remains in solution; the carbonate of zinc is ignited and weighed as oxide of zinc.

Determination of Arsenic and Copper.—Arsenic and copper are precipitated by hydrosulphuric acid from the solution of the iron in aqua regia, separated from each other by sulphide of ammonium, and determined in the usual manner.—*Polytechn. Centralbl.*, 1856, p. 366–379.

*“The Law of the Squares.”—Is it Applicable or no, to the Transmission of Signals in Submarine Circuits?** By WILDMAN WHITEHOUSE.

Referring to the proceedings of this section last year at Glasgow, the author quoted Professor W. Thomson's paper on this subject, where he stated “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 not to be 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 smaller wires twisted together, under a gutta percha covering, to give tolerably convenient action by a submarine cable of six times the length.”

Mr. Whitehouse then stated that circumstances had enabled him to make very recently a long series of experiments upon this point, the result of which he proposed to lay before the section, adding that an opportunity still existed for repeating these experiments upon a portion of cable to which he could obtain access, and that he was ready to show them before a committee of this section in London, if the important nature of the subject should seem to render such a course desirable.

Although the subject of submarine telegraphy had many points of the highest importance requiring investigation, and to the consideration of which he had been devoting himself, the author proposed to confine his remarks on this occasion to the one point indicated in the title—inasmuch as a decision of that one, either favorably or otherwise, would have on the one hand the effect of putting a very narrow limit to our progress of telegraphy, or, on the other, of leaving it the most ample scope. He drew a distinction between the mere transmission of a current across the Atlantic (the possibility of which he supposed everybody must admit), and the effectual working of a telegraph at a speed sufficient for “commercial success,” and we gathered from his remarks that there were those ready to embark in the undertaking as soon as the possibility of “commercial success” was demonstrated. He then gave a description of the apparatus employed in his researches, of the manner in which the experiments were conducted, and lastly of the results obtained. The wires upon which the experiments were made were copper of No. 16 gauge, very perfectly insulated with gutta percha, and spun into two cables containing three wires each of equal length (83 miles), covered with iron wires, and coiled in a large tank in full contact with moist

*From the Lond. Civ. Eng. and Arch's. Jour., Sept., 1856.

earth, but not submerged: the two cables were subsequently joined together, making a length of 166 miles of cable, containing three wires. In addition to this, in some of the latest experiments he had also the advantage of another length of cable, giving with the above an aggregate of 1020 miles.

The instruments, one of which was exhibited, seemed to be of great delicacy, capable of the utmost nicety of adjustment, and particularly free from sources of error. The records of the amount of retardation were all made automatically by electro-chemical decomposition on chemically prepared paper. The observations of different distances recorded themselves upon the same slip of paper—thus 0·83, and 249 miles were imprinted upon one paper; 0·83, 498 upon another slip; 0·249, 498 upon another; 0·535, 1020 upon another; thus, by the comparison of the several simultaneous records on each slip, as well as by the comparison of one slip with another, the author has been enabled to show most convincingly that the Law of the Squares is not the law which governs the transmission of signals in submarine circuits. The author showed next, by reference to published experiments of Faraday and Wheatstone, (*"Philosophical Magazine,"* July, 1855), that the effect of the iron covering with which the cable was surrounded, was, electrically speaking, identical with that which would have resulted from submerging the wire, and that the results of the experiments could not on that point be deemed otherwise than reliable. He next alluded to the objection raised by Prof. Thomson against experiments in "multiple" cables. Faraday, he said, had experimented upon wires laid in close juxtaposition, with reliable results (See *"Experimental Researches,"* vol. 3, page 575); but, in addition to this, our author proposed to himself to answer this objection by an appeal to direct experiments. To decide the question, he first weighed the amount of induction produced in one wire during the use of interrupted or alternating currents in an adjacent wire. A current of eighteen thousand grains thrown into one produced in a length of 166 miles, a current of one grain and a-half by induction in the adjacent and parallel wire; in round numbers, one grain in 10,000,—so slight an amount of interference, in fact, that it may be practically disregarded in this investigation. Indeed, it was found impossible to produce any appreciable difference with reference to the retardation by varying the arrangement of the wires, so that at one time they should carry like currents, at another time unlike, as long as the earth remained part of the circuit. But the author was not without the testimony of most competent persons as additional evidence upon this point. Lieutenant Col. Biddulph, director of the Black Sea Telegraph—the very line to which Prof. Thomson refers—happening to be in England at the time of these researches, went through a series of experiments with our author, and gave as his opinion, "that the phenomena of induction and retardation were as marked, and that there was fully as much, if not more, embarrassment of instruments in these 498 miles of cable as he had met with in the Black Sea line." We may take it as proved, therefore, that experiments on such a cable, fairly and continuously conducted, may be regarded as real practical tests, and the results obtained as a fair sample of what will ultimately be found to hold good

