ART. XXVII.—*Notice of Dr. Gould's Report on the Trans-Atlantic Longitude.**

Dr. GOULD's able Report on the trans-Atlantic longitude has at last been published. It is full three years, however, since the field work to which it relates was finished, and more than two, as we learn from a prefatory note, since the paper in its present form (except the last chapter) was ready for the press. Astronomers have waited for it until patience was well nigh exhausted. Yet, thankful now that it is at last within reach, they will scarcely trouble themselves to press the question, why, or by whom, it has been so long withheld, or why it is now published by the Smithsonian Institution, and not by the Coast Survey itself, under whose auspices the work was done. For ourselves, waiving these, and like queries, as probably admitting of satisfactory answers, or, at least, such as would free the author of the Report from responsibility for the unexplained delay, we turn rather to the more welcome task of examining the Report itself, and laying some of its points before the readers of this Journal.

The paper fills a hundred quarto pages of the Smithsonian Contributions, and is one of the most important yet published on telegraphic longitude. For not only was the undertaking to which it relates among the most difficult and delicate, as well as important, of its kind, but the party put upon it brought to their task nearly the sum total of all the experience and practical skill that had then been developed in this special field; their chief having for fifteen years had exclusive charge of the longitude operations of the United States Coast Survey, and his associates, likewise, a long training in the same service. The Report may be taken, therefore, as a good exemplification of the telegraphic method in one of its latest and most difficult applications, and at the same time, as but a sample of the vast store of similar material that has been accumulating under the same hand from the entire longitude work of the Survey—material which embraced, before the war, no less than twenty-four independent determinations in the Atlantic and Gulf States—and which, surely, ought not to be much longer lost to science.

The Telegraphic method, it is now well understood, is distinctively American, and has had its chief development in the work of our great national Survey. That Survey has, in fact, among its many important contributions to science, given to the world

* The Trans-Atlantic Longitude, as determined by the Coast Survey Expedition of 1866. A Report to the Superintendent of the U. S. Coast Survey. By Benjamin Apthorp Gould, late assistant. Washington City: published by the Smithsonian Institution, 1869. New York: D. Appleton & Co.

the best methods yet devised of determining both latitude and longitude—Talcott's for the one, and the *Telegraphic* for the other; the former, devised in 1834, giving us in a single night's work with the zenith telescope, or a transit instrument used as such, the latitude to a small fraction of a second, and the latter, the longitude, also, for the first time in the history of geodesy, with corresponding facility and precision—a facility and precision, taken together, wholly unattainable by other methods, and in the case of longitude especially, unapproachable even, particularly in extended operations.

It was not till some years after the telegraphic method had been well elaborated here, and in successful use, that it received much attention abroad; and even then, the modes of practice recommended as original, by even eminent astronomers, particularly in France, were only such as had been long employed in our Coast Survey, or superseded by better, and had been published to the world repeatedly, and through various channels.

Almost simultaneously with the invention of the telegraph must have occurred to astronomers the idea of using it in determining longitudes. It was natural enough, therefore, that Arago should, in 1837, as is said, have suggested such a use of it to Morse. But the first actual experiment, so far as we know, was made, on the Baltimore and Washington line, in 1844, by comparison of chronometers at the two termini. The experiment was conducted by Captain Wilkes.

The method had, before this, engaged the attention of Prof. Bache, and he, quick to discern its advantages, in November, 1845, ordered its use in the longitude work of the Coast Survey—then in charge of the eminent astronomer, S. C. Walker; who, the next year, made a successful trial of it on the line between Philadelphia and Washington. From 1848, on, it was the method employed systematically in all determinations between points in the United States. Mr. Walker tried both the method of clock-signals, used in the experiment between Baltimore and Washington, and also the superior one of star-signals, used in the Coast Survey practice, almost exclusively, from the first. Dr. Gould succeeded Mr. Walker in 1851; and in the widely extended operations of the next ten years, brought the method to a very high degree of efficiency, and, with his aids, acquired that thorough mastery of the subject, which so well fitted both him and them for the difficult task, the history and results of which are set before us in this Report.

Dr. Gould's paper contains twelve chapters: 1, Origin of the Coast Survey Expedition; 2, Previous determinations of the trans-Atlantic longitude; 3, History of the Expedition; 4, Observations at Valencia; 5, Observations at Newfoundland; 6, Observations at Calais; 7, Longitude-signals between Foilhom-

merum and Heart's Content; 8, Longitude-signals between Heart's Content and Calais; 9, Personal error in noting signals; 10, Personal equation in determining time; 11, Final result for longitude; 12, Transmission-time of the signals.

