THE COMING TRANSIT OF VENUS *

V.

IT is probable that the observations of contact will be very materially supported by additional observations made with the double-image micrometer. This instrument was devised many years ago by Sir George Airy.† It is the most convenient eye-piece micrometer which can be used for measuring the distance between a pair of stars, or, as in the present case, between the limbs of the sun and Venus. The peculiarity of Airy's double-image micrometer consists in this, that one of the lenses forming an ordinary terrestrial eye-piece is divided in two, like the object-glass of a heliometer. The one half can be slid past the other, and the amount of displacement accurately measured by a divided circle, concentric with the screw which gives this motion. When the halves of this lens are relatively displaced, two images of the object are seen, as in the heliometer. If the distance between a pair of stars be the subject of measurement, the line of separation of the half-lenses is made to coincide with the line joining the two stars. The screw is now turned in one direction, until the image of one star given by one half of the lens coincides with the image of the other star given by the other half of the lens. The amount of displacement is now read off. The halves of the lens are again brought to coincidence. The screw is now turned in the opposite direction, and a similar observation made. Knowing the value of the divisions on the divided circle, these two observations give us a means not only of determining the distance between the two stars, but also of fixing accurately the reading of the instrument when the half-lenses are in coincidence.

It is easy to see that after the internal contact at ingress, and before the internal contact at egress, measurements may thus be made of the distance of Venus from the sun's limb, from which the true time of contact may be deduced, just as in the Janssen photographic method.

But, besides, this double-image micrometer gives a means of estimating the true time of contact in a manner which may possibly be one of very great accuracy indeed. Consider the case of ingress two minutes before the time of true contact. From this time up to the actual contact the distance between the cusps, where the limbs of Venus and the sun meet, will diminish with very great rapidity. By turning the micrometer so that the line of junction of the half-lenses is in a line with the points of these two cusps, the distance between them may be very accurately measured. The observation may be repeated a number of times. The great rapidity with which these cusps approach, with a very slight motion of the planet, makes it probable that each of these observations will give the means of determining very closely the true time of contact.

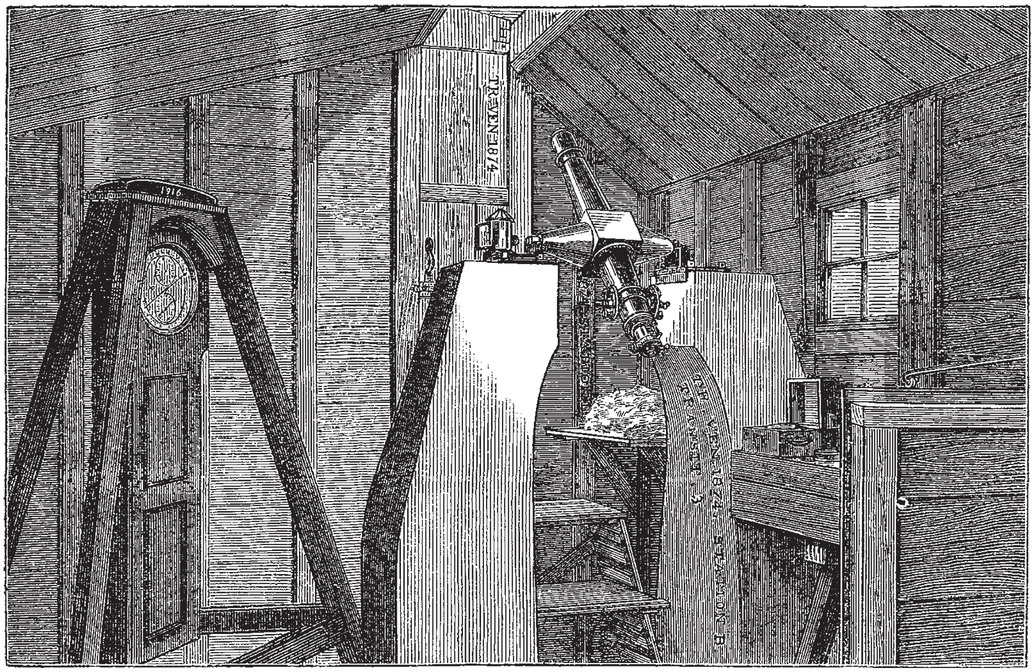

FIG. 16.—The Transit-instrument of the British Expedition.

