this purpose on the summit of a high mountain.

Remarks on the Chronology of the Formations of the Moon. By Prof. NICHOL.—Prof. Nichol stated that, through the munificence of the Marquis of Breadalbane, he had been enabled to bring to bear on the delicate inquiries, whose commencement he intended to explain, a very great, if not a fully adequate, amount of telescopic power. A speculum of twenty-one inches, originally made by the late Mr. Ramage with the impracticable focal length of *fifty-five feet*, had, at the expense of that noble Lord, been re-ground, polished, mounted as an equatorial, and placed

in the Glasgow Observatory, in its best state, only about six weeks ago. Prof. Nichol showed some lunar photographs, which indicated the great light with which the telescope endowed its focal images, and entered on other details as to its *definition*. The object of the present paper is the reverse of speculative. It aims to recall from mere speculation, to the road towards positive inquiry, all observers of the lunar surface. To our satellite hitherto those very ideas have been applied, which confused the whole early epochs of our terrestrial geology, the notion, viz: that its surface is a *chaos*, the result of primary, sudden, short-lived, and lawless convulsion. We do not now connect the conception of irregularity with the history of the earth:—it is the triumph of science to have analyzed that apparent chaos, and discerned order through it all. The mode by which this has been accomplished, it is well known, has been the arrangement of our terrene mountains according to their relation to time: their relative ages determined, the course of our world seemed smooth and harmonious, like the advance of any other great organization. Ought we not then to attempt to apply a similar mode of classification to the formations in the moon,—hoping to discern there also a course of development, and no confusion or manifestation of irregular convulsion? Prof. Nichol then attempted to point out that there appeared a practical and positive mode by which such classification might be effected. It could not, in so far as he yet had discerned, be accomplished by tracing, as we had done on earth, relations between lunar upheavals and stratified rocks; but another principle was quite as decisive in the information it gave, viz: the intersection of dislocations. There are clear marks of dislocation in the moon—nay, the surface of our satellite is overspread with them. These are the rays of light, or rather bright rays, that flow from almost all the great craters as their centres, and are also found where craters do not at present appear. Whatever the substance of this highly reflecting matter, it is evidently no superficial layer or stream, like lava, but extends downwards a considerable depth into the body of the moon. In short, we have no likeness to it on earth, in the sense now spoken of, except our great trap and crystalline *dykes*. It seemed clear, then, that the intersection of these rays are really *intersections of dislocations*, from which we might deduce their chronology. Can the intersection, however, be sufficiently seen?—In other words, is the telescope adequate to determine which of the two intersecting lines has disturbed or cut through the other? Prof. Nichol maintained the affirmative in many cases, and by aid of diagrams, taken down from direct observation, illustrated and enforced his views.

On the Achromatism of a Double Object-Glass. By Prof. STOKES.—The general theory of the mode of rendering an object-glass achromatic, by combining a flint-glass with a crown-glass lens, is well known. The achromatism is never perfect, on account of the irrationality of dispersion. The defect thence arising cannot possibly be obviated except by altering the composition of the glass. It seemed worthy of consideration, whether much improvement might not be effected in this direction; but the problem which the author proposed for consideration was only the following:—Given the kinds of glass to be employed, to find what ought to be done so as to produce the best effect; in other words, to determine

