

APPENDIX III.

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DETERMINATION

OF THE

LONGITUDE OF VALENCIA IN IRELAND

BY

GALVANIC SIGNALS,

IN THE SUMMER OF 1862.

GREENWICH OBSERVATIONS, 1862.—APP. III.

A



**D E T E R M I N A T I O N**

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1. In the summer of 1844, the longitude of the Feagh Main Station in Valencia was determined by the laborious operation of transmitting chronometers from the Royal Observatory, Greenwich, to a temporary observatory at Feagh Main, and comparing them with the sidereal clocks at both extremities of the arc. For convenience, the arc was broken into two parts at Kingstown, and, as a comparison was made at Liverpool at the passage of the chronometers, the arc was astronomically divided into three parts. Thirty pocket chronometers were transmitted ten times from Greenwich to Kingstown and ten times from Kingstown to Greenwich, and the same chronometers were transmitted the same number of times between Kingstown and Valencia. The results adopted were ;

Longitude of Liverpool Observatory West from Greenwich	}	m.	s.
Observatory - - - - -	}	12.	0·05
" " Kingstown station - - - - -		24.	31·20
" " Valencia, Feagh Main Station - - - - -		41.	23·23

The details of the operation are printed in full, in the Memoirs of the Royal Astronomical Society, volume XVI., and in the Greenwich Observations for 1845.

The operation was, I apprehend, one of the most accurate ever effected by the method of transmitting chronometers. But, although the different individual results from different chronometers agreed so well as to indicate a very small probable error of general result, yet it was impossible not to feel some misgivings as to the certainty of a conclusion derived from so much movement of instruments whose construction is so delicate as that of pocket chronometers.

2. In the course of years, the Galvanic Telegraph was invented, the Submarine Telegraph was successfully established, and the enterprise of the Atlantic Telegraph was undertaken. The Island of Valencia having been adopted as the European end of the Atlantic Telegraph, telegraphic wires were laid by the British and Irish Magnetic Telegraph Company, connecting that island with the main system of telegraphs in Ireland, and thus making direct galvanic communication with London. The Royal Observatory of Greenwich had previously been connected with London; and, after the failure of some underground portions of its first connexion, its communication, entirely by open-air wires, was brought to the following state. By favour of the London District Telegraph Company, several wires were led from the Royal Observatory to the office of that Company in the town of Greenwich. The larger portion of these were led on to the Greenwich station of the South-Eastern Railway, whence they were distributed, partly to the various stations of the Railway and to Deal, partly to the office of the Electric and International Telegraph Company. Two wires, however, were continued to a house in Deptford called "Gothic House," where for the first time they came into close proximity with wires of the British and Irish Magnetic Telegraph Company. One of the wires of the Telegraph Company was cut, and its ends were led into the same box in which the Observatory wires terminate. It is evident therefore that (among the various combinations possible) it was now easy to place one of the Observatory wires in connexion with one of the wires leading to the Telegraph Company's office in London. And as that Company possessed the power of making communications along the entire line to Valencia, it follows that an unbroken metallic communication could at any time be established between the Royal Observatory and Valencia. The line is by no means direct, passing by Cambridge, Doncaster, Sheffield, Liverpool, Carlisle, Port Patrick (nearly), Belfast, Dublin, Mallow, and Killarney; its length is probably not far short of 800 miles; but, as will be seen below, while the weather was fine, little inconvenience was produced by that length.

On the failure of the Atlantic Telegraph, the commercial interest in the maintenance of the wires to Valencia ceased. But the intention to use them, if possible, for a new determination of the longitude, had become known to Sir Charles T. Bright, engineer of the Company; and I believe that it was principally due to his instances with the Company, for the advantage of the longitude determination, that the wires were maintained in a serviceable state.

3. In the autumn of 1857, Mr. Struve visited England on his way from France. His chief business had been to visit the Central Offices of Survey of the principal Continental States, and to endeavour to make arrangements for effectually carrying out the observations (where needed), and the computations, which would serve to exhibit the comparison of measure with theory in one or more extensive arcs of parallel. Mr. Struve was shortly afterwards struck with very serious illness, and the plans for a time lay dormant. Subsequently they were taken up by Mr. Otto Struve. The arc

of parallel to which attention was mainly to be given was now defined to be that extending from València in the west to Orsk on the river Oural in the east. The operations required on the part of England were, a repetition of the geodetic connexion between England and the Continent by way of Belgium, (which has since been effected in the most complete and accurate manner by Colonel Sir Henry James, Director of the Ordnance Survey), and a repetition of the determination of the longitude of València. On 1862 May 8, I represented these circumstances to the Lords Commissioners of the Admiralty, and immediately received from their Lordships the necessary powers for the prosecution of the work.

4. As soon as it was understood that I proposed to proceed in no long time to the operations for the determination of longitude, I received the assurance of the British and Irish Magnetic Telegraph Company that every thing in their power should be done to render the operation efficient. Their Secretary and Manager, E. B. Bright, Esq., and their Engineer, Sir Charles T. Bright, issued the necessary orders for junctions of wires and appropriation of them to my service at the times when they should be required. Their London resident officer, T. B. Moseley, Esq., came repeatedly to the Royal Observatory for the purpose of trying experiments and making arrangements. The batteries required at València, 192 cells, were entirely provided by the Company, and when it appeared probable that an addition to the Greenwich batteries was required, 144 cells were lent by the Company, making the whole Greenwich power 324 cells. When, in the course of operations, the submarine cable between Caherciveen and València failed, their engineer was immediately despatched to repair it. To this I have to add that the requisite junctions (to make an unbroken metallic communication from the Greenwich Observatory to the temporary València Observatory) were, I believe, rigorously completed on each of the appointed days at every station except Mallow, where several failures occurred.

5. For the arrangements in València itself, I received every possible aid from the Knight of Kerry (to whose hospitality my Assistants were greatly indebted), and from R. J. Lecky, Esq., engineer and manager of the works at the well known València slate-quarries. In fixing upon a locality, the selection of which was combined with the decision upon the choice of astronomical instruments, I had to consider the difficulty of making a secure galvanic communication between the Telegraph Office and the Temporary Observatory, unless they could be in close proximity; and also the possible difficulty of establishing a satisfactory meridian-mark. Finally I selected, under the advice of Mr. Lecky, a point in Knightstown on the premises of the València Slate Company, south of the reservoir; and I decided to rely, for my determinations of local time, upon zenith distances of stars, to be observed with an Altazimuth. In this place therefore I planted an observing-hut, for the use of which I am indebted to the kindness of Colonel Sir Henry James, and of which the roof had at my request been

altered by Sir H. James so as to admit of observations of stars in all azimuths to a zenith distance of about  $50^\circ$ ; in this hut were placed an Altazimuth, and a Sidereal Clock; and in its neighbourhood were an Auxiliary Clock for making signal contacts, a barometer, and a thermometer. Further allusion will hereafter be made to these instruments.