There are great difficulties connected with observations of the sun at such low altitudes as are required for the application of De l'Isle's and other methods. These will materially affect the definition of the cusps, and it is not certain that the micrometer method will give results so valuable as might have been anticipated.

But even in the eye-observation of contact the low altitude of the sun will be a serious drawback. This difficulty has been fully recognised by the Astronomer Royal, and, with the assistance of Mr. Simms, he has devised an ingenious eye-piece, which is likely largely to reduce the inconvenience.* The chief difficulty is, that at such low altitudes not only are the rays of light enormously refracted by the earth's atmosphere, but the colours are actually dispersed, as with a prism. Hence the definition cannot be perfect. The principle of the new eye-piece consists in employing a hemispherical lens for the one next the eye. The surface of this lens next to the eye is plane; and the lens can be moved, by means of a screw and slight spring, in a socket which is a portion of a sphere the same radius as the lens. By turning the screw, various inclinations can be given to the plane surface next the eye. But the curvature of the other surface remains the same, though a different portion of it is used. The practical result, then, of such an inclination of the lens in its socket is simply the introduction of a prism whose angle can be so varied as to correct totally the atmospheric dispersion.

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* Continued from p. 30.

† Greenwich Observations, 1840.

* Monthly Notices of the R.A.S. vol. xxx. p. 58.

But in the case of photography the low altitude of the sun introduces a much more serious difficulty. The light has in this case to pass through a great length of the earth's atmosphere, in its lowest and densest regions. Much of the light is absorbed by the atmosphere, as is shown by the fact that the rising or setting sun may be gazed at with impunity. But further, it is found that of all the colours composing the sun's light, those which affect most powerfully a photographic plate are the most greedily absorbed. Hence it has been found at St. Petersburg that at mid-winter a photographic plate must be exposed to the sun 360 times as long as at the equinoxes, when the altitude of the sun is about 6° or 7°. This is a difficulty which cannot be surmounted except by exposing the plate a longer time than is desirable.

It has been already stated that considerable discrepancies in determining the times of contact might arise from observers noting different phenomena. The employment of the Model Transit of Venus ensures concordance among the observers of each nation; but all European observers will be much indebted to M. Struve, who has actually compared his own observations with those of the Russian, German, English, and French observers, so that comparisons will be possible between the observations of these different nations.

Everything being now prepared for observing as successfully as possible the actual phenomenon of contact, it remains to describe the means by which the time can be determined accurately. All clocks and watches are set and regulated by observations of the stars, or by comparison with other clocks so regulated. An astronomical clock counts the hours up to 24h. The clock is set to oh. at the instant when a particular star passes the meridian. If then we have a means of determining the time when this happens, we can set our clock accurately to local time. But a star does not pass the meridian of Greenwich at the same time as it passes the meridian of a place having any other longitude. By the aid of a transit instrument the local time can be determined; but to determine actual Greenwich time at another place we must, as before stated, know accurately the longitude of that station. *These two things, the absolute time and the longitude, are so connected, that if we know the one, the other can be immediately deduced.*

The longitude may be determined in a variety of different ways. If the two places whose difference of longitude is to be determined be not very distant, a simple method may be employed. A rocket is sent up from some point between the two stations. An observer at each station notes the local time at which the rocket is seen to burst. The difference between these times gives the difference of longitude. A flash from a lamp or reflected sunlight may be similarly employed.