The expedition had for its object a more exact determination of American longitudes, as reckoned from a European zero, than had yet been made. It was important, for the purposes of the Coast Survey, and of astronomical science, that the trans-Atlantic longitude should correspond in precision with those determined telegraphically within our own borders. The previous determinations of that longitude whether by moon-culminations, by eclipses and occultations, or by chronometers, differed very widely from each other. The range of difference was no less than five or six seconds in time. We take for comparison, the longitude from Greenwich, of the Naval observatory at Washington. This depends mainly on that of Cambridge, and this again, in part, on that of New York, Philadelphia, and other places. Between these points the longitude has been measured telegraphically. That between Washington and Cambridge was found by Walker, in 1848-9, to be $23^m 41^s 54$, and has never been re-determined; but the link between New York and Jersey City was determined only geodetically, and that between Jersey City and Washington by simple clock-comparisons instead of the better method of star-signals; while another geodetic link was that of $12^s 44$, between the Naval observatory and the Seaton station in Washington, to which all the telegraphic longitudes of the Coast Survey are referred. On account of these circumstances, and the omission, besides, in these earlier determinations, of many of the refinements and precautions since introduced, it seems highly desirable, as Dr. Gould suggests, that the longitude between New York and Washington, which he regards as the only weak link in our whole chain of telegraphic longitudes, should be carefully re-determined. Walker's value, however, as here given, is presumed to be very near the truth. Using the above quantities, Dr. Gould finds for the determinations of the longitude of the Naval observatory from Greenwich, which have appeared best entitled to confidence in recent years, the following:—

1. *From Eclipses and Occultations.*—The value adopted in the volume of Observations for 1845 is $5^h 8^m 14^s \cdot 64$. Peirce, in 1845, from occultations observed by Bond from 1839 to 1841, gave $5^h 8^m 13^s \cdot 9$. Walker, from all available observations between 1767 and 1842, obtained $5^h 8^m 14^s \cdot 16$, a value subsequently reduced to $13^s \cdot 85$, by change in the adopted longitude of Philadelphia, Cambridge, and Washington. A correction of the lunar parallax, deduced by Airy, Walker and others, from various observations, required a still further diminution of all

American longitudes; so that we have at present from eclipses and occultations:

Walker, corrected value from observations before 1843, 5 ^h 8 ^m , 11 ^s ·14	
Peirce, from eclipse of 1851, July 28,	11·57
Peirce, from emersions of Pleiades, 1839, Sept. 26,	11·45
Peirce, " " " 1856-1861,	13·13

2. *From Moon Culminations:*—

				h.	m.	s.
Walker,	from Cambridge obs'tions,	1843-45		5	8	10·01
Loomis,	" Hudson "	1838-44				9·3
Gilliss,	" Capitol Hill "	1838-42				10·04
Walker,	" Washington "	1845				9·60
Newcomb,	" " "	1846-60				11·6 ±0·4
Newcomb,	" " "	1862-3				9·8

Walker considered 9^s·96 as the most probable value from moon-culminations, and Newcomb assigned 11^s·1 as that indicated by the Washington observations from 1846 to 1863, inclusive.

3. *From chronometers transported between Boston and Liverpool.*

				h.	m.	s.
Mean from 373 previous to 1849,				5	8	12·46
Bond's discussion of 175, expedition of 1849,						11·14
Walker's " " " "						12·00
Bond's " " " "						12·20 ±1·20
Bond's " of 52, 6 trips, expedition of 1855,						13·43 ±0·19

The new telegraphic determination of the longitude between Liverpool and Greenwich adds 0^s·06 to all these values.

The discordance of the most elaborate and trustworthy results obtained by the old methods is thus seen to exceed four seconds. The value employed by the Coast Survey from 1852 to 1859 was 5^h 8^m 11^s·2; since 1859 it has been 5^h 8^m 11^s·8.

This great uncertainty of the trans-Atlantic longitude—from 30 to 60 times greater than that of our Coast Survey determinations—required that the earliest opportunity presented by the successful laying of an ocean cable should be seized for determining this longitude telegraphically, and with all possible precision. Measures were early taken to use, for this purpose, the cable of 1858, and, again, that of 1865; but, of course, in vain. In 1866, however, the cable of that year, and the recovered cable of the year before, afforded the desired opportunity. The expedition, organized under authority of the Coast Survey, was directed by Dr. Gould, and composed of officers of the survey, who were among the most skilled in longitude work. Messrs. Gould and Mosman went to Valencia, Ireland, Messrs. Dean and Goodfellow to Heart's Content, Newfoundland, and Messrs. Davidson and Chandler to Calais, Maine; for, the complete solu-

tion of the problem in hand, required the determination of three separate longitudes: 1, between Greenwich and Valencia; 2, between Valencia and Heart's Content, and 3, between Heart's Content and Calais, the easternmost station of the series connected by the telegraphic determinations of the Coast Survey. The first was accomplished by the ready co-operation of the Astronomer Royal. The second and third involved the principal labor, and presented the chief difficulties.

And these difficulties were by no means trifling. Wretched climate, defective land lines, unprecedented distance, with other untoward conditions, all conspired to render success by no means easy. Neither Valencia, nor Newfoundland, had, it would seem, any attractions for an astronomer. At Valencia, the observers were informed, on their arrival, early in October, that it had rained every day, without exception, for eight weeks. It was not until the 14th that any instrumental adjustments whatever, could be effected. During the seven weeks of their sojourn "there were but four days on which no rain fell, and there was but one really clear night during the period while the instruments were in position. The observations were, in general, made during the intervals of showers; and it was an event of frequent occurrence for the observers to be disturbed by a copious fall of rain while actually engaged in noting the transit of a star." The climate of Newfoundland appears to have been no better for astronomical work than that of Valencia. Messrs. Dean and Goodfellow reached Heart's Content on the 20th September, but saw neither sun, moon, nor stars until the 16th October, when they made their first imperfect adjustments of the transit and clock. In such circumstances, it is certainly most creditable to the skill and energy of the observers, that so many and excellent observations were obtained, as are discussed in this report.

The most formidable difficulty encountered, however, was the defective condition of the long land line of 1,100 miles between Newfoundland and Maine. On this line there were no less than four "repeaters," or double relay magnets, and two stations at which messages were re-written, without even an automatic "repeater," until these indispensable instruments were furnished for those stations by Mr. Davidson. But even with these provided, and with the most laborious precautions taken in other respects, all efforts at direct communication proved unavailing, day after day, and week after week. Mr. Davidson's health becoming impaired, his place at Calais was taken by Mr. Boutelle, one of the most experienced officers of the survey. Singularly enough, it was only a couple of hours before his arrival, on the 11th December, that, suddenly, the long-desired communication was found to be established. "A

sharp frost had thrown the otherwise defective line into a condition of admirable insulation, so that an interchange of clock signals was effected without difficulty." The signals afterward obtained over this line were, in the main, quite satisfactory, and sufficiently numerous to ensure a trustworthy result; still this third link in the chain of longitudes is undoubtedly its weakest part.

A further difficulty was presented by the unprecedented interval between the meridians; necessitating the use of simple clock-comparisons instead of star-signals, and preventing the interchange of observers for eliminating the effects of personal equation. Star-signals—i. e., signals transmitted at the instant a given star passes the several wires of the transit instrument, first at one station and then at the other, and registered at both—have these advantages over mere clock-signals—beats sent from each station and similarly registered—that they give results independent of the star's right ascension, which, cannot be said of the other; that, unlike the other, they are independent also of absolute time-determinations, though not of instrumental errors; and, finally, that they yield results affected only by the clock's rate, and not its error, in the interval of the star's passage between the two meridians. Where this interval is large, as in the case before us, the special advantage of star-signals mainly disappears; and even if it did not, they would require too protracted an occupation of the cable, and in the climate encountered, or indeed, in any climate, the chances would be great, with an interval of three hours, against the same star being observed at both stations.

With the superior catalogue of time-stars, however, which had been prepared, and with the careful experiments for personal equation which were made both before and after the expedition, there was good reason to believe that, even with the clock-method, necessarily used, no sensible, or at least, considerable error depending on the right ascensions would be introduced, nor any, also, arising from uneliminated personal equation. That this was true, in both cases, the discussion of these points in the report seems conclusively to show.