6. In discussing with Sir H. James the general course of operations, it was suggested that a party of Sir H. James' surveyors should encamp on a hill called "The Coomb" near to Knightstown, whose form and position promised considerable immunity from local attraction; that they should there determine their local time; and that they should make comparison with the galvanic signals by the two methods, 1st, of observing the time of galvanic signals by a chronometer to be carried from and to the Surveyors' Station, and, 2nd, of comparing the two clocks (that in the Temporary Observatory and that in the Surveyors' Station) by means of an intermediate visible signal. It was also wished by Sir H. James that a meridian line should be laid down accurately. Circumstances prevented the first part of this plan from being carried out; but every assistance was given by Sir H. James and Captain Clarke, R.E., and the parties under their command, for carrying out the plans of the Observatory party.

7. Before entering upon technical explanations, I will state in a chronological form the general order of operations.

1862.

- May 26. Mr. Dunkin conveyed the Greenwich instruments to Southampton, where they were embarked in the schooner "Pilgrim," the property of the Ordnance Survey Office.
- May 29. The "Pilgrim" sailed, carrying the instruments, &c., of the Survey Party and of the Observatory Party.
- June 6. The "Pilgrim" arrived at Valencia.
- June 9. Mr. Dunkin and Mr. Criswick, Assistants of the Royal Observatory, started for Valencia.
- June 11. Arrived at Valencia.
- June 12. The observing-tent was erected.
- June 13. The clocks were mounted.
- June 14. The galvanic wires were attached. Signals were given at Greenwich but not received at Valencia, and signals were given at Valencia but not received at Greenwich. (In this and similar instances following, there is reason to believe that there was interruption at Mallow).
- June 15. Signals were given at both ends, but none were received. Two stars were observed at Valencia, at the first cessation of continued rain.
- June 16. Signals were given at both ends, but none were received. Two stars were observed at Valencia.
- June 17. Signals were given, but none received; no stars.

- June 18. By direction of Mr. Bright, a continuous current was to be sent from the Valencia Telegraph Office, and Mr. Moseley kindly attended at the Royal Observatory to make observations on it. No signals however could be received from stations beyond Dublin.
- June 19. Signals were sent from Greenwich, 8<sup>h</sup>. 15<sup>m</sup> to 8<sup>h</sup>. 30<sup>m</sup>, and 8<sup>h</sup>. 45<sup>m</sup> to 9<sup>h</sup>. 0<sup>m</sup>; the latter set from 8<sup>h</sup>. 48<sup>m</sup> were received at Valencia. Signals were sent from Valencia, 8<sup>h</sup>. 30<sup>m</sup> to 8<sup>h</sup>. 45<sup>m</sup> (Greenwich Mean Solar Time), and 9<sup>h</sup>. 0<sup>m</sup> to 9<sup>h</sup>. 15<sup>m</sup>; the latter set from 9<sup>h</sup>. 4<sup>m</sup> were received at Greenwich. The signals were sufficiently strong, the galvanometer needle striking the pin on the face of the dial. No stars were observed at Valencia.
- June 20. By arrangement with the Telegraph Company, no signals were sent. No stars were observed at Valencia.
- June 21. Greenwich signals were sent, 8<sup>h</sup>. 15<sup>m</sup> to 8<sup>h</sup>. 30<sup>m</sup>, and 8<sup>h</sup>. 45 to 9<sup>h</sup>. 0<sup>m</sup>; received at Valencia 8<sup>h</sup>. 26<sup>m</sup> to 8<sup>h</sup>. 30<sup>m</sup>, and 8<sup>h</sup>. 45<sup>m</sup> to 9<sup>h</sup>. 0<sup>m</sup>. Valencia signals were sent 8<sup>h</sup>. 30<sup>m</sup> to 8<sup>h</sup>. 45<sup>m</sup>, and 9<sup>h</sup>. 0<sup>m</sup> to 9<sup>h</sup>. 15<sup>m</sup>; all seen, but so weak that, among the deflexions produced by induced currents, they could not be accurately observed. Mr. Watlock, engineer to the Company, examined the Submarine Cable between Valencia and the mainland on the 20th and 21st, and ascertained that it was defective.
- June 22. Signals were sent from both ends, but none received.
- June 23. The cable was found to be very defective. A piece was cut off by Mr. Watlock, and a new piece was substituted; and the cable then appeared to be perfect. Signals were sent as usual, but none passed; evident signs of interruption.
- Experiments were made this day on the stability of the foundation-block of the Altazimuth, and arrangements were made, with the assistance of Mr. Lecky, for establishing a larger block, and for insulating it more completely.
- June 24. Signals were almost perfectly successful. At Valencia, there were 16 star-observations for time, and 4 for latitude.
- June 25. No signals passed (interruption at Liverpool). No stars were observed at Valencia. The new foundation-block was fixed.
- June 26. Signals were entirely successful. Two stars were observed at Valencia.
- June 27. By arrangement, no signals were sent.
- June 28. Signals were entirely successful. No stars were observed at Valencia.
- June 29. Signals were successful; no stars.
- June 30. Signals were partially successful; a constant current during one part of the time; no stars.
- July 1. Signals failed; no stars.

- July 2. Signals failed ; no stars.  
 July 3. All the Greenwich signals were received, but none of the Valencia signals ;  
 it was believed that a relay instrument was by mistake placed in circuit ;  
 8 observations for time, and 4 for latitude.  
 July 4. By arrangement, no signals were sent.  
 July 5. Signals failed ; no stars.  
 July 6. Signals failed ; 4 observations of stars.  
 July 7. Signals were successful ; 16 observations of stars.  
 July 8. Signals failed ; no stars.  
 July 9. Signals were successful : no stars.  
 July 10. Signals were nearly all successful ; 12 observations of stars.  
 July 11. By arrangement, no signals were sent.  
 July 12. Signals were successful ; 16 observations of stars.  
 July 13. Signals failed ; no stars.  
 July 14. Signals failed ; no stars.  
 July 15. Signals were partially successful ; no stars.  
 July 16. The cable failed ; no stars.  
 July 17. The cable was still defective ; some stars were observed.  
 July 18. The cable still defective ; no stars.  
 July 19. Signals were successful ; 16 observations of stars.  
 July 20. Signals failed ; no stars.  
 July 21. The instruments and observatory were dismantled.  
 July 22. The packages were embarked in the "Pilgrim," for return to Greenwich.  
 July 23. The observers quitted Valencia.  
 July 26. Arrived at Greenwich.

