On the Means which will be available for correcting the Measure of the Sun's Distance, in the next twenty-five Years. By the Astronomer Royal.

At the Meeting of the Society, on the 8th of April, the Astronomer Royal gave an oral statement "on the means which will be available for correcting the measure of the Sun's distance in the next twenty-five years;" the substance of which is contained in the following abstract.

The members of the Society will not be surprised at our looking so far in advance as twenty-five years. The special opportunity, which will then present itself, is the last which will occur for nearly a century and half from the present time. Some years of preparation will be required to enable us to secure the full advantages which may then be within our reach. But, with all possible care, it will be found that the risk of total failure is not inconsiderable. The recognition of this danger naturally leads us to examine whether there will not be some earlier opportunity, of a different kind, for arriving at the same determination. And it will appear (in the judgment of the Astronomer Royal) that circumstances will be favourable, in the course of a few years, for obtaining a very good measure by the use of a different principle; less accurate, undoubtedly, in each of its individual applications than the method upon which reliance has usually been placed, but admitting of almost indefinite repetition, demanding no co-operation of distant observers, and requiring only that, in each instance, the observations which are to be compared be made with the same instrument and by the same observer (or with observers only so far changed that any personal equation would correct itself). But even this method requires appliances, which cannot be constructed at the moment of observation; and it is necessary to study well, some time before the operations shall actually commence, what equipment, instrumental and literary, is desirable for giving the best chance of success. It will appear that we are not beginning too soon to direct our attention to these matters in the present year.

The measure of the Sun's distance has always been considered the noblest problem in astronomy. One reason for this estimation is, that it must be commenced as a new step in measures. It is easy to measure a base-line a few miles long upon this Earth, and easy to make a few geodetic surveys, and easy to infer from them the dimensions of the Earth with great accuracy; and, taking these dimensions as a base common to every subsequent measure, it is easy to measure the distance of the Moon with trifling uncertainty. But the measure of the Moon's distance in no degree aids in the measure of the Sun's distance, which must be undertaken as a totally independent operation. A second reason is that, in whatever way we attack the problem, it will require all our care and all our ingenuity, as well as the application of almost all our knowledge of the antecedent facts of astronomy, to give the smallest chance of an accurate result. A third reason is, that upon this measure depends every measure in astronomy beyond the Moon; the distance and dimensions of the Sun and every planet and satellite, and the distances of those stars whose parallaxes are approximately known.

The received measure of the Sun's distance depends on the transits of *Venus* of 1761 and 1769, but mainly on the latter. Very careful discussions of these will be found in the two books published by Encke, and in a memoir of great value by Don Joachim Ferrers, printed in our own *Memoirs*. On examining these, it will be found that, though there is very close accordance in the results obtained by the different investigators and from the different transits, yet all investigators have expressed their doubts upon those results. In the transit of 1761, the result depended almost entirely upon an accurate knowledge of the differences of longitude of very distant stations, which are undoubtedly subject to great uncertainty. In the transit of 1769 it happened that the result depended almost entirely upon the observations made by Father Hell at Wardhoe; and to these great suspicion has attached, many astronomers having, without hesitation, designated them as forgeries. It is evidently desirable to repeat the practical investigation when opportunity shall present itself.

It is desirable, for clearness, to begin with a reference to the simplest operation for measuring distance by parallax; as applied, for instance, to the Moon. In figure 1, let A and B be two observatories on the same meridian; and at A let the star C be observed to touch the Moon's limb, and at B let the star D be observed to touch the limb. (It will readily be understood that it is not essential that the observatories should be on the same meridian; if, as is in fact true, the Moon's apparent change of place can be exactly computed; nor is it necessary that the star touch the limb, if its angular distance can be very exactly measured.) After communication of the observations, the observer at A can measure the angle CAD. This angle differs from AMB by the angle ADB; but such is the distance of the stars, that the angle ADB is in every case unmeasurably small; and AMB, therefore, is to be taken as equal to CAD. Now, the dimensions of the Earth being known, the length and direction of the line AB will be known; and the directions of AM, BM, are known; and therefore the length of AM, BM, or of any other line drawn from M to any other part of the Earth, is easily found. A small error in the angle at M, that is, in the angle CAD, will produce a great error in the result for AM or BM. With this caution the problem is completely solved.

The question naturally rises, Cannot the same method be applied to the Sun? Practically, it cannot, for the following reasons. First, if errors of equal amount were committed in determining the inclination of the two lines AM, BM, for the Sun and for the Moon, their effects on the results would be enormously unequal. Thus, if the error were 2″, it would produce an error of 100 miles in the Moon's distance; but it would produce an error of 16,000,000 miles in the Sun's distance. Secondly, no stars can be seen for observation in apparent contact with the Sun's limb. Thirdly, if for want of observable stars we rely upon the instrumental measure of the angular elevation of the Sun's limb, we introduce the risk of instrumental errors, and (far worse) of errors in the computation of atmospheric refraction at the most unfavourable of all times of observation; and these are sufficient completely to vitiate the method.

In consequence of these difficulties, astronomers have always sought to determine the distance of the Sun indirectly, by determining the distance of a planet, either by referring the planet's apparent place to stars, or by referring it to the Sun. In order to make this indirect process available, it is necessary to rely upon the *antecedent* determination of the *proportion* of the distances of the different planets from the Sun.

It is a historical fact that, in the time of Copernicus and Keppler, when astronomers did not know whether the Sun's distance from the Earth was nearer to ten millions or to a hundred millions of miles, the *proportion* of the distances of the different planets was known almost as exactly as at present. The first and rudest means of obtaining these proportions may be understood from figure 2. Commence with the assumption that the planets move in circular orbits. At the Earth E, the apparent angle S E V between the Sun and *Venus* reaches, but does not overpass, a certain value. At this time, then, the angle E V S is a right angle. Therefore, in the triangle E V S two angles are known (namely, at E and at V), and therefore the proportions of the three sides can be found; and two of these sides are the distances of the Earth and *Venus* from the Sun. Again, conceive that from the Earth E′ the planet *Mars* is seen in the direction E′M′. By an acquaintance with the movements of *Mars*, derived from the observations of many preceding years, it is known that his position, as seen from the Sun, is in the direction S M′. The angular difference between these two directions is the angle S M′E′. Also we know the angle S E′ M′, the apparent angular distance of *Mars* from the Sun. Hence (as in the instance of *Venus*) we know two angles of the triangle S E′M′, and therefore we know the proportion of its three sides, two of which are the distances of the Earth and *Mars* from the Sun. These, at first, are very rude determinations; but they aid materially in introducing more exact ones. It is found by degrees that some alteration must be made in the inferred mean distances of the planets from the Sun; it is found by degrees that this will not suffice, and that the supposition of different degrees of ellipticity and in different directions must be introduced; and at length, by infinite repetitions of the process of trial-and-error, of which scarcely a trace remains, except in the results, proportions of very considerable accuracy are obtained. In all this there is not the smallest reference to any of the absolute distances.

In figure 3 is shown the first practical inference from this knowledge of proportion of distances, as applied to a transit of *Venus*. Let *Venus* V be so exactly between the Sun and the Earth that she can be seen upon the face of the Sun. An observer at A sees her upon the point S, and an observer at B sees her upon the point S′. Suppose the relation between the points S and S′ to be such as to admit of record (the mode of making this record will be considered shortly), and suppose, by means of that record, the angle S A S′ is measured. The angle which we desire to obtain, in order to measure the Sun's distance, is A S′ B. Now the proportion of our measured angle S A S′ to the desired angle A S′ B is sensibly the same as the proportion of S′ V to A V, or as 72 : 28, very nearly. Thus it appears that we measure a large angle in order to infer from it a small one: and this is the circumstance which is the most favourable of all for obtaining an exact result. (If we tried to use a transit of *Mercury* in the same way, it would be found that the measured angle at A is to the required angle at S′ in the proportion of 4 to 6 nearly, that is, that we measure a small angle in order to infer from it a larger; hence the transits of *Mercury* are inapplicable to the measure of the Sun's distance.) It is further to be considered that in this reference of the apparent place of *Venus* to the disk of the Sun, no use is made of stars, and nothing depends on the difficulty of computing refraction, inasmuch as *Venus* and the Sun are, at the time of the observation, subject to the same refraction.

This method then appears likely to be excellent, provided that we possess a practical process for measuring the angle S A S′. The mode of finding this will be our next consideration.

In the central diagram of figure 5 is represented by a black line the path which *Venus* will appear to describe across the Sun's disk in the transit of 1882 (reversed in regard of right and left, for the convenience of subsequent investigations) as seen from the centre of the Earth. For the present let us lay aside the consideration of the Earth's rotation. An observer in the northern portion of the Earth will see *Venus* describe, not the black line, but the fainter line below the black line and parallel to it. An observer in the southern portion of the Earth will see *Venus* describe the fainter line above the black line. The path seen by the southern observer is longer than that seen by the northern observer, and therefore occupies a longer time. Consequently the mere observation of the duration of the transit at these two stations would give information on the lengths of the two chords, and therefore would give means of computing the amount of separation of the two chords: and this apparent separation corresponds to the angle S A S′ in figure 3. We have therefore all the means of computing the angle A S′ B, and of inferring from it the Sun's distance: although, as may be imagined, the intervening calculations are sufficiently complicated.

But this is on the supposition that the Earth has no motion of rotation. Let us introduce the consideration of rotation, and see how it modifies the result.

In the right-hand diagram of figure 5 is represented the face of the Earth, which is turned towards the Sun at the ingress of *Venus* on the Sun's disk; and in the left-hand diagram of the same is the face which is turned towards the Sun at the egress. The reversed form given to the solar disk in the central diagram refers all lines of the three diagrams to corresponding geometrical direction. Now, fixing our attention on a northern station, in the United States of America for instance, it will be seen that the translation of this place by the movement of rotation carries it to meet the motion of *Venus*. Consequently it tends to shorten the duration of the transit. But by virtue of the northerly position of that station, the duration of transit is already shortened. Consequently, by combination of these two effects, the duration of the transit at the northern station is very much shortened.

Now can we select a southern station such that the same rotation of the earth shall tend to lengthen the duration of transit as seen there? The transit at southern stations is already the longer by virtue of their southern position: and if to this we could superadd a further lengthening by virtue of the Earth's rotation, we should have a very long duration of transit there which we might hope to compare with the very short duration at the north station, with the prospect of obtaining a combination which would be most advantageous for obtaining the measure sought.

We can select such a station.

It is essential to remark that the transit will take place in the month of December, and that at that time the Earth's south pole will be turned towards the Sun, and therefore that those regions of the Earth which are included between the south pole and the southern limit of illumination will be carried by rotation in a direction opposite to the direction of movement of all the northern parts of the Earth. If we fix our attention on a small darkly-shaded part of the Antarctic Continent; and if we compare its positions in the right-hand diagram and the left-hand diagram, it will be seen that it is carried in the same direction as *Venus*, that the apparent movement of *Venus* is therefore made slower, and that the duration of transit is thereby lengthened. And as it is lengthened already by the southern position of the station, it will by the combination of these causes be very much lengthened. Comparing this with the observed duration in the United States, where it is very much shortened, we shall have a large difference, depending entirely upon the proportion which the Earth's radius bears to the distance of the Sun, and most favourably available for the determination of that proportion. The difference of the times of duration would probably be not less than twenty-five minutes.

