TRANSITS OF VENUS.

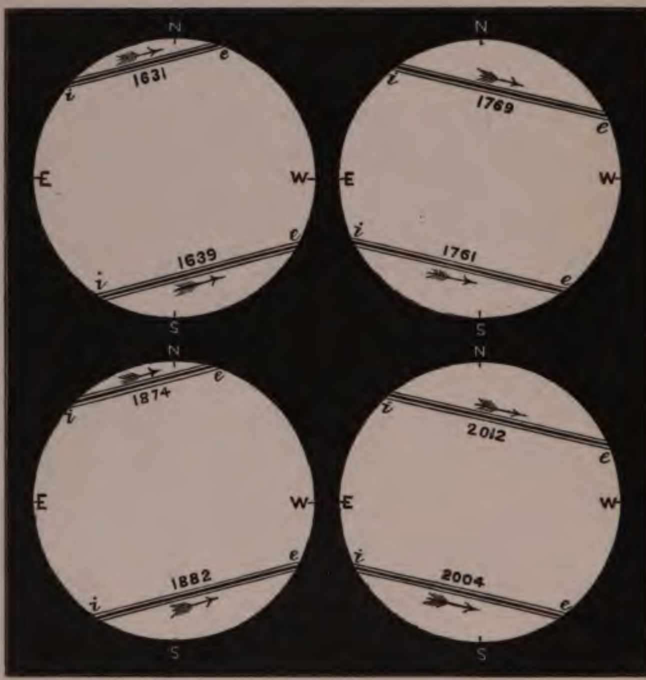
LONDON: PRINTED BY
SPOTTISWOODE AND CO., NEW-STREET SQUARE
AND PARLIAMENT STREET

PLATE I.

PATHS OF VENUS

(MOST NORTHERLY, CENTRAL, AND MOST SOUTHERLY)

ACROSS THE SUN'S FACE



R. A. Proctor del.

DURING THE TRANSITS OF

A.D. 1631, 1639, 1761, 1769, 1874, 1882, 2004, AND 2012.

(The regions where the ingress *i*, the egress *e*, or the whole transit could be seen, are shown in the eight coloured Plates II.—IX.)

TRANSITS OF VENUS.

A POPULAR ACCOUNT

OF

PAST AND COMING TRANSITS

FROM THE FIRST OBSERVED BY [illegible] A.D. 1639

TO THE TRANSIT OF [illegible]

BY

RICHARD A. PROCTOR, B.A. CAMB.

AUTHOR OF [illegible] 'OTHER WORLDS THAN OURS' ETC.

> A spot [illegible]
> Astronomer in the Sun's lucent orb
> Through his glazed optic tube yet never saw.
> [illegible]
>
> [illegible]
> [illegible]

WITH 20 PLATES (12 COLOURED) AND 37 WOODCUTS

LONDON:

LONGMANS, GREEN, AND CO.

1874.

[illegible]

PLATE I.

PATHS OF VENUS

[illegible]

ACROSS THE SUN'S FACE

R. A. Proctor del.

DURING THE TRANSITS OF

A.D. 1631, 1639, 1761, 1769, 1874, 1882, 2004, AND 2012.

(The regions where the ingress i, the egress e, or the whole transit could be seen, are shown in the eight coloured Plates II.—IX.)

TRANSITS OF VENUS.

A POPULAR ACCOUNT

OF

PAST AND COMING TRANSITS

FROM THE FIRST OBSERVED BY HORROCKS A.D. 1639

TO THE TRANSIT OF A.D. 2012.

BY

RICHARD A. PROCTOR, B.A. CAMB.

AUTHOR OF 'SATURN' 'THE SUN' 'OTHER WORLDS THAN OURS' ETC.

A spot like which
Astronomer in the Sun's lucent orb
Through his glazed optic tube yet never saw.
MILTON.

Et vera incessu patuit Dea.
VIRGIL.

WITH 20 PLATES (12 COLOURED) AND 37 WOODCUTS.

LONDON:
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1874.

PREFACE.

THIS WORK is intended to be partly historical and partly explanatory. So far as I know, no book has hitherto been published in England giving a complete account of the transits of 1639, 1761, and 1769, and of the various interesting circumstances connected with them. This want I have endeavoured here to meet, illustrating by maps the conditions under which those transits were observed. In the chapters relating to the transits of 1761 and 1769, I sketch the causes of the partial failure of the observations then made, and give an account of the attempts made in recent years to reconcile those observations with the present estimate of the sun's distance. It will be observed that in dealing with the latest of these attempts I adopt the opinion of Continental and American astronomers, no longer regarding that attempt as in any sense removing the difficulties recognised before it was made.

In Chapter IV. I have given a simple account of the principles on which the recurrence and observation of transits depend.

In the last chapter I carry on the history of the subject to the present time. It would be impossible, as Sir Edmund Beckett points out in the latest edition of his fine work 'Astronomy without Mathematics,' to present the subject adequately without a short account of the occurrences of 1869 and 1873—now belonging to the history of transits, and instructive in many respects. It has seemed to me best to quote the original papers of 1868 and 1869, and then briefly sketch the progress of events which led to the arrangements finally adopted.

The plans of the various scientific nations for the transit now at hand are worthy of the occasion. Astronomers attach just value to the beautiful method of Delisle, while not losing sight of the favourable opportunity presented for applying the simple method invented by Halley. They have wisely noted the fact that all the best Halleyan stations are excellent also for Delisle's method, and have taken such measures, that if bad weather should prevent the beginning or end from being both observed, one or other may still be utilised. In this way new Delislean stations have been obtained by the very arrangements which provided for the employment of Halley's method; and thus the chance of absolute failure through bad weather has been very largely diminished. The long-neglected region in North India has been occupied, and useful observations will doubtless be made there. Southern observing-stations are also now amply provided for—first-class Halleyan stations having been quadrupled in number since last year, when it was pointed out that the want of them endangered the whole scheme of operations.

The suggested Antarctic expeditions for viewing the transit of 1882 have been very properly abandoned.

A brief account is given at the end of Chapter V. of the conditions of the transits of 2004 and 2012.

RICHARD A. PROCTOR.

LONDON: *October* 1874.

For the use of Plates X., XI., XII., XIII., and XIV., I have to thank the Editor of the 'Astronomical Register,' Rev. S. J. Jackson. These pictures originally appeared in the 'Illustrated London News,' for which journal I drew them. Electros were supplied to Mr. Jackson for use in the 'Register,' and were lent to me by him. These plates have also appeared, slightly reduced by some process unknown to me, in the New York 'Daily Graphic.'

CONTENTS.

ILLUSTRATIONS.

PLATES.

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[1] Plates II.—IX., and Plates XVII., XVIII., are to be arranged so that all the titles, TRANSIT OF 1631, 1639, &c., lie towards the left when the book is opened between any pair of plates and held in the usual manner. Plate XX. is to read the other way; that is, it is to have its title, Plate XX., towards the right.

WOODCUTS IN TEXT.

TRANSITS OF VENUS.

CHAPTER I.

TRANSITS OF THE SEVENTEENTH CENTURY.

As soon as the Copernican theory of the solar system was established, astronomers perceived that the inferior planets, Mercury and Venus, must from time to time appear to cross the face of the sun. For although on most of these occasions when either planet passes between the sun and the earth, no transit was to be expected, the planet either passing above or below the face of the sun, yet it could not but happen that, in the course of many such conjunctions, the planet would make a passage at so small a distance north or south of the sun's centre as to appear for a time to be upon the sun's disc. To make this clear, without entering into any nice details at this stage, let E *e* and V *v* (fig. 1) be the paths of the Earth and Venus, respectively, around the Sun, S.[1] Then if we suppose, the path E *e* to lie in the level of the paper, we must imagine one-half of the path V *v*—the half V′ *v* *v*′—to lie above the level of the paper, the other half, *v* V V′, lying below that level—not greatly above or below; in fact, the short white lines near *v* and V show how much these parts of the path of Venus are to be supposed respectively above and below the level of the paper. Still, it will be clear that when Venus is,

Fig. 1.—Showing the paths of Venus and the Earth, and indicating their inclination and the line of intersection of their planes.

as at V, between the Sun and the Earth at S and E, she is not really on the line S E, but considerably below it; and, as supposed to be seen from the Earth, she passes below the Sun. When in conjunction on the other side of V′ *v*′, Venus passes above the Sun. Only when she is in conjunction nearly at V′ (the Earth near E′), or nearly at *v*′ (the Earth near *e*′), will she

[1] E is supposed to be the place occupied by the earth at the time of the autumnal equinox.

appear to cross the face of the Sun. But in the course of many conjunctions there must be some which take place when the two planets are thus placed; that is, either near V′ and E′, or near *v*′ and *e*′. Similar remarks apply to the case of Mercury.

This was early perceived by the followers of Copernicus. The Ptolemaic system did not, indeed, preclude the possibility of such phenomena as transits; but since the older astronomers regarded the planets as shining by their own inherent lustre, it was not to be expected that, even if a transit occurred, the planet would be discernible while crossing the sun's face. And as in reality the Ptolemaic system gave no means of inferring the relative distances of the several planets (including the sun[1]), there could not even be any certain assurance that Venus or Mercury ever came between the earth and the sun. It was only by a mere assumption that the old astronomy assigned to the sun a sphere outside the spheres of Mercury and Venus.

Still we find that, even so far back as the ninth century, Mercury was supposed to have been seen as a dark spot on the face of the sun. Doubtless one of those large sun-spots, which from time to time are visible to the naked eye, had attracted attention, and was regarded by the ignorant as caused by the passage of one or other of the planets, Mercury or Venus, between the earth and the sun. Venus being probably

[1] The sun and moon were 'planets' in the old astronomy; and we still find traces of the usage in some modern expressions.

conspicuous at the time, either as a morning or evening star, the less familiar planet Mercury afforded a convenient explanation of the dark spot. The account of the phenomenon accords well with the belief that a solar spot was seen, for we are told by the author of the 'Life of Charlemagne' that Mercury was visible as a black spot upon the sun for eight consecutive days in April of the year 807. Kepler, who was perfectly well aware that Mercury moves too rapidly to remain even for as many hours on the sun's disc, endeavoured to show that the expression originally used in the manuscript had not been *octo dies,* but *octoties,* a barbaric form of *octies,* for 'eight times.'

It is now well known that Mercury is far too small to be seen by the naked eye when crossing the sun's disc. And this fact disposes of the statement made by the famous physician Ebn Roschd (commonly called Averroës), in his Ptolemaic Paraphrase, to the effect that he saw the planet on the sun in the year 1161, at a time when Mercury really was in inferior conjunction. We need not, however, question the veracity of the learned doctor, seeing that Kepler himself supposed he had seen the planet upon the sun on one occasion. When, a few years later, the existence of sun-spots was detected by the telescope, Kepler admitted that in all probability he had seen such a spot, and not the planet Mercury.

After Kepler had completed his Rudolphine tables of the planetary motions he was able to arrive at tolerably accurate results as to the epochs of the transits of Mercury and Venus over the solar disc. In fact, he announced, in 1627, that in the year 1631 both Mercury and Venus would pass over the sun's face—Mercury on November 7; and Venus on December 6; and that in 1761 Venus would again pass across the face of the sun.

As the first occasion on which the transit of an inferior planet was ever witnessed, the transit of Mercury in 1631 has an interest resembling that which attaches to the first observation of a transit of Venus, eight years later. Therefore I think the reader will be interested to hear how Gassendi succeeded in observing Mercury in transit.

Gassendi made preparations for the observation of the transit at Paris. The manner in which he observed the phenomenon was somewhat remarkable. Through a small aperture in a shutter the solar light was admitted into a darkened room, and an image of the sun, some nine or ten inches in diameter, was formed upon a white screen. A carefully divided circle was traced upon this screen, and the whole was so arranged that the image of the sun could be made to coincide exactly with the circle. As Gassendi was anxious to ascertain the exact moment of the ingress of the planet upon the sun's disc, or—supposing he should fail in that respect—at least to determine the moment of egress, and as he had no trustworthy clock, he determined that the altitude of the sun should be carefully estimated several times during the progress of the transit, and particularly at the moment of egress. It was necessary, therefore, that he should have an assistant, and, further, that his assistant should work in another room; for from the room in which Gassendi was working the sun's light, as I have said, had been carefully excluded, save at the minute aperture in the window-shutter. Accordingly, Gassendi placed his assistant in a room above him, with a large quadrant for taking altitudes, instructing him to observe the height of the sun as soon as he heard Gassendi stamp upon the floor of the room beneath. A clumsy arrangement, truly, when compared with the subtle devices of modern astronomers—with the aid which they derive from powerful telescopes, all but perfect clocks, and, where need arises for communicating with one another from distant stations, the instantaneous indications of telegraphy. Yet we cannot but admire the spirit in which Gassendi worked, the readiness with which, for want of more perfect instruments, he set himself to invent arrangements which suited his requirements, and the skill with which he availed himself of those imperfect adaptations.

And if we admire these qualities in Gassendi, still more must we admire the patience with which he waited for the commencement of the phenomenon. Modern astronomy is able to announce, within three or four minutes, the instant at which a transit will commence at any given spot upon the earth's surface. But Kepler's prediction respecting Mercury's motions did not lay claim to any accuracy of this sort. So uncertain did the epoch of the occurrence appear to be, that Gassendi began to watch for the transit *two days* before the date assigned by Kepler for its occurrence.

The 5th of November proved unfavourable for observation, the day being rainy. The next day was also unsuitable, clouds having overspread the sky during nearly the whole day. The morning of the 7th, the day appointed by Kepler for the transit, was also cloudy. Thus Gassendi began his watch on that day with the uncomfortable feeling that during some part of the two preceding days the planet might already have passed over the sun's disc,—perhaps that the transit had been completed but a few minutes before the clouds broke up on the morning of the 7th.

A little before eight the sun shone for a few minutes through the openings between the clouds, but there still remained enough mist to prevent Gassendi from being able to determine whether any spot existed upon the image of the sun in his observing-room. Nearly an hour passed before the sun was sufficiently clear of clouds to enable Gassendi to make any satisfactory observations. Towards nine, however, the sun became distinctly visible, and turning to the image on the screen, the astronomer perceived upon it a small black spot. He could not believe, however, that this was Mercury, as the received estimate of the planet's dimensions had led him to look for a spot nearly twice as large. As he was familiar with the nature of solar spots, and the rapid manner in which they form, he concluded that one had made its appearance on the sun's surface since the preceding day. At nine o'clock he had another opportunity of observing the spot, and he carefully estimated its position, intending to make use of it as a point of reference for determining the path of the planet in transit, if he should be fortunate enough to witness that phenomenon. Soon after, he had another view of the spot, and was surprised to find that it had moved away considerably from its former position. He felt assured that no ordinary solar spot could have moved so rapidly; but still he could not persuade himself that he was looking at Mercury in transit, having so fully satisfied his mind respecting the dimensions which the planet would exhibit. Besides, the hour had not yet arrived at which Kepler had predicted that the transit would begin.

Gassendi was still in doubt, and endeavouring to recall the circumstances of his former measurement, in order to convince himself that he had made no mistake, when the sun again made his appearance through the clouds, and it was apparent that the spot had moved yet farther from its original place. No room now remained for doubt. It was clear that the phenomenon which had been so long and so anxiously awaited by the astronomer was already in progress. He immediately stamped upon the floor to attract the notice of his assistant. But this person, whose name has not reached us, was possessed of less patience than Gassendi. He probably felt much less interest in the phenomenon; possibly, he placed very little faith in the calculations of Kepler. Whatever was the reason, he had grown weary of watching, and had left his post. Gassendi had to continue his observations alone, hoping that at least his assistant would return before the planet had passed completely off the sun's face. Fortunately this happened; the requisite observations were made for determining the time of egress; and thus an important addition was made to our knowledge of the motions of the innermost planet of the solar system.

Gassendi sent an amusing account of his observations to Professor Shickhard, of the University of Tübingen. 'The crafty god,' he wrote, 'had sought to deceive astronomers by passing over the sun a little earlier than was expected, and had drawn a veil of dark clouds over the earth in order to make his escape more effectual. But Apollo, acquainted with his knavish tricks from his infancy, would not allow him to pass altogether unnoticed. To be brief, I have been more fortunate than those hunters after Mercury who sought the cunning god in the sun. I found him out, and saw him where no one else had hitherto seen him.' He states that the planet, as seen projected on the image of the sun, did not appear altogether black, but was greyish, and somewhat ruddy round the margin. Doubtless these peculiarities were due to the method of observation employed by the astronomer. He estimated the apparent diameter of the spot at about one-ninetieth part of the sun's apparent diameter, an estimate considerably exceeding the true dimensions, but still more considerably below the dimensions which astronomers had been disposed to assign to the planet.

Gassendi, although he did not observe the commencement of the transit, was yet able to compute the time of its occurrence. He found that the transit had begun nearly five hours before the time assigned by Kepler.[1]

I have mentioned that Kepler had predicted that a transit of Venus would take place on December 6, 1631. It need hardly be said that Gassendi, after his

[1] The second observed transit of Mercury took place on November 3, 1651. The observations of Gassendi had enabled astronomers to estimate the epoch of the transit much more exactly than in the former instance. It resulted from their calculations that the phenomenon would not be visible in England, or indeed in Europe; but would be well seen over a large part of Asia. Accordingly a young Englishman, Jeremiah Shakerley, went to Surat, in India, for the purpose of witnessing the phenomenon. Such a journey undertaken for such a purpose in an age when sea-voyages were not only much more protracted, but also far more dangerous than in the present day, must be looked upon as a remarkable and commendable instance of devotion to scientific pursuits. It is pleasing to be able to record that the energy of the young Englishman was rewarded by complete success.

The third observed transit took place on May 3, 1661. It was observed by Hevelius at Dantzic, and at London by Huyghens, Street, and Mercator. Hevelius was surprised to find that the diameter of the planet was very much smaller than he had been led to expect. He found on measurement that Gassendi's estimate was nearly twice as great as the true diameter of the planet.

The fourth transit of Mercury was observed by Halley at St. Helena on November 7, 1677. He was the first astronomer who had ever observed the complete passage of the planet across the solar disc.

Later transits of Mercury have no special historical interest, though observations of considerable importance were made during the transits of 1736, 1799, and 1868.

success in observing the transit of Mercury, had good hopes of observing Venus to even greater advantage. It is true that, according to Kepler's calculation, the transit might be expected to begin only towards sunset, and it was therefore possible that the phenomenon would not be visible at all in Europe. But it was equally possible that any error in the calculation might lie in the other direction, and so the whole transit be favourably seen before sunset. Gassendi took the same measures for observing the transit as in the case of Mercury. He had proposed to observe the sun on December 4 and 5, but 'an impetuous storm of wind and rain rendered the face of the heavens invisible on both those days. On the 6th he continued to obtain occasional glimpses of the sun till a little past three o'clock in the afternoon, but no indication of the planet could be discerned upon the sun's disc as depicted upon the white circle. On the 7th he saw the sun during the whole forenoon, but he looked in vain for any trace of the planet.' 'It is now well known,' proceeds Prof. Grant, from whose account I have just quoted, 'that the transit of the planet took place during the night between December 6 and 7.' I do not know where any calculation of the circumstances of the transit can be found; but an investigation of my own (sufficiently accurate for a past and unseen phenomenon) shows that in the South-eastern parts of Europe the egress might have been observed, occurring for those parts after sunrise on the morning of December 7.[1] Plate II. shows the parts of the earth whence the transit might have been seen wholly or in part. The position of the line C D, which, according to my calculation, marks the boundary between the places where day and night were in progress in the Northern hemisphere at egress, shows the parts of Europe whence the end of the transit might have been observed.

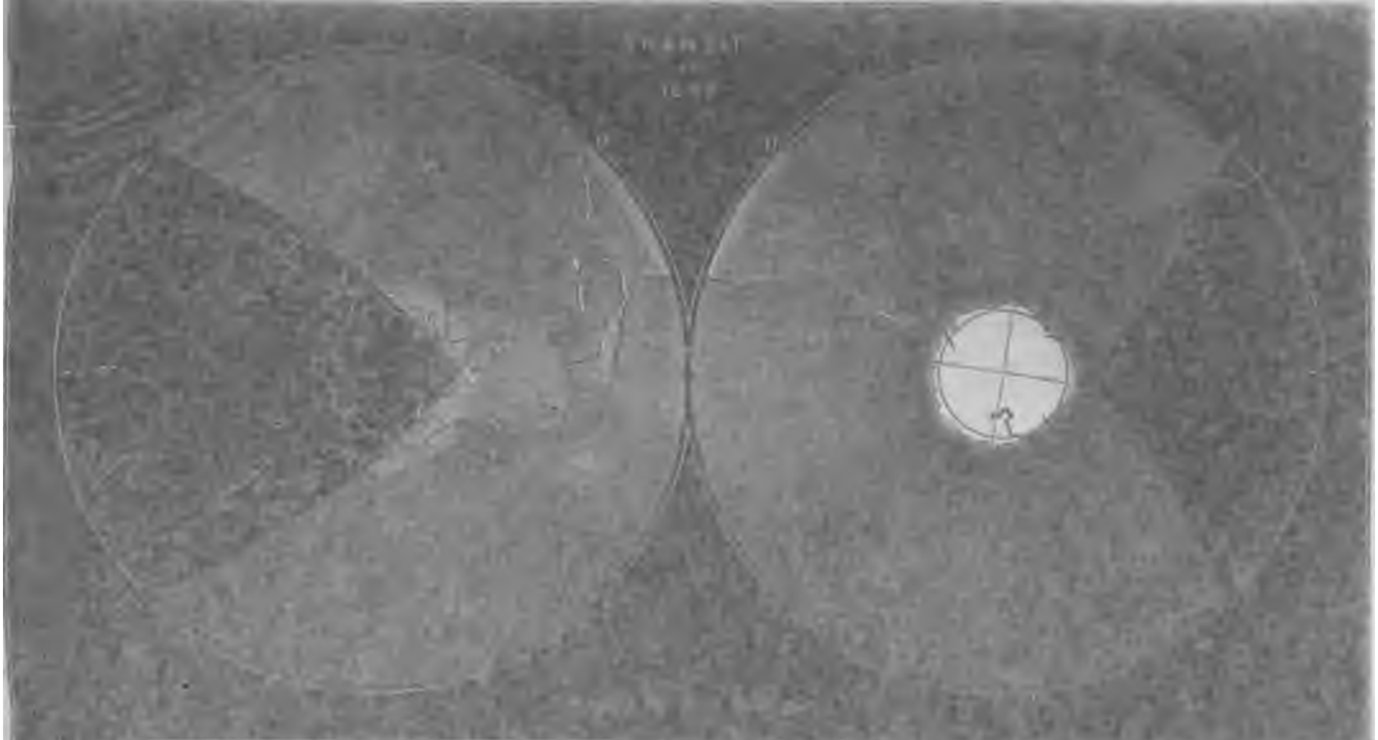
Kepler had stated that after the transit of 1631 there would be none till the year 1761. According to his calculations, Venus, when in her inferior conjunction on December 4, 1639, would pass very near to the sun's centre, but not quite near enough for a transit to occur, the planet passing *below*—that is, south of the sun. On the other hand, the tables of Lansberg, a Belgian astronomer, who followed the old system of computation, seemed to show that Venus would on this occasion transit the upper or northern part of the sun's face. Horrocks, a young and then unknown astronomer, having been led to examine the tables of Venus, found that though Kepler's were much more exact than Lansberg's, a transit would really occur, Venus passing below the centre of the sun, as Kepler had predicted, but not so low as to miss the

[1] M. Dubois, in his admirable work, 'Les Passages de Vénus,' gives the following humorous explanation of Gassendi's failure:—'Le Passage de Vénus, qui sans doute n'était pas prédit avec une précision suffisante, ne fut pas observé—*d'abord* parce que Gassendi, qui s'apprêtait à l'observation, en fut empêché par la pluie, mais *surtout* parce que le passage eut lieu pendant la nuit pour les observateurs européens.' [The italics are mine.]

PLATE II.

A B and A′ B′ mark the boundary between the sunlit and dark hemispheres at the beginning of the transit.
C D and C′ D′ mark the boundary between the sunlit and dark hemispheres at the end of the transit.

PLATE [illegible]



A B and A' B' mark the boundary between the sunlit and dark hemispheres at the beginning of the transit.
C D and C' D' mark the boundary between the sunlit and dark hemispheres at the end of the transit.

PLATE II.

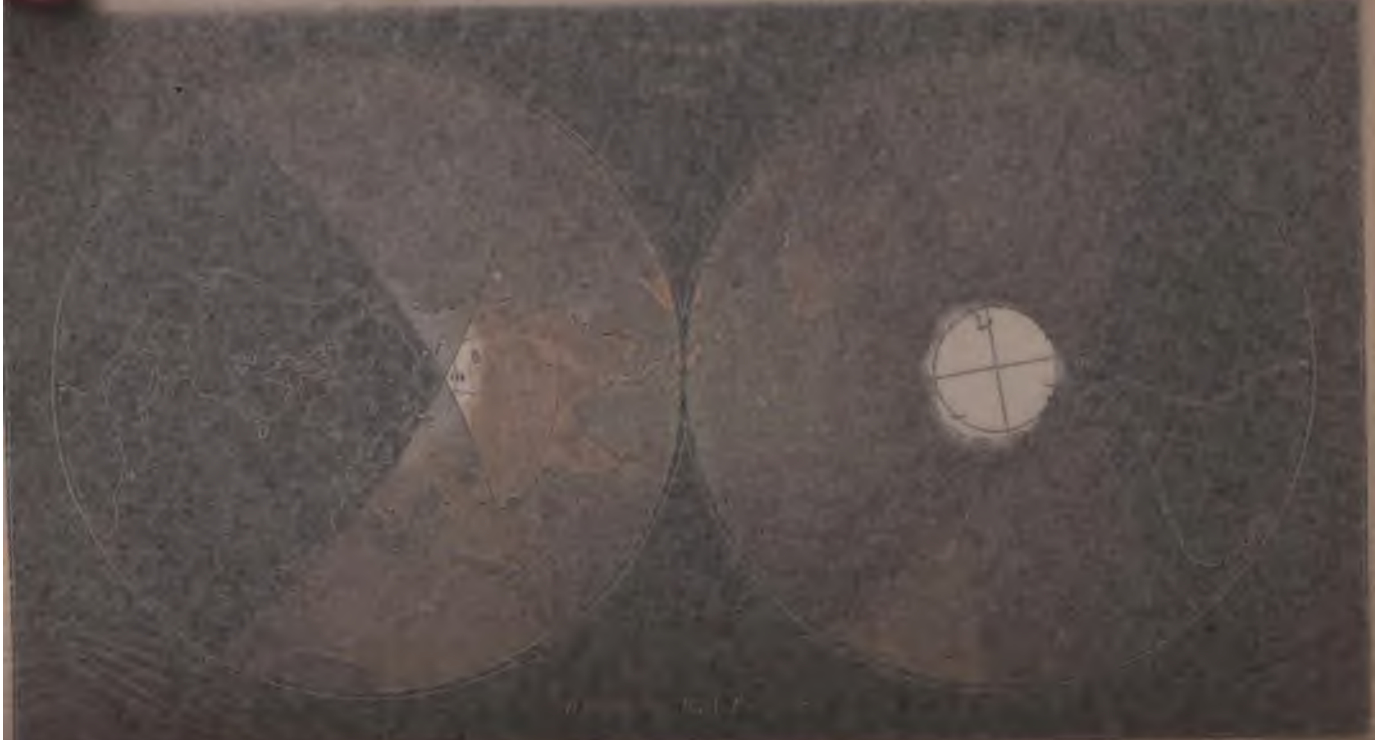
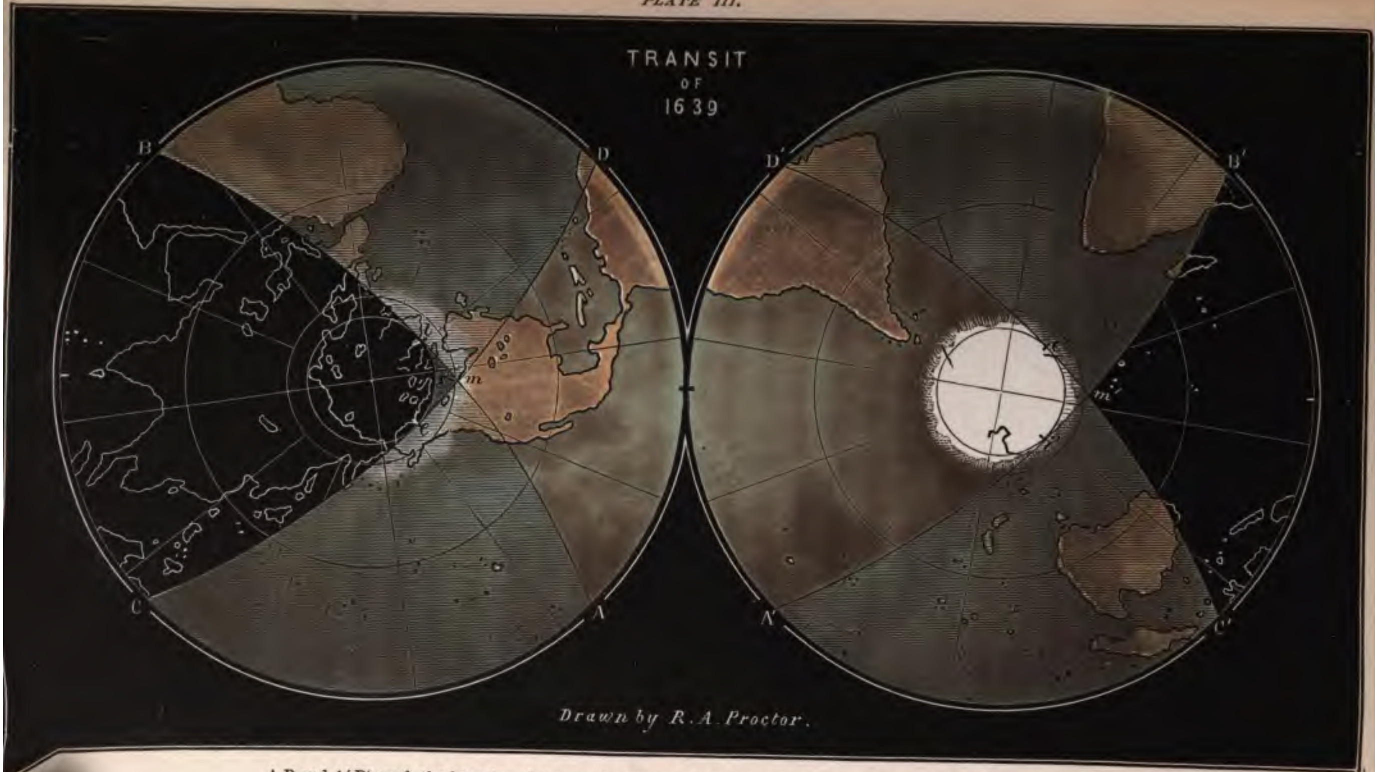


PLATE III.



A B and A′ B′ mark the boundary between the sunlit and dark hemispheres at the beginning of the transit.
C D and C′ D′ mark the boundary between the sunlit and dark hemi[illegible]

sun's disc altogether. The circumstances under which Horrocks made this discovery possess considerable interest, and I propose to devote some space to his account of them.

While preparing himself for practical observation, Horrocks undertook (apparently from sheer love of science) the computation of Venus's motions from the tables of Lansberg. These tables were so highly valued by their author that he had spoken of them as superior to all others, *quantum lenta solent inter viburna cupressi.* But Horrocks recognised many imperfections in them, and at length, as he says, 'broke off the useless computation, resolved for the future with his own eyes to observe the positions of the stars in the heavens; but, lest so many hours should be entirely thrown away,' he made use of his results to predict the positions of the planets. 'While thus engaged, I received,' he proceeds, 'my first intimation of the remarkable conjunction of Venus and the sun; and I regard it as a very fortunate occurrence, inasmuch as about the beginning of October it induced me, in expectation of so grand a spectacle, to observe with increased attention.' Nevertheless, his heart was wroth within him against Lansberg, insomuch that he could not refrain from the extreme step of 'forgiving' him in the following agreeable terms: 'I pardon, in the meantime, the miserable arrogance of the Belgian astronomer who has overloaded his useless tables with such unmerited praise, and cease to lament the misapplication of my own time, deeming it a sufficient reward that I was thereby led to consider and to foresee the appearance of Venus in the sun. But, on the other hand, may Lansberg forgive me' (this is charming) 'that I hesitated to trust him in an observation of such importance, and from having been so often deceived by his pretensions to universal accuracy that I disregarded the general reception of his tables.' 'Lest a vain exultation should deceive me,' he proceeds, 'and to prevent the chance of disappointment, I not only determined diligently to watch the important spectacle myself, but exhorted others whom I knew to be fond of astronomy to follow my example; in order that the testimony of several persons, if it should so happen, might the more effectually promote the attainment of truth, and because by observing in different places our purpose would be less likely to be defeated by the accidental interposition of clouds, or any fortuitous impediment.' He was particularly anxious, because Jupiter and Mercury seemed by their positions to threaten bad weather. 'For,' says he, 'in such apprehension I coincide with the opinion of the astrologers, because it is confirmed by experience; but in other respects I cannot help despising their puerile vanities.' Among the astronomers to whom he wrote was his friend Crabtree.[1]

[1] Both these ardent students of astronomy died young. Horrox (or Horrocks, as his name is now more commonly spelt) was but twenty years old when he calculated the transit, so that his feat may not inaptly be compared to that of Adams in calculating the place of the unknown planet Neptune within a few months of taking his degree. Each instance of an early mastery of difficult problems was fated to

In what follows I quote the account given by Horrocks himself of the observations made upon this occasion, using the translation given by the Rev. Mr. Whatton.[1]

'Following the example of Gassendi,' says Horrocks, 'I have drawn up an account of this extraordinary sight, trusting that it will not prove less pleasing to astronomers to contemplate Venus than Mercury, though she be wrapt in the close embraces of the sun:

> Vinclisque nova ratione paratis
> Admisisse Deos.

Hail! then, ye eyes that penetrate the inmost recesses of the heavens, and, gazing upon the bosom of the sun with your sight-assisting tube, have dared to point out the spots on that eternal luminary! And thou, too, illustrious Gassendi, above all others, hail! thou who, first and only, didst depict Hermes' changeful orb in hidden congress with the sun. Well hast thou restored the fallen credit of our ancestors, and triumphed o'er the inconstant wanderer. Behold thyself, thrice celebrated man! associated with me, if I may venture so to speak, in a like good fortune. Contemplate, I repeat, this most extraordinary phenomenon, never in

meet with neglect; but Horrocks died before justice had been done him. Adams was quickly able to prove that his work was sound, notwithstanding the coolness with which it had been received by official astronomers. Horrocks died in 1641, in his twenty-second year. Crabtree is supposed to have been killed at the battle of Naseby Field.

[1] The memoir accompanying Mr. Whatton's translation will be found full of interest. The complete work is published by Macintosh, 24 Paternoster Row.

our time to be seen again! the planet Venus, drawn from her seclusion, modestly delineating on the sun, without disguise, her real magnitude, whilst her disc, at other times so lovely, is here obscured in melancholy gloom; in short, constrained to reveal to us those important truths, which Mercury on a former occasion confided to thee.

'How admirably are the destinies appointed! How wisely have the decrees of Providence ordered the several purposes of their creation! Thou, a profound divine, hast honoured the patron of wisdom and learning; whilst I, whose youthful days are scarce complete, have chosen for my theme the queen of love, veiled by the shade of Phœbus' light.

'Whilst I was meditating in what manner I should commence my observation of the planet Venus, so as effectually to realise my expectations, the recent and admirable invention of the telescope afforded me the greatest delight, on account of its singular excellence and superior accuracy above all other instruments. For although the method which Kepler recommends in his treatise on Optics, of observing the diameter and eclipse of the sun through a plain aperture without the aid of glasses, is very ingenious, and in his opinion, on account of its freedom from refraction, preferable to the telescope; yet I was unable to make use of it, even if I had wished to do so, inasmuch as it does not show the sun's image exactly, nor with sufficient distinctness, unless the distance from the aperture be very great, which the smallness of my apartment would not allow. Moreover, I was afraid to risk the chance of losing the observation; a misfortune which happened to Schickard and Möstling, the astronomer to the Prince of Hesse, as Gassendi tells us in his "Mercury:" for they, expecting to find the diameter of Mercury greater than it was reasonable to anticipate, made use of so large an aperture that it was impossible to distinguish the planet at all, as Schickard himself has clearly proved; and even though Venus gave promise of a larger diameter, and thereby in some measure lessened this apprehension, and I was able to adapt the aperture to my own convenience, yet in an observation that could never be repeated I preferred encountering groundless fears to the certainty of disappointment. Besides, I possessed a telescope of my own, of such power as to show even the smallest spots upon the sun, and to enable me to make the most accurate division of his disc; one which, in all my observations, I have found to represent objects with the greatest truth.

'This kind of instrument, therefore, I consider ought always to be preferred in such experiments.

'Having attentively examined Venus with my instrument, I described on a sheet of paper a circle whose diameter was nearly equal to six inches, the narrowness of the apartment not permitting me conveniently to use a larger size. This, however, admitted of a sufficiently accurate division; nor could the arc of a quadrant be apportioned more exactly, even with a radius of fifty feet, which is as great a one as any astronomer has divided; and it is in my opinion far more convenient than a larger, for although it represents the sun's image less, yet it depicts it more clearly and steadily. I divided the circumference of this circle into 360° in the usual manner, and its diameter into thirty equal parts, which gives about as many minutes as are equivalent to the sun's apparent diameter; each of these thirty parts was again divided into four equal portions, making in all 120; and these, if necessary, may be more minutely subdivided; the rest I left to ocular computation, which, in such small sections, is quite as certain as any mechanical division. Suppose, then, each of these thirty parts to be divided into 60″, according to the practice of astronomers. When the time of the observation approached I retired to my apartment, and having closed the windows against the light, I directed my telescope, previously adjusted to a focus, through the aperture towards the sun and received his rays at right angles upon the paper already mentioned. The sun's image exactly filled the circle, and I watched carefully and unceasingly for any dark body that might enter upon the disc of light.

'Although the corrected computation of Venus's motions which I had before prepared, and on the accuracy of which I implicitly relied, forbade me to expect anything before three o'clock in the afternoon of the 24th; yet since, according to the calculations of most astronomers, the conjunction should take place sooner—by some even on the 23rd—I was unwilling to depend entirely on my own opinion, which was not sufficiently confirmed, lest by too much self-confidence I might endanger the observation. Anxiously intent, therefore, on the undertaking through the greater part of the 23rd, and the whole of the 24th, I omitted no available opportunity of observing her ingress. I watched carefully on the 24th from sunrise to nine o'clock, and from a little before ten until noon, and at one in the afternoon,—being called away in the intervals by business of the highest importance, which for these ornamental pursuits I could not with propriety neglect.[1] But during all this time I saw nothing in the sun except a small and common spot, consisting as it were of three points at a distance from the centre towards the left, which I noticed on the preceding and following days. This evidently had nothing to do with Venus. About fifteen minutes past three in the afternoon, when I was again at liberty to continue my labours, the clouds, as if by Divine interposition, were entirely dispersed, and I was once more invited to the

[1] Presumably, as Mr. Whatton points out, 'the business of the highest importance' here referred to was the duty of conducting divine service, as November 24, Old Style, was a Sunday. Mr. Whatton quotes the following passage from one of Thomas Hearne's pocket-books, dated February 8, 1723: 'Mr. Horrox, a young man, minister of Hoole, a very poor pittance, within four miles of Preston, in Lancashire, was a prodigy for his skill in astronomy, and had he lived, in all probability he would have proved the greatest man in the whole world in his profession. He had a very strange unaccountable genius, and he is mentioned with great honour by Hevelius upon account of his discovery of Venus in the sun, upon a Sunday; but being called away to his devotions and duty at church, he could not make such observations as otherwise he would have done.'

grateful task of repeating my observations. I then beheld a most agreeable spectacle, the object of my sanguine wishes, a spot of unusual magnitude and of a perfectly circular shape, which had already fully entered upon the sun's disc on the left, so that the limbs of the sun and Venus precisely coincided, forming an angle of contact. Not doubting that this was really the shadow of the planet, I immediately applied myself sedulously to observe it.

'In the first place, with respect to the inclination, the line of the diameter of the circle being perpendicular to the horizon, although its plane was somewhat inclined on account of the sun's altitude, I found that the shadow of Venus at the aforesaid hour—namely, fifteen minutes past three—had entered the sun's disc about 62° 30', certainly between 60° and 65°, from the top towards the right. This was the appearance in the dark apartment; therefore out of doors beneath the open sky, according to the laws of optics, the contrary would be the case, and Venus would be below the centre of the sun, distant 62° 30' from the lower limb,—or the nadir, as the Arabians term it. The inclination remained to all appearance the same until sunset, when the observation was concluded.[1]

'In the second place, the distance between the centres of Venus and the sun, I found by three observations, to be as follows:—

[1] Horrocks observed Venus close by the place marked i. in Plate I. She had barely completed ingress when he first saw her, and when his observations closed she had advanced nearly two diameters of herself

The Hour	Distance of the Centres
At 3.15 by the clock	14 24″
„ 3.35 „	13′ 30″
„ 3.45 „	13′ 0″
„ 3.50 the apparent sunset.	

The true setting being 3.45, and the apparent about five minutes later, the difference being caused by refraction. The clock, therefore, was sufficiently correct.

'In the third place, I found, after careful and repeated observation, that the diameter of Venus, as her shadow was depicted on the paper, was larger, indeed, than the thirtieth part of the solar diameter, though not more so than the sixth, or at the utmost

along the line of transit from the place just noted. His picture is too elaborate to be given in full, but the accompanying drawing (fig. 2)

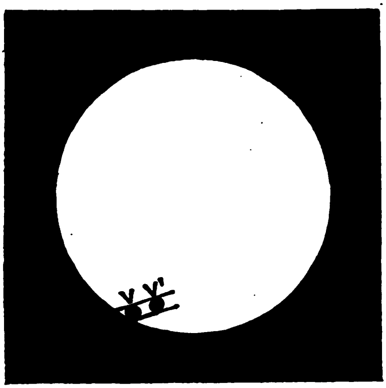


Fig. 2.—The Transit of 1639, as observed by Horrocks.

serves sufficiently to show what he observed—v being the position of Venus when first observed, v′ the position she had reached when the sun was about to set.

the fifth of such a part. Therefore, let the diameter of the sun be to the diameter of Venus as 30′ to 1′ 12″. Certainly her diameter never equalled 1′ 30″, scarcely perhaps 1′ 20″, and this was evident as well when the planet was near the sun's limbs as when far distant from it.

'This observation was made in an obscure village, where I have long been in the habit of observing, about fifteen miles to the north of Liverpool, the latitude of which I believe to be 52° 20′, although by the common maps it is stated to be 54° 12′; therefore, the latitude of the village will be 53° 35′, and the longitude of both 22° 30′ from the Fortunate Islands, now called the Canaries. This is 14° 15′ to the west of Uraniburg, in Denmark, the longitude of which is stated by Brahé, a native of the place, to be 36° 45′ from these islands.

'This is all I could observe respecting this celebrated conjunction during the short time the sun remained in the horizon: for although Venus continued on his disc for several hours, she was not visible to me for longer than half an hour, on account of his so quickly setting. Nevertheless, all the observations which could possibly be made in so short a time I was enabled by Divine Providence to complete so effectually that I could scarcely have wished for a more extended period. The inclination was the only point upon which I failed to attain the utmost precision; for, owing to the rapid motion of the sun, it was difficult to observe with certainty to a single degree; and I frankly confess that I neither did nor could ascertain it. But all the rest is sufficiently accurate, and as exact as I could desire.'

Horrocks was not the only observer of the transit of 1639. 'I had written,' he says, 'to my most esteemed friend William Crabtree, a person who has few superiors in mathematical learning, inviting him to be present at this Uranian banquet, if the weather permitted; and my letter, which arrived in good time, found him ready to oblige me. He therefore carefully prepared for the observation, in a manner similar to that which has been before mentioned. But the sky was very unfavourable, being obscured during the greater part of the day with thick clouds; and as he was unable to obtain a view of the sun, he despaired of making an observation, and resolved to take no further trouble in the matter. But a little before sunset—namely, about thirty-five minutes past three—the sun bursting forth from behind the clouds, he at once began to observe, and was gratified by beholding the pleasing spectacle of Venus upon the sun's disc. Rapt in contemplation, he stood for some time motionless, scarcely trusting his own senses, through excess of joy; for we astronomers have, as it were, a womanish disposition, and are overjoyed with trifles, and such small matters as scarcely make an impression upon others; a susceptibility which those who will may deride with impunity, even in my own presence; and if it gratify them, I too will join in the merriment. One thing I request: let no severe Cato be seriously offended with our follies; for, to speak poetically, what young man on earth would not, like ourselves, fondly admire Venus in conjunction with the sun, *pulchritudinem divitiis conjunctam?*

'But to return, he from his ecstacy and I from my digression. In a little while the clouds again obscured the face of the sun, so that he could observe nothing more than that Venus was certainly on the disc at the time. What he actually saw in so short a space was as follows: In the apartment Venus occupied the right side of the sun, being higher than its centre, and therefore in the heavens lower, and on the left. She was distant at the aforesaid hour—namely, thirty-five minutes past three—a sufficiently appreciable space from the sun's left limb, but Crabtree's opportunity was so limited that he was not able to observe very minutely either the distance itself or the inclination of the planet. As well as he could guess by his eye, and to the best of his recollection, he drew upon the paper the situation of Venus, which I found to differ little or nothing from my own observation; nor indeed did he err more than Apelles himself might have done in so rapid a sketch. He found the diameter of Venus to be seven parts, that of the sun being 200, which, according to my calculations, gives about 1′ 3″.

'This observation was made near Manchester, called by Antoninus, Mancunium, or Manucium, the latitude of which Mr. Crabtree makes 52° 24′; and the common tables 54° 15′; the longitude 23° 15′; or three minutes of time to the east of Liverpool, from which it is distant twenty-four miles.

'I wrote also of the expected transit to my younger brother, who then resided at Liverpool, hoping that he would exert himself on the occasion. This indeed he did, but it was in vain; for on the 24th the sky was overcast, and he was unable to see anything, although he watched very carefully. He examined the sun again on the following day, which was somewhat clearer, but with no better success, Venus having already completed her transit.

'I hope to be excused for not informing other of my friends of the expected phenomenon; but most of them care little for trifles of this kind, preferring rather their hawks and hounds, to say no worse; and although England is not without votaries of astronomy, with some of whom I am acquainted, I was unable to convey to them the agreeable tidings, having myself had so little notice. If others, without being warned by me, have witnessed the transit, I shall not envy their good fortune but rather rejoice, and congratulate them on their diligence. Nor will I withhold my praise from anyone who may hereafter confirm my observations by their own, or correct them by anything more exact.

'Venus was visible in the sun throughout nearly the whole of Italy. France, and Spain; but in none of those countries during the entire continuance of the transit.

'But America!

O fortunatos nimium, bona si sua norint.

Venus! what riches dost thou squander on unworthy regions which attempt to repay such favours with gold, the paltry product of their mines. Let these barbarians keep their precious metals to themselves, the incentives to evil which we are content to do without. These rude people would indeed ask from us too much should they deprive us of all those celestial riches, the use of which they are not able to comprehend. But let us cease this complaint, O Venus! and attend to thee ere thou dost depart.'

On which Horrocks bursts into strains of poetry, imploring Venus not to seek those barbarous regions for which, even as his eyes were gazing upon her, she was hastening. 'But ah!' he sighs, 'thou fliest,

And torn from civil life,
The savage grasp of wild untutored man
Holds thee imprisoned in its rude embrace.
Thou fliest, and we shall never see thee more;
While heaven, unpitying, scarcely would permit
The rich enjoyment of thy parting smile.
Oh! then farewell, thou beauteous queen! thy sway
May soften natures yet untamed, whose breasts,
Bereft of native fury, then shall learn
The milder virtues. We, with anxious mind,
Follow thy latest footsteps here, and far
As thought can carry us; my labours now
Bedeck the monument for future times
Which thou at parting left us. Thy return
Posterity shall witness; years must roll
Away, but then at length the splendid sight
Again shall greet our distant children's eyes.'

CHAPTER II.

THE TRANSIT OF 1761.

FROM the way in which Horrocks showed how the apparent place of Venus on the sun's face must be affected by the observer's position, it is tolerably clear that he would have been led to perceive how observations made from different places could be used to determine the sun's distance, had time permitted him to correspond with other astronomers. For at the beginning of Chapter VI. he says: 'I beheld Venus during the transit, not from the centre, but from the surface of the earth, therefore I observed her apparent and not her true situation. Her true situation, which chiefly concerns us, is only to be obtained by the correction of the parallaxes, into which subject I now proceed to inquire. The hypotheses of all astronomers make the parallax of Venus in so near an approach to the earth sufficiently apparent; but this I shall leave to be further considered in a separate treatise.' He then shows how the sun's distance enters into the determination of the true from the apparent position. At the end of the work he speaks again of the proposed treatise. 'I had intended,' he says, 'to offer a more extended treatise on the sun's parallax; but as the subject appears foreign to our present purpose, and cannot be dismissed with a few incomplete arguments, I prefer discussing it in a separate treatise—"*De syderum dimensione*"—which I have in hand. In this work I examine the opinions and views of others; I fully explain the diagram of Hipparchus, by which the sun's parallax is usually demonstrated, and I subjoin sundry new speculations. I also show that the hypotheses of no astronomer (Ptolemy not excepted—nor even Lansberg, who boasts so loudly of his knowledge of this subject) answer to that diagram, but that Kepler alone properly understood it. I show, in fact, that the hypotheses of all astronomers make the sun's parallax either absolutely nothing or so small that it is quite imperceptible, whereas they themselves, not understanding what they are about, come to an entirely opposite conclusion, a paradox of which Lansberg affords an apt illustration. Lastly, I show the insufficiency and uselessness of the common mode of demonstration from eclipses. I give many other certain and easy methods of proving the distance and magnitude of the sun, and I do the same with regard to the moon and the rest of the planets, adducing several new observations.'

There cannot be a doubt, I think, that had Horrocks lived to complete this treatise, the methods subsequently devised by Halley and Delisle would have been found included among the 'certain and easy methods of proving the sun's distance and magnitude.' They are so obvious, when once the connection between transits and the solar parallax has been noticed, that they could not possibly have escaped the keen insight of the young astronomer, especially as he had actually observed Venus in transit.

Passing, however, from what might have happened, let us consider how, during the interval between Horrocks's transit and the next, the idea of utilising transits for the determination of the sun's distance presented itself to astronomers.

Priority in this matter has been claimed for James Gregory; but, as Sir Edmund Beckett points out in the last edition of his 'Astronomy without Mathematics,' on insufficient grounds. In a scholium to the 87th problem of his *Optica Promota*, Gregory says that 'the problem has a very beautiful application, although perhaps laborious, in observations of Venus or Mercury when they obscure a small portion of the sun; for by means of such observations the parallax of the sun may be investigated.' But the method described in the problem, the object of which is to determine the parallaxes of two planets by observations of their conjunctions, has no practical value. I cannot understand on what grounds Prof. Grant, in his 'Physical Astronomy,' claims for Gregory the credit usually attributed to Halley. For if the mere mention of the connection between the phenomena of a transit and the solar parallax be the point insisted upon, Horrocks seems clearly to have anticipated Gregory; if the method described by Gregory be insisted upon, then, since that method never has been and never could be applied successfully, Gregory cannot be regarded as having anticipated Halley, the inventor of a practicable method. The very fact that Mercury is associated with Venus, in the sentence quoted from Gregory's work, shows how little he had grasped the idea of Halley's problem, in the solution of which transits of Mercury are useless. It is not because of the intrinsic importance of the invention that I discuss the rival claims; for I think that the approach of the transits of 1761 and 1769 would probably have forced the attention of astronomers to the very simple considerations on which the matter depends. But, as Halley had in all probability read the *Optica Promota* (Admiral Smyth thinks Halley had certainly done so[1]), the much more important question whether Halley treated Gregory with fairness is really involved. As Gregory died in 1675, only four years before Halley mentioned the utility of observations of Venus in transit, it would seriously affect our estimate of Halley's character if we adopted Prof. Grant's conclusion. I think, however, there can be very little question, when Gregory's remarks have been carefully studied, that Halley must be acquitted of all unfairness.

On November 7, 1677, Halley, stationed at St. Helena, witnessed a transit of Mercury. He noticed that the duration of the transit could be observed very exactly, and was thus led to believe that the apparent

[1] Nevertheless, this may be doubted, as Halley was but twenty-one years old when the idea of utilising transits first occurred to him; and it was only two years later that he announced the idea.

position of the path of transit of Mercury or Venus could be very accurately determined. In 1679, in the *Catalogus Stellarum Australium*, we find his first public mention of the idea. Later, he gave it closer attention, and at last, in 1716 (three years before he became Astronomer Royal), he contributed to the Proceedings of the Royal Society the following paper[1] (I quote Ferguson's translation):—

'There are many things exceedingly paradoxical, and that seem quite incredible to the illiterate, which yet, by means of mathematical principles, may be easily solved. Scarce any problem will appear more hard or difficult than that of determining the distance of the sun from the earth, very near the truth; but even this, when we are made acquainted with some exact observations, taken at places fixed upon and chosen beforehand, will, without much labour, be effected. And this is what I am now desirous to lay before this illustrious Society (which I foretell will continue for ages), that I may explain beforehand to young astronomers, who may perhaps live to observe these things, a method by which the immense distance

[1] 'It must be admitted,' says Grant of this essay, 'that the ability with which Halley expounded the peculiar advantages attending the determination of the solar parallax by observations of the transits of Venus, the earnestness with which he recommended the practical application of the method, and the weight of his authority on questions relating to astronomical science, were mainly instrumental in inducing the different Governments of Europe to adopt those liberal proceedings for observing the transits of 1761 and 1769 which have led to a more accurate knowledge of the dimensions of the solar system than could otherwise be hoped for.'

of the sun may be truly obtained to within a five-hundredth part of what it really is.

'It is well known that the distance of the sun from the earth is by different astronomers supposed different; according to what was judged most probable from the best conjecture that each could form. Ptolemy and his followers, as also Copernicus and Tycho Brahe, thought it to be 1,200 semidiameters of the earth; Kepler, 3,500, nearly; Ricciolus doubles the distance mentioned by Kepler, and Hevelius only increases it by one-half. But the planets Venus and Mercury, having, by the assistance of the telescope, been seen in the disc of the sun, deprived of their borrowed brightness, it is at length found that the apparent diameters of the planets are much less than they were formerly supposed; and that the semidiameter of Venus, seen from the sun, subtends no more than a fourth part of a minute, or fifteen seconds, while the semidiameter of Mercury, at its mean distance from the sun, is seen under an angle only of ten seconds; that the semidiameter of Saturn, seen from the sun, appears under the same angle; and that the semidiameter of Jupiter, the largest of all the planets, subtends an angle of no more than a third part of a minute in the sun. Whence, trying the proportions, some modern astronomers have thought that the semidiameter of the earth, seen from the sun, would subtend a mean angle between that larger one subtended by Jupiter and that smaller one subtended by Saturn and Mercury; and equal to that subtended by Venus

—namely, fifteen seconds—and have thence concluded that the sun is distant from the earth almost 1,400 of the earth's semidiameters. But the same authors have, on another account, somewhat increased this distance; for inasmuch as the moon's diameter is a little more than a fourth part of the diameter of the earth, if the sun's parallax should be supposed fifteen seconds, it would follow that the body of the moon is larger than that of Mercury; that is, that a secondary planet would be greater than a primary, which would seem inconsistent with the uniformity of the mundane system. And, on the contrary, the same regularity and uniformity seems scarcely to admit that Venus, an inferior planet, that has no satellite, should be greater than our earth, which stands higher in the system, and has such a splendid attendant. Therefore, to observe a mean, let us suppose the semidiameter of the earth seen from the sun, or, which is the same thing, the sun's horizontal parallax, to be twelve seconds and a half—according to which the moon will be less than Mercury, and the earth larger than Venus—and the sun's distance from the earth will come out nearly 16,500 of the earth's semidiameters. This distance I assent to at present as the true one, till it shall become certain what it is by the experiment which I propose. Nor am I induced to alter my opinion by the authority of those (however weighty it may be) who are for placing the sun at an immense distance beyond the bounds here assigned, relying on observations made upon the vibrations of a pendulum, in order to determine those exceeding small angles; but which, as it seems, are not sufficient to be depended upon: at least, by this method of investigating the parallax, it will come out sometimes nothing, or even negative—that is, the distance would either become infinite, or greater than infinite, which is absurd. And indeed, to confess the truth, it is hardly possible for a man to distinguish, with any degree of certainty, seconds, or even ten seconds, with instruments, let them be ever so skilfully made. Therefore it is not at all to be wondered at that the excessive nicety of this matter has eluded the many and ingenious endeavours of such skilful operators.

'About forty years ago, when I was in the island of St. Helena, observing the stars about the south pole, I had an opportunity of observing, with the greatest diligence, Mercury passing over the disc of the sun; and (which succeeded better than I could have hoped for) I observed, with the greatest degree of accuracy, by means of a telescope twenty-four feet long, the very moment when Mercury, entering upon the sun, seemed to touch its limb within, and also the moment when going off it struck the limb of the sun's disc, forming the angle of interior contact; whence I found the interval of time, during which Mercury then appeared within the sun's disc, even without an error of one second of time. For the lucid line intercepted between the dark limb of the planet and the bright limb of the sun, although exceedingly fine, is seen by the eye, and the little dent made on the sun's limb, by Mercury's entering the disc, appears to vanish in a moment; and also that made by Mercury leaving the disc seems to begin in an instant. When I perceived this it immediately came into my mind that the sun's parallax might be accurately determined by such kinds of observations as these, provided Mercury were but nearer to the earth, and had a greater parallax from the sun; but the difference of these parallaxes is so little as always to be less than the solar parallax which we seek, and therefore Mercury, though frequently to be seen on the sun, is not to be looked upon as fit for our purpose.

'There remains, then, the transit of Venus over the sun's disc; whose parallax, being almost as great as the solar parallax, will cause very sensible differences between the times in which Venus will seem to be passing over the sun at different parts of the earth. And from these differences, if they be observed as they ought, the sun's parallax may be determined even to a small part of a second. Nor do we require any other instruments for this purpose than common telescopes and clocks, only good of their kind; and in the observers nothing more is needful than fidelity, diligence, and a moderate skill in astronomy. For there is no need that the latitude of the place should be scrupulously observed, nor that the hours themselves should be accurately determined with respect to the meridian; it is sufficient that the clocks be regulated according to the motion of the heavens, if the times be well reckoned from the total ingress of Venus into the sun's disc to the beginning of her egress from it; that is, when the dark globe of Venus first begins to touch the bright limb of the sun within; which moments I know, by my own experience, may be observed within a second of time.

'But, on account of the very strict laws by which the motions of the planets are regulated, Venus is seldom seen within the sun's disc; and during the course of 120 years it could not be seen once—namely, from the year 1639 (when this most pleasing sight happened to that excellent youth Horrocks, our countryman, and to him only since the Creation) to the year 1761, in which year, according to the theories which we have hitherto found agreeable to the celestial motions, Venus will again pass over the sun on May 26,[1] in the morning; so that at London about five o'clock in the morning we may expect to see it near the middle of the sun's disc, and not above four minutes of a degree south of the sun's centre.[2] But the dura-

[1] June 6, according to new style.

[2] The true time of mid-transit was almost twenty-three minutes past five, and Venus, instead of being only 4′ south of the sun's centre at mid-transit, passed more than 9½′ below that point. The difference in the latter respect was much the more important. Halley was not unaware of the possibility of error in his computation, since the error arose from his neglecting the shifting of the nodes of Venus, described farther on (p. 108); and he notes that possibly the nodes may shift.

Any exact discussion of the phenomena which the transit would have presented if Halley's computations had been correct would, of course, be idle; but it may be as well roughly to indicate the actual difference between the transit as it occurred and as Halley computed it.

In fig. 3, C is the centre of the sun's disc, *i* E; *i e* is the path of

tion of this transit will be almost eight hours—namely, from two o'clock in the morning till almost ten. Hence

Venus, as computed by Halley; I E is the path she actually traversed. The time occupied in traversing I E was about 6¼ hours, whereas the

Fig. 3.—Illustrating the Transit of Venus in 1761, as it actually occurred, and as Halley computed it.

time which would have been occupied in traversing *i e* amounts to close upon eight hours, being very little less than that occupied in a central transit. It is manifest, at once, that the chords of transit *i e* are much more nearly equal than the chords, I E, so that as far as mere length of transit chord is concerned it would be useless to set the observers far apart in a northern and southern direction. But what Halley hoped to do was this:—

Let P, fig. 4, be the North pole of the earth, travelling in the direction indicated by the arrow, P being in sunlight, as the date is June 6. The equator is represented by *e′* E *e*. Now, suppose for a moment that an observer at E sees Venus in the direction E V (Venus herself being supposed to lie far beyond the picture on the right, and above the level of the paper, to correspond to the shape given to the terminator between light and darkness on the earth). Then, at this moment an observer at A sees Venus in direction A *v*, or apparently not so far advanced (since she comes between the earth and sun, moving in the same direction as the earth around the sun, and with a greater velocity). On the other hand, the observer at A′ sees her in direction A′ *v′*, or apparently farther the ingress will not be visible in England; but as the sun will at that time be in the sixteenth degree of

advanced. Hence the effect of being carried from A to A′ is to throw Venus forward on her path. But an observer at A, when transit began,

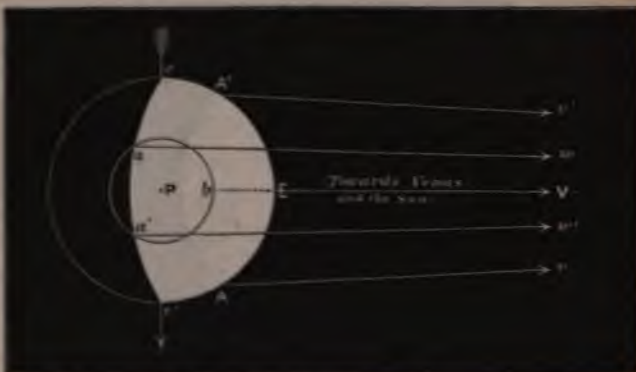


Fig. 4.—Illustrating the Conditions of the Transit of 1761, as computed by Halley.

would be carried by the earth's rotation, during the transit (lasting nearly eight hours) to the position A′; to him, therefore, the duration of the transit would be shortened by the earth's rotation. But next consider three observers in the latitude parallel *a′ b a*, near to the pole. The observer at *b* sees Venus in direction *b* V; from *a* she is seen towards *w*, or thrown forwards; from *a′* she is seen towards *w′*, or thrown backwards. The effect of being carried from *a* to *a′* is, then, to throw Venus back, or lengthen the duration of her transit. Now, if we set an observer so that at the beginning of the transit he is at *a*, he will be carried to the position *a′* at the end of the transit, if only we so select the latitude parallel *a a′* that the part in the darkened hemisphere corresponds to rather less than eight hours' rotation; in other words, take a latitude where, on June 6, or 15 days before midsummer, the night lasts less than eight hours. We find latitude 56° North suitable. This would give the beginning of the transit at sunset and the end at sunrise; and the whole of the transit between the contacts invisible. But as the sun must not be exactly on the horizon at the critical moments, we must take a place in somewhat higher latitude than 56°; and of course, the

Gemini, having almost twenty-three degrees north declination, it will be seen without setting at all,

longitude of *a*, as of A, would depend on the time at which transit began, since we must have the station which is at A at the beginning carried to the position E, at the middle. According to Halley's computation the middle of the transit would occur at about 5 in the evening, or E must be in seven hours east longitude at mid-transit. This, then, is the longitude of the equatorial station; and the longitude of the northern station is therefore to be in five hours west longitude.

Plate IV. would have to be thus altered to illustrate the circumstances of the transit as computed by Halley:—The two projections, instead of touching in Sumatra, should touch about a third of an hour farther east; since C A corresponds to the length of transit, the points A and D should be brought nearly two hours in longitude nearer together; and of course A′ and D′ should be shifted to correspond. The point I would move to a place near the new position of A′, I′ to a point near the new position of B, E near to the new position of D, and E′ near the new position of C′. Thus, H, which is the middle of the arc E I, would come close to Sumatra, and H′ would be near the Galapagos Islands. It would have been easy to find a number of stations near H in its new position; but the region *e m i*, much increased by the shifting of A B

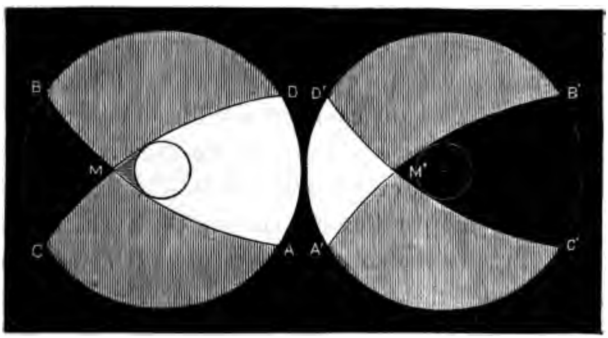


Fig. 5.—Illustrating the changes to be made in Plate IV., in order that it may correspond to the transit of 1761, as computed by Halley.

and C D, would be the best part, so far as approach to the new position of H′ was concerned. It will be seen that, under the actual conditions of the transit, the region *m e i* was quite unsuited for the purpose which in almost all parts of the north frigid zone: and therefore the inhabitants of the north coast of Norway, beyond the city of Nidrosia, which is called Drontheim, as far as the North Cape, will be able to observe Venus entering the sun's disc; and perhaps the ingress of Venus upon the sun when rising will be seen by the Scotch, in the northern parts of the kingdom, and by the inhabitants of the Shetland Isles, commonly called Thule. But at the time when Venus will be nearest the sun's centre the sun will be vertical to the northern shores of the Bay of Bengal, or rather over the kingdom of Pegu; and therefore in the adjacent regions, as the sun, when Venus enters his disc, will be almost four hours towards the east, and as many towards the west at the time of her egress, the apparent motion of Venus on the sun will be accelerated by almost double the horizontal parallax of Venus from the sun; because Venus at that time is carried with a retrograde motion from east to west, while an eye placed upon the earth's surface is whirled the contrary way, from east to west. Supposing the sun's parallax (as we have said) to be 12½″, the parallax of Venus will be 43″; from which, subtracting the parallax of the sun, there will remain 30″ at least for the horizontal parallax of Venus from the sun; and therefore the motion of Venus will be increased 45″ at least by that parallax, while she passes over the sun's disc in those elevations of the pole

had led Halley to indicate it for occupation; and the nearest approach to H′ was within the space D′ m' A′, near m'. Fig. 5 illustrates the conditions of transit as computed by Halley.

which are in places near the tropic, and yet more in the neighbourhood of the equator. Now, Venus at that time will move on the sun's disc very nearly at the rate of four minutes of a degree in an hour; and therefore eleven minutes of time at least are to be allowed for 45″, or three-fourths of a minute of a degree; and by this space of time the duration of this eclipse caused by Venus will, on account of the parallax, be shortened. And from this shortening of the time only we might safely enough draw a conclusion concerning the parallax which we are in search of, provided the diameter of the sun and the latitude of Venus were accurately known. But we cannot expect an exact computation in a matter of such subtility.

'We must endeavour, therefore, to obtain if possible another observation, to be taken in those places where Venus will be in the middle of the sun's disc at midnight; that is, in places under the opposite meridian to the former, or about six hours or ninety degrees west of London, and where Venus enters upon the sun a little before its setting, and goes off a little after its rising. And this will happen under the abovementioned meridian, and where the elevation of the north pole is about fifty-six degrees; that is, in a part of Hudson's Bay near a place called Port Nelson. For, in this and the adjacent places, the parallax of Venus will increase the duration of the transit by at least six minutes of time; because while the sun from its setting and rising seems to pass under the pole, those places on the earth's disc will be carried with a motion from east to west contrary to the motion of the Ganges; that is, with a motion conspiring with the motion of Venus; and therefore Venus will seem to move more slowly on the sun, and to be longer in passing over its disc.

'If therefore it should happen that this transit should be properly observed by skilful persons at both these places, it is clear that its duration will be seventeen minutes longer as seen from Port Nelson, than as seen from the East Indies. Nor is it of much consequence (if the English shall at that time give any attention to this affair) whether the observation be made at Fort George, commonly called Madras, or at Bencoolen, on the western shore of the island of Sumatra, near the equator. But if the French should be disposed to take any pains herein, an observer may station himself conveniently enough at Pondicherry, on the west shore of the Bay of Bengal, where the altitude of the pole is about twelve degrees. As to the Dutch, their celebrated mart at Batavia will afford them a place of observation fit enough for the purpose, provided they also have but a disposition to assist in advancing, in this particular, the knowledge of the heavens. And indeed I could wish that many observations of this famed phenomenon might be taken by different persons at separate places, both that we might arrive at a greater degree of certainty by their agreement, and also lest any single observer should be deprived by the intervention of clouds of a sight which I know not whether any man living in this or the next age will ever see again; and on which depends the certain and adequate solution of a problem the most noble, and at any other time not to be attained to. I recommend it therefore again and again to those curious astronomers who (when I am dead) will have an opportunity of observing these things, that they would remember this my admonition, and diligently apply themselves with all their might in making this observation, and I earnestly wish them all imaginable success: in the first place, that they may not by the unseasonable obscurity of a cloudy sky be deprived of this most desirable sight, and then, that having ascertained with more exactness the magnitudes of the planetary orbits, it may redound to their immortal fame and glory.

'We have now shown that by this method the sun's parallax may be investigated to within its 500th part, which doubtless will appear wonderful to some. But if an accurate observation be made in each of the places above marked out, we have already demonstrated that the durations of this eclipse made by Venus will differ from each other by 17 m. of time; that is, upon a supposition that the sun's parallax is 12½″. But if the difference shall be found by observation to be greater or less, the sun's parallax will be greater or less nearly in the same proportion. And since 17 m. of time are answerable to 12½″ of solar parallax, for every second of parallax there will arise a difference of more than 80 s. of time; whence if we have this difference true to two seconds it will be certain what the sun's parallax is to within a 40th part of 1″; therefore his distance will be determined to within its 500th part at least, if the parallax be not found less than what we have supposed: for 40 times 12½ make 500.

'And now I think that I have explained this matter fully, and even more than I needed to have done to those who understand astronomy; and I would have them take notice that on this occasion I have had no regard to the latitude of Venus, both to avoid the inconvenience of a more intricate calculation, which would render the conclusion less evident, and also because the motion of the nodes of Venus is not yet discovered, nor can be determined but by such conjunctions of the planet with the sun as this is. For we conclude that Venus will pass four minutes below the sun's centre, only in consequence of the supposition that the plane of Venus's orbit is immovable in the sphere of the fixed stars, and that its nodes remain in the same places where they were found in the year 1639. But if Venus in the year 1761 should move over the sun in a path more to the south, it will be manifest that her nodes have moved backwards among the fixed stars; and if more to the north, that they have moved forwards; and that at the rate of 5½′ of a degree in 100 Julian years, for every minute that Venus's path shall be more or less distant than the above-said 4′ of the sun's centre. And the difference between the duration of these eclipses will be somewhat less than 17 m. of time, on account of Venus's south latitude; but greater if by the motion of the nodes forwards she should pass on the north of the sun's centre.'

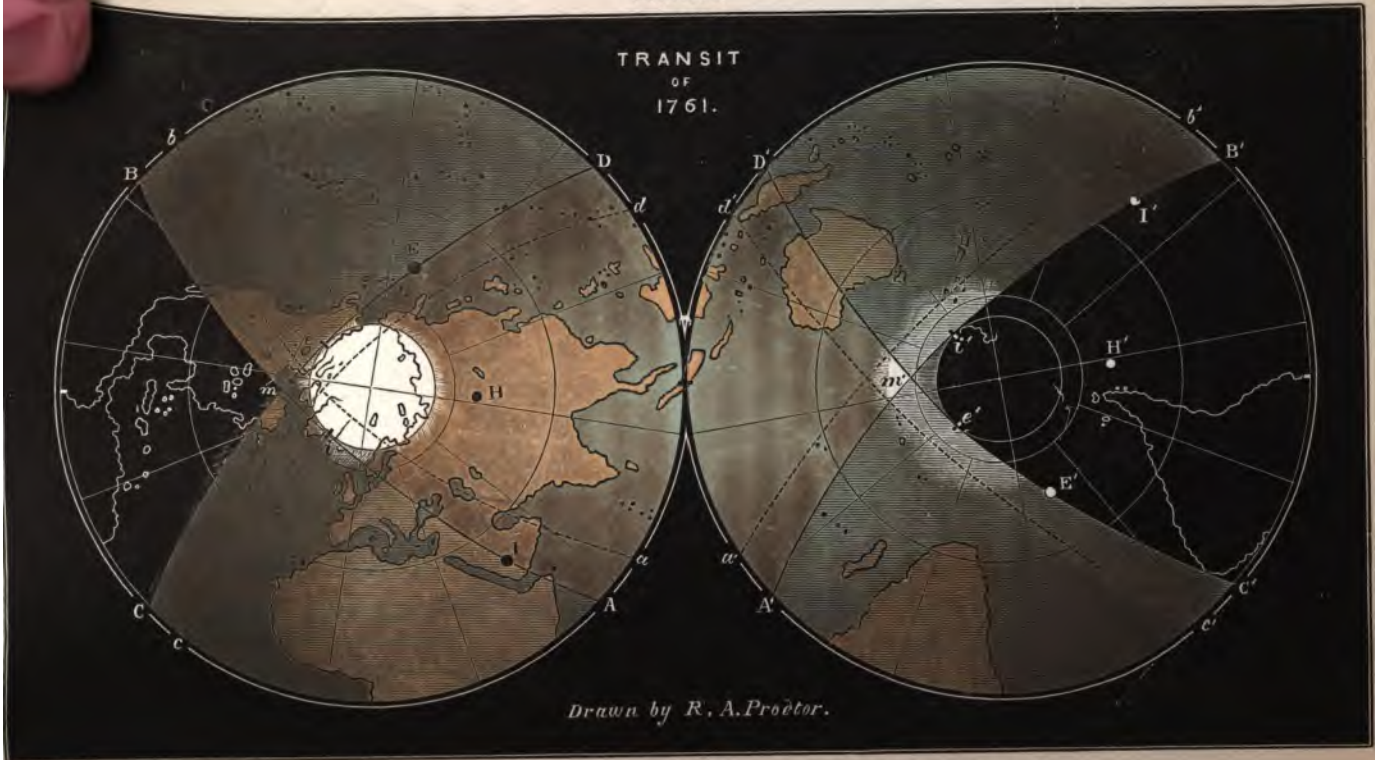
The rest of Halley's dissertation I omit, because it relates to the details of the transit as incorrectly computed by him, and therefore possesses no present interest.

As I have said it was not until three years after his essay appeared that Halley became Astronomer Royal. It does not appear that during the remaining years of his life he made any farther contribution to the subject. He died on January 14, 1742, more than nineteen years before the transit occurred.

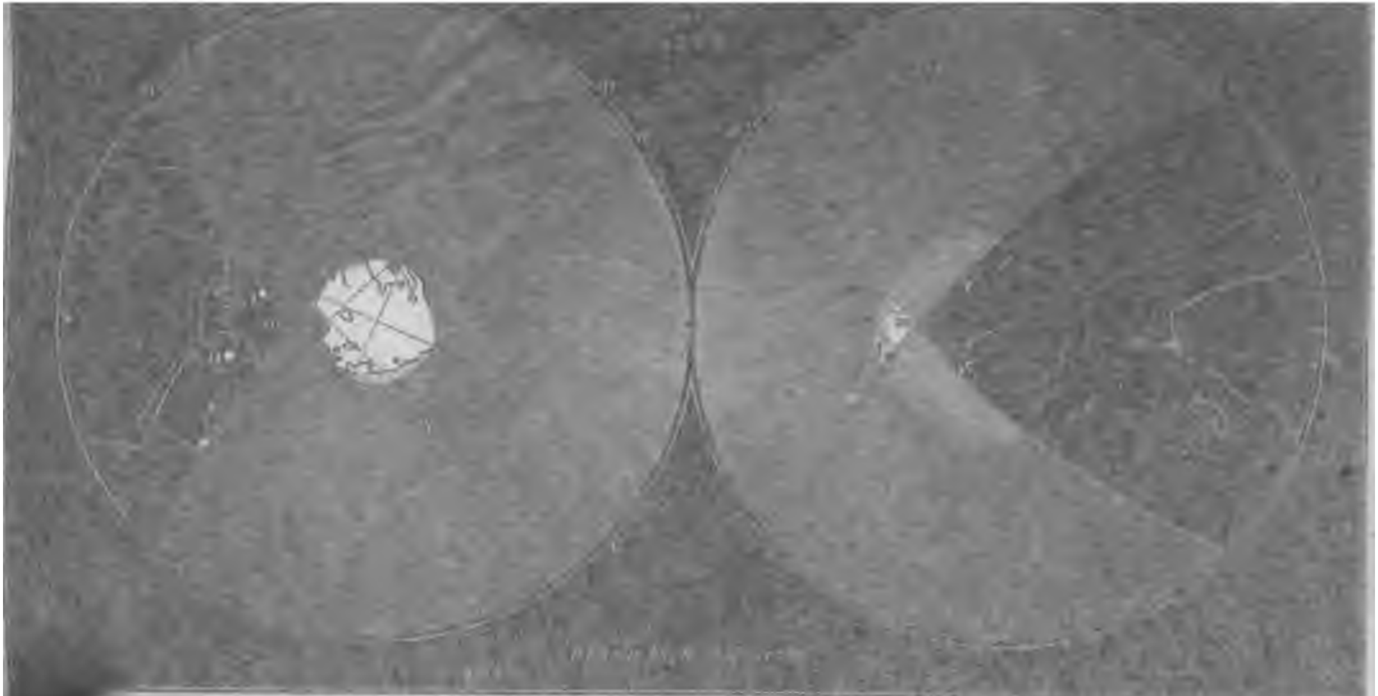
As the time for the transit drew near astronomers began to examine carefully the motions of Venus, in order to ascertain how far the conditions on which Halley's computation had been based were really fulfilled. Passing over, however, a paper by Trébuchet, pointing out inaccuracies in Halley's dissertation, it was not until August 1760, or less than a year before the transit took place, that the conditions on which successful observation depended were pointed out by Delisle. He published a chart of the earth on an equatorial projection, showing the hour at which the transit would begin or end. The chart corresponded, in fact, to Plate IV., meridional projections being substituted for the equatorial projections there used. It will be understood, however, that Delisle did not claim for his chart the degree of accuracy aimed at in Plate IV. He showed that the stations selected by Halley were not suited to the actual conditions of the transit, and that in fact the transit could not be well observed by the method of durations. He showed how, at suitably selected stations, whose longitude had been accurately determined, the single observation of a contact, whether at ingress or egress, would supply the means of determining the solar parallax. For the description of his method the reader is referred to Chapter IV.

Ferguson, in England, seems to have independently arrived at the same conclusion, not long after; at least his treatise on the subject suggests the impression that he had selected his own method of dealing with it, and had carried his analysis nearly to its completion when Delisle's paper and map reached him. He found that 'instead of passing only four minutes of a degree below the sun's centre, Venus will pass almost ten minutes of a degree below it, on which account the line of the transit will be so much shortened as will make her passage over the sun's disc about an hour and twenty minutes less than if she passed only four minutes below the sun's centre at the middle of her transit; and therefore her parallax from the sun will be so much diminished, both at the beginning and end of her transit, and at all places from which the whole of it will be seen, that the difference of its duration, as seen from them, and as supposed to be seen from the earth's centre, will not amount to eleven minutes of time. But this is not all; for although the transit will begin before the sun sets to Port Nelson, it will be quite over before he rises to that place next morning, on

PLATE IV.

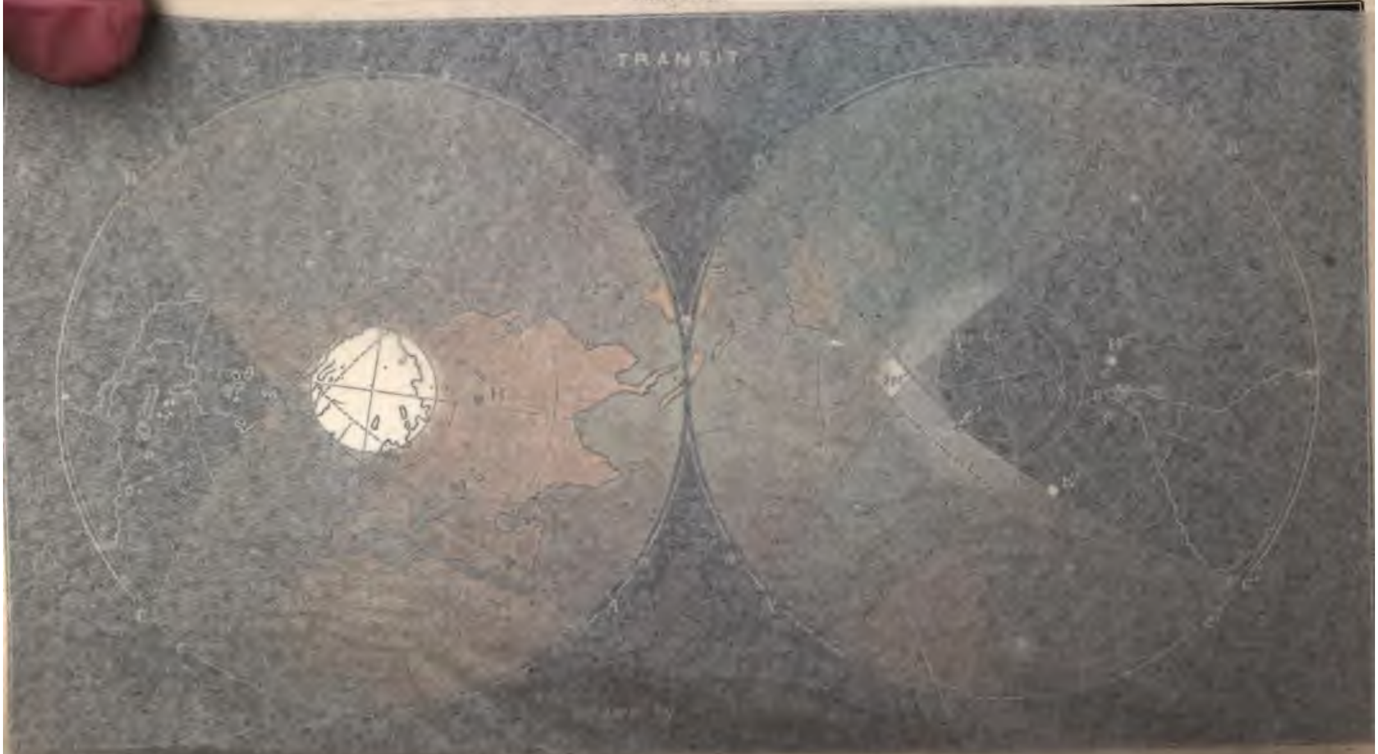


… hemispheres at ingress ; along $a\,b$, $a'b'$, sun 10° high at ingress.



A B and A′ B′ separate visible and darkened hemispheres at ingress; along a b, a′ b′, sun 10° high at ingress.
C D and C′ D′ separate visible and darkened hemispheres at egress; along c d, c′ d′, sun 10° high at egress.
I, I′ are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

PLATE IV.



A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along $a\,b$, $a'\,b'$, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along $c\,d$, $c'\,d'$, sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

account of its ending so much sooner than as given by the tables to which Dr. Halley was obliged to trust. So that we are quite deprived of the advantage that otherwise would have arisen from observations made at Port Nelson.'

Ferguson gave a chart of the transit on the same plan as I have used in Plates II.–IX. The chart was taken directly from Delisle, however, as Ferguson tells us, only 'I have changed,' he says, 'his meridional projections into that of the equatorial; by which I apprehend that the curved lines showing at what places the transit begins or ends with the rising or setting sun appear more natural to the eye and are more fully seen at once than in the map from which I copied; for in that map the lines are interrupted and broken in the meridian that divides the hemisphere, and the places where they should join cannot be perceived so readily by those who are not well skilled in the nature of the stereographical projections.' It shows how clear an insight Ferguson had obtained into the conditions of the transit, that, commenting on his charts, in which the line, B A of Plate IV., passes down the eastern shore of the Red Sea, while A′ B′ crosses Madagascar, he says: 'I question much whether the transit will begin at sunrise to any place in Africa that is west of the Red Sea, and am pretty certain that the sun will not be risen to the northernmost part of Madagascar when the transit begins, so that the line,' corresponding to A B, A′ B′ of Plate IV., 'seems to be a little too far west in the map at all places which are south of Asia Minor; but in Europe I think it is very well.'

The actual circumstances of the transit of 1761 in different parts of the earth can be inferred with sufficient accuracy from what is shown in Plate IV. Here the arcs A I B and A′ I′ B′ separate the dark and light hemispheres of the earth at the beginning of the transit, while the arcs C E D and C′ E′ D′ separate the dark and light hemispheres at the end of the transit. Thus the beginning of transit was visible at all places on the hemisphere formed by combining the sections A I B D and A′ I′ B′ D′, and the end of the transit was visible at all places on the hemisphere formed by combining the sections C E D A and C′ E′ D′ A′. The whole of the transit was visible over the spaces D *e i* A and D′ *m′* A′; but in the space *i m e*, though the beginning and end of the transit were seen, the progress of the transit was not wholly visible. No part of the transit was visible over the spaces B *m* C and B′ *i′ e′* C′; but in the space *i′ m′ e′*, though neither the beginning nor the end were visible, the progress of the transit was partially visible. At all points of the arcs A I B and A′ I′ B′ the ingress occurred with the sun on the horizon; but whereas the sun was rising for the arcs A *i* and A′ *i′*, he was setting for the arcs B *i* and B′ *i′*: at all points of the arcs C E D and C′ E′ D′ the egress occurred with the sun on the horizon; but whereas the sun was rising for the arcs C *e* and C′ *e′*, he was setting for the arcs D *e* and D′ *e′*. At the points *m* and *m′* the sun was on the horizon both at ingress and at egress; but whereas the progress of the whole transit, except ingress and egress, took place during the night at *m*, it took place during the day at *m′*. In all that has here been said, the passage of Venus's centre has been alone considered.

The point I′ was that where ingress occurred earliest, the point I being that where ingress occurred latest. It was around these points, then, that observers of ingress by Delisle's method were to be placed, keeping, of course, to that side of the arcs A′ B′ and A B on which the sun would be above the horizon at the time of ingress. We see that several islands were conveniently placed near I′ for showing accelerated ingress, though they were not very well known in those days. The eastern parts of Arabia and parts of India afforded convenient stations near I for observing retarded ingress.

The point E was that where egress occurred earliest, E′ being that where egress occurred latest. Kamschatka, Japan, and Manchooria afforded convenient stations for observing accelerated egress, while the Cape of Good Hope was well placed for observing retarded egress.[1]

As regarded the application of Halley's method—that is, the observation of duration where greatest and least—the transit was not a favourable one. H was the

[1] Encke, in 1822, found the following elements for the transit of 1761. I quote them from the excellent little treatise, 'Les Passages de Vénus sur le disque Solaire,' by M. Dubois, Naval Examiner in Hydrography for France, substituting Greenwich for Paris time and longitude,

place where transit had the shortest duration, H′ being the place where, if the transit had been visible, its duration would have been least. Stations near H could of course be occupied, as here the summer of Siberia was in progress. But we see that there was no station at all near to H′ whence the whole transit could be seen. The point *m′* was geometrically the most advantageous, but there the sun was upon the horizon. The south-western extremity of Australia or the island of St. Paul were the only regions available, and they were almost as far from H′ as from H. In point of fact, Halley's method failed totally on this occasion. It commonly fails at the earlier transit of a pair separated by eight years, as will be shown in the next chapter; but it is worthy of notice that the circumstances of the transit of 1761 in this respect were very much like those of the coming transit of 1882. Although the transit of 1882 will be the

and correcting a misprint, by which in one place north and south latitudes are interchanged:—

	h.	m.	s.	
Ingress of Venus's centre	14	12	48·5	Greenwich apparent solar time.
Middle of the transit	17	23	0·0	
Egress of the centre	20	29	13·5	
Duration of the transit	6	16	25·0	
Least distance of centres.	0	9	34·2	

	Latitude		Longitude	
	°	′	°	′
Pole of accelerated ingress	20	56 S	132	28 W
„ „ retarded „	20	56 N	47	32 E
„ „ accelerated egress	46	47 N	167	59 E
„ „ retarded „	46	47 S	12	1 W
„ „ shortened durations	52	31 N	92	42 E
„ „ lengthened „	52	31 S	87	18 W

second transit of a pair, its geometrical superiority is counterbalanced by the inaccessibility of the antarctic as compared with the arctic regions.

I do not know that any useful purpose could be served by inserting here an account of the various observations of the transit of 1761 made by the persons, 176 in number, who took a more or less important share in the work at no less than 117 stations. Presently the peculiar phenomena which rendered the observation of internal contact uncertain will be described; but the mere records of time observations have no special interest. A few examples may suffice to show this.

We see from Plate IV. that the beginning of the transit was invisible in the western parts of Europe, but the latter half was visible there, though not under specially advantageous circumstances. We have the following particulars respecting the observations in London at Greenwich: 'Early in the morning, when every astronomer was preparing for observing the transit, it unluckily happened that, both at London and the Royal Observatory at Greenwich, the sky was so overcast with clouds as to render it doubtful whether any part of the transit would be seen, and it was 38 m. 21 s. past 7 o'clock, apparent time, at Greenwich when the Rev. Mr. Bliss, our Astronomer Royal, first saw Venus on the sun. . . From that time to the beginning of egress the Doctor made several observations, both of the difference of right ascension and declination of the centres of the sun and Venus, and at last found the beginning of egress, or instant of the internal contact of Venus with the sun's limb, to be at 8 hours 19 minutes 0 seconds apparent time. . . . By the means of three good observations the diameter of Venus on the sun was 58 seconds of a degree.' 'Mr. Short made his observations at Savile House, in London, 30 seconds in time west of Greenwich, in presence of His Royal Highness the Duke of York, accompanied by Their Royal Highnesses Prince William, Prince Henry, and Prince Frederick.' So the account runs. We are not told whether the Duke of York actually honoured Venus by directing His Royal gaze upon her during her transit, or whether Their Other Royal Highnesses made any observations; but as Venus was under observation for about 3½ hours, we may suppose that these exalted persons did not lose the opportunity of witnessing a phenomenon so seldom seen. Venus, all unconscious of the honour, moved onwards to egress, contact occurring at 8 h. 18 m. 15½ s. apparent Greenwich time, or 8½ s. sooner than at Greenwich. At Stockholm the whole transit was observed by Wargentin, the whole duration (between the internal contacts) being 5 h. 50 m. 45 s., corresponding to a little over six hours for the passage of the centres. At Stockholm, as we see from Plate IV., the transit was shortened as compared with the mean duration.

Chappe d'Auteroche was stationed at Tobolsk, in Siberia—an important station for the Halleyan method (see Plate IV.), if any stations had been available for observing lengthened durations. The transit, as observed by him, lasted 5 h. 48 m. 32½ s., or nearly

2 m. less than at Stockholm. Chappe had some trouble in reaching Tobolsk in time for his observations. He started at the end of November 1760, and reached St. Petersburg readily enough; but the journey thence to Tobolsk was not completed without inconvenience and even serious dangers. He reached Tobolsk on April 10, 1761, the voyage having lasted five months.

England sent out an expedition intended for Bencoolen, in Sumatra, apparently because that station had been mentioned in Halley's dissertation; for Sumatra, almost midway between H and *m′* (Plate IV.), offered no advantages for the observation of durations, and was altogether too far removed both from I and E to be of the least service as a Delislean station. Fortunately the ship was attacked by a Spanish war-ship on the road, and had to put in at the Cape, where very useful observations of the retarded egress were made. Another English expedition was sent to St. Helena, a station where retarded egress was observable, but by no means advantageously. At Madras, Mr. Hirst, and at Calcutta, Mr. Magee (whom M. Dubois converts into Magec) observed the duration of transit, obtaining respectively the periods 5 h. 51 m. 43 s., and 5 h. 50 m. 36 s., values which differ much more from each other than parallax will account for. As Ferguson well remarks of the whole series of observations: 'Whoever compares the times of the internal contacts, as given by different observers, will find such difference among them, even those which were taken from the same spot, as will show that the instant of either contact could not be so accurately perceived by the observers as Dr. Halley thought it could, which probably arises from the difference of people's eyes and the different magnifying powers of those telescopes through which the contacts were seen. If all the observers had made use of equal magnifying powers there can be no doubt that the times would have more nearly coincided, since it is plain that, supposing all their eyes to be equally quick and good, they whose telescopes magnified most could perceive the point of internal contact soonest and of the total exit latest.'

Le Gentil, who had been appointed to observe at Pondicherry, was very unfortunate. The following account is taken from M. Dubois' admirable work already referred to: 'On account of the distance of the station where he was to observe the transit, Le Gentil set out from France on March 26, 1760. The observation he hoped to make at Pondicherry was curious and interesting, says J. D. Cassini; in fact, he would have seen the whole transit, and the middle would have occurred when the sun was nearly on the meridian at about ten degrees from the zenith. Le Gentil arrived at the Isle of France on July 10, 1760, that is to say, nearly a year before the expected transit; but the war which arose at that epoch between France and England rendered it no longer possible for him to go to Pondicherry. He resolved to betake himself to Rodriguez, awaiting meanwhile the progress of events. He was just setting off for this new station, where also De Pingré was to observe, when he learned that a French frigate was about to leave the Isle of France for the coast of Coromandel. Le Gentil resolved to avail himself of this opportunity to go to the place selected by the Academy of Sciences; but he was not able to leave the Isle of France on board this frigate till about the middle of March 1761. It was already very late. The frigate carrying the French astronomer experienced at first long-continued calms, which were enough to cause Le Gentil to despair, and which did not permit him to reach the coast of Malabar before May 24. To increase his ill-fortune, the commander of the frigate learned that the English were masters of Mahe and Pondicherry. The frigate had no other resource but to take flight without delay. This she did; and, to the utter despair of Le Gentil, she retook her way towards the Isle of France. The 6th of June arrived! The frigate was in 87° East longitude (from Paris), and 5° 45′ South latitude. The sky was clear, the sun splendid! The unfortunate Le Gentil, unwilling to be altogether idle, observed the transit on board the ship, taking all possible care. He noted the times of ingress and egress; but with what degree of approximation were those times obtained, even admitting that those he noted coincided exactly with the instant of the contacts? The voyage of the French Academician ended thus in failure. Le Gentil then experienced one of those mishaps which assume to the man of science all the proportions of a real misfortune—to have traversed so large a portion of the globe, to have endured all the weariness, all the privations, all the perils of a long sea-voyage, and to effect nothing! This was enough to have disgusted anyone with scientific observation, or at least with Halley's method. We shall presently see, however, when dealing with the transit of 1769, that Le Gentil, so far as that method was concerned, had not yet seen the last of his troubles.'

De Pingré reached Rodriguez in May 1761; and although he had to observe in the open air, and could scarcely find a place where to keep his clock out of the wind, his observations were among the best of those effected during the transit of 1761.

The results of the observations were far from satisfactory, the values of the solar parallax deduced by mathematicians ranging between 8″·5 and 10″·5, corresponding to a distance of the sun ranging from 96,162,840 miles to 77,846,110 miles. From a comparison of a great number of observations made by Short the parallax 8″·5 was deduced for the day of the transit, corresponding to 8″·65 for the earth's mean parallax, or a distance of 94,498,420 miles. In 1822, Encke, then sub-director at Seeberg, deduced from the observations made in 1761 a parallax lying between the extreme limits 8″·429813 and 8″·551237, corresponding to the distances 97,000,000 miles and 95,600,000 miles.

These discrepancies were no doubt due to two chief causes. In the first place, the observations were mostly Delislean, and in the last century means did not exist for the determination of the longitude with the degree of accuracy which was required. Secondly, it was found that the phenomena attending the ingress and egress of Venus are not so simple as Halley had supposed, when he stated that the time of internal contacts can be determined within a single second of time. Halley had reckoned on the appearances presented during a transit such as he had observed at St. Helena, when the sun was high above the horizon, and the small disc of Mercury was little disturbed by atmospheric effects. But at most of the stations for effectively observing the transit of Venus in 1761, and at all those best suited for applying Delisle's method, the sun was not far from the horizon, and the outline of Venus was seriously affected by atmospheric undulations. Moreover, an optical phenomenon which had not attracted Halley's attention was presented during the transit of 1761, and caused the observations to be much less reliable than they would otherwise have been. The disc of Venus was found to assume near the time of internal contact a distorted form. In some cases she seemed to be attached to the edge of

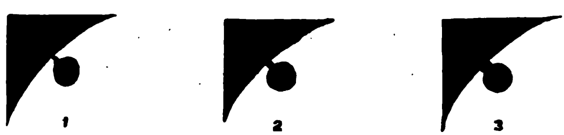


Fig. 6.—Illustrating the Black Drop.

the sun by a dark ligament of greater or less breadth, as shown at 1, 2, and 3, fig. 6; in other cases she appeared shaped like a pear, while in others she was altogether distorted by the combined effect of atmospheric disturbances and the optical distortion (whatever its real nature may be) which causes the black drop and pear-shape figures.

This was the first occasion on which the peculiar appearances in question were noted; but as the difficulty thus introduced affected the discussion of the observations made during both the transits of the last century, this will be a convenient place for describing what was seen in 1769 as well as in 1761. Professor Grant has collected together in his fine 'History of Physical Astronomy' several of the most interesting observations of this kind, and from his work I quote the following cases:—

Mr. Hirst, who observed the transit of 1761 at Madras, stated that 'at the total immersion the planet, instead of appearing truly circular, resembled more the form of a bergamot pear, or, as Governor Pigott then expressed it, *looked like a ninepin*; yet the preceding limb of Venus was extremely well defined.' With respect to the end of the transit, he remarked 'that the planet was as black as ink, and the body truly circular, just before the beginning of egress, yet it was no sooner in contact with the sun's preceding limb, than it assumed the same figure as before at the sun's subsequent (following) limb; the subsequent limb of Venus keeping well-defined and truly circular.'

A similar appearance was observed by Salunde at Paris, by Bergman at Upsal, and also by several other individuals.

Dr. Maskelyne, who observed the transit of 1769 at Greenwich, gives the following description of a phenomenon of a similar nature witnessed by him at the egress of the planet :—

'The regularity of Venus's circular figure was disturbed towards the place where the internal contact should happen by the addition of a protuberance dark like Venus and projecting outwards, which occupied a space upon the sun's circumference which bore a considerable proportion to the diameter of Venus. Fifty-two seconds before the thread of light was formed, Venus's regular circumference (supposed to be continued as it would have been without the protuberance) seemed to be in contact with the sun's circumference, supposed also completed. Accordingly, from this time Venus's regular circumference (supposed defined in the manner just described) appeared wholly within the sun's circumference, and it seemed, therefore, wonderful that the thread of light should be so long before it appeared, the protuberance appearing in its stead. At length when a considerable part of the sun's circumference (equal to one-third or one-fourth of the diameter of Venus) remained still obscured by the protuberance, a fine stream of light flowed gently round it from each side, and completed the same in the space of three seconds of time. But the protuberance, though diminished, was not taken away till about twenty seconds more; when, after being gradually reduced, it disappeared, and Venus's circular figure was restored.

Dr. Bevis states in the account of his observations that 'the planet seemed quite entered upon her disc, her upper limb being tangential to that of the sun; but instead of a thread of light, which he expected immediately to appear between them, he perceived Venus to be still conjoined to the sun's limb by a slender tail, nothing near so dark as her disc, and shaped like the neck of a Florence flask. The said tail vanished at once; and for a few seconds after, the limb of Venus, to which it had been joined, appeared more prominent than her lower part, somewhat like the lesser end of an egg, but soon resumed its rotundity.'

The Rev. Mr. Hirst thus describes the appearance presented during the transit: 'The same phenomena of a protuberance which I observed at Madras in 1761, at both internal contacts, I observed again at this last transit. At both times the protuberance of the upper edge of Venus diminished nearly to a point before the thread of light between the concave edge of the sun and the concave edge of the planet was perfected, when the protuberance broke off from the upper edge of the sun, but Venus did not assume its circular form till it had descended into the solar disc some distance.'

Mr. Dunn, who observed the transit at Greenwich, remarks that 'he saw the planet held as it were to the sun's limb by a ligament formed of many black cones whose bases stood on the limb of Venus, their vertices pointing to the limb of the sun.'

'Mr. Pigott states that Venus, before she separated from the sun, was considerably stretched out towards his limb, which gave the planet nearly the form of a pear; and even after the separation of the limbs Venus was twelve or nine seconds before she resumed her rotundity.'

The following cases, with their accompanying illustrations, serve at once to indicate the nature and suggest the explanation of the peculiar appearances presented by Venus when nearly at internal contact.

Fig. 7 represents the appearance presented by Venus as observed by Mayer at St. Petersburg, in 1769. A reference to Plate V. will show that at St.

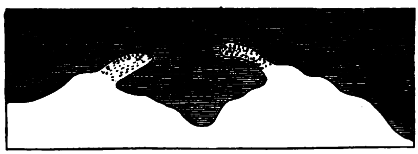


Fig. 7.—Appearance presented by Venus at Internal Contact, as observed by Mayer.

Petersburg the sun was almost upon the horizon at the moment of ingress, and close to the horizon at the moment of egress. There can be no doubt that the distorted appearance of Venus is due to atmospheric disturbances, such as are always recognisable when the sun is observed low down. I may remark that fig. 7 corresponds precisely to what I observed when examining the artificial transit of Venus as arranged at Washington, on a morning when the atmosphere was unusually disturbed. The American astronomers consider that the corresponding arrangements at Greenwich are not so good as their own, because the distance between the observer and the artificial 'Sun and Venus' is not great enough to permit the study of these atmospheric effects. We see clearly enough from Mayer's observation that such effects, though they would not be nearly so great with the sun even moderately raised (say 10°) above the horizon, must always be taken into account. The edge of the sun even at a considerable height is always rippled by the effects of atmospheric undulations. So also necessarily must the outline of Venus be rippled, and it is the contact of two rippling outlines, not of two sharply defined discs, that the astronomer is called upon to observe.

The next picture (fig. 8) is from a drawing by Bayley at Nord Cap. In this case the sun was raised about

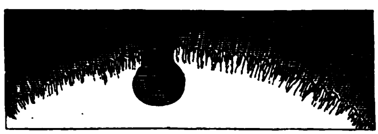


Fig. 8.—Contact of Venus, as observed by Bayley at Nord Cap.

10° from the horizon, but the blurred outline given to the sun indicates the existence of imperfect atmospheric conditions, and we may partly attribute to this cause the wideness of the connecting ligament when contact was actually established.

Fig. 9 is from a drawing by Hirst, who observed the ingress at Greenwich; while fig. 10 shows how

Venus appeared to Bevis, who observed at Kew under nearly the same conditions.

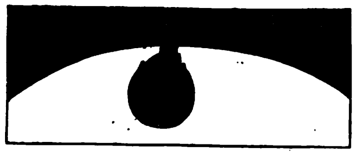


Fig. 9.—The Black Drop, as observed by Hirst.

There has been much discussion as to the cause of the 'black drop,' and in some instances considerable energy has been evinced in the attempt to show that

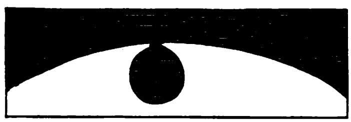


Fig. 10.—The Black Drop, as observed by Bevis.

this or that cause is the true one. It appears probable that the phenomenon is occasioned by the combination of several causes, and is widely variable in its extent. The general cause—by which I mean the resultant of the various causes in operation—is manifestly an apparent extension of the sun's disc, and an apparent contraction of the disc of Venus. Suppose, for instance, that the arc s s′v′ (fig. 11) represents part of the true outline of the sun, then this outline appears shifted outside its true place, or to the position indicated by the boundary between light and shade in the figure; and the apparent outline of Venus is shifted from s′v v′, its true position, to that shown by the outline of the black disc. Supposing this shifting of the outline to be uniform, and to continue unchanged in

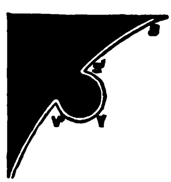


Fig. 11.

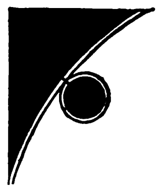


Fig. 12.

Illustrating the Formation of the 'Black Drop.'

extent, as Venus gradually passes on to the sun's face, it is clear that at the moment of true contact, when the real outlines touch, as shown in fig. 12, the apparent outlines will belong to two circles which are far from touching. But at the actual point of contact, where the widening of one outline and the contraction of the other cannot be supposed to act, there will still remain a fine black ligament. Under less perfect conditions this moment of true contact would not be attained, and instead of a fine ligament being seen just before Venus separated from the sun's edge, a wider ligament would be observed.

Thus far I have only indicated the general cause. And it may be said that this general cause is demonstrated by the observed effects. But we must now consider how this general cause is itself brought about; and herein lies the difficulty of the matter, whether regarded as a problem or considered with reference to the practical mastery of this occasion of error.

First, we have the rippling I have spoken of. Taking any point on the outline of the sun's disc or of Venus's, that point is swayed backwards and forwards across its true position by the effect of atmospheric undulation, the range of oscillation being greater or less according as the atmosphere is more or less perturbed, and as the sun is observed nearer to or farther from the horizon. A moment's consideration will show that the effect of such oscillations, operating all round both discs, must be to cause the sun's disc to cover (on the whole) a larger space than it should, while the disc of Venus covers a less space than it should. For there is a certain fringe of space all round both discs which is partially illuminated by these oscillatory movements, and this partial illumination extends the sun's disc outwards and contracts that of Venus. Probably this cause has but a small share in producing the general effect, except when the sun is low down.

Secondly, there is the optical effect caused by the fact that the image of a bright point is not itself a point. And here we have three causes in operation, which we may consider together. First, in the most perfect telescope the image of a point is what is called the 'circle of least confusion' between the two linear (or almost linear) foci. Secondly, diffraction affects the dimensions of the focal image of a point of light. And thirdly, if the telescope is defective, spherical aberration may operate so as seriously to affect the definition. All these causes combine to alter the image of each point of the outlines of the sun and Venus into a small disc of light instead of a point. The result necessarily is that the outlines extend beyond the true boundary of light and dark; that is, the disc of the sun is enlarged and that of Venus is contracted.

Thirdly, there is a cause which might, perhaps, have been combined with the last—the qualities of the eye regarded as an optical instrument; for the image of a point on the retina is not a point but a minute circle, even when the object is viewed directly.

Fourthly, there is the effect called irradiation, by which the apparent size of a bright object is enlarged. This effect will be greater or less according as the contrast between the bright object and the dark background on which it is projected is greater or less. Moreover, it appears that irradiation not only differs in amount with different observers, but varies even with the same observer at different times. Nay, its amount varies from moment to moment with the varying mental effort made by the observer to ascertain, more or less exactly, the true outline of the observed object.

We cannot wonder if the observations of the transit of 1761, affected as they were by peculiarities of appearance resulting from these various causes, for the operation of which observers were not on that occasion prepared, led to no trustworthy results.

CHAPTER III.

THE TRANSIT OF 1769.

THE general impression among astronomers, after the observations of 1761 had been discussed, was that too much reliance had been placed on Delisle's method. 'Experience,' wrote J. D. Cassini, later, in his 'Histoire du Passage de 1769,' 'is our chief instructor; the fruit of its lessons indemnifies us for the value of the years they cost us. The principal end had been missed, in 1761, for want of observations in places where the durations differed sufficiently. It was essential not to experience a second time the same disadvantage.'

Among the first statements published respecting the transit of 1769 was that by the ingenious Ferguson, who wrote as follows in 1762: 'On the 3rd of June, in the year 1769, Venus will again pass over the sun's disc, in such a manner as to afford a much easier and better method of investigating the sun's parallax than her transit in the year 1761 has done. But no part of Britain will be proper for observing that transit,[1]

[1] This was an error, due to Ferguson's reliance on Halley's tables; not, I need hardly say, the tables by which Halley had arrived at his

so as to deduce anything with respect to the sun's parallax from it, because it will begin but a little before sunset, and will be quite over before two o'clock next morning. The apparent time of conjunction of the sun and Venus, according to Dr. Halley's tables, will be at thirteen minutes past ten o'clock at London, at which time the geocentric latitude of Venus will be full ten minutes of a degree north from the sun's centre; and therefore, as seen from the northern parts of the earth, Venus will be considerably depressed by a parallax of latitude on the sun's disc; on which account the visible duration of the transit will be lengthened; and in the southern parts of the earth she will be elevated by a parallax of latitude on the sun, which will shorten the visible duration of the transit with respect to its duration as supposed to be seen from the earth's centre; to both which affections of duration the parallaxes of longitude will also conspire. So that every advantage which Dr. Halley expected from the late transit will be found in this, without the least difficulty or embarrassment. It is, therefore, to be hoped that neither cost nor labour will be spared in duly observing this transit, especially as there will not be such another opportunity again in less than 105 years afterwards.'

Ferguson also showed accurately the places where advantage could be best taken of Halley's method: 'The most proper places for observing the transit in

incorrect ideas respecting the circumstances of the earlier transit, but those which Halley had formed subsequently.

the year 1769 are in the northern parts of Lapland, and the Solomon Isles in the Great South Sea, at the former of which the visible duration between the two internal contacts will be at least twenty-two minutes greater than at the latter, even though the sun's parallax should not be quite 9″. If it be 9″ (which is the quantity I had assumed in a delineation of this transit which I gave in to the Royal Society before I had heard what Mr. Short had made it from the observations of the late transit), the difference of the visible durations, as seen in Lapland and in the Solomon Isles, will be as expressed in that delineation; and if the sun's parallax be less than 9″ (as I now have very good reason to believe it is) the difference of durations will be less accordingly.'

Later, Hornsby in England, and De Lalande and Pingré in France, discussed very carefully the circumstances of the transit of 1769. De Lalande, in 1764, illustrated the conditions of the transit of 1769 by a projection of the earth planned like that which Delisle had made for the transit of 1761. The tables of Cassini formed the basis of the calculations made by these astronomers; and as Cassini had had the advantage of later observations of Venus, his tables were necessarily more accurate than those which Halley had completed earlier in the century.

The circumstances of the transit of 1769 in different parts of the earth can be inferred from what is shown in Plate V. Here the arcs A I B and A′ I′ B′ separate the dark and light hemispheres of the earth at the beginning of the transit, while the arcs C E D and C′ E′ D′ separate the dark and light hemispheres at the end of the transit. Thus the beginning was visible at all places on the hemisphere formed by combining the sections A I B D and A′ I′ B′ D′; and the end of the transit was visible at all places on the hemisphere formed by combining the sections C E D A and C′ E′ D′ A′. The whole of the transit was visible over the spaces D *e i* A and D′ *m′* A′; but within the space *i m e*, though the beginning and end of the transit were seen, the progress of the transit was not *wholly* visible. No part of the transit was visible over the spaces B *m* C and B′ *i′ e′* C′; but within the space *i′ m′ e′*, though neither the beginning nor the end were visible, the progress of the transit was partially visible. At all points of the arcs A I B and A′ I′ B′ the ingress occurred with the sun on the horizon; but whereas the sun was rising for the arcs A *i* and A′ *i′*, he was setting for the arcs B *i* and B′ *i′*. At all points of the arcs C E D and C′ E′ D′ the egress occurred with the sun on the horizon; but whereas the sun was rising for the arcs C *e* and C′ *e′*, he was setting for the arcs D *e* and D′ *e′*. At the points *m* and *m′* the sun was on the horizon both at ingress and at egress; but whereas the whole transit, except ingress and egress, took place during the night at *m*, it took place during the day at *m′*. All that has here been said has related to the passage of Venus's centre.

The point I was that where ingress occurred earliest,[1] the point I′ being that where ingress occurred latest. It was around these points that observers of ingress by Delisle's method were to be placed, on that side of the arcs A B and on A′ B′ where the sun would be above the horizon at the time of ingress. We see that Great Britain was admirably placed for observing accelerated ingress, Greenwich being almost as well placed as any station could possibly be, though having the sun rather low (and unfortunately it appeared from Halley's tables as though the sun would be still lower). At Greenwich sunset was approaching when transit began. I′ was in a little known part of the Southern Seas.

The point E′, where egress occurred earliest, was, like I′, placed in a part of the Southern Seas about which little was at that time known. E, where egress occurred latest, was so placed that the whole of

[1] Encke, in 1822, found the following elements for the transit of 1769. I quote these, like the elements for 1761, from M. Dubois' 'Les Passages de Vénus sur le disque Solaire':—

	h.	m.	s.	
Ingress of Venus's centre	7	26	54·5	Greenwich apparent solar time.
Middle of the transit	10	27	20·8	
Egress of the centre	13	27	51·3	
Duration of the transit	6	0	56·8	
Least distance of centres	0	10	8·1	

	Latitude °	′	Longitude °	′
Pole of accelerated ingress	49	33 N	7	23 E
„ „ retarded „	49	33 S	172	37 W
„ „ accelerated egress	22	30 S	122	46 W
„ „ retarded „	22	30 N	57	14 E
„ „ shortened durations	38	37 S	143	2 W
„ „ lengthened „	38	37 N	39	58 E

India and the region between the north-west of India and the Sea of Aral was suitable for observing this phase.

But chief interest was attached, as I have said, to the application of Halley's method. The Halleyan poles were at H and H′, these being respectively the points where, in a geometrical sense (that is, without taking into account the actual visibility of ingress or egress), the transit would be respectively most lengthened and most shortened. H, we see, lay in a region whence no part of the transit could be seen, and the point *m* was the nearest to H where both the beginning and end would be visible, but with the sun upon the horizon. The space *i m e* was that which presented the most promising conditions, except that the sun would be low within this space, both at ingress and at egress (passing below the horizon for a greater or less portion of the progress of the transit). Wardhuus, in Lapland, close to this region, was selected for the most northerly Halleyan stations; and as the polar regions could not be occupied, the stations next in order of value were necessarily those lying on the opposite side of the arctic circle, from Kamschatka, through Alaska, &c., round to Hudson's Bay. These, however, were too far away from H to be of great value as Halleyan stations, while their great distance from I and E prevented them from having any special value as Delislean stations. The southern Halleyan pole H′ was in the same unknown quarter of the Southern Seas in which I′ and E′ are seen to lie. In fact, the whole region around H′ was of extreme importance for the observation of the transit of 1769, since any station placed there would not only be excellent for Halley's method, but also for Delisle's, both as respects retarded ingress and accelerated egress.

In passing, let it be noted how the superiority of the second transit of a pair (in general) shows itself by the positions of H and H′ in Plates IV. and V. respectively. In Plate IV. we see that H and *m* lie on opposite sides of the north pole, H′ and *m′* on opposite sides of the south pole; whereas in Plate V. we see that H and *m* are on the same side of the north pole, H′ and *m′* on the same side of the south pole. Now, necessarily one Halleyan pole lies in the region whence no part of the transit can be seen, and we see that in such a case as that illustrated by Plate V. the point *m* indicates how near that particular Halleyan pole H can be approached without losing either the beginning or end of the transit; whereas in the case illustrated by Plate IV., *m′*, the point of nearest available approach, lies very much farther away from the corresponding Halleyan pole H′. Still, it is to be noticed that, even in the case of a transit like that of 1769 (Plate V.), the really effective use of Halley's method requires that a station should be reached near the space corresponding to *e m i*. If this cannot be arranged, the stations next in order of value are those lying on the farther side of the arctic or antarctic circle (as the case may be), and such stations will commonly not be much better than one near *m′* for the transit of 1761 (Plate IV.), and may be even far inferior to stations available in the case of such a first transit as that of 1874 (Plate VI.). This is, in fact, the reason why Halley's method fails totally in 1882 (see Plate VII.), though this is the second transit of a pair.

The actual operations for viewing the transit of 1769 were carried out on a widely extended scale. Preparations were made for sending observers to the South Sea, California, Mexico, Lapland and Kamschatka. The King of Denmark invited Father Hell, the eminent German astronomer, to observe the transit at Wardhuus, in Lapland, and thither Hell betook himself with Borgrewing, the Danish astronomer. They arrived in the autumn of 1768, and passed the winter in that desolate region. Chappe d'Auteroche was selected by the French Academy to observe the transit from the Solomon Isles, in the South Sea; but, says M. Dubois, 'the South Sea at that epoch was under the rule of Spain, and it was only possible to visit those seas in a Spanish vessel, and with the permission of the Court of Spain. The Spanish Government refused such permission, but gave Chappe leave to embark in the Spanish fleet then about to sail for Western America.' Chappe eventually observed at St. Joseph, in California.

'England,' says M. