REPORT

ON THE

RECENT DETERMINATION OF THE LONGITUDE OF MADRAS.

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This Report is divided into three parts:—

PART I—CONTAINS A NARRATIVE ACCOUNT OF THE EXPEDITION,
PART II.—GIVES THE RESULTS OBTAINED, AND IN
PART III.—THESE RESULTS ARE COMPARED WITH FOREIGN TELEGRAPHIC VALUES.

PART I.

NARRATIVE ACCOUNT OF THE EXPEDITION.

The following table has been compiled to show the previous determinations that have been made by Indian officers of the longitude of Madras:—

Date.	Longitude of Madras. Arc.	Time.	Authority.	Book of Reference.
1798	80° 16′ 30″	5h 21m 6s 0	Lambton	Asiatic Researches, Vol. X.
1805	80 18 30	5 21 14.0	Lambton	Asiatic Researches, Vol. XII.
1815	80 17 21	5 21 9.4	Warren	G T. Survey of India, Vol. II.
1826	80 17 15	5 21 9.0	Goldingham	Records of the Madras Observatory.
1830	80 15 55·5	5 21 3·7	Taylor	Vol XVI, Memoirs, R. A. S.
1840	80 13 55·5	5 20 55·7	Riddle	Vol. XII, Memoirs, R. A. S.
1845	80 14 19·2	5 20 57·28	Taylor	Vol. XVI, Monthly Notices, R. A. S.
1847	80 15 56·55	5 21 3·77	Everest	Great Arc of India.
1858	80 14 19·5	5 20 57·3	Jacob	G. T. Survey of India, Vol. II.
1878	80 14 51·24	5 20 59·416	Campbell	Annual Report on the Great Trigonometrical Survey of India for 1876-77.
1883	80 14 50·03	5 20 59·335	Walker	G. T. Survey of India, Vol IX.
1890	80 14 51·08	5 20 59·405	Strahan	G. T. Survey of India, Vol. XV.
1893	80 14 51·33	5 20 59·422	Strahan	G. T. Survey of India, Vol. XV.

The close accordance between the last four values must not be regarded as evidence of accuracy, as these four values are not the results of different observations, but merely different discussions of the same determination. No account exists of Lambton's method of observation: his results alone remain on record. Warren's and Goldingham's values were deduced from the observations of Jupiter's satellites, and Taylor had recourse to moon culminations. Riddle's, Everest's and Jacob's values were obtained from discussions of Taylor's lunar observations, and the value quoted by General Walker in 1872, in his paper on "The Longitude of Teheran", Volume XXXII, Monthly Notices, R. A. S., was derived from the same source.

In 1874 the telegraphic longitude of Suez was measured by members of the Transit of Venus Expedition, and two years later the difference of longitude between Suez and Madras was telegraphically determined by Colonels Campbell and Heaviside. The combination-result of these two operations was to place the Madras Observatory in longitude $5^h\ 20^m\ 59^s.416$, a value subsequently modified by General Walker to $5^h\ 20^m\ 59^s.335$.

The next change made was in 1889: in the fourteen years previous to this date, a net-work of longitude triangles had been gradually thrown over the Indian Peninsula, the accuracy of the arcs of each triangle being tested by the smallness of the closing error; as early as 1876 the closing errors of the triangular circuits were considered unsatisfactorily large, in 1881 they averaged a quarter of a second of time, and in 1885 they became so large that it was considered useless to proceed with the work, unless their cause were discovered.

In 1889 these errors were proved to be due to imperfections in the object glasses of the collimators, and to eliminate the effect of these imperfections a new method of calculating the collimation-constant was introduced: all the Indian arcs of longitude, including Bombay-Aden and Aden-Suez, had consequently to be re-computed *de novo*; the large circuit errors were then found to have disappeared, and the longitude of Madras became $5^h\ 20^m\ 59^s.405$.

When all the Indian longitude arcs were finally adjusted in 1893, by a simultaneous reduction by the method of minimum squares, the difference of longitude between Bombay and Madras was increased by $0^s.017$, and the longitude of Madras made $5^h\ 20^m\ 59^s.422$.

The following are the values which have been adopted in the Nautical Almanac list:—

From 1816 to 1834	Goldingham's value;
„ 1835 to 1861	Taylor's first value;
„ 1862 to 1881	Jacob's value;
„ 1882 to the present time	Campbell's value.

Error of the Indian Triangulation.

In the last eighty years no change has been made in the value adopted for the Great Trigonometrical Survey of India; that value is Warren's value, 80° 17′ 21″, and was introduced by Colonel Lambton in 1815.

The precise error in the longitude of our principal triangulation is not, however, identical with the error in Warren's longitude of Madras: Kalianpur is the origin of our triangulation, and its longitude was fixed by Colonel Everest as follows:—

Warren's longitude of Madras	80°	17′	21″
Difference of longitude between Madras and Kalianpur found by triangulation	2	35	36·25
Everest's longitude of Kalianpur	77	41	44·75

In Volume II, Great Trigonometrical Survey of India, General Walker contemplates the possibility of the quantity 2° 35′ 36″.25 having to be altered in the future, when his revisionary triangulation between Kalianpur and Madras had been finally reduced, and when possibly Clarke's values of the semi-axes had been substituted for Everest's, but in 1884 it was pointed out by Mr. Hennessey that the difference of longitude between Kalianpur and Madras must be independent of triangulation and semi-axes and be astronomically determined.

This astronomical determination was made in 1889, and the true difference, finally corrected, is 2° 35′ 29″.49. The actual error therefore in the adopted longitude of the Great Trigonometrical Survey of India is 6″.76 *less* than the error of Warren's Madras value. In 1840 Everest estimated the error in longitude of the Indian triangulation at about +3′ 30″: in Vol. II, Great Trigonometrical Survey of India, General Walker, using Jacob's value of Madras, gives it as about +3′, but in the interim between the printing and publication of that volume the first telegraphic determination of longitude was carried out, and in the preface to the volume General Walker reduces his estimate to about +2′ 30″, the difference between Campbell's and Warren's values. In Vol. XV, Great Trigonometrical Survey of India, Colonel G. Strahan calculates the error to be +2′ 22″.92.

Origin of the recent Expedition.

In 1891, at the meeting of the International Geographical Congress at Berne, the question was raised as to why the Government of India did not correct the longitude of its maps instead of continuing to publish them with an acknowledged error of 2′ 30″.

A discussion followed in India as to whether any such alteration was feasible, and upon examination the longitude of India was found even then to be not known with *sufficient accuracy* to justify a change. Colonel Everest had to deal with this same problem half a century ago. The longitude of his triangulation had long before been made dependent on Warren's value for Madras, but as time went on Taylor had improved on Warren's value, and Colonel Hodgson, the Surveyor General of India, had had other independent observations taken at Calcutta: Everest had to decide whether he would make use of these later and better observations at Madras and Calcutta and substitute some new value of longitude for Warren's. His decision had best be given in his own words: "These data seem to me by no means *sufficiently* conclusive to warrant any alter- "ation in the quantities employed by Colonel Lambton in all previous operations "of the Great Trigonometrical Survey, for just in the same manner as Mr. Taylor "has assigned a new value for the longitude of Madras, some future astronomer "may introduce another alteration. In fact the actual determination of the ter- "restrial longitude of any place is too difficult and delicate a question to rest on "a small number of observations, and if every new set of determinations were ap- "pealed to as a test, there would be no end to the shifting of the origin: where- "fore it seems to me better for the present at least to use the same value as that "employed by Colonel Lambton".

The question may be asked as to what constitutes "sufficient" accuracy, or as to what Everest would have considered "sufficiently conclusive data" to warrant an alteration. From the great mass of latitude and longitude observations that have been taken in all parts of India, excluding those portions under the influence of Himalayan attraction, it has been ascertained* that in any astronomical determination, the probability of error arising from the deviation of the plumb-line from the vertical is about 2″.83, and at all events, it will be conceded, that no alteration in the adopted longitude of Madras would be desirable, until a value has been obtained, whose probable error is at least as small as this. It might then perhaps be argued that it would be useless to go further or to attempt the attainment of a degree of accuracy in observation greater than that which local attraction admits in final results; but local attraction is an uncertain and unsatisfactory standard, and in seeking a definition for the "sufficiently conclusive data" of Everest, it is hardly practicable to insist upon the probability of error being confined within named limits. Nothing more definite can well be laid down than that the longitude of a Great Trigonometrical Survey and of a great national observatory should be determined with the highest accuracy attainable. By this expression is implied no unreasonable multiplicity of observations but merely the employment of that same refinement and care which distinguished the operations of the Trigonometrical Survey itself.

In 1815 Captain Warren's value was probably the best attainable, but in 1877 it was rendered obsolete by the telegraphic determination *viâ* Mokattam and Suez. Unfortunately this latter measurement, superior as it is to all previous results, has itself been subjected to somewhat severe criticism, and though it was sufficiently accurate to prove conclusively the existence of considerable errors in Warren's and Taylor's values, it is held to have been by no means determined with the "highest accuracy" attainable. In Volume I of the Annals of the Cape Observatory, Mr. David Gill, F.R.S., H.M.'s Astronomer at the Cape, discussing the longitude of the Cape of Good Hope, which like that of Madras depends on the telegraphic determinations between Greenwich, Mokattam, Suez and Aden, writes: "The weak point of this result is un- "questionably the determination of the longitude Greenwich-Aden. Neither of "the two series of operations on which it depends was executed with such refine- "ments or precautions as are necessary for the determination of fundamental "longitudes, nor indeed, so far as I know, were these operations planned with a

* Annual Report of the Survey of India Department, 1893-94.

"view to the securing of more accuracy than would amply suffice for Transit of "Venus purposes.

"For refined purposes, the results of the British Transit of Venus party "are vitiated by the extraordinary variations of the Personal Equation of the "observers engaged in the determination of the Greenwich-Mokattam longi-"tude, the results varying over a range of six-tenths of a second of time on the "seven nights upon which Personal Equation was determined.

"For the longitude Mokattam-Suez there is no comparison of the Per-"sonal Equation of the observers before the expedition and only a somewhat un-"satisfactory one after it".

The two series of operations referred to by Mr. Gill are the determination *viâ* Mokattam and Suez, and the German determination *viâ* Berlin and Alexandria, and though the results of both were utilised to obtain the longitude of the Cape of Good Hope, that of Madras has been made to only depend on one: the anomaly thus exists that whilst the longitude of the observatories of Madras and the Cape are both equally dependent on the longitude of Aden, a different value for the latter is now being adopted in the two cases.

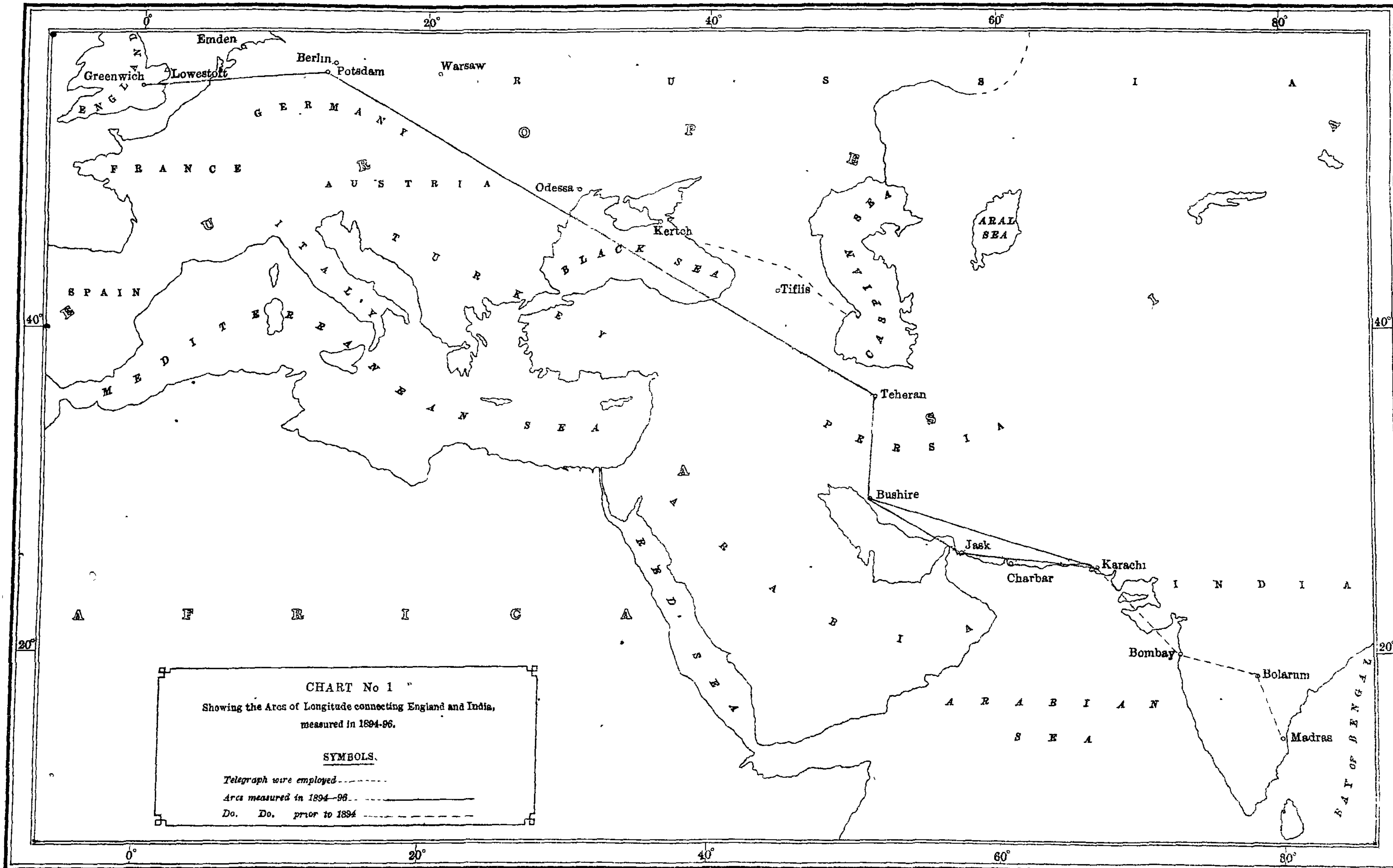
About the time that the reliability of the Mokattam-Suez-Aden arcs was being discussed, it was decided to organise and despatch an astronomical party to Baluchistan and Persia with the object of determining telegraphically the longitude of points on the Makran Coast and the Persian Gulf, Captain Lenox Conyngham and myself being designated as the observers. An opportunity was thus presented of obtaining *viâ* Teheran a new and refined determination of the longitude of Madras.

The opportunity was taken, and the party was ordered to extend their operations through Persia and Europe to Greenwich.

Equipment.

The instrumental equipment consisted of two complete sets of instruments, each set comprising a transit telescope, a chronograph with accompanying electrical apparatus, an astronomical clock and a break-circuit chronometer. The transit telescopes were sister instruments, specially made for this expedition by Messrs. Troughton and Simms of Charlton, Kent. The focal length is $35\frac{1}{2}$ inches and the diameter of the object glass $3\frac{1}{2}$ inches, and each possesses an apparatus by which the telescope can be lifted out of the Y's and rapidly reversed. The diaphragm carries twenty-five vertical wires for the observation of transits The value of one division of the micrometer was found to be almost identical in the two instruments, *viz.*, $0^s.03884$. These divisional values were determined in India, in Europe, and for a third time in Persia, owing to apprehensions that they might vary with temperature and focus; the difference between the expansions by heat of the brass tubes of the telescopes and of the steel screws of the micrometers has, however, been found to be to a great extent compensated by the simultaneous expansion of the object glasses and the alteration in their co-efficients of refraction, and no sensible variation in value has been traced. With each transit telescope is an excellent collimator having an object glass of $3\frac{1}{2}$-inch diameter and $35\frac{1}{2}$-inch focal length.

The chronographs were made by Messrs. Warner and Swasey of Cleveland, Ohio, U.S.A., and gave great satisfaction, but owing to their weight being too great for a mule, the clock-work, drum and fittings of one of them were transferred from the heavy metal base-plate to a wooden fac-simile, which was made under Colonel Gore's direction. The clocks were eight-day astronomical clocks by Frodsham with mercurial pendulums furnished with the usual arrangement for breaking an electric circuit every second. The break-circuit chronometers, which were made by Messrs. Bond & Co. of Boston, were included in the equipment, as a reserve to fall back upon, in the event of the clocks receiving damage during their transport on the backs of mules and in Arab boats: their purchase was amply justified by results, for the only instruments that failed throughout the whole expedition were the two astronomical clocks, one of which received a jar when being landed in the surf at Bushire, the other being injured on the march from the Caspian to Teheran. The rate of the break-circuit chronometers proved as steady and as reliable as that of the clocks.



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Photozincographed at the Office of the Trigonometrical Branch, Survey of India, Dehra Dún, September 1896

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The observers were also provided with portable observatories consisting of canvas roof and walls on a wooden frame-work with a meridional aperture. These observatories were intended to hold the transit telescopes only, as they were not sufficiently substantial to protect the clocks from changes of temperature : a room about ten feet square was built for the chronographs and clocks when an existing building was not available.

Selection of Arcs.

The whole area of India had already been covered with a net-work of longitude arcs, and by their means the difference of longitude between Madras and Karachi had been accurately determined, and it was thus only necessary to commence at Karachi and determine its difference from Greenwich.

The following table shows the six arcs into which it was originally proposed to divide this distance :—

Arcs.	Length of Wire.	Telegraph Lines.
	English miles.	
Karachi to Bushire	1,193	Submarine cable, Persian Gulf Telegraphs.
Bushire to Teheran	655	Land-line, Persian Telegraphs.
Teheran to Odessa	1,516	Land-line, Indo-European Telegraph Company.
Odessa to Emden	1,402	Land-line, Indo-European Telegraph Company.
Emden to Lowestoft	280	Submarine cable, British Government.
Lowestoft to Greenwich	135	Land-line, British Government.

This scheme of division was based on three principles: (1) The number of arcs must be kept as small as possible; (2) on each arc telegraphic connection must be direct without intermediate translating relays; and (3) owing to the supposed impracticability of joining up submarine cables and land-lines direct without the interposition of a translating apparatus, every place at which a change from cable to land-line occurred was considered an essential station of observation.

As will be seen from the accompanying chart, Bushire, Emden and Lowestoft were the junctions of a cable and land-line, and were thus at once selected as the terminal stations of arcs. Teheran was introduced partly because its geographical longitude was required, and partly because it is the junction of the Indo-European Telegraph Company's wire with that of the Persian Telegraphs. The remaining distance, Teheran to Emden, was considered too long for one arc and was divided at Odessa.

The desirability of having as few divisions as possible need hardly be insisted upon. Every extra link tends to weaken the chain, and every additional arc increases the probable error of the terminal longitude. The length of an arc is, however, in practice limited by such telegraphic considerations as the condition of the wires, the battery power available and the sensitiveness of the relays. Intermediate translating relays on a line-wire are objectionable: the observers have no control over their adjustment, and their effect on the rate of the current in its two directions may not be the same. One intermediate translating relay is perhaps just permissible, the whole line-wire being still under the control of either one or the other of the observers; the accidental insertion on the line of other wayside translations by forgetful telegraph-masters can then generally be detected by the alteration in the character of the distant signals as marked on the drum of the chronograph, but if two intermediate translations exist, that portion of the line between them is free of all control from the observatories.

In addition to the places named above, we were ordered to fix the positions of Jask and Charbar for geographical purposes.

Preliminary Operations.

Having determined the values of the micrometer divisions and of the equatorial wire intervals and the level scales, we tested the instruments for flexure and measured the inequality and irregularity of their pivots. During these observations we made frequent determinations of collimation, *firstly*, by intersecting the cross of the collimator with the central transit wire both before and after reversing the telescope in its Y's, and, *secondly*, by pointing the telescope to the nadir and taking the readings of the micrometer before and after reversal, when the central transit wire coincided with its own reflection in mercury. Our object was to determine the quantity which we call C_o, *viz.*, the reading of the telescope micrometer, when the central transit wire is exactly collimated. We were disturbed to find that the value of C_o was not the same when determined from the collimator, as it was when determined from the nadir: inequality of pivots would have accounted for a small constant difference, but in neither telescope was the difference small or constant. We at first thought that there must be a loose joint in the tubes of the telescopes, and that the lines of their collimation were unsteady, but eventually we found that the difference was caused by the pressure of the uprights of the lifting apparatus against the horizontal axes of the telescopes. These uprights are capped with friction wheels, which are pressed vertically upwards by interior springs against the axes of the telescopes, the object of the arrangement being to relieve the Y's of part of the weight of the telescopes, and the axes are encircled by grooves for the friction wheels to work in. After many experiments and trials it was found that the friction wheels, forced upward by the springs, exerted a lateral pressure, varying in amount, against the sides of these grooves, and that this lateral pressure prevented the telescopes when horizontal from taking up the same position in azimuth after their reversal in their Y's, as they had occupied before, and that consequently the readings of the collimator before and after reversal were not properly comparable: in a similar way also this lateral pressure affected the dislevelment of the horizontal axis, and vitiated the results of the nadir observations. We removed the springs from the uprights, thus relieving the telescopes from the pressure of the friction wheels, and the apparent unsteadiness of the collimation disappeared. Throughout the operations we have worked without these springs, and by doing so we have thrown more weight on to the Y's and pivots than the makers intended: no symptom of weakness or shakiness has appeared, neither the Y's nor pivots have shown any signs of wear, and the telescopes throughout have exhibited a perfect steadiness in collimation.

Having tested our equipment and observed our personal equation, we proceeded to measure an experimental arc in Dehra Dun: the two transit telescopes were set up on the same meridian, one a few feet south of the other, so that there might be no difference in longitude between them, and we took a complete series of longitude observations on four successive nights. Our batteries, clocks, chronographs and observatories were connected telegraphically in the same way as on regular longitude work, the only difference being that the length of our line-wire was ten feet instead of hundreds of miles. We each observed 98 time stars and made 12 comparisons of clocks, and the resulting difference of longitude between the two telescopes, corrected for personal equation, was given at $0^s.017$. Colonel Gore, the Superintendent of Trigonometrical Surveys in India, had been present in the observatories during the measurement, and, taking into consideration that we were new to the telescopes, that our electrical apparatus had been just designed and was not working smoothly, and that our observations had been interfered with by clouds, he decided that the error $0^s.017$ in our result was sufficiently small to justify the commencement of work between Karachi and Greenwich. As we had had no experience of working over submarine cables, we were ordered to introduce a check on the determination of Karachi-Bushire by not only measuring it direct, but also by inserting Jask and determining the arcs Karachi-Jask and Jask-Bushire.

Jask-Bushire Arc.

Having again measured our Personal Equation we left Dehra on November 10th, and on November 26th we sailed from Karachi. The steamer was bound for Bushire, and not for Jask, but at our request the British India Steam Navigation Company directed the Commander to drop one Party off Jask. Mr. Woodsell, the Telegraph Superintendent at Jask kindly sent off two native boats to meet us, and on 29th November I landed at Jask. Six days later Captain Lenox Conyngham reached Bushire. Whilst the masonry pillars were being built for the telescopes, collimators and clocks, we carried out some experimental trials with the electrical apparatus, to test if our Siemens relays were sufficiently sensitive to receive signals from line, which we found they were.

We commenced observations on the Jask-Bushire arc on December 9th. As the submarine cables were only available for our use during periods of 15 minutes at intervals of one or two hours, our method of measurement was to compare the clocks when the cable was available, and to take transits locally at each station during the intervals. Clock comparisons were direct, no hand signals being employed, and the method was as follows:—The clock at the east station was put in circuit with the line-wire, and its beats were automatically transmitted to the west station for five minutes, and recorded in conjunction with the beats of the west clock on the west chronograph. The procedure was then reversed, and a similar set of signals was sent from west to east, and no clock comparison was considered complete, unless a record had been obtained at both stations.

We had a preconcerted programme of stars in order that the two observers might use the same; this method has the great advantage that no accurate knowledge of right ascensions is required. Occasionally this programme was broken into by telegraphic interruptions at clock comparison and by passing clouds, and then we each determined the errors of our clocks by transits of well-placed stars, which we selected independently of one another. Following General Campbell, on whose writings all longitude work in India has been based, we term those stars that have been taken by both observers and whose right ascensions are not required, "Longitude stars", and those which have only been used by one observer to find his clock error, "Clock stars".

On this arc the following was our nightly programme of work, which lasted about six hours:—

Determination of Collimation and Level.
First Clock Comparison.
Transits of stars for one hour.
Second Clock Comparison.
Determination of Collimation and Level.
Transits of stars for one hour.
Third Clock Comparison.
Determination of Collimation and Level.
Transits of stars for one hour.
Fourth Clock Comparison.
Determination of Collimation and Level.
Transits of stars for one hour.
Determination of Collimation and Level.
Fifth Clock Comparison.

The telescopes were reversed in their Y's in the middle of each group of transit stars.

The observation of circumpolar stars for azimuth was attended to, and as a general rule a pair was obtained during each group of transit stars. To cancel the effects of errors in azimuthal determination, our transit stars were selected half north of the zenith and half south.

The above programme we worked on six successive nights, December 9th to 14th; the weather was fine throughout, and the kind co-operation of the Telegraph Officers, Messrs. McMullen and Campbell at Bushire and Woodsell and Huntly at Jask, rendered the measurement successful. Two hundred and sixty-one transit stars had been observed at Jask, and two hundred and seventy-three at Bushire: twenty-eight comparisons of clocks had been taken. An astronomical azimuth and latitude were also observed at Jask, the latter with the zenith telescope.

Karachi-Bushire Arc.

I left Jask on December 21st and arrived at Karachi on December 24th. Our experiences with the Jask-Bushire cable led us to expect some difficulty in exchanging clock signals through the double length of cable between Karachi and Bushire and we commenced trials without delay: we very soon discovered that our Siemens relays were not sufficiently sensitive to receive signals from the distant station, and we had no alternative but to abandon them. Our friends in the Telegraph Department came to our aid and supplied us on loan with two excellent Brown-Allan relays : these answered well but they were difficult to manage and adjust, and rendered the presence of a Telegraph Officer necessary at night in our observatories. Mr. Campbell at Bushire and Mr. Wartenby at Karachi kindly volunteered to assist us, and they superintended the interchange of clock signals throughout the arc.

For the Karachi-Bushire arc the nightly programme of work was very similar to that of the former arc : owing, however, to the difference of longitude exceeding an hour, the longitude stars observed at Karachi did not transit at Bushire till 64 minutes afterwards : if we had adhered to the principle of only observing the same stars at both stations, the Bushire observer would have had an idle hour after the first clock comparison, and the Karachi observer would have had nothing to do for the hour preceding the last comparison. To avoid this a group of well-placed clock stars was observed at Bushire during the first hour and a similar group but of different clock stars was taken at Karachi during the last hour: the resulting clock errors were brought to a mean epoch by the application of corrections for rate, and a value obtained for the difference of longitude : in addition to these clock stars, four groups of longitude stars were observed nightly at each station.

We worked this programme through on five successive nights, December 28th to January 1st, two nights December 27th and January 2nd being lost through clouds. The loss of these nights was unfortunate, as it prevented us carrying out our original intention of observing the nightly programme six times through. The Government Telegraph steamer *Patrick Stewart* was to leave Bushire on January 4th, and Mr. Possmann, the Director of Persian Gulf Telegraphs, had kindly offered to take Captain Lenox Conyngham to Charbar : this offer was a great boon, and we were most anxious to avail ourselves of it : not only should we save all expense of transhipment, but the party and equipment would be comfortably landed by day-light in Charbar bay : the British India Company's steamers do not call at Charbar, and though the Company was most obliging in stopping them for our convenience off Jask and Charbar, the Commanders did not enter the bays, and we had to tranship our equipment out at sea and perhaps at night into Arab boats. If Captain Lenox Conyngham was to catch the *Patrick Stewart*, January 2nd was the last night that we could observe; and that night we had to decide whether the Bushire equipment should be dismantled early next morning, or whether we should continue the arc, and trust to one of us getting to Charbar on some future occasion. Nothing is laid down as to how many observations are the necessary minimum for an arc : the number of clock comparisons, the number of nights' work and the number of stars to be observed are left to the discretion of the observers. As a single night's work is liable to be burdened with some small constant error, our original intention had been, in accordance with the practice of our predecessors, to devote six nights' work at least to each arc, and on this arc we had so far only got five. During those five, twenty-two comparisons of clocks, lasting fifteen minutes each, had been made, and two hundred and thirteen transit stars had been observed at Karachi and two hundred and twenty-one at Bushire. Taking into consideration that the difference of longitude between Karachi and Bushire was to be measured not only directly, but also with Jask as an intermediate observing station, we decided that having got five-sixths of our full complement of work, we should not be justified in missing this good opportunity for one of us to get to Charbar for the sake of the extra sixth.

Longitude of Charbar.

Captain Lenox Conyngham left Bushire on January 4th 1895, landed his equipment at Jask on January 9th, and himself reached Charbar on January 10th. The longitude of Charbar was only required for topographical purposes and does not enter in any way into the main scheme for the determination of the longitude of Madras : to save the longitude equipment from risk of damage, it was not taken to Charbar, the only instruments landed there being a zenith telescope, an 8-inch theodolite, a tappet and a chronometer. Our method of observing the longitude was as follows:—Captain Lenox Conyngham set his zenith telescope up in the meridian, and connected his tappet through the line-wire with my chronograph at Karachi. The chronograph was started and on it the Karachi clock made to record its seconds. With my transit instrument I then observed twelve time stars at Karachi to determine the clock error, recording them on the chronograph, and when this had been done Captain Lenox Conyngham commenced observing the transits of stars at Charbar across the five vertical wires of the zenith telescope. With his tappet in his hand he could break the line current at every transit of a star across a wire, and thus record his transits on my chronograph.

When he had taken twenty stars over five wires each, I observed twelve more at Karachi. From the same record therefore we were able to get the error of the clock as determined by transits of stars across both the Karachi and Charbar meridians, and the corrected difference between the two resulting errors is the required difference of longitude. This programme was worked through on three nights : the theoretical defect of the method is that the retardation of the current remains undetermined, but this is immaterial, as Charbar is on the land-line between Karachi and Jask, two-thirds of the distance of the latter from Karachi, and the retardation of a signal between Jask and Karachi has been subsequently found to be $0^s.056$, so that the time of transmission of a signal from Charbar to Karachi can be approximately calculated to be $0^s.04$.

A British India steamer passes Charbar once a fortnight for the Persian Gulf, and one of these steamers, the *Chanda*, was timed to pass on January 17th: an astronomical latitude and azimuth had to be observed at Charbar in addition to the longitude, and it was rather a question whether all these measurements could be made in six days : if the *Chanda* was missed, Captain Lenox Conyngham would have to wait in Charbar till January 31st.

Mr. Janes, the Telegraph Officer at Charbar, had been good enough to build the astronomical pillars before Captain Lenox Conyngham's arrival, the six nights were perfectly cloudless, and the observations were completed in time. On January 15th, the *Chanda* passed Karachi and I arranged for her to stop in her course off Charbar : Captain Lenox Conyngham met her in his boat on January 17th, was taken on board, and the following day was landed at Jask.

Karachi-Jask Arc.

The clock comparisons on the Karachi-Jask arc were made with a land-line.

We commenced work on January 21st, but owing to clouds did not finish till February 2nd. Our nightly programme consisted of 5 clock comparisons, 5 determinations of collimation and level, 6 circumpolar stars for azimuth and 40 time stars, the telescopes being reversed in their Y's after every group of eight. The same stars were observed by both observers. By February 3rd, when we closed work, we had taken 39 clock comparisons and had each observed 219 time stars, 111 being north of our zeniths and 108 south.

On February 9th, Captain Lenox Conyngham reached Karachi, and on the four following nights we observed together for personal equation, taking 50 stars with each telescope. On March 14th, we sailed for England and on April 10th arrived at Liverpool.

This account of the measurement of the three Persian Gulf arcs would not, however, be complete without an acknowledgment from the observers of the cordial assistance they received from Mr. Possmann, the Director of the Persian Gulf Telegraphs, and his staff. To Messrs. McMullen, Woodsell, Huntley, Campbell, Hopkins, Janes and Wartenby, the officers of the expedition are especially indebted, not only for the generous aid rendered on every occasion, but for their kindly hospitality in receiving them into their homes on the shores of the desert.

"The fact cannot be too strongly expressed", the American observers well remark, "that without the zealous and persevering co-operation of the telegraph "officials no such undertaking as this can be successful, and no one without "absolute experience can realize the discouraging delays and difficulties which "would be almost insurmountable without the cordial assistance of the members "of the cable staff".

Potsdam-Greenwich Arc.

Having reported our arrival at the India Office, we were placed under Mr. Danvers and Mr. Rowlands, and were given letters of introduction to the

Astronomer Royal and to Mr. Preece, C.B., F.R.S., the Engineer-in-Chief of the General Post Office. Colonel O'Brien, R.E., kindly gave us permission to use the observatory of the India Store Depôt at Lambeth, and this we made our head-quarters during our whole stay in Europe. At our first interview with Mr. Preece we learnt that there was no objection to joining up a land-line and cable direct, and that we could work without translation between Greenwich and Emden; and at his suggestion we decided to cut Lowestoft out from the programme. Professor Christie, F.R.S., the Astronomer Royal, was good enough to interest himself in our work, and he advised us to substitute Berlin for Emden: both he and Mr. Preece were of opinion that on an Emden-Greenwich measurement, where the wire was partly land-line and partly cable, the retardation of the current would not be the same from Greenwich to Emden as from Emden to Greenwich, and that some inland station should be selected in place of Emden: the North Sea cable would then be interposed between two land-lines, and our connecting wire be symmetrical in its two directions. This valuable advice we accepted without hesitation, and having learnt from Mr. Preece that no translation at Lowestoft or Emden would be necessary in signalling between Greenwich and Berlin, we decided to substitute the latter for Emden.

The Astronomer Royal wrote on our behalf to Dr. Förster, Chief of the Observatory at Berlin, who, whilst cordially offering us a site, made the suggestion, that Potsdam would be found a more suitable station, owing to the want of available space at Berlin. We accordingly addressed Professor Helmert, Chief of the Royal Geodetic Institute of Prussia, who gave us ready permission to work at his observatory and promised us every assistance in his power.

The next change made in our original scheme of connection was the elimination of Odessa: this station had been included, because we had considered the distance from Emden to Teheran too great for direct signalling, and we had wished to avoid translation. But the Astronomer Royal was of opinion that the introduction of an additional station of observation was more likely to cause an error in the resulting longitude of India than the presence of a single translation at Odessa, and that we might omit this station with advantage; this we accordingly did.

The remodelled scheme, therefore, for the connection of Bushire and Greenwich included the following arcs :—

Teheran-Bushire.
Teheran-Potsdam.
Potsdam-Greenwich.

Lowestoft, Emden and Odessa had been cut out and Potsdam introduced, and the five original arcs had now been reduced to three.

The new scheme was in every way superior to the old: it was less liable to error, would occupy less time, would be less laborious and less costly.

All these modifications of the original plan, the discussions which led up to them, and the fresh negotiations and correspondence which they involved, though their results can be given in a few lines, occupied many days, and it was not till May 25th that all had been arranged. In the interim one of the transit telescopes had been sent to Messrs. Troughton and Simms to be given more focussing play; both astronomical clocks had been thoroughly cleaned by Messrs. Frodsham and Co. and our Siemens relays, which had been proved unsuitable for the work, had been replaced by two relays of the G. P. O. pattern, which we purchased from Messrs. Elliott Brothers.

By May 28th my instruments were ready at Greenwich, and on that date we began the observation of our Personal Equation. This we finished on June 1st, and Captain Lenox Conyngham proceeded to Potsdam. He was cordially welcomed by Professor Helmert and every assistance was given him: his instruments were passed by the German Customs authorities at Hamburg without question or delay, and he was fortunate at Berlin to find a careful and intelligent assistant in Herr Globisch, who remained with us for over six months, until our work in Germany was finished, and who performed all the observatory duties to our entire satisfaction.

With regard to the line-wire which was to connect our two observatories, we had at first asked permission from Mr. Andrews, the Managing Director of the Indo-European Telegraph Company, to use the Company's line from London to

Berlin, and this he had been good enough to grant us. A line-wire from Greenwich to London had been promised us by the Engineer-in-Chief of the General Post Office, and special arrangements had been made to connect these two wires at St. Martins Le Grand without translation. When, however, Potsdam came to be substituted for Berlin, it became necessary to borrow a line-wire from the German Government to connect these two cities, and telegraphic connection between our observatories thus became dependent on three sources of direction—from Potsdam to Berlin the line was under the German Government, from Berlin to London under the Indo-European Telegraph Company, and from London to Greenwich under the British Post Office. It was difficult to work a line under three separate managements, and as the two Governments owned many wires running between London and Berlin, Mr. Andrews pointed out that the inclusion of his wire seemed an unnecessary complication, and suggested that it should not be brought into use, until we were working between Teheran and Potsdam. This suggestion was acted upon, and Mr. Preece not only allowed us the use of a land-line from Greenwich to Lowestoft and of a cable from Lowestoft to Emden, but also obtained for us from the German Government the loan of a wire connecting Emden and Potsdam.

Dr. Von Stephan, the German Post Master General, and Herr Scheffler, the Chief of the German Imperial Telegraphs, ordered the necessary cells and electrical instruments to be supplied to us at Potsdam, and Herr Post-Inspector Arpurth was attached to the observatory at Potsdam to render us all possible assistance.

On June 17th observations were commenced on the Potsdam-Greenwich arc. We at first tried the Indian method of both observing the same stars, but the constant presence of clouds rendered a preconcerted programme impossible; stars visible at one end were obscured at the other, and the two observers had of necessity to work independently, each taking every well-placed star that he could. By July 1st we had secured a sufficient number of star observations and clock comparisons, and we proceeded to interchange stations. On July 11th we were again ready for work, Captain Lenox Conyngham now being at Greenwich and I at Potsdam.

The weather was very unfavourable and we were unable to complete the arc till July 26th. Thirty-five clock comparisons had by then been made, each lasting 10 to 15 minutes, 268 time stars had been observed at Potsdam and 237 at Greenwich. On July 31st we met at Greenwich and again measured our Personal Equation.

Our work at Greenwich was much facilitated by the great assistance that we received from Mr. Christie, the Astronomer Royal, and his assistants. We were not only given a site for our station on the Greenwich meridian but an excellent observatory was placed at our disposal, and we were allowed the use of the standard clock and any other instruments that we required.

Teheran-Potsdam Arc.

Captain Lenox Conyngham had now to move from Greenwich to Teheran whilst I stood fast at Potsdam, and on August 31st his instruments left Hull in a steamer of the Wilson Line bound for Odessa. Here he was met by Babu Hanuman Prasad, his former recorder in India, and by four *khalásis* trained to observatory work, who had been despatched from Dehra Dun. During his stay he was able to arrange for telegraphic connection between Teheran and Potsdam. At Odessa there is no permanent connection between the wire to Berlin and that to Teheran, and all messages are re-transmitted by hand. Captain Lenox Conyngham now arranged for direct connection without translation, but as an alternative, in the event of the failure of the direct signals, he had the translating relays all placed ready for insertion. At the advice of Herr Arpurth, the translating apparatus was made with two relays instead of the usual four, thus halving the chances of unequal retardation. "It is unfortunate", Captain Lenox Conyngham wrote to me, "that Odessa is by no means half-way between Potsdam "and Teheran; for the equalisation of retardation in the two directions this trans-"lation should have been half-way. Sukhum-Kali would have been a more "satisfactory place. Odessa, however, has many advantages: it has a large, "important telegraph office, a mechanician is available to arrange and watch "the translating relays, and any amount of battery power is to be had".

From Odessa the party proceeded by steamer to Batoum and thence by rail to Baku, and from Baku a Russian steamer took them to Enzeli. Here, when they were landing, the steamer was rolling badly and the passing down of the heavy instruments to the little Persian rowing boats was a precarious proceeding: twice the collimator disappeared under the waves of the Caspian, but nothing was actually lost. Resht was easily reached from Enzeli on the calm waters of the Murdab, but the road from Resht to Teheran was very difficult. Some of the loads were unavoidably heavy for the mules, and the mule-drivers were untrustworthy. Captain Lenox Conyngham saw each load himself tied on every morning, accompanied the loaded mules throughout the march, and did not leave them till they were unloaded and disposed of for the night.

At Teheran Captain Lenox Conyngham selected a site for his observatory in an open space used for exercising horses in the grounds of the British Legation, where he was allowed by Sir Mortimer Durand to erect his pillars: his clock and chronograph he set up in a small room in a disused bowling alley to the north of his station. No suitable place for an observatory could be found near the telegraph office, which was in the heart of the city, and a temporary connecting wire had to be erected. Colonel Wells, R.E., Director of the Persian Section of the Indo-European Telegraph Department, arranged to have this done, and Mr. Meyer, the mechanician in charge of the telegraph office, undertook to remain in the office during the hours of observation to superintend the arrangements and see that the special observatory wire was joined to the main line at the proper time. When all was ready, we endeavoured to exchange direct signals between Potsdam and Teheran, but except now and then when a few signals would pass, we were unsuccessful. After two nights of failure we were prepared to order the insertion of translation at Odessa, but at the urgent representations of Herr Arpurth, who declined to be beaten, we determined to try again. On Sunday when wires in Europe were silent and no trouble was to be apprehended from induced currents, we sat up all night—Captain Lenox Conyngham with Mr. Meyer at Teheran, and I with Herr Arpurth at the Berlin Central Office—direct signals were got through, but they were not sufficiently reliable for clock comparison. Herr Arpurth urged further experiments and promised success, but we were hard pressed for time, and on October 31st with great reluctance we decided to accept translation at Odessa. Direct signalling might have been possible, if there had been more battery power at Teheran: as it was, there were only 200 Bichromate cells available, and though I could have obtained the loan of any number at Potsdam, I thought it undesirable to have my battery of a greater strength than that of my colleague.

On October 31st we began work and obtained good observations on October 31st, November 1st, 4th and 6th: on November 3rd, 5th and 8th several stars were observed at Potsdam, but their results were useless, as clouds prevented work at Teheran. On November 2nd and 7th many observations were made at Teheran, but Potsdam was overclouded. On November 9th floods in the Caucasus carried away our telegraph line and twelve days were occupied in restoring it, during which no telegraphic connection existed between Berlin and Teheran, and no longitude work was possible.

On the first night that we had observed, although Odessa had been in translation, our clock signals were frequently obliterated by induced currents. To obviate this the German Telegraph authorities with characteristic liberality were good enough to grant us the immense concession, that all telegraph traffic on the thirty-two wires running parallel to our wire between Berlin, Warsaw and Odessa should be stopped, whilst we were comparing our clocks. Even then the traffic on the Indo-European Telegraph Company's second wire, between Odessa and Teheran, would frequently cause induction currents on our wire, and stop our clock comparisons, and I was compelled to again address Mr. Andrews and to beg the further concession, that he would not only allow us to use one of his wires, but that he would order the other to be kept silent during our exchange of signals. Mr. Andrews granted this request at once, and sent telegraphic directions to his staff at Odessa and Teheran. Clock comparisons were then at last rendered possible, though owing to the great length of the line-wire and to the numbers of intermediate stations, which required warning, they were by no means easy for Herr Arpurth to arrange. Herr Arpurth began by working from my observatory at Potsdam, but finding it impossible there to control all the lines and stations and to deal with the numerous telegraphic interruptions, he decided to direct from the great Central Office at Berlin, where he was connected with the Potsdam Observatory by telephone. At first we had decided to take two clock comparisons per night, but when we saw the amount of time and trouble that was necessary to get all the intermediate stations to go direct, and the great delays to traffic caused by the stoppage of so many wires in the midst of populous Europe, we thought it incumbent on us to take as little advantage as possible of the generous concessions extended to us and to cut down our telegraphic communication to a minimum, and we therefore eventually exchanged our clock signals but once in a night.

On this arc the nightly programme of work for each observer was made as simple as possible: it consisted of one clock comparison at a fixed time, and 40 time stars, 20 to be observed before the comparison and 20 after. Ten to sixteen circumpolar stars were to be taken for azimuth and 3 to 5 determinations to be made of the collimation and level errors: in the selection of stars the observers were to be independent of each other. Though it was our original intention to observe the time stars half before and half after the comparison of clocks, the constant presence of clouds upset such plans, and we used to take all well-placed stars that were visible.

By November 21st the telegraph line was restored, but owing to clouds either at Potsdam or Teheran, we could not recommence work till November 24th. On November 25th and 28th we obtained good work, but after that we were stopped by snow storms, rain and clouds, sometimes at one end, sometimes at the other, frequently at both. The nights, on which good observations were taken, were far apart, but every night for seven weeks without one exception we were both present in our observatories.

On December 6th we reached the climax of misfortune: out of the preceding nine nights we had been able to work on but one. On December 6th about 10 P.M. Berlin time, Herr Arpurth was able to inform Teheran that the sky was cloudless at Potsdam, and an hour later he received a message from Teheran —"weather perfect." We set to work with a will, and when the time arrived for clock comparison, we had both observed some 30 stars. Teheran being the easterly station had to send clock to Potsdam first, and I at once saw by the character of the signals, that all was not right on the line: I thought at first that some intermediate station must be cutting in, but Herr Arpurth telephoned me from Berlin, that there was a great storm on the line near Warsaw, and the line-wires were in contact. All of a sudden this contact presumably ceased, for the signals of the Teheran clock arrived sharp and clear for the space of 40 seconds. Before, however, I could send my clock signals to Teheran, the contact must have recommenced: Captain Lenox Conyngham received none, and a first-rate night's work was lost through the want of half a clock comparison.

On December 15th we decided to close the arc: we had spent nearly two months on it, but had only got ten nights of really good work. Twelve clock comparisons had been made, 274 time stars had been observed at Potsdam and 291 at Teheran. These numbers do not include those stars observed at either place on nights when the other was overclouded, but are the actual numbers that can be utilised in deducing the resulting longitude. On an average six circumpolars were observed every night at Potsdam and eight at Teheran.

On December 16th I commenced dismantling the instruments, and on December 18th despatched them to Hamburg to be shipped to London. They arrived in London on January 2nd, and on January 4th I put them on board the British India S. S. *Thorndale*, bound for Karachi. As the steamer was starting, an accident happened to her engines in the Thames, and she had to put back into dock. I then took the instruments to Liverpool and caught the *Branksome Hall* by one day.

The *Branksome Hall* arrived at Karachi on February 12th. On February 17th I put the instruments on board the S. S. *Pemba*, and the following day sailed for Bushire.

I cannot conclude this account of our work in Germany without saying how indebted we both feel to Professor Helmert. Not only for the cordial welcome that he extended to us and for the kind permission that he gave us to live in the rooms of the Institute, but also for his valuable professional assistance in providing a site for our station, and in placing at our disposal his observatory, his clocks and his instruments. Herr Schnauder, his assistant, voluntarily sat up with me till 2 or 3 A.M. every night, and superintended for me all the telegraphic communications with Berlin. He is an officer of high scientific attainments, thoroughly versed in geodetical work, and I gained much experience through our association; his nightly presence in the observatory enhanced the value of my work, and was no small factor in ensuring the success of the Teheran-Potsdam arc.

I have already had occasion to mention the generous concessions that were granted us by the German Telegraph authorities and the Managing Director of the Indo-European Telegraph Company, but it remains for me to add that Herr Post-Inspector Arpurth, the officer deputed to superintend our telegraphic arrangements, took an enthusiastic interest in our work and carried out many experiments for us at no small inconvenience to himself. From his intimate knowledge of scientific telegraphy he was able to point out to us several precautions, that we should otherwise have omitted to take, to equalise the retardation of the electric current in its two directions.

Triangulation at Teheran.

Whilst I was moving from Germany to the Persian Gulf, Captain Lenox Conyngham had to stand fast at Teheran; during these weeks he occupied himself in connecting his longitude station with that of General Stebnitzki's, and in fixing the position of his observatory with reference to conspicuous points. Dr. Auwers in Berlin gave Captain Lenox Conyngham many particulars about Stebnitzki's operations, which had been executed in 1874 in connection with the Transit of Venus, but the actual identification of the places would have been very difficult, had it not been for the friendly co-operation of General Schindler at Teheran. This officer was well acquainted with Stebnitzki's work and was able to point out the place from which he had observed. Unfortunately in the last twenty years the town had changed much: houses had been demolished, others built, and new trees had grown, so that points which were conspicuous enough in 1874 were now no longer visible.

The points given by General Stebnitzki are :—

(1) The Kiosk near the house of the Sipah Salar Muhammad Khán.
(2) The centre of the instrument room of the Telegraph Office.
(3) The western tower of the Darwazeh Nassrieh.
(4) The central staircase of the Russian Legation.

Of these No. (2) now forms the women's quarters in the house of a Persian nobleman and was thus unapproachable even in the interests of science. The description of No. (3) was wanting in exactitude, as there were several small minarets on this gate, and it was uncertain which was referred to. No. (4) was situated in the heart of the town at a long distance from Captain Lenox Conyngham's longitude station, and owing to the absence of suitable conspicuous points their connection by triangulation was impracticable. Fortunately, with the kind assistance of General Schindler, Captain Lenox Conyngham was able to identify No. (1) and the position of Stebnitzki's observatory.

The high walls surrounding the houses in Teheran rendered triangulation difficult, but Captain Lenox Conyngham found four accessible points, which were visible from both his station and Stebnitzki's, and he was thus able to make a satisfactory connection. With the object of fixing the position of his observatory with reference to surrounding points and thus facilitating its identification in the future, Captain Lenox Conyngham measured a base along the ramparts on the north side of the town, and at each end of it measured the angles between his longitude station and several conspicuous objects. He then observed the azimuth of his base, and thus possessed sufficient data to enable him to calculate the distances from his station, in both latitude and longitude, of all the points that he had fixed.

Teheran-Bushire Arc.

On February 27th I arrived off Bushire in the S. S. *Pemba*, and was met by two sailing boats, which Mr. McMullen, the Telegraph Superintendent, had kindly engaged. Unfortunately, the sea was rough and the large astronomical clock received a jar when being lowered into a boat.

Mr. McMullen gave me a good room for my chronograph and we commenced work on March 4th. By comparisons with the chronometer the clock at first showed a variable rate, but after it had been taken to pieces and cleaned of the minute glass fragments from its broken face, it went very well; one result of its jar, I was, however, unable to rectify, and that was that the act of winding caused it to temporarily stop. It is an eight-day clock, and this stoppage thus occurred but once during the arc, and on this occasion, in order to measure the exact loss of time entailed, we arranged that the act of winding should be performed during a clock comparison.

On March 16th the arc was satisfactorily completed. We had by that date obtained eight full nights' work and had made 25 clock comparisons; 231 time stars equally distributed north and south of the zenith had been observed at each station, 54 circumpolar stars had been taken for azimuth at Bushire and 62 at Teheran. For the measurement of this arc Colonel Wells, R.E., the Director of Persian Telegraphs, placed one of his line-wires at our disposal, arranged for our direct communication without translation, and gave orders that we should be assisted in every way. Captain Lenox Conyngham was much indebted, during the measurement of the Teheran arcs, to Mr. Nelson, the Superintendent of Indo-European Telegraph Company's Line from Teheran to Odessa, and to Mr. Meyer, to whom allusion has already been made. The hours of observation on the Teheran-Potsdam arc were very unseasonable, for the use of the line-wire could only be obtained after 10-30 P.M. Greenwich Time, which is equivalent to 2 A.M. Teheran Time; but in spite of this Mr. Meyer was always punctually at his post in the telegraph office and did all in his power to promote the success of the work.

Captain Lenox Conyngham left Teheran immediately after completing this arc and returned to India *viâ* Resht, Baku, Batoum and Port Said. The two observers met at Dehra on May 18th, and there on four successive nights observed a final value of personal equation. This closed the work of the expedition and completed our determination of the longitude of Madras.

It was estimated that the cost of the expedition, excluding the salaries of the observers, but including all their travelling and contingent expenses, the transport of the instruments and the pay of the temporary assistants in Europe, would amount to Rs. 49,328 (£ 3,083), but the actual cost proved to be Rs. 36,404 (£ 2,275).

The chronograph sheets were to a certain extent read off contemporaneously with the observations to insure against the risk of loss, which was not improbable in moving to and from such places as Teheran, Bushire and Jask. The reading was completed during the summer of 1896, and the computations are now all ready to be printed and published.

PART II.

RESULTS OF THE OBSERVATIONS.

The resulting values of our six arcs of longitude are as follows :—

Arc.	Observed Difference of Longitude.	Probable Error.
Karachi-Jask	$0^h\ 36^m\ 59^s.697$	$\pm\ 0^s.0057$
Jask-Bushire	0 27 45.057	± 0.0095
Karachi-Bushire	1 4 44.812	± 0.0097
Teheran-Bushire	0 2 21.438	± 0.0094
Teheran-Potsdam	2 33 24.223	± 0.0080
Potsdam-Greenwich	0 52 15.929	± 0.0060

The first three of these arcs form a circuit, and its closing error may be deduced thus :—

Karachi-Jask		$0^h\ 36^m\ 59^s.697$
Jask-Bushire		0 27 45.057
	Sum .	1 4 44.754
Karachi-Bushire		1 4 44.812
	Closing error .	0.058

The value to be finally adopted for the difference of longitude between Karachi and Bushire is obtained as follows :—

Karachi-Jask	$0^h\ 36^m\ 59^s.697$	$\pm 0^s.0057$
Jask-Bushire	0 27 45.057	±0.0095
Karachi-Bushire (indirect) . . .	1 4 44.754	±0.0110
Karachi-Bushire (direct) . . .	1 4 44.812	±0.0097
Mean value, regard being paid to weights	1 4 44.787	±0.0073

The longitude of Madras can now be calculated, the values of the three arcs connecting Madras and Karachi being taken from pages 440 and 441 of Volume XV, G. T. Survey of India.

Difference of Longitude.		Probable Error.	Longitude East of Greenwich.		Probable Error.
Potsdam-Greenwich	$0^h\ 52^m\ 15^s.929$	$\pm\ 0^s.0060$	Potsdam . .	$0^h\ 52^m\ 15^s.929$	$\pm\ 0^s.0060$
Teheran-Potsdam	2 33 24.223	± 0.0080	Teheran . .	3 25 40.152	± 0.0100
Teheran-Bushire	0 2 21.438	± 0.0094	Bushire . .	3 23 18.714	± 0.0137
Karachi-Bushire	1 4 44.787	± 0.0073	Karachi . .	4 28 3.501	± 0.0155
Bombay-Karachi	0 23 12.196	± 0.0129	Bombay . .	4 51 15 697	± 0.0202
Bolarum-Bombay	0 22 48.801	± 0 0061	Bolarum . .	5 14 4.498	± 0.0211
Madras-Bolarum	0 6 54.615	± 0.0085	Madras . .	5 20 59 113	± 0.0227

The error in the value of longitude adopted for the triangulation of the Great Trigonometrical Survey of India is thus shown to be $+2'\ 27''.54$.

In calculating the probable error of an arc, I first find x_1=the probable error of the mean value of the arc without the application of the personal equation: then x_2=the probable error of the personal equation : and x_0=adopted value for the probable error of the arc$=\sqrt{x_1^2+x_2^2}$. This method renders the probable error of the resulting longitude of Madras too large for two reasons : *firstly*, no allowance has been made for the tendency of errors in personal equation to cancel on double arcs such as Bushire-Teheran-Potsdam, when one observer moves from Bushire to Potsdam, the other remaining at the intermediate station: and, *secondly*, no allowance has been made for the fact, that the three arcs, connecting Karachi and Madras, have not only been measured direct, but enter into many verificatory circuits, all of which show closing errors smaller than $0^s.10$—*vide* page 433, Vol. XV, G. T. Survey of India.

The details of all observations and results will be published hereafter and cannot be included in this preliminary account, but as the accuracy of our Potsdam-Greenwich arc may possibly be questioned for reasons given in Part III of this Report, I have considered it advisable to show here the several observed values from which the final mean was derived. Each value in this table is dependent on a separate clock comparison and separate star observations :—

OBSERVED DIFFERENCE OF LONGITUDE BETWEEN POTSDAM AND GREENWICH.

Date.	Lenox Conyngham at Potsdam and Burrard at Greenwich.	Date.	Burrard at Potsdam and Lenox Conyngham at Greenwich.
1895.		1895.	
June 21 .	$0^h\ 52^m\ 15^s.547$	July 12 .	$0^h\ 52^m\ 16^s.241$
" 22 .	15.592	" 12 .	16.263
" 22 .	15.591	" 12 .	16.267
" 22 .	15.545	" 15 .	16.279
" 27 .	15.624	" 15 .	16.282
" 27 .	15.653	" 15 .	16.358
" 27 .	15.686	" 19 .	16.277
" 29 .	15.556	" 19 .	16.254
" 29 .	15.593	" 19 .	16.282
July 1 .	15.508	" 21 .	16.246
" 1 .	15.544	" 21 .	16.250
		" 22 .	16.274
		" 22 .	16.262
		" 22 .	16.281
Mean .	$0^h\ 52^m\ 15^s.585$	Mean .	$0^h\ 52^m\ 16^s.273$
	Probable error ± 0.0108		Probable error ± 0.0051

The mean value of the arc is $0^h\ 52^m\ 15^s.929$ with a probable error of $\pm 0^s.0060$.

Personal Equation.

It has always been the Indian practice to introduce into each arc a correction for personal equation deduced from observations made both previously and subsequently to the measurement, and our own experiences had led us to believe that these corrections were reliable. Between 1889 and 1893 I had measured my personal equation with Captain Lenox Conyngham on seven different occasions: the results are given in Vol. XV, G. T. Survey of India, from which this table has been abstracted :—

Place.	Date.	No. of Stars.	Observed Value of Personal Equation (B-C.).
Dehra Dun .	October 1889 .	86	—0.230
Agra .	December 1889 .	98	—0.253
Mooltan .	March 1890 .	56	—0.233
Karachi .	April 1890 .	48	—0.249
Karachi .	November 1891 .	48	—0.210
Bombay .	February 1892 .	96	—0.254
Dehra Dun .	April 1892 .	69	—0.270

During the present expedition we observed for personal equation on five different occasions, with the following results :—

Place.	Date.	No. of Stars.	Observed Value of Personal Equation (B-C.)
Dehra Dun .	October 1894 .	89	$-0^s.222$
Karachi .	February 1895 .	98	-0.269
Greenwich .	May 1895 .	65	-0.318
Greenwich .	August 1895 .	12	-0.273
Dehra Dun .	May 1896 . .	71	-0.266

It will be seen that at Greenwich in May 1895, the observed value is exceptionally large.

This may be accidental and due to bad observing, or to actual variation, but perhaps the following explanatory suggestion may be offered. Personal equation is probably an expression of the form ($C_1 \pm C_2$ sec. δ), where C_1 is a constant representing the time taken for an eye-impression to actuate the hand, and C_2 is the constant equatorial interval between a transit wire and the point, at which an observer estimates a star's bisection: C_1 is an interval of time, C_2 of space. The time taken by a star to traverse the space C_2 will increase with the declination and will be C_2 sec. δ. Now the mean declination of the stars observed for personal equation at Greenwich in May 1895 is 15° larger than in the case of any personal equation measured in India. In August 1895 we decreased this mean declination at Greenwich, but the August value of personal equation deserves less weight than that of May, owing to its being based on but 12 stars, which were all that the clouds allowed us to take in the six nights which we devoted to the determination.

An independent value of personal equation may be deduced from the interchange of observers on the Potsdam-Greenwich arc :—

Lenox Conyngham at Potsdam . . . . . . Burrard at Greenwich . . . . . . }	$0^h\ 52^m\ 15^s.585$
Burrard at Potsdam . . . . . . Lenox Conyngham at Greenwich . . . }	$0\ 52\ 16.273$
Difference	$-0^s.688$
Personal equation = half the difference	-0.344
Personal equation as directly observed at Greenwich . .	-0.318
Discrepancy . . .	-0.026

At Karachi we measured our personal equation with General Campbell's idiometer, and the results agreed well with those of the star observations.

In a paper entitled "A peculiarity of Personal Equation", published in Volume XXXVII of the Monthly Notices of the Royal Astronomical Society, General Campbell pointed out that his personal equation with Colonel Heaviside depended on whether the star crossed the field of the telescope from right to left or from left to right, and we have consequently always considered it advisable to keep the values of personal equation derived from north and south stars separate. This distinction was, however, only feasible when the numbers of time stars observed on an arc were equally distributed north and south, and it had to be abandoned on our two European arcs.

Though we made many direct determinations of personal equation, the resulting longitude of Madras, it should be noted, is almost independent of them. The arcs connecting Madras and Karachi may be considered free from errors of personal equation, these two places having been connected in five different ways, *viâ* Bolarum, Bellary, Jubbulpore, Agra and Calcutta, and by different pairs of observers. On the arc Potsdam-Greenwich, the effect of these errors was cancelled by the interchange of observers, and on the two Teheran arcs, the same observer remained at Teheran throughout, an arrangement that should protect the deduced difference of longitude between Potsdam and Bushire. There only remains the Karachi-Bushire difference, and of this there was made, as I explained before, both a direct and indirect measurement. Owing to this duplication we considered an alternate arrangement of observers unnecessary, and allowed our plans to be governed solely by considerations of economy: in a circuit consisting of but three arcs, no method of posting will cancel errors arising from differences of personal habit, but the smallness of the closing error will sufficiently demonstrate the accuracy of the correction adopted.

Result of the Teheran Triangulation.