8. The instrument employed at Greenwich for the determination of local sidereal time was the transit-circle. It is unnecessary here to give more than a few particulars upon the state of its adjustments on the days of effective observations, viz., June 24, June 26, July 7, July 10, July 12, July 19.

The following are the readings of the South Collimator at coincidence of its wire with the image of that of the North Collimator. One revolution =  $32''\cdot 29$ .

1862.	d	h	r
June	22.	22	0'095
	29.	22	0'124
July	6.	22	0'151
	11.	22	0'126
	13.	22	0'113
	20.	21	0'114

(In making the determinations of the Line of Collimation, that position of the South Collimator was used which was defined by the Reading upon the North Collimator next preceding in order of time.)

Reading of Transit-Micrometer for Line of Collimation. One revolution =  $14''\cdot78$ .

1862	d	r	r
June 24		reading for the day, $30\cdot370$	adopted for mean of several days, $30\cdot374$
26	"	" " $30\cdot355$	" " " $30\cdot354$
July 7	"	" " $30\cdot373$	" " " $30\cdot372$
10	"	" " $30\cdot368$	" " " $30\cdot372$
12	"	" " $30\cdot363$	" " " $30\cdot363$
19	"	" " $30\cdot420$	" " " $30\cdot426$

## Error of Level of the Axis of the Transit-Circle.

1862.	d	h	"	"
June 24.	11		for the day, + $4\cdot42$	adopted for mean of several days, + $4\cdot30$
26.	11		" + $3\cdot99$	" " " + $3\cdot92$
July 7.	10		" + $3\cdot83$	" " " + $3\cdot97$
10.	10		" + $4\cdot42$	" " " + $4\cdot42$
12.	10		" + $4\cdot64$	" " " + $4\cdot64$
19.	9		" + $4\cdot20$	" " " + $4\cdot18$

## Azimuthal Error of the Transit-Circle.

1862.	d	"	"
June 24 :		Polaris S.P. and $\zeta$ Virginis.....	- $1\cdot33$
June 24 :		$\delta$ Ursæ Minoris and Cephei 51 S.P....	- $1\cdot79$
June 25 :		Polaris S.P. and Spica .....	- $4\cdot13$
27 :		Polaris S.P. and $\alpha$ Serpentis.....	- $3\cdot83$
27 :		$\delta$ Ursæ Minoris and Cephei 51 S.P....	- $3\cdot30$
28 :		$\delta$ Ursæ Minoris and Cephei 51 S.P....	- $3\cdot53$
July 7 :		$\delta$ Ursæ Minoris and Cephei 51 S.P....	- $0\cdot40$
8 :		Polaris S.P. and Polaris .....	- $0\cdot35$
July 10 :		$\delta$ Ursæ Minoris and Cephei 51 S.P.....	- $0\cdot04$
July 12 :		$\delta$ Ursæ Minoris and Cephei 51 S.P.....	- $0\cdot38$
July 19 :		$\delta$ Ursæ Minoris and Cephei 51 S.P....	- $0\cdot67$
19 :		$\lambda$ Ursæ Minoris and $\theta$ Aquilæ .....	- $0\cdot97$
20 :		Polaris and Polaris S.P. ....	- $0\cdot84$

} adopted..... -  $1\cdot56$   
 } adopted for June 26, -  $3\cdot70$   
 } adopted for July 7. -  $0\cdot38$   
 } adopted for July 10, -  $0\cdot38$

It will appear from these numbers that the state of adjustments of the transit-circle was extremely well known.

9. The adopted Clock Losing Rates on June 23, 24, and 25, were respectively  $0^s\cdot32$ ,  $0^s\cdot38$ ,  $0^s\cdot36$ ; with the rate  $0^s\cdot38$ , and with transits of 9 stars by the chronographic method, from June 24,  $7^h$  to  $11^h$  (solar), the clock error at  $0^h$  sidereal was  $60^s\cdot81$  slow. For June 25, 26, 27, the rates were  $0^s\cdot36$ ,  $0^s\cdot37$ ,  $0^s\cdot47$ ; with clock rate  $0^s\cdot37$ , 4 clock stars (chronographic) from June 26,  $8^h$  to  $10^h$ , gave clock error at  $0^h$  sidereal  $61^s\cdot53$ . On July 7 an alteration was made in the clock adjustments, but its subsequent rate was steadily  $3^s\cdot92$ , and 5 stars from July 7,  $8^h$  to  $11^h$ , gave for  $0^h$  sidereal  $32^s\cdot80$ . About July 10 the rate was steadily  $4^s\cdot12$ ; and 7 stars from July 10,  $10^h$  to  $12^h$ , gave for  $0^h$  sidereal  $44^s\cdot78$ . On July 12 the adjustments were disturbed, but the rates

scarcely differed from  $1^s.09$ ; and 7 stars from July 12,  $9^h$  to  $10^h$ , gave error at  $0^h$  sidereal  $10^s.68$ . On July 18, 19, 20, the rates were  $1^s.16$ ,  $1^s.12$ ,  $1^s.17$ ; with rate  $1^s.12$ , 4 stars from July 19,  $8^h$  to  $9^h$ , gave error for  $0^h$  sidereal  $18^s.26$ . These clock-errors are given as corrected for personal equation, so as to reduce them to those which would have been given by Mr. Dunkin's observation of chronographic transits.

It will appear from these that there is no room for the smallest possible doubt on the state of the clock-error at Greenwich through the whole series of signal observations.

10. The instrument used at Valencia was an Altazimuth, constructed by Messrs. Troughton and Simms, the property of T. Coventry, Esq., and for the loan of which (obtained through the kind offices of Mr. James Simms) I am indebted to that gentleman. The circles, horizontal and vertical, are 12 inches in diameter, the length of the telescope is  $16\frac{3}{4}$  inches, its clear aperture 1.8 inch. A horizontal tripod with adjusting screws, resting on a wooden tripod, carries the foundation-plate of gun-metal, on which the azimuth-circle is fixed, and which has a cone projecting upwards, on which turns an exterior cone rising upwards from the base-plate of the rotating frame. Upon that base-plate are planted two pillars; one bears a Y, two micrometer-microscopes (carried by a horizontal bar) for reading the divisions of the graduated vertical circle, and the spirit-level (carried by the same bar) parallel to the plane of the vertical circle; the other bears a Y, the clamp of the clamping vertical circle, and the stand for the illuminating lamp. The graduated vertical circle and the clamping vertical circle are of the same diameter, but on opposite sides of the telescope; a horizontal axis 10 inches long, consisting principally of two cones meeting at their bases, carries the telescope in the middle of its length, and a circle on each side. The pivots are of gun-metal; that which is next the clamping-circle is perforated for illumination of the telescope-field. The numeration of the divisions on the graduated circle is continuous from  $0^\circ$  to  $360^\circ$ . The graduation is to spaces of  $5'$ ; a revolution of each micrometer corresponds sensibly to  $1'$ ; and their heads are divided to  $60''$ . The divisions of the level are sensibly  $1''$  each. The horizontal wires in the field of view of the telescope (which alone were used at Valencia) are at such a distance that, in the azimuths at which stars were most usually observed, a star occupied about  $14^s$  in passing from one to the next.

As the azimuthal motion was not used for any accurate purposes, I shall simply remark on it that the circle-graduation is read by three verniers attached to the base-plate of the rotating frame, and that, besides coarse levels for ordinary adjustment of verticality of vertical axis, there is a striding level of great delicacy, to be planted, in azimuthal observations, on the pivots of the horizontal axis.

I have only to add, finally, that a spring, placed between the cone which rises from the foundation-plate and that which rises from the base-plate, serves to alleviate the friction in azimuthal motion. It was sometimes suspected that this spring was a

little too strong, and that a trifling unsteadiness was occasioned by it; but of this there was no certain evidence.

In observation of the zenith-distance of stars, the vertical circle was clamped and was not touched; the horizontal circle also was clamped, but the observer kept his fingers on the tangent-screw, and moved the instrument in azimuth sufficiently to cause the star's vertical transit to fall on corresponding parts of all the vertical wires. The times of the successive transits were noted as in ordinary eye-and-ear transits, by listening to the beats of the clock Earnshaw, a clock belonging to the Royal Observatory, furnished with gridiron pendulum, but bearing a good character for steadiness of rate.

I will here give the determinations of the Zenith Point of the Vertical Circle. As, in the determinations of time, every object was, when possible, observed in both positions of the instrument, the exact value of Zenith Point was unimportant; the determinations were principally useful as showing the state of the instrument.

	°	'	"
1862.			
June 16. Two double observations of a mark on Feagh Main, gave . . . .	90. 0.		49.83
June 17. Two double observations of a mark on a hill distant $1\frac{1}{2}$ mile ..	90. 1.		4.65
June 20. Two double observations of a mark on a house distant $3\frac{1}{2}$ miles	90. 0.		54.54
June 21. Similar observations .....	90. 0.		55.18
June 23. Similar observations .....	90. 1.		0.00
Again .....	90. 0.		56.22
June 24. Seven double observations .....	90. 0.		57.95
June 25. Two double observations .....	90. 0.		59.06
The new foundation-block was fixed.			
June 27. Similar observations .....	90. 0.		56.69

After this time, the zenith points were determined solely by observations of stars. With an approximate colatitude and approximate hour-angle for one observation, but with exact difference of time between the two observations, the exact difference of zenith distance at the two observations (one with Face Right, the other with Face Left) was computed. The circle readings were corrected for refraction (with the use of an approximate zenith-point), and, the difference just found being applied to one of the corrected circle-readings, the two readings became similar to those for a fixed object, and the zenith point could be inferred in the same way as with a fixed object. Thus the following were found:—

	°	'	"
July 3, by $\alpha$ Aquilæ.....	90. 0.		52.01
by Arcturus.....	90. 0.		52.20
July 6, by $\alpha$ Lyræ .....	90. 1.		7.42
by Arcturus.....	90. 1.		3.75
July 7, by $\alpha$ Aquilæ.....	90. 0.		55.11
by Arcturus.....	90. 0.		53.22
July 10, by $\alpha$ Aquilæ.....	90. 1.		1.04
by Arcturus.....	90. 1.		2.66
July 12, by Arcturus.....	90. 1.		3.81
by $\zeta$ Cygni.....	90. 1.		1.53

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	°	'	''
July 17, by Arcturus.....	90.	1.	4.08
by $\alpha$ Aquilæ.....	90.	1.	0.60
July 19, by $\alpha$ Aquilæ.....	90.	0.	59.36
by $\eta$ Boëtis .....	90.	0.	57.53

From these determinations of zenith-point, a zenith-point was adopted each evening at the observer's discretion, and with it the apparent zenith distances were calculated. The refraction was computed by the Tables in the Appendix to the Greenwich Observations 1853, and the true instrumental zenith-distances were found. All the observations of zenith-distance, except those on July 17, were made by Mr. Dunkin.

11. I now proceed to describe the process of computing the clock-error. It will be convenient to premise here, that the colatitude of the station, as inferred from that of Feagh Main, (where the latitude was observed several years ago,) by measure on the Ordnance Map, and likewise by a small triangulation effected under Sir Henry James' direction, is without great error  $38^{\circ}. 4'. 30''$ .

As the observations were to be made in a place whose latitude was not positively known, the observers were instructed to observe the zenith-distances of known stars in azimuths differing by  $90^{\circ}$  approximately. The zenith-distances of two such stars, in whatever azimuths, (provided the difference is nearly  $90^{\circ}$ ,) give the colatitude and local time, with the same probable error as the observation of one star in the meridian and one star near the prime vertical. The computation is most easily conducted by using two trial-colatitudes, one supposed to be too great and the other too small, by finding, with each of these, the clock-errors given by both stars, and by interpolating or extrapolating a colatitude which will make the clock-errors by both stars to coincide. The mean R.A. and N.P.D. of the stars were taken from the latest Catalogues in the Royal Observatory, including the Seven-Year Catalogue now in progress. In this way the reductions were begun.