Thus the circumstances of the transit of 1882 are peculiarly favourable (subject only to certain practical considerations, to be noticed hereafter,) for the determination of the proportion of the

Earth's radius to the Sun's distance (usually called the Sun's horizontal parallax, or more strictly the sine of the Sun's horizontal parallax). A discussion of the transit of 1874 will show what are the conditions on which this favourable state depends.

In figure 4, where *Venus* is seen crossing the northern part of the Sun's disk, it will be perceived that the northern station has (independently of the Earth's rotation) the longer duration of transit, and the southern has the shorter. Now when we introduce the consideration of rotation, no selection of stations on the principle adopted for 1882 will tend to exaggerate this difference. If both stations are on the north side of the south pole, the movement of rotation shortens both durations in no very unequal degrees. If we take a station between the south pole and the southern limit of illumination, the motion of rotation tends to lengthen the duration which by virtue of southernly position is shorter, and thereby the inequality is diminished. Thus it appears that the transit of 1874 cannot be used with the same advantage as that of 1882 for determining the Sun's horizontal parallax.

An examination of the characteristics of these transits, and also of those which occur in the month of June (as 1761 and 1769) when the north pole of the Earth is turned towards the Sun, suggests the following remarks. The transits favourable for the determination of the Sun's horizontal parallax are those in which the part of the Sun's disk crossed by *Venus* has the same name (north or south) as the pole of the Earth which is turned towards the Sun. Now in general (but not always) the transits of *Venus* will occur in pairs (as 1761–1769, 1874–1882), with an interval, in each case, of eight years. This interval depends on the circumstances that the transits can only be visible when the conjunction of the Earth and *Venus* takes place very near to one of the nodes of the orbit of *Venus* on that of the Earth; and that in eight years *Venus* has revolved almost exactly thirteen times, so that a conjunction at any one degree of heliocentric longitude is followed by a conjunction very near to the same degree after an interval of eight years. But in consequence of the proportion of 8 : 13 being not quite exact, and because in eight years *Venus* revolves a little more than thirteen times, the successive conjunctions take place in $2\frac{1}{2}$ days *less* than 8 Julian years. Therefore at the second conjunction *Venus* is less advanced in respect of the node than at the first. At the December conjunctions *Venus* is near the ascending node, at the June conjunctions she is near the descending node. In the former, therefore, she will be at the second transit more southerly, and in the latter more northerly, than at the first transit. These indications correspond with those of favourable transits. Therefore in all cases the second transit of each pair is the more favourable for determining the Sun's horizontal parallax. The exceptional case is when *Venus* crosses the middle of the Sun's disk, as then the latitude of *Venus* is too great at both the next preceding and the next following 8-year interval to give a visible transit.

In the explanation up to this point, we have gone on the supposition that the observations of transit to be employed are those of duration of transit. And this method possesses the very important advantage, that it is entirely independent of the assumed longitude of the place of observation. But there is another method, namely, that of observing the absolute time (as referred to Greenwich time) of ingress only or of egress only, at different stations on the Earth. The best way of considering this is to conceive that figure 3 is not in the plane of a meridian, but in the plane passing through the observing-station and through the Earth's centre. Then it is plain that the apparent disturbance of the point S from the point at which *Venus* would be seen from the Earth's centre, is in the plane which passes through the observing-station and through the Earth's centre. Now if this plane is parallel to the Sun's limb at the point of ingress, the disturbance of the apparent place of *Venus* will merely cause its place to slide along the Sun's limb, and will not affect the time of ingress. If the plane is perpendicular to the Sun's limb at the point of ingress, the disturbance will tend to throw *Venus* upon or off the Sun's disk in the greatest possible degree, and therefore to accelerate or retard the ingress in the greatest possible degree. But the observed time of ingress must necessarily be expressed, in the first instance, in local time; this can be converted into Greenwich time only by application of the assumed longitude of the place, and therefore when we compare the Greenwich times of ingress as observed at two stations, the result is necessarily affected by the possible errors of two longitudes. The same remarks apply to the egress.