In the report by Major-General Stebnitzki on "The geographical position of the city of Teheran", which was published in the 36th volume of the Records of the Topographical Department of the Russian General Staff, the actual point in Teheran finally fixed is not the General's observing station, which was identified by Captain Lenox Conyngham near the house of the Sipah Salar Muhammad Khân, but the central point of the instrument room of the former office of the Indo-European Telegraph Company. The latter point was probably selected in preference to the former as being the more durable. To avoid confusion, I think it advisable to state that when I mention "Stebnitzki's station", I invariably mean the centre of this telegraph office.

The centre of this office is stated by General Stebnitzki to be $0^s.942$ west of his observing station at the Sipah Salar's house.

From the triangulation executed by Captain Lenox Conyngham, it has been calculated that our new longitude station at Teheran is $2^s.005$ west of the observing station at the Sipah Salar's house.

We are therefore now by great good fortune in possession of the valuable knowledge, so essential to the establishment of a connection between the German work and ours, that

LENOX CONYNGHAM'S STATION IS $1^s.063$ WEST OF STEBNITZKI'S.

Retardation.

The American observers well define retardation as "the wave and armature time" or "the time required for the electric impulse to pass through the conductor and to overcome the inertia of the receiving instrument".

The retardation of the electric current as observed from clock comparisons is shown in the following table:—

Arc.	Length of wire in miles.		No. of Translations.	Retardation of the Current in one direction.
	Land-line.	Cable.		
Karachi-Jask	601	—	0	$0^s.056$
Jask-Bushire	—	598	0	0 .146
Karachi-Bushire	—	1,193	0	0 .243
Teheran-Bushire	655	—	0	0 .058
Teheran-Potsdam	2,626	2	1	0 .151
Potsdam-Greenwich . . .	441	280	0	0 .148

In connection with the retardation, the following facts are perhaps worth noting:—

(1) The first three arcs were measured under similar conditions with the one exception that we used a land-line on Karachi-Jask and a submarine cable on the others. The probable error of the land-line arc is $\pm 0^s.0057$ and of the two cable arcs $\pm 0^s.0095$ and $\pm 0^s.0097$ respectively.

(2) The retardation of the current between Karachi and Jask was measured under three different conditions with the following results:—

By land-line with no intermediate translation .	$0^s.056$
By land-line with translation at Ormara . .	0 .109
By submarine cable	0 .151

(3) It is peculiar that the retardation between Teheran and Potsdam should have been only $0^s.151$, the more particularly as there was a translation at Odessa. It is perhaps possible that this low value denotes faulty insulation of the line-wire, which would explain our difficulties in exchanging signals. General Stebnitzki in 1874 found the retardation between Teheran and Berlin to be $0^s.408$, but he does not mention how many translating stations were interposed.

As we have to be content in longitude observations with assuming, that the retardation affects signals equally in both directions, and that its effect is thus eliminated, it is incumbent on observers to keep the wave and armature time as small as possible and to avoid every source of increase. It was for this reason that we insisted on working with but one translation between Teheran and Potsdam, though the introduction of a second and third would doubtless have facilitated communication. "The retardation", writes General Campbell, "may be affected by the *direction* of the signal relatively to the magnetic poles, "or by the rotatory motion of the Earth, or by the direction of existing Earth "currents, or above all by the nature of the current used, whether by zinc or "copper to Earth at sending station".

Throughout the present longitude operations attention has been paid to the equalisation of resistances and currents, but considerations of expense rendered systematic exchange of relays and electrical apparatus impracticable. Between Karachi and Bushire the direct measurement was made with Brown-Allan relays and with a submarine cable, whilst on the duplicate determination, *viâ* Jask, Siemens relays were employed and the wire was partly land-line and partly submarine. The agreement of the results, notwithstanding these differences of condition, demonstrates the absence of constant error. On the two Teheran arcs one set of electrical apparatus and relays remained at Teheran throughout, the other being moved round from Potsdam to Bushire, and it is hoped that inherent differences of retardation may have had opposite effects on the two arcs.

On the Potsdam-Greenwich arc the receiving relay at Greenwich was repeatedly altered, and the line-wire connecting our observatories seldom ran by the same route any two nights in succession, changes that were accompanied by no discordance of result.

Summary of Results.

Whether or not it is advisable to introduce a change into the longitude of the Indian Survey is a point not now under consideration. But the question may be fairly raised as to whether this new determination is of *sufficient* accuracy, or whether in Everest's words it furnishes "sufficiently conclusive data" to warrant an alteration, in the event of such being pronounced expedient. Will not instrumental improvements and other inventions be likely to necessitate another longitude re-measurement perhaps in the near future?

The reply to this is that the chief sources of error in astronomical work are not nowadays to be sought for in instrumental defects, but rather in the presence of local attraction, in abnormal refraction from adverse meteorological conditions, and above all in the frailty of human observers. As long as gravity and the upper atmospheric strata remain beyond our control, and as long as observers are affected by climate, ill-health, emotions or fatigue, so long will it be impossible to greatly increase the degree of accuracy that has been already attained in star-observations.

With no limit to time and no limit to expense, with more favourable weather and less frail observers, the recent determination could perhaps be *slightly* improved upon; but if future inventions ever greatly enhance the possible accuracy of scientific surveying, the triangulation of the Great Trigonometrical Survey and the last measurement of the Madras longitude will be rendered obsolete together.

The probable error of our final value of longitude, excluding the uncontrollable effects of gravity, is only $0''.33$, and with this result no triangulation, however refined, will be able to compete at the end of seven thousand miles, until a knowledge has been gained of the size and shape of the earth, more intimate than at present seems attainable.

PART III.

COMPARISON WITH FOREIGN VALUES.

The longitude of Madras has now been telegraphically determined by five independent series of observations: the following table and the accompanying Chart II show the several lines of operation:—

	Colored in Chart II.	Route.	Date.	Observers.
Series A .	Yellow and brown.	Greenwich, Moscow, Vladivostok, Hongkong, Singapore, Madras.	1874-75 .	Generals Scharnhorst and Kuhlberg from Moscow to Vladivostok. Commanders Green, Davis and Norris of the U. S. Navy from Vladivostok to Madras.
Series B .	Blue . .	Greenwich, Berlin, Teheran, Karachi, Madras.	1874 .	Dr. Knorre at Berlin. Colonel Stebnitzki at Teheran. Dr Becker at Isfahan. General Addison at Karachi. Mr. Pogson at Madras.
Series C .	Red . .	Greenwich, Potsdam, Teheran, Bushire, Karachi, Madras.	1894-96 .	Captains Burrard and Lenox Conyngham.
Series D .	Blue and red.	Greenwich, Berlin, Malta, Suez, Aden, Madras.	1874-77 .	Drs. Auwers, Knorre, Löw and Mr. Gill from Berlin to Aden Colonels Campbell and Heaviside from Aden to Madras.
Series E .	Green and red.	Greenwich, Mokattam, Suez, Aden, Madras.	1874-77 .	Mr. Criswick at Greenwich. Captain Ord Browne at Mokattam. Mr. Hunter at Suez. Colonels Campbell and Heaviside from Suez to Madras.

Before comparing the final results of the different series, it may be as well to see to what extent our own recent determinations agree with the values of longitude previously adopted for Potsdam and Teheran.

Firstly to take the case of Potsdam.

Arc.	Difference of Longitude.	Probable Error.	Authority.
Berlin-Greenwich	$0^h\ 53^m\ 34^s.910$	$\pm 0^s.010$	Page 219, Verhandlungen der Internation. Erdmessung 1893.
Berlin-Potsdam	0 1 18.721	±0.005	Astronomisch-Geodätische Arbeiten. Berlin 1895.
Difference . .	0 52 16.189	±0.011	

Our direct measurement of Potsdam-Greenwich, given in Part II, is $0^h\ 52^m\ 15^s.929$, or $0^s.260$ less than the value derived above.

The closing error of the Berlin-Potsdam-Greenwich circuit is thus $0^s.260$, and this error, which I cannot but regard as unexpectedly large, has to be distributed among the three arcs composing the circuit. Of these the arc Berlin-Potsdam has been measured twice recently with every refinement, its two independent results differing by only $0^s.02$; and whilst the accepted difference of longitude between the two great European observatories of Berlin and Greenwich is not likely to be in error by a quarter of a second of time, it is difficult for me to admit even the possibility of the existence of an error of this magnitude in our own work. The task of distribution will therefore not prove light.

Teheran results may be compared as follows, Dr. Auwers' modification being accepted of General Stebnitzki's arc, which was given in the original report as $2^h\ 32^m\ 6^s.59$:—

Arc.	Difference of Longitude.	Probable Error.	Authority.
Teheran-Berlin	$2^h\ 32^m\ 6^s.610$	$\pm 0^s.11$	Die Venus-Durchgänge 1874 and 1882 by Dr. Auwers, 1896.
Berlin-Potsdam	0 1 18.721	± 0.005	Astronomisch-Geodätische Arbeiten. Berlin 1895.
Deduct distance between Stebnitzki's and Lenox Conyngham's stations at Teheran.	0 0 1.063	...	Part II of this Report.
Teheran-Potsdam . .	2 33 24.268	± 0.11	

Our recent direct measurement of Teheran-Potsdam, given in Part II, is $2^h\ 33^m\ 24^s.223$, and thus differs by only $0^s.045$ from the value derived above.

Series A.

The following table contains the values of the arcs of longitude measured across Siberia and down the Chinese coast:—

Arc.	Difference of Longitude.	Probable Error.	Authority.
Pulkowa-Greenwich	$2^h\ 1^m\ 18^s.63$	$\pm 0^s.008$	Astronomische Nachrichten No. 3202, Vol. 134.
Moscow-Pulkowa	0 28 58.45	0 .010	General Scharnhorst's Report, Vol. 37. Records of the Russian General Staff.
Kazan-Moscow	0 46 11.97	0 .030	
Ekaterinburg-Kazan	0 45 59.64	0 .043	
Omsk-Ekaterinburg	0 51 1.51	0 .043	
Tomsk-Omsk	0 46 18.61	0 .043	
Kansk-Tomsk	0 43 1.19	0 .043	
Irkutsk-Kansk	0 34 18.09	0 .043	
Chita-Irkutsk	0 36 52.07	0 .043	
Stritensk-Chita	0 16 45.54	0 .043	
Albazin-Stritensk	0 25 31.67	0 .043	
Blagovyeshchensk-Albazin . . .	0 13 45 99	0 .043	
Khabarovka-Blagovyeshchensk . .	0 30 10.96	0 .043	
Sum . .	9 0 14.32	...	
Khabarovka-Vladivostok	0 12 43.04	0 .043	
Vladivostok-Shanghai	0 41 34.82	0 .023	Determinations of longitude, East Indies, China, Japan. By officers of the U. S. Navy.
Shanghai-Hongkong	0 29 17.28	0 .029	
Hongkong-Cape St. James . . .	0 28 21.43	0 .021	
Cape St. James-Singapore . . .	0 12 52.42	0 .021	
Singapore-Madras	1 34 25.58	0 .019	
Sum . .	3 39 14.57	...	
Longitude of Madras = Difference of Sums	5 20 59.75	± 0.155	

The difference of longitude between Singapore and Madras was also measured by Dr. Oudemans and Mr. Pogson, but without any determination of personal equation. An account is given in "Telegraphic Determinations of Longitude", by Norman Pogson, and in the Astronom. Nachrichten, No. 2486 of 1883. In the "Triangulation Von Java" Dr. Oudemans himself in discussing the longitude of Batavia accepts the American value without modification.