But it was soon found that the colatitude required to produce this agreement of clock-errors was one which was quite inadmissible. Thus on June 16, observations of  $\alpha$  Lyræ, 3 hours east of the meridian, and  $\eta$  Boëtis, 2 hours west of the meridian, gave for colatitude  $38^{\circ}. 4'. 21''.3$ ; on July 6, observations of  $\alpha$  Lyræ  $2\frac{3}{4}$  hours east of the meridian, and Arcturus  $1\frac{3}{4}$  hour west of the meridian, gave  $38^{\circ}. 4'. 20''.70$ . It was then remarked that there were some observations of Polaris and  $\delta$  Ursæ Minoris, and observations of southern stars very near the meridian, which might be supposed to decide on this point; they were reduced, and gave as follows:—

	°	'	''
On 1862, June 24, Polaris.....	38.	4.	34.34
July 3, Polaris.....	38.	4.	34.59
July 12, $\delta$ Ursæ Minoris.....	38.	4.	35.70
July 17, Polaris.....	38.	4.	34.45
July 17, $\gamma$ Herculis.....	38.	4.	29.06
July 17, $\beta$ Herculis.....	38.	4.	28.44
July 17, $\delta$ Ophiuchi.....	38.	4.	27.60

These results seemed to throw the matter into still greater confusion. In the mean time, till something should occur which should throw light on this point, all the observations of zenith-distance were reduced for clock-error with the assumed colatitude  $38^{\circ}.4'.20''00$ .

On the return of the Altazimuth to the Royal Observatory, it was planted in the Front Court, in a spot whose colatitude is  $38^{\circ}.31'.21''4$  very nearly, and stars were observed very near the meridian on both sides of the zenith, giving the following apparent values of colatitude:—

Stars N. of the Zenith.		°	'	"
1862.				
September 6,	Polaris .....	38.	31.	25.2
	7, $\lambda$ Ursæ Minoris .....	38.	31.	27.2
	7, Polaris .....	38.	31.	27.3
Stars S. of the Zenith.				
September 6,	$\alpha$ Aquarii .....	38.	31.	18.16
	$\gamma$ Aquarii .....	38.	31.	18.18
	7, $\beta$ Aquilæ .....	38.	31.	16.75
	7, $\epsilon$ Delphini .....	38.	31.	16.40
	7, $\beta$ Delphini .....	38.	31.	20.93

These results led to two conclusions. First, that if the correction for instrumental error, which was indisputably required in the observations of Polaris, &c., at Greenwich, was applied with the same magnitude to the observations of Polaris at Valencia, it would produce a colatitude agreeing very nearly with that obtained by the geodetic connexion with Feagh Main. Secondly, that the instrumental error made the zenith-distances of stars appear too great on both sides of the zenith.

As it was evident now that the hour-angles computed with colatitude  $38^{\circ}.4'.20''00$  and unaltered instrumental zenith-distances must be corrected for changes in both these elements, numerical values of the following factors were prepared for all the observations:—

Correction (in seconds of time) to computed hour-angle, corresponding to a given change in colatitude, =

$$\frac{1}{15} \left\{ -\cot. N. P. D. \times \operatorname{cosecant} \text{ hour-angle} + \cotan. \text{ colatitude} \times \cotan. \text{ hour-angle} \right\} \\ \times \text{change of colatitude expressed in seconds of arc.}$$

Correction (in seconds of time) to computed hour-angle, corresponding to a given change in star's zenith distance, =

$$\frac{1}{15} \frac{\sin. \text{ zen. dist.}}{\sin. \text{ colat.} \times \sin. N. P. D. \times \sin. \text{ hour-angle}} \times \text{change of zenith distance expressed in seconds of arc.}$$

And finally, after examination of all results, and remarking that, from want of acquaintance with the cause of the error, we were scarcely justified in assigning a law to it; that the meridian observations might admit of small errors; and that the

condition of giving nearly equal clock-errors from observations on both sides of the meridian ought to be secured; the following coefficients of corrections were assumed:—

"
   
Correction to assumed colatitude..... + 8'00
   
Correction to instrumental zenith distances of stars — 4'11

The corresponding corrections to the computed hour-angles being found and applied, the following values were obtained for the clock-error:—

**ERRORS OF THE CLOCK AT VALENCIA, AS FOUND FROM OBSERVATIONS OF STARS WITH THE ALTAZIMUTH.**

Day. 1862.	Stars East of Meridian.			Stars West of Meridian.			Mean of Sidereal Times.	Mean Clock Slow.	Clock's Losing Rate.
	Star's Name.	Approximate Sidereal Time.	Clock Slow.	Star's Name.	Approximate Sidereal Time.	Clock Slow.			
June 24	$\alpha$ Aquilæ ...	h m s 16. 23 —40'22		$\beta$ Leonis ...	h m s 16. 5 —39'43		16. 55	—39'56	
	"	16. 29 39'50		"	16. 14 39'39				
	"	16. 35 39'52		$\eta$ Boötis ....	16. 44 38'90				
	"	16. 40 39'85		"	16. 48 38'93				
	$\beta$ Aquilæ ...	17. 8 40'11		"	16. 54 39'34				
	"	17. 13 40'41		"	16. 59 39'67				
	"	17. 19 40'32		"	17. 36 38'93				
June 26	"	17. 24 39'20		"	17. 43 39'25				1'37
	$\alpha$ Lyræ.....	15. 27 36'64		$\beta$ Leonis ...	15. 38 37'20		15. 33	—36'90	
July 3	"	15. 33 —37'11		"	.. —36'65				1'41
	$\alpha$ Aquilæ ...	15. 51 +33'11		Arcturus ...	16. 6 +32'44		16. 19	+33'06	
	"	15. 59 33'14		"	16. 12 32'43				
	"	16. 19 33'62		$\eta$ Boötis ....	16. 29 33'53				
"	16. 24 33'23		"	16. 35 32'99					
July 6	$\alpha$ Lyræ.....	15. 43 36'20		Arcturus ...	15. 52 36'47		15. 50	+36'30	
	"	15. 47 36'58		"	15. 56 35'93				0'94
July 7	$\alpha$ Aquilæ ...	16. 43 37'36		$\eta$ Boötis ....	16. 52 37'05		17. 19	+37'30	
	"	16. 47 37'63		"	16. 57 37'20				
	"	17. 1 37'50		"	17. 8 37'05				
	"	17. 5 37'37		"	17. 11 37'42				
	$\epsilon$ Cygni ....	17. 25 37'39		Arcturus ...	17. 33 36'83				
	"	17. 29 37'31		"	17. 37 37'26				
	"	17. 42 37'38		"	17. 50 37'22				
July 10	"	17. 46 37'42		"	17. 53 37'41				1'49
	$\alpha$ Aquilæ ...	16. 34 41'86		Arcturus ...	16. 48 41'61		17. 5	+41'77	
	"	16. 38 41'64		"	16. 51 41'65				
	"	16. 55 42'26		"	17. 2 41'79				
	"	16. 59 41'74		"	17. 7 41'63				
	"	17. 20 41'61		"	17. 38 41'62				
"	17. 33 +42'47		"	17. 42 +41'43					