The absolute time (and consequently the longitude) can also be found by transporting chronometers from one station where it is known to another where it is not known. First-rate chronometers must be used, and a large number to check one another's errors. The main error of a chronometer is due to the influence of temperature on the momentum of the balance wheel and the strength of its spring. The Russians have of late years introduced with great success a method of secondary correction for this error. Along with the compensated chronometer at least one is sent without any compensation. The difference between this chronometer and others is a measure of the sum total of the temperatures to which they have been exposed; and by the aid of a table carefully drawn up from a number of observations, the amount of secondary correction necessary can be fairly estimated. It is said that the employment of this device is of the very greatest service. Ten well-tried chronometers, accompanied by a single uncompensated one, if carried between stations ten days apart (*e.g.* St. Petersburg and Cazan) will, in one journey, give the longitude of an intermediate station (such as Moscow) correctly within $\frac{1}{10}$ of a second of time. By the aid of this contrivance chronometers may be employed, even for very long journeys, to determine the longitude. This method is quite new, and has not been tested by any nations except the Russians. The results obtained by them are, however, perfectly satisfactory. Theoretically the idea is almost perfect; the outstanding temperature error being the main fault of chronometers, and the employment of an additional chronometer uncompensated giving us a means of determining the amount of this error, the time deduced by this means ought to give very satisfactory results. There is but one objection to the method, which is only a partial one. After a series of alternately very hot days and very cold nights, the difference between the compensated and uncompensated chronometers might be the same as after the same period, with a tolerably uniform temperature; but the correction necessary in these two cases might be very different indeed. It is easy, however, to keep chronometers at a temperature which does not vary rapidly, and the experiments made by the Russians warrant us in saying that by the aid of this method longitudes may be determined, with very great accuracy indeed, in voyages of such length that the ordinary chronometric method would be unavailing, and that in every case where longitudes are required by the use of chronometers this method should be employed.

A third way of determining the absolute time is by the use of telegraphic signals. An operator at Greenwich may arrange to telegraph a signal to another at Alexandria at a certain definite time of day. If the transmission of the current from Greenwich to Alexandria were instantaneous the person at Alexandria would at that instant receive the exact time. But a current through a submarine cable is retarded. Suppose it to be retarded two seconds; the time received at Alexandria will be *too late* by two seconds. If now an operator at Alexandria telegraphs to Greenwich he will dispatch the signal two seconds *before* it reaches Greenwich. The longitudes determined by the two currents in opposite directions will therefore differ by four seconds. The mean of these values gives the true longitude, and half the difference between the two determinations is the time of transit of the currents. It is found, however, both from theory and experiment, that if there be a leak in the cable nearer to Greenwich than to Alexandria the current will pass more slowly in going to Alexandria than in the reverse direction. This difference, however, can never be very great.

Considerable differences have been found by the Americans to exist between comparative observations of longitude by the telegraphic method and by the lunar method, which will presently be described. The Americans rushed to the conclusion that the error existed in the lunar method. This is not necessarily so. The American system of telegraphing over long distances consists in using a *relay*. A relay is an arrangement to overcome the difficulty of sending a current through a long line. It is placed at an intermediate station. It consists essentially of an electro-magnet which attracts a piece of iron when a current which has originally been sent through the primary station passes through its coils. This attraction of a piece of iron makes contact with a new electric circuit with a separate battery, and so the current is passed on to the final station, or to a second relay. The piece of iron must move through a sensible distance before the second circuit is completed. It has hitherto been supposed that the time lost in employing a number of relays could be eliminated by sending the current in alternate directions as above described. This is certainly not the case. The time elapsing before contact is made by a relay depends upon the strength of the current. The strength of the current depends upon the length of the wire through which it is passing, and also upon the strength of the battery. Consider now the case of a relay at the junction of a long and short wire. The current passing through the long wire is weaker than the other. Hence if the current first pass through the short wire, the loss of time introduced by the relay is less than when the current is first sent through the long wire. For this reason the time taken by the current to pass in one direction is less than in the other direction. It appears then that the employment of a number of relays is injurious in longitude determinations, and if extraordinary precautions be not taken the resulting longitude will be erroneous. The same takes place with a submarine cable, with a leak near one end of it.