We are now in a state to consider the applicability of the two methods to the transits of *Venus* in 1874 and 1882. The calculations of the places of the Earth and *Venus*, upon which the diagrams of figure 4 and figure 5 are founded, have been made by Mr. Breen, Assistant to the Royal Observatory, and may be accepted as accurate. At the commencement of this evening's meeting, an independent set of calculations was handed to the Astronomer Royal by Mr. Hind, Superintendent of the Nautical Almanac, which do not sensibly differ from Mr. Breen's. In the exhibitions of the illuminated side of the Earth, the nearest integral hour of Greenwich mean time is taken, because (as will be mentioned) there is yet a little uncertainty on the exact time.

First. On the application of the method of difference of duration of transit to the transit of 1874.

It has already been remarked that in this transit there is no possibility of combining the effect of Earth's rotation with the effect of difference of latitude of stations, so as to exaggerate the difference of durations of transit depending on difference of latitude alone. And if we consider the effect of difference of latitude only, we find that circumstances are not very favourable. The most northerly stations are to be found in Siberia, Tartary, and Thibet (which will scarcely be visited by astronomers in December), on the coasts of China, and in North British India. The most southerly stations will be Kerguelen's Island, Van Dieman's Land, and New Zealand. But the observable difference of duration will probably not be half of that in 1882.

Second. On the application of the method of the difference of absolute times to the transit of 1874.

For the ingress, favourable positions will be found at Owhyhee (where the displacement tends to throw *Venus* upon the Sun's disk, or to accelerate the ingress), and at Bourbon, Mauritius, and Kerguelen's Island (where the displacement tends to throw *Venus* from the Sun's limb, or to retard the ingress). For the egress, Sicily, Italy, and portions of Europe west of the Black Sea, are so situate as to throw *Venus* upon the Sun's disk, or to retard the egress; and New Zealand, New Caledonia, Van Dieman's Land, and Eastern Australia, are well situated for accelerating the egress. But it is doubtful whether the longitudes of any of the stations named, except those in Europe, are yet known with sufficient accuracy.

Third. On the application of the method of difference of duration of transits to the transit of 1882.

It has been already pointed out that there are two tracts, each sufficient to contain a number of observing stations, which are particularly well adapted to these observations. And it is specially to be remarked that the command of a number of stations, sufficiently near together to see the astronomical phenomenon in nearly the same way, but sufficiently separated to take the chances of different states of the sky, is very important. On occasion of the eclipse of 1842, the astronomers at Turin saw nothing, in consequence of the cloudy state of their sky, while the Astronomer Royal on the Superga, not five miles distant in a straight line, saw all the phenomena of the eclipse. Bearing this caution in mind, we will consider the circumstances of the two tracts in question.

The northern tract includes the whole of the United States of North America. The observatories are numerous, and they possess an advantage which even yet is little known in Europe: namely, that from the extent of galvanic telegraph, the habit of using it in the United States, the public spirit of the nation and of the telegraph-companies, which would assuredly induce them to devote that wonderful auxiliary to the exclusive use of astronomy on an occasion so important, and the absence of political suspicions, all the observing-stations would for an observation like this be connected by the galvanic telegraph. (The Astronomer Royal adverts to the political suspicions, not without some bitterness, for he has been prevented by them from using a European telegraph for a single hour to determine the longitude of an important continental point.) The peculiar advantage of connecting, at least of comparing, all the observers' clocks, would be of this kind. Suppose that there were ten observing-stations, and that, in consequence of the changeable weather, the ingress only was observed at five of these stations, and the egress only at the other five. If the clocks of the observatories were not connected or compared, these observations would be totally lost. But if they are connected, then every observation is referred to the absolute time of one clock, say the Washington clock; and from a knowledge of the geographical position, a correction of the absolute time may be computed, so as to deduce, from every observation of absolute time of ingress at any station, what would have been the absolute time of ingress had it been observed at Washington, and from every observation of the absolute time of egress at any station, what would have been the absolute time of egress had it been observed at Washington; and thus we shall have five observations of ingress and five observations of egress, all as if they had been observed at Washington and noted by the Washington clock. Humanly speaking, therefore, we may say that the probabilities for the accurate and efficient observation of these phenomena in the United States are vastly superior to any that could have been reckoned on in any former time, or to any that could now be reckoned on in any other region.

