pinnæ of a fern, or a few fragments of the stems of Ulodendron or Sigillaria, give every inadequate ideas of the plants to which they had belonged in their state of original entireness.

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 1,125 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 hundredths 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 (1,125 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 magneto-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 favour 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 1,125 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 submarines cables?

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 equatoreal, 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 of 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.

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. Piazzi Smyth had endeavoured 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.

ERRATUM.—The Lithographer of the Map of the Township of Colchester, which accompanied the conclusion of Major Lachlan's paper in the last number of the Journal, has introduced an error in the direction of one of the Canoe Canals, which we take this opportunity of rectifying. Instead of running straight through Round Marsh and Long Marsh, it should run due north along the borders of Long Marsh as far as the 8th Concession, and then across *Roach's Marsh only*, until it approached the River Canard. A drain from Round Marsh into the Canoe Canal will accomplish all that is required with respect to the drainage of Round Marsh.