practically in lines laid out *in extenso*. Attention was then called to a table showing the amount of retardation observed at various distances, and under as nearly similar conditions as was possible to obtain. In this are embodied the results of far more labor than meets the eye. At the head of each column is stated the number of observations upon which the result given was computed, every observation being rejected on which there could fall a suspicion of carelessness, inaccuracy, or uncertainty as to the precise conditions; and on the other hand, every one which was retained being carefully measured to the 100th part of a second.

This table is subject to correction for variation in the state of the battery employed just as barometrical observations are subject to correction for temperature. "Of this variation, as a source of error," said our author, "I am quite aware, but I am not yet in possession of facts enough to supply me with the exact amount of correction required. I prefer, therefore, to let the results stand without correction. Now it needs not long examination of this table to find that we have the retardation following an increasing ratio, that increase being very little beyond the simple arithmetical ratio.

"I am quite prepared to admit the possibility of an amount of error having crept in these figures in spite of all precautions. Indeed, I have, on that account, been anxious to multiply observations, in order to obtain trustworthy results; but I cannot admit the possibility of error having accumulated to such an extent as to entirely overlay and conceal the operation of the law of the squares, if, in reality, that law had any bearing on the results. Taking 83 miles as our unit of distance, we have a series of 1, 2, 3, 6, and 12. Taking 166 miles as our unit, we have then a series of 1, 3, and 6. Taking 249 miles, we have still a series of 1, 2, and 4, in very long distances. Yet, even under these circumstances, and with these facilities, I cannot find a trace of the operation of that law."

Table of Retardation observed at Various Distances.

Voltaic Current.—Time stated in parts of a second.

Mean of 550 observations.	Mean of 110 observations.	Mean of 1840 observations.	Mean of 1960 observations.	Mean of 120 simultaneous observations.	
83 miles	166 miles	249 miles	498 miles	535 miles	1020 miles.
.08	.14	.36	.79	.74	1.42

The author then examined the law of the squares, as shown by the value of a current taken in submarine or subterranean wires, at different distances from the generator thereof.

Magneto-electro currents were weighed at Liverpool, which had traveled respectively 40, 120, 240, and 320 miles—from Manchester, Birmingham, London, and Belfast. The results did not confirm the laws of the squares, and the most distant come out with less loss than a comparison of the shorter lengths had led him to anticipate. Another result was even more striking. The voltaic current from a battery of very moderate power, weighed at a distance of 498 miles, in the cable before mentioned, 250 grains on its exit from the wire to earth. Divide the cable into six lengths, and, in accordance with the law of the squares,

at some part of the first length this current ought to be found weighing 36 times $250=9000$ grains. This is nearly half as much again as the current actually did weigh at 83 miles, and nearly 1500 grains more than the same current (by observations both at the beginning and end of the experiment,) gave as its maximum lift on short circuit. These observations, by weight, though not possessing, if taken by themselves, the full force which would be due to them had the subject of loss by traveling, and the ratio thereof, been fairly worked out, are yet strongly corroborative of the general results.