It must be noticed that in all the methods here described for determining the longitude, the local time must be accurately known. This is done by aid of a transit instrument as before described. One of the transit instruments of the British Expedition, in its wooden hut, is shown in Fig. 16.

Another class of method for determining the longitude depends upon the motions of the moon. It has already been stated that what we want is to know at some instant the absolute Greenwich time. If then we could get something analogous to a huge clock in the heavens which an observer at any part of the world could see we should be able to determine our longitude. The moon may be taken to represent the hand of such a clock, and the stars the hours and minutes. The moon is chosen in preference to the planets because she moves more rapidly among the stars. She moves around the earth, that is through 360°, in 27½ days, or through 1° in two hours, or through one second of arc in two seconds of time. If then the tables in the *Nautical Almanac* predicting the place of the moon are absolutely correct, an observer by watching the instant at which she seems to come to the position of any star, and knowing from the tables the Greenwich time at which she reaches that position, receives an intimation of the absolute time from this gigantic celestial clock. Or, if there be no star, it will suffice to observe the time when the moon reaches any definite position among the stars. As a matter of fact the tables of the moon are by no means perfect; but this difficulty is overcome by the regular series of observations of the moon's place made at Greenwich on every possible occasion. Thus while the tables are sufficiently accurate to give the navigator a fair knowledge of his longitude, an observer in any country can, when convenient, compare his observations with those made at Greenwich, and so determine the longitude with great accuracy.

It is a fact of interest in connection with the present subject, that the transits of Venus will aid materially in perfecting the Lunar Tables. The motions of the moon are rendered irregular by the disturbing attraction of the sun. But we cannot determine with great accuracy either the amount or the direction of the sun's attraction upon the moon until we know accurately the sun's distance. Hence if we wish to be able to compute tables of the moon sufficiently correct for the exact determination of longitude, we must employ every means in our power to perfect our knowledge of the sun's distance.

Of the methods available for determining the moon's position, three will be employed in the coming transit. The first is by observing, with a powerful telescope, the exact time at which the moon extinguishes the light of a star in front of which it is passing. This is technically called an occultation of a star by the moon; and when the occultation is made by the non-illuminated portion of the moon the observation has great precision, and, the position of the star being known, is very valuable for determining longitude.

The second method is by observing, with a transit instrument, the exact time at which the moon passes the meridian, and by observing about the same time the transits of stars whose positions are well known.

The third method is by employing an instrument called an altitude-and-azimuth instrument, or shortly, an alt-azimuth. This instrument is shown in Fig. 17, and consists essentially of a telescope mounted upon two divided circles so arranged that the one shall give the altitude of an object towards which the telescope is pointing, while the other gives its azimuth or its angular distance from the meridian measured in a horizontal direction. An instrument of this class has long been employed at Greenwich with great success for determining the position of the moon when out of the meridian. It thus acts as a supplement to the transit-circle, of the utmost value in so cloudy a climate as our own. One disadvantage of this instrument is that the numerical reductions are extremely troublesome; but no trouble is too great in an observation of so much importance.