Series B.

An account, written by General Addison, of Series B is to be found in Vol. XXXVIII of the Monthly Notices of the Royal Astronomical Society, and the resulting longitude of Madras is there given as $5^h\ 20^m\ 59^s\ 65$. But in "Die Venus-Durchgänge 1874 and 1882", published at Berlin in 1896, Dr. Auwers discusses this series of longitudes, and considerably modifies General Addison's results, decreasing the value of every arc. The following table shows Dr. Auwers' values :—

Arc.	Difference of Longitude.	Probable Error.	Authority.
Berlin-Greenwich	$0^h\ 53^m\ 34^s.91$	$\pm 0^s.010$	Die Venus-Durchgange 1874 and 1882, Berlin 1896
Teheran-Berlin	2 32 6.61	± 0.048 (Teheran-Berlin and Isfahan-Teheran)	Die Venus-Durchgange 1874 and 1882, Berlin 1896
Isfahan-Teheran	0 0 58.70		Die Venus-Durchgange 1874 and 1882, Berlin 1896
Karachi-Isfahan	1 1 12.94	± 0.11	Die Venus-Durchgange 1874 and 1882, Berlin 1896
Madras-Karachi	0 53 5 85	± 0.11	Pogson's "Telegraphic Longitudes".
Sum = Longitude of Madras . .	5 20 59 01	± 0.163	

It is difficult to obtain satisfactory values of the probable errors of this series owing to a serious mistake made in the observations at Karachi, and I have consequently no confidence in the final probable error that I have deduced for this value of the longitude of Madras. A constant error of about 10 seconds of time runs throughout the observations at Karachi. Mr. Pogson writes "an error of *exactly* ten seconds in General Addison's time determi- "nation is the only possible explanation", but Dr. Auwers states the error to be "nearly eleven seconds". General Addison, the observer, attributes it to his neglect to obtain the pivot error of his transit instrument, and is of opinion, though the resulting longitude of Karachi may be wrong, that "the difference "between Isfahan and Madras remains unaffected, the errors on the east and "west sides of Karachi correcting each other". Mr. Pogson considers that "no "conceivable pivot correction nor any other instrumental error could account for "so large a difference", but on the supposition that the observer mistook the time by exactly ten seconds, he agrees with General Addison that the effect would be reversed east and west of Karachi and "would therefore be eliminated in the "final difference between Greenwich and Madras". Both Dr. Auwers and Dr. Oudemans, the Surveyor General of Java, adopt the assumption, that the large error, whatever may be its cause, is cancelled, and they accept General Addison's observations.

General Addison's error is, however, not the only weak point in Series B, for no determination was made of personal equation between the Madras and Isfahan observers, an omission that cannot be satisfactorily allowed for in calculating probable errors.

Series C.

The results of this series are given in Part II of this Report. The value obtained for the longitude of Madras is—

$5^h\ 20^m\ 59^s.113$ with a probable error of $\pm\ 0^s.0227$.

Series D.

This series is discussed by Dr. Copeland in Vol. III, Chapter XXIII, of the Dunecht Observatory Publications, and by Dr. Auwers in a paper on "The Longitude of Aden", published in the Astronom. Nachrichten, No. 3180, dated August 15th, 1893. Dr. Auwers' discussion is reprinted in "Die Venus-Durchgänge 1874 and 1882", with the slight modification, that the difference between Alexandria and Berlin is decreased by $0^s.025$. His later value has been employed in the following table :—

Arc.	Difference of Longitude.	
	By Dr. Copeland.	By Dr. Auwers.
Berlin-Greenwich	0^h 53^m $34^s.865$	0^h 53^m $34^s.910$
Alexandria-Malta-Berlin	1 5 58.942	1 5 58.866
Suez-Alexandria	0 10 38 923	0 10 39.266
Aden (Gill's station)-Suez	0 49 43.750	0 49 43.630
Longitude of Gill's station at Aden . . .	2 59 56.480	2 59 56.672

The difference of longitude between Gill's station at Aden and the Madras Observatory can be found as follows :—

Gill's point at Aden is $0^s.877$ east of Campbell's. Then from pages 410, 440 and 441 of Vol. XV, G. T. Survey of India

Madras-Bellary	0^h 13^m $16^s.545$
Bellary-Bombay	0 16 26.871
Bombay-Aden (Campbell's station)	1 51 20.022
Sum	2 21 3.438
Deduct difference between Campbell's and Gill's stations	— 0.877
Madras is east of Aden (Gill's station) by . . .	2 21 2.561

The resulting longitude of Madras by Series D thus becomes :—

Copeland's reduction	5^h 20^m $59^s.041$
Auwers' reduction	5 20 59.233

In Volume I of the Annals of the Cape Observatory, Mr. David Gill, who was one of the observers on this series, writes :—"In the case of Lord Lindsay's Expe-" dition (*i. e.* of Series D) the observations lay no claim to high refinement. They " were made throughout in the open air, with small portable instruments, which " in the case of Alexandria were placed on the roof of a hotel, where the observer " had to abstain from movement during each complete observation, otherwise the " level was disturbed by the change of his position. At Aden and Alexandria the " chronometers had to be carried a long distance between the observing station " and the telegraph office. The observers were without personal assistance and " the crucial observations for time had often to be made under conditions of " extreme fatigue, amounting on one or two occasions nearly to exhaustion on " the part of the observers engaged.

"In fact the character of the work was only such as it was possible to " organise and execute *en route*, and the results fully realised the accuracy " expected from them".

Regr No D. 816, S I D - Oct. 96 — 500

" 165, S I D - June 97 — 300

Photozincographed at the Office of the Trigonometrical Branch, Survey of India, Dehra Dún, October 1896

No. 156 — S. 96

Series E.

The following table gives the results of series E :—

Arc.	Difference of Longitude.	Probable Error.	Authority.
Mokattam-Greenwich	$2^h\ 5^m\ 6^s.24$	$\pm 0^s.067$	Airy's Transit of Venus, 1874.
Suez-Mokattam	0 5 6·93	±0·089	Ditto ditto
Aden-Suez	0 49 42.813	±0.041	Vol. XV, G. T. Survey of India.
Bombay-Aden	1 51 20.022	±0.033	Ditto ditto.
Bellary-Bombay	0 16 26.871	±0.012	Ditto ditto.
Madras-Bellary	0 13 16.545	±0.007	Ditto ditto.
Longitude of Madras	5 20 59.421	±0.123	

In the Annals of the Cape Observatory Mr. Gill points out, that this Series E was not executed with such refinements or precautions as are necessary for the determination of fundamental longitudes, and Mr. Hunter, the Suez observer, in his report to Sir George Airy states, that the micrometer screw of his transit instrument was "decidedly drunken", and that he had to abandon its use, and "to trust to observing the pole-star in both positions of the instrument over "the fixed wires for collimation" (page 322 of the Account of Observations of the Transit of Venus, 1874, by Sir George Airy). The personal equation of the Greenwich and Mokattam observers varied from $0^s.025$ to $0^s.655$ (page 288, Transit of Venus, 1874).

The results of the five series of operations may thus be tabulated as follows :—

	Longitude of Madras.	Probable Error.
Series A	$5^h\ 20^m\ 59^s.750$	$\pm 0^s.155$
" B	5 20 59·010	±0·163
" C	5 20 59·113	±0·0227
" D	5 20 59·233	±0·127
" E	5 20 59·421	±0·123

The deduction of the final value from the results of the five series will now be largely dependent on individual judgment and cannot be made with certitude. One of the four following courses will, I think, have to be adopted :—

(1) To obtain a mean value by combining the results of the series in accordance with their relative weights.
(2) To accept Series C without modification.
(3) To accept Series C with a modification of the Potsdam-Greenwich arc.
(4) To adjust all the errors of the several circuits, shown in the Chart, by a simultaneous reduction by means of minimum squares.

Whichever course be selected, its result will be governed by the weights given to the different series, and to a great extent these weights are but the outcome of opinion and bias.

At the suggestion of Babu Shiv Nath Saha and with his assistance, I commenced to adjust the errors of all the circuits shown in Chart II, by a simultaneous reduction by the method of minimum squares, and to solve the equations of condition presented by the several arcs of longitude with a view to obtaining the most probable value of each arc. I had to abandon this calculation, because I could devise no satisfactory method of taking account of the fact, that the Berlin-Greenwich arc enters into many European longitude circuits not shown on the Chart and the arc Madras-Karachi into many similar circuits in India. A simultaneous adjustment of the errors of *all* the European and Indian longitude circuits would be a work of great labour, quite incommensurate to the value at which its results would be appraised.

A change in the longitude of Madras affects not only British India, but the Malay Peninsula, Tonquin, China, the Dutch Indies and Australia, and the value adopted by the Government of India should be the value recognised by the scientific world.

The disagreement at Potsdam with European work is the main feature of our recent determination, and we do not possess the necessary data in India to enable us to adjust the discrepancy. Three of the series out of the five depend on the longitudes of Berlin and Potsdam, and it seems to me anomalous to discuss their discordance at Madras, as long as an initial difference exists at their first junction in Europe.

Captain Lenox Conyngham and myself were deputed together to determine the longitude of Madras, and our specific duty terminated with the completion of the calculations of Series C; and though I have appended a comparison with foreign values to this report, it would be beyond my province to deduce a final result from their combination.

MUSSOOREE;
The 21st November, 1896.

S. G. BURRARD,
Captain, R.E.

G. I. C. P. O.—No. 17 S. G. G.—27-7-97.—80.