Her engines have each two piston rods, the air pumps being worked by beams from the cylinder cross-heads, and so assisting to balance the weight of piston and connecting-rods.

There is also a large balance wheel keyed on to the crank shaft, which answers for turning engines by hand, and in some degree as a fly-wheel.

The screw is three-bladed, and projects beyond the after stern-post, as in Beattie's patent, the rudder being enclosed in the space between the two stern-posts, and the screw shaft dividing it in two pieces, which are connected by an oval looped rudder post. The screw is 18 feet diameter, and 36 feet pitch, so that with 35 revolutions of engines, the speed of screw equals 12·4 knots per hour; and, as the diameter is so very large (for the power transmitted through it) there is, of course, but little slip.

The tubular boilers have twelve furnaces, 3·8 wide, and 7 ft. long; thus giving 308 sq. ft. of grate surface; the tubes, 290 in number, are 7 ft. long, and $3\frac{1}{4}$ outside diameter. We hope to give some particulars of this vessel in our next.

The *Belgique*, belonging to the same Company, which sailed from here for New York last month, arrived at that port after a run of nineteen days, having met with very strong head winds the whole way across.

*On Electrical Conduction in Submarine Cables.** By WILLIAM THOMSON.

There are some conclusions from my theory of electrical conduction in submarine cables which, if correct, must be found valuable in planning the best dimensions and arrangement of wires and insulating tubes for telegraphic communication between Britain and America:—an undertaking at present actually in progress, as appears from statements in the *Athenæum*. It is, therefore, of importance that such of your readers as may be interested in the subject should, without delay, have some evidence to allow them to judge as to the validity of the objections to those conclusions which Mr. Wildman Whitehouse made in his communication to the British Association at Cheltenham (reported *Athen.* August 30,) and which he still maintains in his letter recently addressed to you (*Athen.* Oct. 11,) notwithstanding my confident assertion of their correctness and my reference to the theoretical demonstration (*Proceedings of the Royal Society*, May 10, 1855, and *Philosophical Magazine*, vol. Jan. to June, 1856,) as supporting this assertion by unassailable evidence (*Athen.* Oct. 4). On this account, rather than from any desire to continue in your columns a controversy which I believe will be easily adjusted by private correspondence, I offer you the following remarks.

The points of greatest practical importance in my conclusions are, 1, "the law of squares," as it has been called by Mr. W. Whitehouse; and, 2, the influence of lateral dimensions of wire and insulating coat on the rapidity of signals by a submarine cable of stated length. It is on these points chiefly that Mr. W. Whitehouse brings his attack, and it is to these that I shall confine my present defence.

* From the *Lond. Athenæum*, November, 1856.

1. *Law of Squares*.—In his Cheltenham communication, Mr. W. Whitehouse brings forward the following table of results, as conclusive against the law of squares:—

Amount of Retardation observed at various Distances. Voltaic Current. Time stated in Parts of a Second.

Mean of 550 obsrvns.	Mean of 110 obsrvns.	Mean of 1840 obsrvns.	Mean of 1960 obsrvns.	Mean of 120 simultaneous observations.	
83 miles. ·08	166 miles. ·14	249 miles. ·36	498 miles. ·79	535 miles. ·74	1020 miles. 1·42

The retardation here shown as the results of undoubtedly trustworthy observations, are not proportional to the squares of the distances ; but it depends on the nature of the electric operation performed at one extremity of the wire, and on the nature of the test afforded by the indicating instrument at the other extremity, whether or not any *approach to the law of squares is to be expected* in the observed results. Thus, the following table, which I have derived solely from theory,* shows for different lengths of cable, of certain lateral dimensions, the delays, from the instants of beginning various definitely specified electrical operations at one extremity experienced in waiting for the indications by each of two definite tests of electrical effect at the other extremity :—