The southern tract is a part of the Antarctic land discovered by Lieut. Wilkes of the U. S. Navy, included between Sabrina Land and Repulse Bay, and occupying an extent of about 400 miles. The Astronomer Royal is informed by General Sabine that the 6th of December is rather early in the season for a visit to this land, but probably not too early, more especially as firm ice will be quite as good for these observations as dry land. It must, however, be borne in mind that it is indispensable to secure observations both of ingress and of egress in this tract, without which all the advantages of the North American observations will be useless. For this purpose it appears absolutely necessary to establish a chain of observing-posts, and to furnish some means of comparing the clocks. We are in possession now of two powers, unknown in former times, applicable to this purpose. One is the galvanic telegraph, which possibly (but not very probably) might be laid down in a temporary way. The other is the use of steamers, by which the observers would be distributed to their several posts, and which would be constantly employed for some days before and some days after the transit, in running up and down the line of coast with a number of chronometers, and comparing them with the stationary chronometers at each observing-post. It would be extremely desirable that the country should be reconnoitred some years before the transit, in order to ascertain at a sufficiently early time the practicability of these or some equivalent plans, without which the risk of entire failure would be great.

Fourth. On the application of the method of the differences of absolute times to the transit of 1882.

For the ingress, the islands of Bourbon, Mauritius, and Kerguelen's Island, are very favourably situated for accelerating the ingress; and the United States of North America for retarding it. For the egress: Van Dieman's Land, Eastern Australia, New Zealand, and New Caledonia, will have the egress much retarded;

while the United States, the West India Islands, and the coast of South America as far as the Rio Plata, will have it accelerated.

In the transits of 1761 and 1769 great difficulty was found to attach to the observations of the internal contact of the limb of *Venus* with the Sun's limb, from the phenomenon which in late years has attracted attention under the name of Baily's beads. The Astronomer Royal expressed his opinion as entirely coinciding with that of Professor Powell, that this phenomenon is simply due to irradiation, as arising partly from diffraction, partly from fault of the telescope, and partly from the nervous excitement of the eye. From his own experience in two total eclipses of the Sun, in which he had taken great pains to see the phenomenon, and had (as he believes simply because he took care to see the Sun very distinctly) been unable to see the slightest trace of it, he had not the smallest doubt that, when proper care is taken for distinct vision, the phenomenon will not be seen at all. He referred specially to his delightful view of the very beautiful phenomenon of the disappearance of the last portion of the Sun in the valleys between the lunar mountains, in the eclipse of 1851; which with less distinct vision would probably have created strings and beads. This distinctness of vision he ascribed principally to the use of a graduated dark glass, constructed under his direction by Mr. Simms. It consists of a long wedge of red glass and a long wedge of green glass, their edges turned the same way, combined with an equivalent wedge of colourless glass, its edge turned the opposite way. Nobody would suppose without trial how fastidious the eye is as to the proper intensity of shade; and how distinctly, when intent on clear vision, it rejects a shade in the most trifling degree lighter or darker. He thought it highly important that such shades should be used for observing the transits of *Venus*. It is desirable also that the colour left by the shade-glass should be agreeable to the observer's eye.