## ERRORS OF THE CLOCK AT VALENCIA—concluded.

Day. 1862.	Stars East of Meridian.			Stars West of Meridian.			Mean of Sidereal Times.	Mean Clock Slow.	Clock's Losing Rate.
	Star's Name.	Approximate Sidereal Time.	Clock Slow.	Star's Name.	Approximate Sidereal Time.	Clock Slow.			
July 12	$\alpha$ Cygni . . . .	h m s 17. 7 +44'83	Arcturus . . .	h m s 16. 58 +44'97	17. 31	+44'92	17. 31	+44'92	1'10
	"	17. 11 45'09	"	17. 2 44'67					
	"	17. 25 44'71	"	17. 17 44'85					
	"	17. 29 45'53	"	17. 20 44'41					
	$\zeta$ Cygni . . . .	17. 42 45'22	"	17. 35 44'03					
	"	17. 47 44'83	"	17. 38 44'80					
	"	17. 57 45'06	$\eta$ Boötis . . . .	17. 50 44'96					
"	18. 0 44'79	"	17. 53 45'11						
July 17	$\alpha$ Aquilæ . . .	16. 40 50'50	Arcturus . . .	16. 29 50'19	16. 36	+50'34	16. 36	+50'34	0'83
	"	16. 43 50'04	"	16. 33 50'64					
July 19	$\alpha$ Aquilæ . . .	16. 45 51'90	Arcturus . . .	16. 52 51'73	17. 12	+52'01	17. 12	+52'01	0'83
	"	16. 49 52'13	"	16. 55 52'43					
	$\zeta$ Cygni . . . .	16. 59 51'77	"	17. 8 52'14					
	"	17. 4 51'97	"	17. 11 51'89					
	"	17. 14 52'04	$\eta$ Boötis . . . .	17. 21 52'13					
	"	17. 18 51'96	"	17. 25 52'02					
	"	17. 28 52'14	"	17. 34 51'97					
"	17. 31 +51'90	"	17. 40 +52'16						

The values of "Clock Slow" correspond to observations taken with the Instrument alternately Face Right and Face Left, in all cases except those of  $\beta$  Leonis June 26, for which a single observation only was made, with the instrument Face Right. But as the reduction of the observations of  $\alpha$  Lyræ, with a zenith-point derived from a fixed mark, appeared to show that that zenith-point was erroneous (the results Face Right and Face Left being sensibly different), a corresponding difference was applied to the Clock Slow —  $37^s.20$ , derived from  $\beta$  Leonis Face Right, and thus was formed the Clock Slow —  $36^s.65$ , which is assumed to correspond to an observation Face Left.

There is no doubt that, on the whole, the mean of the results on each day possesses all desirable accuracy. The only day which can present the slightest difficulty is June 26, and that, I believe, is quite trustworthy.

Very little depends upon the corrections to the colatitude and zenith-distances, because the observations on the east and west sides of the meridian were almost exactly balanced. But I have thought it desirable to enter, in some detail, into the difficulties which first presented themselves; as the account may prove to others, who are to engage in similar operations, a useful lesson on the anomalies for which they ought to be prepared, and which, perhaps, a few preliminary observations may enable them to overcome without difficulty.

12. In deciding on the method to be used for comparison of the sidereal clocks at Greenwich and Valencia, I had to select, not the method which might be the most

accurate under very favourable circumstances, but that which might be best under rather unfavourable circumstances. And I was led to prefer that of simple galvanometer-signals. I adopted this the more readily, because I knew that I could place the greatest confidence in the coolness and skill of my excellent Assistants, Mr. Dunkin and Mr. Ellis, by whom the observations of the signals would be made.

The battery at Greenwich consisted, in the first instance, of 180 cells, subsequently increased to 324. The battery at Valencia consisted of 192 cells. This number was amply sufficient, even for a much longer communication, when the air was dry; but in very humid weather, or in rain, its signals were feeble.

For completing the galvanic circuit by which the signal was given, a small auxiliary clock was used at each station; on whose 60-second wheel were fixed 4 pins, which, in the rotation of the wheel, forced into momentary contact a pair of springs carried by an insulating block. The two segments of the galvanic wire, one leading to the battery, the other leading to the long telegraph-wire, were connected with these two springs; and thus, at every 15° nearly, the telegraph-wire was brought for an instant into metallic connexion with the battery, and the galvanic current passed and exhibited its signal on the galvanometers at both stations. The regular recurrence of signals is attended with this great advantage, that the observer is prepared to fix his attention on the signals and on the beats of the transit-clock: at the same time, by proper adjustment of the rate of the auxiliary clock, the signals are made to fall on all portions of the temporal interval between two beats of the transit clock. As soon as the period of sending signals from one station was terminated, the long wire was disconnected from the clock and was connected with earth, and at the same time the opposite change was made at the other station.

The phenomenon observed was the clock-time (as obtained by listening to the beats of the transit-clock) of the start of the needle.

13. I shall now exhibit the result given by these observations for the length of time occupied by the Galvanic Current in passing between Greenwich and Valencia.

First, by comparison of the observed clock-times of signals on those days on which no stars were observed, namely, June 28, June 29, July 9. On these days no attempt was made to apply an approximate clock-error. But as the signals were given in different quarters of the hour from Greenwich and from Valencia, it is necessary to take account of the clock-rates, so as virtually to refer all observations to the states of the clocks in the same quarter. I shall adopt as standard the first quarter. I shall exhibit only the excess of seconds of Greenwich Clock over seconds of Valencia Clock.