Durations of battery action.	150 miles.		300 miles.		600 miles.		1200 miles.		2400 miles.	
	Times from beginning of battery-action till strength of current rises to									
	$\frac{3}{4}$ max.	max.	$\frac{3}{4}$ max.	max.	$\frac{3}{4}$ max.	max.	$\frac{3}{4}$ max.	max.	$\frac{3}{4}$ max.	max.
seconds	seconds.	seconds.	seconds.	seconds.	seconds.	seconds.	seconds.	seconds.	seconds.	seconds.
0	·011	·0164	·044	·0656	·175	·2622	·7	1·049	2·8	4·196
·333	·0378	·333	·145	·354	·353	·49	·84	1·17	2·93	4·27
·667	·0378	·667	·151	·667	·495	·78	1·08	1·45	3·07	4·4
1·000	·0378	1·000	·151	1·000	·564	1·104	1·25	1·70	3·20	4·53
8	·0378	8·000	·151	8·000	·604	8·000	2·416	8·33	6·93	10
16	·0378	16·000	·151	16·000	·604	16·000	2·416	16·00	9·00	17·47
24	·0378	24·000	·151	24·000	·604	24·000	2·416	24·00	9·60	25·33
∞	·0378	∞	·151	∞	·604	∞	2·416	∞	9·664	∞

The electrical operation to which the first horizontal series of numbers in the table refers is in strictness an infinitely short application of an infinitely intense electro-motive force ; but, practically this condition will be realized to a sufficient degree of approximation for verifying the result by applying a powerful battery or an electro-magnetic impulse during one-twentieth, or any smaller fraction of the time, shown as the delay of the maximum in each case,—for instance, during $\frac{1}{260}$ of a second, or any less time, for the 150-mile cable, or during $\frac{1}{8}$ th of a second or any less

* I have obtained the tabulated results merely by inspection from a diagram of curves published with my first communication to the Royal Society on the subject. The decimals will, no doubt, require some correction when exact calculations are made, as will very soon be the case. The lateral dimensions for which the table has been calculated are those of a single wire cable, copper wire ·065 in. diameter, gutta purcha tube $\frac{1}{4}$ -in. outer diameter.

time for the cable of 2400 miles. The electrical operations referred to in the other horizontal lines consist of applications of one pole of a constant battery during the stated times. In every case the extremity operated on is supposed to be put in connexion with the earth at the instant when it is disconnected from the battery. The other extremity is supposed to be always kept in connexion with the earth. The conductor of the indicating instrument through which this connexion is established is supposed to exercise but a small resistance compared with that of any number of miles of the wire, and is assumed to give rise, by electro-magnetic induction, to no delay comparable with that due to the induction of the other kind (peristaltic induction) in the cable in each case. This last condition will be far from being fulfilled, except for the longest cable, when large coils with soft iron cores are used in the indicating instrument, as any one will be ready to admit who has seen Faraday's beautiful magneto-optical experiment, and has remarked the gradually rising illumination consequent on the sudden completion of the voltaic circuit.

Now, it will be observed that the law of squares is fulfilled throughout no horizontal line of the table, except the first and the last. If, for instance, we take the series of retardations of $\frac{3}{4}$ of maximum strength, and of maximum strength consequent on an application of the battery for one-third of a second, we have the following comparative view:—

Lengths,	150	300	600	1200	2400
Retardation of $\frac{3}{4}$ maximum,	·0378	·145	·352	·84	2·93
Retardations of maximum,	·333	·354	·49	1·17	4·27

Here the retardation of the $\frac{3}{4}$ maximum is a little short of four times as much in the 300 mile cable as in the 150; it is only $2\frac{1}{2}$ times as much in the 600 as in the 300; only $2\frac{1}{4}$ times as much in the 1200 as in the 600; and only $3\frac{1}{2}$ times as much in the 2400 as in the 1200; instead of being 4 times greater in each of the longer cables than in the other of half-length, as it would be if the law of squares were applicable.