Still there is one caution which must not be put out of sight. The selection of places depends entirely upon the portion of the Earth which is illuminated at the times of ingress and egress; and if the tables of the movements of *Venus* are erroneous in 1882 to the amount of an hour's motion, the illuminated face of the Earth will be altered to the amount of two or three hours' rotation of the Earth, and the selection of stations may be totally changed. It is, therefore, most important that the tables of *Venus* should be thoroughly examined, and where necessary rectified. A great mass of observations of *Venus* exist, already reduced so far as to require only the very last step of substitution of errors of planetary elements. The Astronomer Royal referred particularly to the Greenwich Planetary Reductions from 1750 to 1830, to the reduction of certain Cambridge observations, to the reduction (in the annual Greenwich volume) of the Greenwich observations down to the present time, and to the discussion of some of the Greenwich observations by Mr. Main and Mr. Glaisher. And he took the opportunity of expressing his opinion that fifty pounds spent on calculations with an object like this would confer much greater benefit on astronomy than a thousand pounds employed in the foundation and equipment of an observatory.

On viewing the expense and the risk of the determinations of the Sun's distance by transits of *Venus*, as well as the distance of time, which must necessarily place them beyond the knowledge of many observers of the present day, it appears natural to consider whether other methods cannot be used, less stringent as to the moment of observation, requiring less co-operation of observations, and occurring at an earlier time. Such are the direct determinations of the parallaxes of *Venus* and of *Mars*, when near to the Earth, by simultaneous observations at northern and southern stations as in figure 1, or by successive observations at the same observatory when it is brought to different positions by the Earth's rotation as in figure 6.

Venus cannot be compared with stars on the meridian. She may be compared with stars in extra-meridional observations before sunrise or after sunset, but she is then uncomfortably bright, and rarely well defined; and she has only one illuminated limb admitting of observation, and therefore in the comparison of observations made at different stations there is great risk of error from difference in the estimation of her semidiameter. Moreover she does not remain long in the position nearest to the Earth; and the nearer she is, the more contracted are the daily hours of observation. It seems unlikely that trustworthy results will be deduced from the observations of *Venus*.

The circumstances of *Mars* in opposition to the Sun (figure 6), are much more favourable. *Mars* may then be compared with stars through the whole night: he has two observable limbs, both admitting of good observation: he remains much longer in proximity to the Earth; and the nearer he is, the more extended are the hours of observation.

Here, however, a circumstance is to be considered which has not previously called for attention. The orbit of *Mars* is much more excentric than those of *Venus* and the Earth. At some oppositions, therefore, he will be so far from the Earth that little advantage will be derived from attempting to observe his parallax. (It is understood that such observations were made in the United States' expedition of a few years past, which, from the great distance of *Mars*, must have been nearly useless.) At other oppositions he is almost as near as *Venus* is about conjunction. The following table expresses roughly the distance of *Mars* from the Earth, at some of the nearest and some of the most distant oppositions. The unit of measure is the Earth's mean distance from the Sun:—

1860 about	July 21	0·38
1862	October 1	0·39
1869	February 13	0·68
1871	March 22	0·64
1877	September 3	0·37

The years 1860, 1862, and 1877, are therefore favourable for the determination of parallax. But they require the following special considerations.

When, as in figure 1, the method of comparison of observations made at a northern observatory and a southern observatory is employed, the most favourable position of the planet is that of verticality to the point midway between the two observatories. The north latitude of the northern observatories (Greenwich, Berlin, Pulkowa) is greater than the south latitude of the southern observatories (Cape of Good Hope, St. Jago). Hence, *cæteris paribus*, a north declination of *Mars* will be preferable to a south declination. In this respect the opposition of 1862 is preferable to that of 1860.