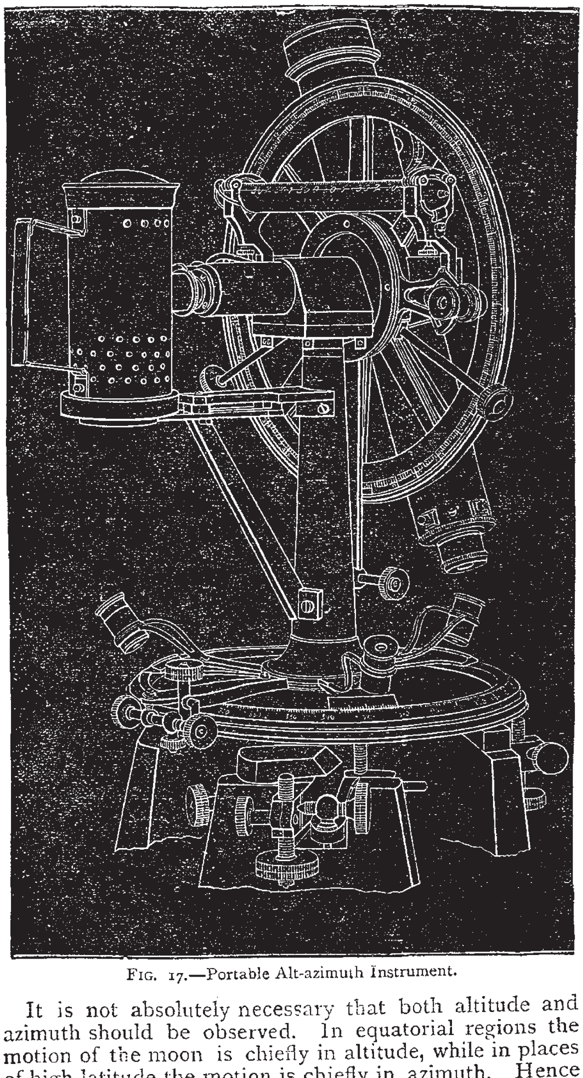

Fig. 17.—Portable Alt-azimuth Instrument.

It is not absolutely necessary that both altitude and azimuth should be observed. In equatorial regions the motion of the moon is chiefly in altitude, while in places of high latitude the motion is chiefly in azimuth. Hence among the English stations the vertical circles alone are provided for the stations within 30° of the equator, while at Rodriguez, Kerguelen's Island and New Zealand the azimuth circles are accurately divided. All these instruments have been well tested, and are found to be remarkably perfect. Not only the alt-azimuths but also most of the other instruments to be employed by the British have been constructed by Troughton and Simms; they have all been well tried, and the results have been so satisfactory that these makers deserve great credit for the help they have thus given to the success of the expeditions.

In all observations of the moon for determining the longitude there are of course numerous corrections which must be applied. Among these none is more important than the correction for the parallax of the moon.

RECAPITULATION.—In the case of every nation depending upon De l'Isle's method and in the case of every expedition when only one contact is observed, the longitude must be determined with very great accuracy. This can be done by any of the following methods :—

1. By rockets, or flashing signals.
2. By a trigonometrical survey.
3. By the aid of chronometers, in which it would be unwise to neglect the method lately introduced of adding to the chronometers one which is uncompensated.
4. The telegraphic method, in which it is not desirable to use relays, since very long lines with a Thomson's reflecting galvanometer will give good results, while the employment of relays is objectionable.
5. By observations of the moon's position which may be made by either of the three following methods :—
   - (α) By occultations of the moon.
   - (β) By transit observations of the moon and moon-culminating stars.
   - (γ) By aid of an alt-azimuth.

GEORGE FORBES

(*To be continued.*)

OCEAN CURRENTS

I OBSERVE that in NATURE, vol. ix. p. 423, Dr. Carpenter re-states and maintains his opinion that polar cold rather than equatorial heat is the *primum mobile* of his general oceanic circulation. In my papers in the *Philosophical Magazine* for Oct. 1871 and Feb. 1874 I have proved, I trust, to the satisfaction of any physicist who will be at the trouble to examine what I have written on the subject, that this notion is based upon a confusion of ideas in regard to the way in which difference of specific gravity produces motion. It is not my object at present to enter into any further discussion of this elementary matter ; but I wish briefly to refer to a new and somewhat plausible-looking objection advanced in Dr. Carpenter's article against the views I advocate in reference to under-currents. The following is the paragraph to which I refer :—