Still another serious obstacle in the way of accurate longitude determination was the absence of any means for the automatic registration of the signals received; inasmuch as the loss of time in noting the signals was, in the method employed, not only considerable, but quite uncertain. The most sensitive electro-magnet tried, could not be actuated unless by a charge of the cable too strong to allow the transmission of signals with sufficient rapidity. When the Valencia clock was breaking circuit during an eighth part of every second, a permanent deflection only was observed at Newfoundland.

The instrument actually employed for receiving signals was Sir W. Thomson's very ingenious reflecting galvanometer, the construction and performance of which are thus described. "A small mirror, to the back of which is attached a permanent magnet, the joint weight of the two being from five to six centigrams, is held, by means of a single fiber above and below, in the center of a coil of fine wire, which forms part of the galvanic circuit; and its position and sensitiveness are regulated by movable bar-magnets placed in the immediate vicinity. Upon the mirror is thrown a beam of light through a slit in front of a bright kerosene lamp, and the deflections of the needle are noted by the movements of the reflected beam, which is received upon a strip of white paper. The exquisite delicacy of this galvanometer, as well as the electrical excellence of the telegraph cables, may readily be appreciated after the beautiful experiment in which the electricians at Valencia and Newfoundland conversed with each other on a circuit not far from 700 myriameters (4,320 statute miles) in length, formed of the two cables joined at the ends, using a battery composed of a percussion gun-cap, a morsel of zinc, and a drop of acidulated water."

It is obvious that, with this instrument, there must be an appreciable loss of time in noting signals, due to inertia of needle, etc., which would be avoided, or more definitely measurable, with an automatic register. But no such register was available. Fortunately, this loss, as is shown in the chapter on "Personal error in noting signals," proved to be more constant and measurable than was anticipated, and, affects the final result only in a very slight and limited degree. A very delicate automatic receiving apparatus has since been suggested by Dr. Gould for recording the signal on the chronograph, by contact of the galvanometer needle, whenever slightly deflected, with wires placed close on each side of it, and properly connected with the battery.

The other instruments used were the regular apparatus of the telegraphic party of the Coast Survey; at each station, a 46-inch transit instrument, with reversing apparatus, and a reticule of five "tallies," of five spider lines each; a chronograph, (Bond's "spring governor," at Valencia and Heart's Content, and a "Kerrison's regulator" at Calais;) and a circuit-breaking clock. Upon all the chronographs one pen, which is constantly tracing a line upon a revolving cylinder, records the signals, both of the clock and of the observer, by offsets from this normal line.

At Valencia, observations were obtained on fifteen nights, on no one of which was the sky unclouded. "On only two of the five nights on which longitude signals were exchanged with Newfoundland, was it possible to obtain observations after

the exchange, and this was possible, too, on only one of the three nights, when signals were successfully exchanged with Greenwich. Fifty-three complete transits were observed over all the threads; on and after November 5th, by Mr. Mosman alone; on the 25th and 28th October, by Dr. Gould; at other times by both. The clock corrections, however, whenever possible, were deduced from Mr. Mosman's observations; when otherwise, a constant correction of $-0^s.08$ was applied to Dr. Gould's. For both clock correction and azimuth, the conditional equations were solved by the method of least squares, a method introduced into this branch of the coast survey work by Dr. Gould, and an important means of giving precision to the telegraphic longitudes. We are indebted, indeed, to this eminent astronomer for the particular notation and methods of observation and reduction, which have been employed in the longitude work of the coast survey since it came under his charge, and which are described in the C. S. Report for 1856. The transit observations are given in full, with their reductions for the groups immediately preceding and following the longitude-signals, and with the normal equations and resultant values for each group.

A glance at these observations reveals at once, in the small residuals, both the accuracy of the star-places and the skill of the observers. For, necessarily, different sets of fundamental stars for clock and instrumental corrections were employed at the two stations, owing to their distance, and also at the same station on different nights, owing to the various hours at which the signals could be exchanged. Considering all the unfavorable circumstances, the degree of precision attained is certainly remarkable. This must be attributed, in great part at least, to the extreme accuracy of Dr. Gould's own carefully elaborated catalogue of fundamental stars, prepared in 1862, for the use of the longitude parties of the Coast Survey, and in its second edition, furnishing still more precise places, especially for circumpolar stars.

The observations at Heart's Content, and at Calais, for time and instrumental corrections, were made, as at Valencia, in very unfavorable circumstances, yet, similarly, exhibit remarkable accordance. Fortunately, at the former place excellent series of transits were obtained, both before and after every exchange of longitude signals, whether with Valencia or with Calais. At the latter place, however, the clock gave serious trouble as well as the weather; yet the observations, though not as good as could be wished, proved to be much more accordant than might have been expected in the circumstances, and give, it appears, a small probable error of the final result.

Longitude signals were successfully transmitted between

Foilhommerum and Heart's Content on but five dates; October 25th and 28th, and November 5th, 6th, and 9th. The method of giving and receiving these signals is thus described in the Report. We quote at length, that a clear understanding may be obtained of that portion of the operations on which the precision of the result chiefly depends.

"Three series, of two sets each, were exchanged on every occasion; each set consisting of ten signals alternately positive and negative, at intervals of about five seconds, except that the fifth and eighth were preceded by pauses of ten seconds, which was also the interval between the two sets. The purpose of this arrangement was to discover whether the velocity of transmission was perceptibly affected by a longer time being allowed for the cable to recover its electrical equilibrium, and also to facilitate the identification of the individual signals. Some slight convenience in the practical details also arose from the circumstance that each set occupied one minute, and that each series consisted of ten positive and ten negative signals. Those signals were considered positive by which the platinum was put in connection with the cable and the zinc with the ground.

In receiving the signals, the observer (Mr. Dean at Newfoundland, and myself at Valencia) watched the deflections of the light-spot, while his thumb rested on the button of a delicately adjusted break-circuit key, which was pressed at the instant in which the deflection was perceived. This instant was thus recorded upon the chronograph, after a certain amount of delay, which we will call the personal error of noting, and which depended upon a considerable number of influences to be discussed hereafter. The keys by which the signals were transmitted were made by the American Telegraph Company, under the supervision of Mr. Dean, and are constructed according to the arrangement devised by Prof. Thomson for the Atlantic Telegraph, in such a manner that pressure upon one button produces a positive, and upon the other a negative signal, while no current flows at other times. To this arrangement an additional contrivance was applied by which the local circuit to the chronograph passed through the same key, and was interrupted by pressure upon either button, so that every signal transmitted through the cable was recorded upon the chronograph at the station whence it was sent.

It is thus manifest that the times of sending the signals were accurately recorded, while the times of receiving signals were recorded after an interval of time dependent on the personal error of noting, and inseparable from the time of transmission through the cable, except by some independent means of measurement. If this interval were the same for both observers, it would be eliminated entirely from the longitude and merged

with the time of transmission. Otherwise it would effect the resultant longitude by one-half the difference between the personal errors of noting for the two observers. Happily it proved to be very nearly the same for Mr. Dean and myself, and also measurable; so that it has been possible to eliminate its influence from the measure of velocity, as well as from the longitude."—*Rep.*, p. 54.

The relations of the several quantities derived from the observations, and entering into the final results, are expressed in the following equations: in which T and T' denote the clock times at Valencia and Newfoundland, respectively, Δt and $\Delta t'$ the clock errors, x the transmission time of the signals, and λ the longitude; these quantities being also distinguished by a subscript ₁ and ₂ for Valencia and Newfoundland signals respectively. Including in x the personal error of noting signals, the signals given and recorded at Valencia at the time T_1 will be registered upon the Newfoundland record at $T_1' = T_1 + \Delta t_1 - \Delta t_1' - \lambda + x_1$ and the signals given and recorded at Newfoundland at T_2 will be registered upon the Valencia record at $T_2 = T_2' + \Delta t_2' - \Delta t_2 + \lambda + x_2$. Thus the comparison of the records of Valencia signals, at the two stations gives $T_1 - T_1' = \Delta t_1' - \Delta t_1 + \lambda - x_1$, while the comparison of the records of Newfoundland signals gives

$$T_2 - T_2' = \Delta t_2' - \Delta t_2 + \lambda + x_2,$$

and consequently

$$2\lambda = (T_2 - T_2') + (T_1 - T_1') + (\Delta t_2 - \Delta t_2') + (\Delta t_1 - \Delta t_1') + (x_1 - x_2)$$

$$x_1 + x_2 = (T_2 - T_2') - (T_1 - T_1') + (\Delta t_2 - \Delta t_2') - (\Delta t_1 - \Delta t_1')$$

If we assume the personal error of noting to be the same for the two observers, and the signals to travel with equal velocity in the two directions, the term $x_1 - x_2$ will disappear from the first equation, while the second will give a measure of the sum of the transmission-times and the personal errors of noting.

The several quantities above indicated are given in detail in the Report for the different series of signals, and exhibit excellent accordance in the results. There appears to have been no difference of clock rates to affect the deduced value of x , nor of velocity for eastern and western signals to require a correction of λ depending on the clocks.

The resultant values for the longitude, subject, however, to a correction for personal equation in determining time, are as follows:—

1866, October	25,	2 ^h 51 ^m 56 ^s ·477
	28,	56·487
November	5,	56·455
	6,	56·481
	9,	56·460

The mean interval between the moments of giving the signals and of their record upon the chronograph sheet is similarly found to have been

October	25,	0 ^s ·62	± 0 ^s ·008
	28,	0·64	·010
November	5,	0·59	·004
	6,	0·55	·007
	9,	0·54	·005

in which the quantities appended are the probable errors of the respective determinations as deduced from the total results of the several sets, there being six sets for each determination except that of November 6th.

Between Heart's Content and Calais the clock signals were much less satisfactory; being obtained only on four nights, on only two of which the clock errors could be determined either immediately before, or soon after, the exchange, and on one of these two there was special trouble from the clock. By some misunderstanding, also, the signals were not exchanged strictly according to programme, and thus a further source of uncertainty was introduced from the partial confusion occasioned in the record, and the greater difficulty of eliminating personal equation. The resulting longitude and time of transmission exhibit, however, better accordance than was to have been expected, and are as follows (subject to a correction for personal equation):

	1866.	λ .	α .
December 11,	0 ^h 56 ^m	37 ^s ·89	0 ^s ·24
12,		37·53	0·31
14,		37·84	0·27
16,		37·78	0·28

From the description of the mode of receiving signals, quoted on page 236, it will be seen that there is introduced, in all the observations, a special error due to the different methods employed at the two stations of recording a given signal. This is called in the report, "the personal error of noting." It is the interval between the arrival of the signal at the galvanometer and its record on the chronograph; for it will be remembered that the signal sent through the cable does not pass at once, automatically, to the chronograph, as at the transmitting station, but reaches it only through the mediation of an observer, who, after noticing the deflection of the light-spot from the galvanometer, sends a second telegraphic signal to his own chronograph. The whole interval, α , therefore, which elapses between the giving of a signal at one station and its chronographic record at the other, is made up of several parts.

These parts, as given in the Report, consist of the time requisite,—

1. For the signal to arrive at the other station.
2. For the galvanometer needle to move through a perceptible arc.
3. For the observer to notice the motion and tap his break-circuit key.
4. For this observation-signal to be recorded upon the chronograph.

Of these the second and third constitute the “error of noting,” which is, therefore, partly instrumental and partly personal; the one due to the inertia of the galvanometer-magnet, the other being the “transmission-time” along the nerves from eye to brain, through brain, and thence to finger-tips; consisting, therefore, of three distinct intervals, viz.: 1, between phenomenon and perception; 2, between perception and volition; 3, between volition and giving signal. The sum of these three intervals, according to the experiments made by Dr. Gould, could hardly exceed, for good observers, $0^{\circ}3$; which would give for velocity of transmission through nerves about 13 feet, unless, indeed, some considerable time is occupied in the act of volition after perception, as seems probable from the experiments of de Jaager and Hirsch. Helmholtz made the velocity of nerve-sensation in the frog (dead), by different experiments, 87 and 89 feet per second. Dr. Schelske found that velocity in the nerves of common sensation and along the spinal cord in living men, to be about 97 feet. Others make the velocity not more than half as great.

With respect to the four intervals which make up the quantity x , it is remarked that, if equal at the two stations, they become wholly eliminated in the resultant longitude; if unequal, the longitude must be increased by one half the excess of their sums for westward signals. In either case, the operations for longitude give only their total sum at the two stations. The chronographs at both stations being similar, it might be presumed that the time lost by both in making the record after the key is tapped would be the same; and repeated examinations showed that this was sensibly the case. It therefore becomes eliminated in the resultant longitude; as does also the transmission-time through the cable, if sensibly the same in both directions; as Dr. Gould shows that it is.

The method employed for determining the “error of noting” was alike ingenious and successful. It was by carefully observing a series of signals similar to those exchanged for longitude, and so arranged that both the original signal and the observation of the consequent deflection were recorded on the same chronograph. The similarity requisite in respect to

intensity of current, was secured by an ingenious adjustment of battery force to both galvanometer and chronograph, by means of a divided circuit, the branch of it through the galvanometer being a German silver wire so fine as to give an intensity, as compared with that in the other branch, of 1 to 100; the requisite reduction. Other equally skillful devices secured similarity and freedom from error in other respects.

These experiments showed the range of variation of this error of noting for different individuals to be very small; the mean error for Dr. Gould at Valencia being $0^{\circ}271$, and for Mr. Dean at Newfoundland, $0^{\circ}335$; the difference being due to the galvanometer, probably, rather than the observer. The sum of the two $0^{\circ}606$, is the quantity to be deducted from the value of $x_1 + x_2$ to obtain the true time of transmission, and half their difference, or $0^{\circ}032$, that to be deducted from the longitude after all other corrections are applied.

In conducting these experiments a very curious source of error was detected, in the effect of hearing the click of the key, upon observations supposed to depend only on seeing the deflection of the needle. This is a point of so much importance to observers, that we quote the suggestion of the Report, "that a very marked effect upon the observation of transits of stars is likely to be produced when the chronograph is in the same apartment, so that the regular beats of the magnet are audible. When the intervals between the transit-threads are approximately multiples of half a second, the tendency is very great so to tap upon the observing key as to produce a rhythmical beat in the armature; and when the interval differs from the multiple of a second, the occurrence of that magnet-beat which records an even second often precipitates the tap of the observer, whose nerves are in keen tension awaiting the instant of bisection. Only a strong effort of will can obviate these perturbing influences—which are akin to those exhibited in the measurements just described."

This curious effect seems to show that eye and ear do not report to the brain with the same promptness, and that, when addressed simultaneously, the mind cannot recognize which makes the report, but takes that of one or the other, or both together, indiscriminately, the range of uncertainty being equal to the difference in promptness of the two senses. Possibly part of the effect may be due to a definite lapse of time between the occurrence of the audible and the visible signal; which, however, should give a more constant effect.

Want of space compels us to pass the interesting chapter on "Personal equation in determining time," with simply the remark that the ordinary methods of measuring that equation being inapplicable, by reason of the distance and other circumstances,

comparisons were made between the observers for this purpose as soon as practicable after their return; and the values deduced, though in many respects quite unsatisfactory, were adopted as the best approximation that could be obtained for the different observers. They are these:

Gould—Mosman	$= +0^{\circ}02$
Dean—Mosman	$= +0^{\circ}11$
Goodfellow—Dean	$= +0^{\circ}14$
Boutelle—Goodfellow	$= -0^{\circ}14$
Boutelle—Chandler	$= -0^{\circ}04$

A special arrangement, by which it was intended to entirely eliminate personal equation at Heart's Content, unfortunately failed through some misapprehension of the observers. The fact, however, that they had been in the practice of observing together for many years and had always found the equation between them to vary between very narrow limits, on the two sides of zero, rendered the failure of little practical consequence.

We hasten to give briefly the final results for longitude.

Between Foilhommerum and Heart's Content we have for the several dates, after corrections applied:—

1866, Oct. 25,	$2^{\text{h}} 51^{\text{m}} 56^{\text{s}}.457$
28,	.468
Nov. 5,	.455
6,	.481
9,	.460

The final longitude deduced, after correction for personal equation in determining time, Dean—Mosman $= +0^{\circ}11$, and in noting signals, Dean—Gould $= +0^{\circ}03$, becomes

$$\lambda = 2^{\text{h}} 51^{\text{m}} 56^{\text{s}}.54$$

Between Heart's Content and Calais, the results, similarly corrected, are for the several dates:—

Dec. 11,	$0^{\text{h}} 55^{\text{m}} 37^{\text{s}}.93$
12,	[37.53]
14,	37.84
16,	37.82

And the final result, corrected by $-0^{\circ}14$ for personal equation between Boutelle and Goodfellow, and omitting Dec. 12, is—

$$\lambda = 0^{\text{h}} 55^{\text{m}} 37^{\text{s}}.72$$

Between Greenwich and Foilhommerum, the longitude was obtained by satisfactory signals on two nights, and compared with two previous determinations by Mr. Airy for other points at Valencia, by short geodetic connections. The two nights exchanges gave

	λ .	α .
1866, Nov. 5,	0 ^h 41 ^m 33 ^s .305	0 ^s .115
13,	33.280	0.110
Mean,	0 41 33.29	

This differs by $-0^s.10$ from that adopted by Mr. Airy as deduced from the great chronometric expedition of 1844, and the telegraphic determination of 1862.