But there is another method of making observations for parallax, not applicable to *Venus*, but applicable to *Mars;* namely, by observing the displacement of *Mars* in right ascension, when he is far east of the meridian, and far west of the meridian, as seen at a single observatory. Thus, in figure 6, conceive the pole of the Earth to be turned towards the eye, and conceive the Earth and *Mars* to be stationary in space, the Earth however rotating round its axis. By the diurnal rotation, an observatory is carried from the position A to A′; and at one of these times *Mars* is seen in contact with one star, and at the other time with another star. These observations give the means, as in figure 1, of determining the distance of *Mars*. And though, in fact, both the Earth and *Mars* are moving, yet the effects of those motions can be so exactly calculated as to give to the determination the same accuracy as if both were at rest.

In order to compare the value of this method with that of observations on the meridian at two observatories, we must estimate the length of the base-line, AB in figure 1, or AA′ in figure 6. The greatest meridional base-line, from Pulkowa to the Cape of Good Hope, is = Earth's radius $\times$ 2 sine 47° nearly. The measure of the greatest base-line AA′ depends on the latitude of the observatory. At Greenwich it is = Earth's radius $\times$ 2 sine 38° 30′; at the Cape of Good Hope and at St. Jago (Chili) it is about = Earth's radius $\times$ 2 sine 57°; at Madras it is nearly = Earth's radius $\times$ 2 sine 77°. Thus it appears that at each of the three last-mentioned observatories, the base-line which can be obtained is considerably greater than the best which can be obtained by meridional combination of two observatories. At Madras, the angle to be measured would be about 44″. To this is to be added that the method is attended with no expense whatever; that the observations, which are compared, are made with the same telescope and by the same observer, or the same series of observers; that there is none of the tediousness, the wearying correspondence, or the doubt, which are inseparable from observations requiring distant co-operation; and that the observer is supported by the feeling that his own unassisted observations will give a perfect system of means for deciding one of the most important questions in astronomy. The Astronomer Royal expressed his opinion that this method is the best of all.

In order to use the process to the greatest advantage, *Mars* ought to be visible at six hours' distance from the meridian on each side; and therefore his declination ought to have the same name as the latitude of the observatory. Thus, 1860 will be a favourable year for the Cape of Good Hope and St. Jago; 1862 will be favourable for North American and European observatories. It is scarcely necessary to discriminate between them for Madras, where both years are good; 1862, however, is preferable to 1860.

The first equipment for this observation, on the necessity for which special stress must be laid, is an equatorial, firm in right ascension. Many modern equatorials are deficient in this important quality. It would be well, in using them, to apply a temporary mechanism for fixing the instrument in right ascension, such as its construction may permit. The next, which will be found advantageous, though not strictly necessary, is the apparatus for the American or chronographic method of transits, by which the number of observations may be greatly increased, and something will be gained in the accuracy of each. These, with the ordinary clocks and chronometers, &c., of an observatory, are all that are required.

The principal rules for the observer would be: To make the observations as near as practicable to the six-hour intervals from the meridian, on both sides; and to repeat the observations in continued sequence, morning and evening, morning and evening. If different observers are employed, to take care that each observer is charged as often with morning as with evening observations. To determine the difference between the right ascension of *Mars* and the right ascensions of two stars, one having greater N.P.D., and the other smaller N.P.D. than *Mars*. To use the same stars in at least two observations of different names, morning and evening; and in as many more consecutive observations as can be conveniently arranged. When it becomes necessary to change the selection of stars, to observe both the old pair and the new pair in one morning or evening observation. In all cases to observe, by such alternation as is most agreeable to the observer, both limbs of *Mars* (the preceding and the following). The observations might with advantage commence a fortnight before opposition and terminate a fortnight after it.

In the nature of external preparation, applying generally to all observatories, the principal requisite is a chart of the apparent path of *Mars*, in considerable detail, giving the place of the planet for every hour or every few hours, and giving the places of all the stars, little and great, in its neighbourhood. The observer in possession of this will be able to select stars of such a magnitude as he judges most agreeable to his eye, and at such intervals as will be convenient for his system of wires; and to attend rigorously to the condition of always comparing the planet with two