"Intimately connected with the consideration of this subject," said our author, "is another which is so closely related to it as to be almost a necessary and integral part thereof. I mean the question of the *size of the conducting wire*. I venture to allude to it now (even though from its importance a subject of special research with me at this moment, and my series of experiments on this point, still in progress and far from complete), because it may be regarded as an appeal to another court for a decision upon the same question put in a different form. The number of wires in the cable enabled me to determine the relative degrees of retardation observable when using a single, double, or treble-sized conductor of the same length in each instance—viz: 166 miles. I had thus to a limited degree the opportunity of testing the application of this law as enunciated by Prof. W. Thomson last year, with reference to the size of conductor necessary, to which I have already alluded. The results, far from confirming the law, are strikingly opposed to it. The fact of trebling the size of the conductor augmented the amount of retardation so as to nearly double that observed in the single wire. I give the actual figures obtained as the mean of one series of thirty-two observations on the point:

Amount of retardation taken at 166 miles:—

Voltaic Current.—Time stated in parts of a second.

Size of conductor.	Mean of 32 observations,
Single,	.15
Double,	.20
Treble,	.17

Velocity as deduced from the foregoing—

Single wire and earth,	1100 miles per second.
Double " "	830 " "
Treble " "	614 " "

"These observations were taken again and again with extreme care, were repeated on several days, and with uniformly corresponding results."

The author then stated that the most searching and most practical test, in fact the *experimentum crucis*, would be found in the limit of the rapidity and distinctness of utterance attainable at different distances. He laid before the Section records taken at 498 and 1020 miles. These being taken by the same instrument, and under like conditions, may be regarded as perfectly reliable for comparison with each other, though from being taken with the magneto-electric, instead of voltaic current, and recorded by a new form of instrument, they are not comparable with

results obtained by any of the ordinary form of telegraphic instrument. Three hundred and fifty was the highest number of distinct signals recorded in the minute through 498 miles, and two hundred and seventy through 1020 miles, the former being at the rate of nearly six in the second, the latter four and a half in the second. These figures bear no relation to the squares of the distance—they are even far beyond the simple arithmetical ratio.

“Now, if the law of the squares be held good in its application to submarine circuits, and if the deduction as to the necessary size of the wire based upon that law can be proved to be valid also, we are driven to the inevitable conclusion that submarine cable of certain length, to be successful, must be constructed in accordance with these principles; and what does this involve? In the case of the transatlantic line, whose estimated length will be no less than 2500 miles, it would necessitate the use, for a single conducting wire only, of a cable so large and ponderous as that probably no ship, except Scott Russell’s leviathan, could carry it; so unwieldy in the manufacture, that its perfect insulation would be a matter almost of practical impossibility; and so expensive, from the amount of materials employed, and the very laborious and critical nature of the processes required in making and laying it out, that the thing would be abandoned as being practically and commercially impossible. It has been calculated by one intimately connected with this branch of manufacture, and to whom the art of submarine telegraphy owes much of its success, that the first cost of the copper wire alone for a single conductor of such a size would exceed the entire cost of an electric cable containing three circuits of the ordinary gauge; while the amount of money and time involved in making and insulating one of the large size perfectly, is, he says, impossible to arrive at.

“If, on the other hand, the law of the squares be proved to be inapplicable to submarine wires, whether with reference to the amount of retardation observable in them, the rapidity of utterance to be obtained, or the size of the conductor required for the purpose, then we may shortly expect to see a cable not much exceeding one ton per mile, containing three, four, or five conductors, stretched from shore to shore, and uniting us to our transatlantic brethren, at an expense of less than one-fourth that of the large one above mentioned, able to carry from four to five times the number of messages, and therefore yielding about twenty times as much return in proportion to the outlay. And what, it may be asked, is the general conclusion to be drawn as the result of this investigation of the law of the squares applied to submarine circuits? In all honesty, I am bound to answer, that I believe nature knows no such application of that law; and I can only regard it as a fiction of the schools, a forced and violent adaptation of a principle in physics, good and true under other circumstances, but misapplied here.”

*German Telegraphs.**

Experience shows that low prices induce the general use of telegraphs. According to official returns published at Berlin the Prussian telegraphs,

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