By comparing, on the same principle, Mr. W. Whitehouse's results quoted above (*Athen.* Aug. 30, p. 1033), we find very analogous *deviations from the law of squares*; but for a strict verification of their *agreement with theory*, it would be necessary to have a precise specification of the electrical operations which were performed at one extremity, and of the exact nature of the test of electrical effect afforded by the receiving instrument at the other extremity. The manuscript of the Cheltenham paper, to which, through the courtesy of Mr. Whitehouse, I have had access, shows that the operations were *certainly not such as could give effects on the whole manifesting the law of squares even approximately*, and makes me think it probable that electro-magnetic induction in the receiving instrument has most sensibly influenced the retardations observed with the shorter lengths of cable, increasing, relatively, those observed with the shortest. Each application of the battery appears to have been maintained for a second, and, therefore, whether this was instantly followed by a reverse application (as appears to have been the case), or by the establishment of a connexion with the ground, the law of squares ought to be verified very closely for all lengths of cable less than 300 miles, if the battery power is arranged so that the maximum strength of the

current is in all cases the same. Accordingly, we have a tolerable verification of the law of squares by Mr. Whitehouse's observed retardations in the 166 mile and the 249 mile cable, which are respectively $\cdot 14$ and $\cdot 36$. These show an even greater increase of retardation in the greater length than according to that law, which would have been exactly verified by the numbers $\cdot 14$ and $\cdot 315$. If the indicating instrument does not show its effect until a somewhat larger proportion than $\frac{2}{3}$ of the maximum strength is reached, the retardation in 498 miles may be either more or less than twice the retardation in 249 miles; and if it be only little more than twice (being $\cdot 79$ as Mr. Whitehouse finds it), it will follow from the theory as illustrated by the table that the retardation in 996 miles ought to be found considerably less than twice the retardation in 498. Thus Mr. Whitehouse finds even the retardation in 1020 miles less than the double of the retardation which he finds in 498 miles. Another cause which may contribute to deviations from the law of squares, and which must most seriously derange the results from agreement with any easily calculated indications of theory, is the inconstancy of the galvanic battery employed. It is well known to all who have used the sand-battery, or any other form of battery which evolves gaseous hydrogen during its continuous action, that, in commencing to act after having been quiescent for any time, it acts with immensely greater electro-motive force than it can keep up, and that during the initial second of its operation the electro-motive force *falls in general with great rapidity*. This circumstance it is probable, largely influences the phenomena which Mr. Whitehouse observed; and if it does, must complicate extremely the character, both relative and absolute, of the electrical operations used in his experiments on different lengths of cable.

It appears then, that in the one case of comparison in which a manifestation of the law of squares could be expected, that law is manifested by Mr. Whitehouse's results, and that in all other cases, so far as the data supplied suffice for testing, they agree with the theory of which the law of squares is a part.

To test the theory extensively and strictly by observations with Mr. Whitehouse's instruments will, probably, not be easy without using either a constant battery or a system of measured electro-magnetic impulses. It will, however, I have no doubt, be quite practicable with a sufficient number of cells of Daniell's battery to verify the law of squares with considerable precision for lengths of cable from 498 miles upwards. All that will be necessary will be to take such battery powers as to give the same permanent current through the cable and instrument in the different cases, that is to say, to make the numbers of cells used proportional to the lengths of the cable; and in each experiment to hold on the battery connexion until after the indicating instrument has made its mark. The most favorable relation between the sensitiveness of the indicator and the battery power, to give a sharp test, is such that the full strength of the permanent current is about four times the strength that just moves the index. But this would probably give too small a retardation in the 498 mile cable to free the comparison from large influence of electro-magnetic induction in the coils, and it may be that a more satisfactory test would be obtained with the actual instruments, by using battery power

capable of maintaining a permanent current more by only $\frac{1}{3}$ d or $\frac{1}{4}$ th than enough to give an indication.