Day. 1862.	Losing Rate of Greenwich Clock.	Losing Rate of Valencia Clock.	Order of Hour-quarter and Place of Signals.	Number of Observations.	Seconds of Excess of Greenwich Clock. Greatest. Least. Mean.	Correction for Greenwich Rate.	Correction for Valencia Rate.	Corrected Excess of Greenwich Clock.
June 28	0 <sup>s</sup> .37	1 <sup>s</sup> .41	First quarter ; signals from Greenwich.	39	32 <sup>s</sup> .7 32 <sup>s</sup> .3 32 <sup>s</sup> .518	0 <sup>s</sup> .000	0 <sup>s</sup> .000	32 <sup>s</sup> .518
			Second quarter ; signals from Valencia.	50	33 <sup>s</sup> .3 32 <sup>s</sup> .4 32 <sup>s</sup> .844	+ 0 <sup>s</sup> .004	- 0 <sup>s</sup> .015	32 <sup>s</sup> .833
			Third quarter ; signals from Greenwich.	52	33 <sup>s</sup> .0 32 <sup>s</sup> .0 32 <sup>s</sup> .587	+ 0 <sup>s</sup> .008	- 0 <sup>s</sup> .030	32 <sup>s</sup> .565
			Fourth quarter ; signals from Valencia.	48	33 <sup>s</sup> .1 32 <sup>s</sup> .5 32 <sup>s</sup> .838	+ 0 <sup>s</sup> .012	- 0 <sup>s</sup> .045	32 <sup>s</sup> .805
June 29	0 <sup>s</sup> .27	1 <sup>s</sup> .41	First quarter ; signals from Greenwich.	49	34 <sup>s</sup> .1 33 <sup>s</sup> .2 33 <sup>s</sup> .796	0 <sup>s</sup> .000	0 <sup>s</sup> .000	33 <sup>s</sup> .796
			Second quarter ; signals from Valencia.	40	34 <sup>s</sup> .5 33 <sup>s</sup> .8 34 <sup>s</sup> .185	+ 0 <sup>s</sup> .003	- 0 <sup>s</sup> .015	34 <sup>s</sup> .173
			Third quarter ; signals from Greenwich.	54	34 <sup>s</sup> .2 33 <sup>s</sup> .4 33 <sup>s</sup> .887	+ 0 <sup>s</sup> .006	- 0 <sup>s</sup> .030	33 <sup>s</sup> .863
			Fourth quarter ; signals from Valencia.	51	34 <sup>s</sup> .6 33 <sup>s</sup> .9 34 <sup>s</sup> .165	+ 0 <sup>s</sup> .009	- 0 <sup>s</sup> .045	34 <sup>s</sup> .129
July 9	4 <sup>s</sup> .12	1 <sup>s</sup> .51	First quarter ; signals from Greenwich.	58	6 <sup>s</sup> .3 5 <sup>s</sup> .9 6 <sup>s</sup> .081	0 <sup>s</sup> .000	0 <sup>s</sup> .000	6 <sup>s</sup> .081
			Second quarter ; signals from Valencia.	42	6 <sup>s</sup> .6 5 <sup>s</sup> .9 6 <sup>s</sup> .231	+ 0 <sup>s</sup> .043	- 0 <sup>s</sup> .016	6 <sup>s</sup> .258
			Third quarter ; signals from Greenwich.	41	6 <sup>s</sup> .2 5 <sup>s</sup> .7 5 <sup>s</sup> .937	+ 0 <sup>s</sup> .086	- 0 <sup>s</sup> .032	5 <sup>s</sup> .991
			Fourth quarter ; signals from Valencia.	33	6 <sup>s</sup> .7 5 <sup>s</sup> .8 6 <sup>s</sup> .261	+ 0 <sup>s</sup> .129	- 0 <sup>s</sup> .048	6 <sup>s</sup> .342
Day, 1862.					June 28.	June 29.	July 9.	
Mean Corrected Excess, with signals from Greenwich . . . .					32 <sup>s</sup> .541	33 <sup>s</sup> .829	6 <sup>s</sup> .036	
Mean Corrected Excess, with signals from Valencia . . . . .					32 <sup>s</sup> .819	34 <sup>s</sup> .151	6 <sup>s</sup> .300	
Half Difference, or Time occupied by Current's Passage . . .					0 <sup>s</sup> .139	0 <sup>s</sup> .161	0 <sup>s</sup> .132	

GREENWICH OBSERVATIONS, 1862.—APP. III.

c

Second, by comparison of the sidereal times of signals on those days on which stars were observed, namely, June 24, June 26, July 7, July 10, July 12, July 19. On these days the clock-error and rate were fully applied to the observed clock-time of each signal, and no further correction for rate is required.

As before, the signals in each first and third quarter were given from Greenwich, and those in each second and fourth quarter from Valencia. The numbers given are the seconds of excess of Greenwich Sidereal Time over Valencia Sidereal Time, omitting 41<sup>m</sup>.

## 1862, June 24.

Signals from Greenwich, 60 obs., greatest	9 <sup>s</sup> .88,	least	9 <sup>s</sup> .37,	mean	9 <sup>s</sup> .567
from Valencia, 52	10 <sup>s</sup> .07	9 <sup>s</sup> .38			9 <sup>s</sup> .777
from Greenwich, 47	9 <sup>s</sup> .86	9 <sup>s</sup> .27			9 <sup>s</sup> .586
from Valencia, 52	10 <sup>s</sup> .06	9 <sup>s</sup> .36			9 <sup>s</sup> .754

Mean with signals from Greenwich, 9<sup>s</sup>.577

from Valencia, 9<sup>s</sup>.766

Time occupied by current's passage, 0<sup>s</sup>.095

## June 26.

Signals from Greenwich, 53 obs., greatest	10 <sup>s</sup> .07,	least	9 <sup>s</sup> .55,	mean	9 <sup>s</sup> .820
from Valencia, 53	10 <sup>s</sup> .35	9 <sup>s</sup> .55			10 <sup>s</sup> .024
from Greenwich, 57	10 <sup>s</sup> .25	9 <sup>s</sup> .53			9 <sup>s</sup> .878
from Valencia, 48	10 <sup>s</sup> .22	9 <sup>s</sup> .42			9 <sup>s</sup> .974

Mean with signals from Greenwich, 9<sup>s</sup>.849

from Valencia, 9<sup>s</sup>.999

Time occupied by current's passage, 0<sup>s</sup>.075

## July 7.

Signals from Greenwich, 47 obs., greatest	10 <sup>s</sup> .03,	least	9 <sup>s</sup> .24,	mean	9 <sup>s</sup> .584
from Valencia, 49	10 <sup>s</sup> .18	9 <sup>s</sup> .46			9 <sup>s</sup> .847
from Greenwich, 55	9 <sup>s</sup> .89	9 <sup>s</sup> .39			9 <sup>s</sup> .613
from Valencia, 35	10 <sup>s</sup> .24	9 <sup>s</sup> .71			9 <sup>s</sup> .919

Mean with signals from Greenwich, 9<sup>s</sup>.599

from Valencia, 9<sup>s</sup>.883

Time occupied by current's passage, 0<sup>s</sup>.142

## July 10.

Signals from Greenwich, 21 obs., greatest	9 <sup>s</sup> .62,	least	9 <sup>s</sup> .03,	mean	9 <sup>s</sup> .309
from Valencia, 50	9 <sup>s</sup> .94	9 <sup>s</sup> .33			9 <sup>s</sup> .579
from Greenwich, 56	9 <sup>s</sup> .65	8 <sup>s</sup> .95			9 <sup>s</sup> .317
from Valencia, 50	10 <sup>s</sup> .00	9 <sup>s</sup> .19			9 <sup>s</sup> .607

Mean with signals from Greenwich, 9<sup>s</sup>.313

from Valencia, 9<sup>s</sup>.593

Time occupied by current's passage, 0<sup>s</sup>.140

## — 19 —

July 12.