The combination of the three longitudes thus determined, gives—

Greenwich—Foilhommerum,	0 ^h 41 ^m 33 ^s .29
Foilhommerum—Heart's Content, 2	51 56.54
Heart's Content—Calais,	0 55 37.72
Greenwich—Calais,	4 29 7.55

“The Valencia observations having been made by, or referred to, Mr. Mosman throughout the whole period, his personal equation is eliminated; the equation between Messrs. Goodfellow and Dean, always small, may be regarded as trustworthy, and by a happy coincidence the personal equations of Mr. Boutelle on the west, and of Mr. Mosman on the east, seem to be almost identical, so that even a total disregard of this quantity would have resulted very nearly in its perfect elimination, the oceanic arc being diminished and the land arc increased, each by about $0^s.14$.”

The only probable influence of personal equation in the entire longitude-measurement, comprising, as it does, three-sixteenths of the whole circumference, lies in the difference between the observations of Messrs. Dunkin and Boutelle.

The longitude of Calais, as heretofore telegraphically determined, is as follows:—

Calais—Bangor,	0 ^h 6 ^m 0 ^s .31
Bangor—Cambridge,	0 9 22.99
Cambridge—New York,	0 11 26.07
New York—Washington,	0 12 15.47
Calais—Washington,	0 39 4.84

whence we have

Greenwich—Washington, 5^h 8^m 12^s.39

The Seaton Station being $12^s.44$, and the dome of the Capitol $10^s.17$, east of the Naval Observatory, to the center of the dome of which the preceding value refers, we have as their longitudes from Greenwich—

Seaton Station,	5 ^h 7 ^m 59 ^s .95
Capitol,	5 8 2.22

From the facts, as exhibited in this report, there would seem to be every reason to believe that this result is within a very small fraction of a second of the exact longitude, and that the small probable error, whatever it may be, is mostly due to the weakest link in the whole chain—that between Newfoundland and Calais.

The indiscriminate mean of all the chronometer determinations, as given early in this article, using the numbers of chronometers as weights, is $5^h 8^m 12^s.14$; leaving out Bond's first discussion of the 175, it is $5^h 8^m 12^s.37$. The mean of Peirce's two from occultations of Pleiades, without regard to weight, is $5^h 8^m 12^s.29$.

We regret that we have not room for even a brief abstract of the valuable closing chapter on the transmission-time of signals, but are compelled to dismiss it with barely stating the general result and some conclusions with respect to a few points, as derived from the experiments.

The transmission-time by cable, after correction for personal error in noting signals ($0^s.303$), was found to be for the several dates:—

1866, October	25,	$0^s.314$	Cable of 1865, with earth and condenser.				
	28,	$.343$	"	"	"	"	"
November	5,	$.280$	Both cables, no earth.				
	6,	$.248$	"	"	"	"	"
	9,	0.240	"	"	"	"	"

The battery-strength on these nights was as follows:—

October	25,	10	cells at Valencia,	10	cells at Newfoundland.
	28,	10	" " " "	10	" " " "
November	5,	3	" " " "	3	" " " "
	6,	3	" " " "	10	" " " "
	9,	4	" " " "	10	" " " "

From these results the inferences seem warrantable, 1st, that the velocity of transmission is greater when the circuit is direct and consists of a good metallic conductor exclusively, than when the signals are given by induction, although the earth may be at the other electrode; and 2d, that an increase of intensity in the electromotive force is attended by an increase in the velocity of propagation of the signal.

From the observations for longitude, and from other experiments made with special reference to particular points, the following general conclusions were reached.

"It appears manifest that not an electrical charge or discharge, but simply an electrical disturbance, is requisite for transmitting a signal; that an inductive impulse, sufficient to deflect the galvanometers employed, was transmitted through one cable,

having at each end a condenser with 10 cells, in somewhat less than the third of a second, five seconds after the transmission of an impulse of the opposite sort; that with a circuit formed by the two cables, a smaller electromotive force sufficed to transmit the signals with yet greater rapidity; that the signals traveled more rapidly through a cable which had not recovered its electrical equilibrium after a current of the opposite character; and that the speed of the signals is modified by the earth-connections, more readily than by changes in the battery-power. And the very marked differences, found in the rates of transmission, between signals given by completing an interrupted circuit and those given by interrupting a closed circuit, may perhaps lead to investigations which will afford an explanation."

C. S. LYMAN.