2. *Influence of Lateral Dimensions of Conductor and Insulating Tube on Rapidity of Electrical Action.*—My remedy for inconvenient slowness of action in a submarine cable is an increased diameter of conductor and of gutta percha coat, in the same proportions, or in different proportions, indicated by economical considerations calculable from theory with data as to the price of copper, gutta percha, and iron, and the relative expense and risk in laying cables of different weights (see my first communication on the subject referred to above). Until the price of copper comes to be a serious consideration in comparison with the whole expense of making and laying the cable, an increase in the diameter of the conductor alone, up to a certain limit, without any increase in the thickness of the gutta percha tube, may be the cheapest remedy. Accordingly, in my statement quoted by Mr. Whitehouse, either on thick copper wire, or (as is preferable for mechanical reasons) “a number of small wires twisted together” is recommended to obviate inconvenient slowness of utterance. Mr. Whitehouse tests this proposal by three wires connected at their ends so as to afford a tripple conducting channel, *but separated throughout their lengths by their gutta percha coats*. Now it is perfectly clear that an electrical impulse through three wires so arranged cannot differ from the sum of three separate electrical impulses of one-third strength each through any one of the wires, except in virtue of mutual peristaltic induction. With these very wires Mr. Whitehouse found the effects of mutual peristaltic induction to be extremely slight, requiring some of his finest tests to be shown at all. How then he found “the retardation nearly twice as great with the triple wire as in one of the wires alone,” is an anomaly which it is not my part to explain. I may be allowed to suggest as a possible explanation, that he used only battery power enough to give the same permanent current through the triple wire as through the single wire in the comparative experiments, and that the triple conductor exhausted the high initial electro-motive force much more rapidly during the first quarter of a second than the single wire. In reality, when justice is done to the triple wire, it will be found to transmit the action *a very little more rapidly than one wire alone*; in consequence of the diminution of electrostatic capacity in each wire, which is caused by mutual induction among the three when equal neighboring parts of them are similarly and equally electrified at each instant. But my law regarding the effect of increasing the diameter of one wire or of using *a number of fine wires twisted together*, is neither proved nor disproved by experiments on the combined effects of a number of wires about as widely separated from one another as from their common iron sheath.

Neither on the two points to which I have especially adverted, nor on any other, will it be possible, I believe, to find the theory at fault; and Mr. Whitehouse has too effectively proved himself to be a sincere searcher for truth to allow me to doubt that he will sooner or later fully admit the correctness of my conclusions, although they have hitherto appeared to him to be at variance with the phenomena he has observed.

On the other hand, I have much pleasure in admitting that he has quite answered my objections to experiments on the retardation of effects of a

current sent through a certain length of wire backwards and forwards in a "multiple" cable, as trustworthy for indicating the retardations to be expected in the same length of wire laid out singly to an untried distance. I am very glad, too, to perceive that he has succeeded in convincing practical men by his experiments, in conjunction with Mr. Bright and Mr. Morse, that a cable of ordinary lateral dimensions may be not too slow in its action for commercial success in the projected sub-Atlantic telegraph.

I should certainly not have expected that this would be found to be the case *if the Varna and Balaclava telegraph, when pushed to the utmost, had been found to be inconveniently slow*; but although I have never been able to learn any particulars as to the experience of those who have worked that telegraph, I have the best possible reason now for knowing that if nine letters per second have not been delivered by it, the fault, or rather I should say the deficiency, has not been in the cable, but in the instruments. Measurements published since the beginning of the present year by that most profound and accurate of all experimenters, Wilhelm Weber, have allowed me to reduce his own previous determination of the electrical conductivity of copper to the proper kind of unit for the telegraph problem, and have so afforded me the data for calculating the retardations of electric signals sent through a copper wire of stated dimensions, insulated in any specified manner. I thus find that the retardation of the maximum strength at one extremity, consequent on a single instantaneous electrical impulse at the other extremity of 400 miles of Black Sea cable,* amounts to only about $\cdot 117$ of a second. The corresponding retardation in a cable of equal lateral dimensions, and 2400 miles long, would be only about 4.2 sec. A mode of operating so as to clear a wire rapidly of residual electricity, which I have worked out from theory, and a plan for telegraphic receiving instruments to take the most full advantage of it which has recently occurred to me, allow me now to feel confident of the possibility of sending a distinct letter every $3\frac{1}{2}$ seconds by such a cable. This, amounting to 17 letters per minute, would give 200 messages of twenty words in the 24 hours, and at 30s. a message would be not a bad return for 1,000,000 *l.* of capital expended. The rate could be augmented to a letter every $2\frac{1}{4}$ seconds, or 280 messages in 24 hours, by augmenting the diameter of copper and gutta percha tube each in the proportion of 5 to 4,—that is to say, by using a copper conductor $\frac{1}{16}$ of an inch diameter, in a gutta percha tube $\frac{1}{8}$ of an inch outer diameter.

From the letter in the *Daily News* (Oct. 9), I can form no idea of the rate which the experiments on the 2000 miles of cable promise for the delivery of the messages. That letter gives no information whatever which could not have been told ten years ago without experiment,† and which was not actually asserted by myself a year and a half ago,—except this, that the eminent men who directed and assisted in those experiments saw something that convinced them of the practicability of telegraphing, at a rate commercially advantageous, between Newfoundland and Ireland, by means of a cable of such dimensions and arrange-

* Or a single wire cable; copper wire $\frac{1}{15.4}$ of an inch in diameter, in gutta percha tube $\frac{1}{8}$ -inch external diameter.

† See my last letter to the *Athenæum*, published October 4.

ment as that on which they made their experiments. If they can satisfy themselves that the effects of mutual induction between wires in a multiple cable of such a length will not be injurious, and that the rates of telegraphing which they can accomplish are as good as those I have indicated, then a multiple cable, such as that proposed by Mr. Whitehouse (*Athen.* Aug. 30, p. 1093), will be undoubtedly satisfactory. But the effects of mutual induction between three or four wires in a cable 2400 miles long will be decidedly more embarrassing in rapid operations than they have been found with shorter lengths; and even without the new plans of working to which I allude may be found so inconvenient that single wire cables will be preferred. If these new plans of wires are to be used, it may be necessary to have only one wire under one iron sheath, as I fear they would not succeed in a multiple cable, although on this point I cannot yet speak with confidence.

I shall only say, in conclusion, that I intend to prepare and publish a table of different sets of dimensions for cables, with estimated rapidity of transmitting messages by each, which those engaged in projecting the Ocean Telegraph may find useful if their experiments leave anything undecided as to the best plan of cable for their purpose, if they have any confidence in scientific deductions from established principles.

INVERCLOY, Isle of Arran, Oct. 24.

*On Nature Printing.** By HENRY BRADBURY, Esq., M. R. I.

(Continued from page 47.)

During the next twenty years Nature Printing was but indifferently prosecuted by various persons for various purposes. Mr. Taylor, of Nottingham, as far back as 1842; printed lace, &c., specimens of which were exhibited at the Great Exhibition; and Mr. Twining, of Nottingham, in 1847, printed ferns, grasses, and plants, which were exhibited by the Botanical Society of London. He adopted the same plans as those used by Kniphof and Hoppe.

In 1847, also, Dr. Fergusson Branson commenced a series of experiments, an interesting paper upon which was read before the Society of Arts in 1851, and therein for the first time was suggested the application of that second and most important element in Nature Printing, which is now its essential feature—the application of the Electrotpe.

"I beg leave," says he, "to bring before the notice of the Society of Arts a new method of engraving plates for printing ferns, leaves, sea-weeds, and other flat plants. . . . Having taken in gutta percha some impressions of ferns, the singularly beautiful manner in which the exact character of the plant was transferred to the gum, suggested to me the possibility of printing from the gutta purcha itself, so as to produce on paper a *fac-simile* of the plant. That experiment partially succeeded, and curiously tested the elasticity of the substance; for the impression remained uninjured, after being subjected to the great pressure of a copper-plate roller. I say that it *partially* succeeded; for the printer found it utterly impossible so thoroughly to cleanse the

* From the Royal Institution of Great Britain.