"According to Mr. Croll's doctrine the whole of that vast mass of water in the North Atlantic, averaging, say, 1,500 fathoms in thickness and 3,600 miles in breadth, the temperature of which (from 40° downwards), as ascertained by the *Challenger* soundings, clearly shows it to be mainly derived from a polar source, is nothing else than *the reflux of the Gulf Stream.* Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf Stream to be about 1-900 that of the North Atlantic underflow ; and as it is admitted that a large part of the Gulf Stream returns into the Mid-Atlantic circulation, only a branch of it going on to the north-east ; the extreme improbability (may I not say impossibility ?) that so vast a mass of water can be put in motion by what is by comparison a mere rivulet, the north-east motion of which as a distinct current has not been traced eastward of 30° W. long. seems still more obvious."

The objection seems to me to be based upon a series of misapprehensions : (1) that the mass of cold water 1,500 fathoms deep and 3,600 miles in breadth is in a state of motion towards the equator ; (2) that it cannot be the reflux of the Gulf Stream, because its sectional area is 900 times greater than that of the Gulf Stream ; (3) that the immense mass of water is, according to my views, set in motion by the Gulf Stream.

I shall consider these in their order : (1) That this immense mass of cold water came originally from the polar regions I of course admit, but that the whole is in a state of motion I certainly do not admit. There is no warrant whatever for any such assumption. According to Dr. Carpenter himself the heating power of the sun does not extend to any great depth below the surface ; consequently there is nothing whatever to heat this mass but the heat coming through the earth's crust. But the amount of heat derived from this source is so trifling that an under-current from the Arctic regions far less in volume than that of the Gulf Stream would be quite sufficient to keep the mass at an ice-cold temperature. Taking the area of the North Atlantic between the equator and the tropic of Cancer, including also the Carribean Sea and the Gulf of Mexico, to be 7,700,000 square miles, and the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, we find that the total quantity of heat derived from the earth's crust by the above area is equal to about 88×10^{15} foot pounds per day. But this amount is equal to only 1-894th of that conveyed by the Gulf Stream, on the supposition that each pound of water carries 19,300 foot pounds of heat. Consequently an under-current from the polar regions of not more than $\frac{1}{35}$ the volume of the Gulf Stream would suffice to keep the entire mass of water of that area within 1° of what it would be were there no heat derived from the crust of the earth. That is to say, were the water conveyed by the under-current at 32°, internal heat would not maintain the mass of the ocean in the above area at more than 33°. The entire area of the North Atlantic from the equator to the Arctic circle is somewhere about 16,000,000 square miles. An under-current of less than $\frac{1}{17}$ that of the Gulf Stream coming from the Arctic regions would therefore suffice to keep the entire North Atlantic basin filled with ice-cold water. In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions either in the form of under-currents or in the form of a general underflow, as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water, for there is no source from which this influx of cold water can derive heat save from the earth's crust. But the amount thus derived is so trifling as to produce no sensible effect. For example, a polar under-current one-half the size of the Gulf Stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient, under such circumstances, to maintain the mass 1° F. above the temperature it possessed when it left the polar regions.

(2) But suppose that this immense mass of cold water occupying the great depths of the ocean were, as Dr. Carpenter assumes it to be, in a state of constant motion towards the equator, and that its sectional area were 900 times that of the Gulf Stream, it would not therefore follow that the quantity of water passing through this large sectional area must be greater than that flowing through a sectional area of the Gulf Stream, for the quantity of water flowing through this large sectional area depends entirely on the rate of motion.

(3) I am wholly unable to understand how it could be supposed that this underflow, according to my view, is set in motion by the Gulf Stream, seeing that I have shown that the return under-current is as much due to the impulse of the wind as the Gulf Stream itself.

I am also wholly unable to comprehend how Dr. Carpenter should imagine that because the bottom temperature of the South Atlantic should happen to be lower,