Signals from Greenwich, 59 obs., greatest	9°52,	least 9°11,	mean 9°311
from Valencia, 52	9°91	9°32	9°568
from Greenwich, 56	9°52	9°11	9°319
from Valencia, 56	9°91	9°31	9°634

Mean with signals from Greenwich, 9°315  
 from Valencia, 9°601

Time occupied by current's passage, 0°143

July 19.

Signals from Greenwich, 56 obs., greatest	9°96,	least 9°36,	mean 9°620
from Valencia, 47	10°26	9°57	9°877
from Greenwich, 55	9°87	9°36	9°607
from Valencia 55	10°27	9°66	9°888

Mean with signals from Greenwich, 9°614  
 from Valencia, 9°882

Time occupied by current's passage, 0°134

Collecting now all the results for the time occupied by the current's passage, we have,—

1862.	
June 24	0°095
26	0°075
28	0°139
29	0°161
July 7	0°142
9	0°132
10	0°140
12	0°143
19	0°134

The mean of the whole is..... 0°129

It appears therefore that the time occupied by the passage of the current between Greenwich and Valencia, or rather the interval between the first visibility of the signal at the battery-station and the first visibility of the signal at the other station, is  $\frac{1}{8}$  of a second of time, very nearly.

14. For the difference of longitude, the numbers corresponding to the days of star-observation at Valencia may be taken from the last section, supplying the minutes 41, which are there omitted. Thus we have—

By the mean of observations on 1862, June 24	41. 9°671
June 26	41. 9°924
July 7	41. 9°741
July 10	41. 9°453
July 12	41. 9°458
July 19	41. 9°748

GREENWICH OBSERVATIONS, 1862.—APP. III.

D

If the observation of signals be regarded as free from sensible error, the value of each day's result will depend mainly on the number of observations of stars on that day. They are as follows :—

	Greenwich.	Valencia.
June 24 .....	9	16
26 .....	4	3
July 7 .....	5	16
10 .....	7	12
12 .....	7	16
19 .....	4	16

No great error will be introduced if, in the formation of combination-weights, we consider every observation as equally good. The combination weights will then be determined by dividing the product of the two numbers, Greenwich and Valencia, for the same day by their sum. Combining the results by this theorem, the mean is found to be—

$$41^m. 9^s. 627.$$

The difference between the results of July 10 and 12 on the one hand and those of the remaining days on the other hand is striking. Nothing has been discovered in the observations or computations which can explain it. But, on making careful inquiry, I find that the state of Mr. Dunkin's health was not perfectly satisfactory. From July 9 to July 17 he suffered from constant severe head-ache and low fever. The symptoms were generally alleviated in the evening, but it is still possible that they in some measure affected the observations. Under these circumstances, I am inclined to attribute less weight to the results of July 10 and July 12, and to adopt as mean—

$$41^m. 9^s. 70.$$

15. It is yet necessary to take account of the differences, in the methods of observing the signals, peculiar to the different observers. On July 29 and July 30 Mr. Dunkin and Mr. Ellis observed the times of the same signals, and, on comparing the registered times, found the following differences :—

From the aggregate of 61 differences, E-D =	+	7.2
” ” 60 ” ”	+	4.8
” ” 60 ” ”	+	1.2
” ” 61 ” ”	+	2.2
The mean of these gives E-D =	+	0 <sup>s</sup> . 063.

The Greenwich times therefore were registered too late by 0<sup>s</sup>. 063, and ought to be diminished by that quantity. This will give as correction to the mean of difference of local times of signals —0<sup>s</sup>. 063.

Also, it is necessary to consider that the clock-error was determined at Greenwich by the chronographic method, and corrected for the personal equations among chrono-

graphic observers so as to reduce it to a result by Mr. Dunkin's chronographic observations. But at Valencia the clock-error was found by Mr. Dunkin's eye-and-ear observations. And it will be necessary to reduce the sidereal times of Greenwich signals from what they were shown by Mr. Dunkin's chronographic observations to what they would have been shown by Mr. Dunkin's eye-and-ear observations. Now the comparisons of three days' observations gives (all results for clock-error being reduced to 0<sup>h</sup> sidereal, and all the observations being made by Mr. Dunkin)—

		Clock Slow by Eye and Ear.		Clock Slow by Chronographic Method.		Excess of Eye and Ear.
		<u>s</u>		<u>s</u>		<u>s</u>
1862.						
October 8,	4 stars	34·10		3 stars,	33·94	+ 0·16
15,	2	39·95		2	39·83	+ 0·12
21,	4	43·66		4	43·42	+ 0·24

The recorded signal-times at Greenwich ought therefore to be increased on this account by + 0<sup>s</sup>·173.

The combination of these two corrections produces +0<sup>s</sup>·11 to be applied to the previously-determined mean of difference of local times of signals. And thus we finally obtain for the difference of longitude between the Transit-Circle-Room at the Royal Observatory and the Knightstown Station at Valencia,

$$41^m. 9^s. 81.$$

16. The west longitude of the Feagh Main Station was found by the operations of 1844 to be 41<sup>m</sup>. 23<sup>s</sup>·23. I am informed by Captain Clarke that the geodetic connexion of the Feagh Main Station with the Knightstown Station gives for the interval of longitude 13<sup>s</sup>·562, by which quantity Knightstown is East of Feagh Main. The west longitude of Knightstown therefore, as inferred from the operations of 1844, would be 41<sup>m</sup>. 9<sup>s</sup>·67. The agreement of these results is very remarkable, and gives great confidence in all the results of the former operation, for Liverpool and Kingstown as well as for Feagh Main.

G. B. AIRY.

*Royal Observatory, Greenwich,  
1863, March 2.*