the ratio of the focal lengths which gives the nearest approach to perfect achromatism. Two classes of methods may be employed for this purpose. In the one, compensations are effected, by trial, on a small scale; in the other, the refractive indices of each kind of glass are determined, for certain well defined objects in the spectrum, such, for example, as the principal fixed lines. The former has this advantage, that compensations on a small scale do not furnish so delicate a test as the performance of a large object-glass. The observation of refractive indices, on the other hand, admits of great precision; but it does not immediately appear what ought to be done with the refractive indices when they are obtained. After alluding to the method proposed by Fraunhofer for combining the refractive indices, which, however, as he himself remarked, did not lead to results in exact accordance with observation, the author proposed the following as the condition of nearest approach to achromatism:—that the point of the spectrum, for which the focal length of the combination is a minimum, shall be situated at the brightest part, namely, at about one-third of the interval DE from the fixed line D , towards E . The refractive index of the flint glass may be regarded as a function of the refractive index of the crown glass, and may be expressed with sufficient accuracy by a series with three terms only. The three arbitrary constants may be determined by the values of three refractive indices, determined for each kind of glass. On applying the resulting formula to calculate r , (the ratio of the chromatic changes of the indices of refraction,) for the object-glass, for which Fraunhofer has given both the refractive indices of the component glasses and the value of r , which, as observation showed, gave the best results, and taking in succession various combinations, of three lines each, out of the seven used by Fraunhofer, the author found that, whenever the combination was judiciously chosen, the resulting value of r was the same, whatever might have been the combination, and equal to 1.980, which is precisely the value determined by Fraunhofer, from observation, as giving the best effect.

On the Polar Decomposition of Water by Frictional and Atmospheric Electricity. By Prof. ANDREWS.—The author having drawn attention to the fact, that water had never been decomposed by the action of the common friction electricity, so as to collect the gases and exhibit them at the opposite poles, stated that the cause of the failure of the experiment was the solution of the gases in the mass of the liquid. By fusing platina wires in thermometer tubes, this difficulty is avoided, and the gases may be then obtained and collected with the same facility as in ordinary eudiometric experiments. By arranging a series of such tubes, the operations may be almost indefinitely repeated. On raising an electrical kite, the author succeeded in obtaining the polar decomposition of water by atmospheric electricity. The observations were made in fine weather, when the atmosphere was not usually charged with electricity. Although the gases were easily collected and measured, from the delicate form of apparatus employed, the quantity of water decomposed in this case amounted only to one 700,000th of a grain in the hour.

On the Phosphorescence and Composition of Plate-Sulphate of Potash. By Prof. PENNY.—The subject of this paper is a chemical product from kelp. It is called plate-sulphate of potash from the circumstance of its

being crystallized in thick plates or slabs, consisting of the aggregate layers of successive crops of crystals. Beautiful specimens of the salt, supplied by Mr. Paterson, one of the most extensive manufacturers of kelp products in Glasgow, were exhibited to the Section. There are several points in the chemical history of this salt possessing a high degree of scientific interest. The principal points alluded to by the author of the paper were, first, the peculiarity of the process by which it is obtained in slabs or slates; second, the brilliant phosphorescence of its crystals at the time of their formation; and third, its remarkable composition. The mode of obtaining it was fully explained, and the various circumstances that influence the emission of light from the salt, at the time of crystallizing, were minutely described.

On a new Form of Cyanic Acid. By BARON LIEBIG.—In the course of some experiments on the fulminate of mercury, I observed that that compound, when kept boiling in water, changed its color, and lost its fulminating properties. On examining the change that had taken place in the composition of the fulminate, I discovered a new acid which had exactly the composition of cyanuric acid, but which differed entirely from that acid in its properties, and in the properties of the salts which are produced with the alkaline bases—salts remarkable for their beauty and for the distinctness of their crystalline form. Taking for the equivalent of hydrated fulminic acid the formula C^2, NO, HO , the new acid is produced in a very similar manner. The elements of three equivalents of fulminic acid unite to form one equivalent of the new acid, to which I shall give the name of fulminuric acid. This acid is monobasic. Its salt of silver is soluble in hot water, and crystallizes from it in long, silky, white needles. The alkaline salts of the new acid are very easily prepared by boiling the fulminate of mercury with an alkaline chloride. The fulminate of mercury is first dissolved; then gradually two-thirds of the oxide of mercury precipitates, and the alkaline fulminate, with a certain quantity of chloride of mercury and potassium, remains in the solution. By employing the chloride of sodium, or the chloride of barium, we obtain, of course, a salt of the new acid, with a base of soda or of barytes. With chloride of ammonium an ammoniacal salt is obtained, the crystals of which are distinguished from all others by their adamantine brilliancy, and their high degree of power and lustre. These crystals belong to the Klinerhombic system, and possess double refraction almost as strongly as Iceland spar. The hydrated acid is easily obtained by decomposing the basic lead salt by means of sulphuretted hydrogen. It has a strongly acid re-action, and when reduced by evaporation to a state of syrup, it is transformed by degrees into a crystalline mass, which dissolves in alcohol, and which, by the action of acids, is changed into carbonic acid and ammonia.

On the Extraction of Metals from the Ore of Platinum. By M. FRÉMY.—M. Frémy treated of the preparation of osmium, rhodium, and iridium, from the residues of the platinum ores. The preparation of osmium according to the old method is attended with great difficulties and actual danger. M. Frémy proposed to prepare osmium by passing atmospheric air over the residual ore, heated in a porcelain tube. The volatile osmic acid is condensed in glass balloons, and the less volatile oxide of ratheni-

um is found at the extremity of the heated tube. The rhodium remaining in the residual mass is separated from the other metal contained by chlorine gas at a high temperature.

On the Allotropic Modifications of Chlorine and Bromine analogous to the Ozone from Oxygen. By Prof. ANDREWS, who explained that ozone could be produced, first, by an electric spark; secondly, by the decomposition of acids and solutions, when coming into contact with the galvanic wire; and lastly, by oxidation.

On a Process for obtaining Lithographs by the Photographic Process. By Prof. RAMSEY.—Prof. Ramsey described a process by which Mr. Robert M'Pherson, of Rome, had succeeded in obtaining beautiful photolithographs.—specimens of which had been hung up in the Photographic Exhibition in Buchanan Street. The steps of the process are as follows:—1. Bitumen is dissolved in sulphuric acid, and the solution is poured on an ordinary lithographic stone. The ether quickly evaporates, and leaves a thin coating of bitumen spread uniformly over the stone. This coating is sensitive to light, a discovery made originally by Mr. Niepce, of Chalons. 2. A negative on glass, or waxed paper, is applied to the sensitive coating of bitumen, and exposed to the full rays of the sun for a period longer or shorter according to the intensity of the light, and a faint impression on the bitumen is thus obtained. 3. The stone is now placed in a bath of sulphuric ether, which almost instantaneously dissolves the bitumen, which has not been acted upon by light, leaving a delicate picture on the stone, composed of bitumen on which the light has fallen. 4. The stone, after being carefully washed, may be at once placed in the hands of the lithographer, who is to treat it in the ordinary manner with gum and acid, after which proofs may be thrown off by the usual process.—Prof. Ramsey then proceeded to state that the above process, modified, had been employed with success to etch plates of steel or copper, without the use of the burin:—1. The metal plate is prepared with a coating of bitumen, precisely in the manner noticed above. 2. A positive picture on glass or paper is then applied to the bitumen, and an impression is obtained by exposure to light. 3. The plate is placed in a bath of ether, and the bitumen not acted upon by light is dissolved out. A beautiful negative remains on the plate. 4. The plate is now to be plunged into a galvano-plastic bath, and gilded. The gold adheres to the bare metal that refuses to attach itself to the bitumen. 5. The bitumen is now removed entirely by the action of spirits and gentle heat. The lines of the negative picture are now represented in bare steel or copper, the rest of the plate being covered by a coating of gold. 6. Nitric acid is now applied as in the common etching process. The acid attacks the lines of the picture formed by the bare metal, but will not bite into the gold surface. A perfect etching is thus obtained.

*On Writing Inks.** By JAMES STARK, M. D., F.R. S. E.

The author stated that in 1842, he had commenced a series of experiments on writing inks, and up to this date had manufactured 229 differ-

* From the Lond. Civ. Eng. and Arch. Jour., August, 1855.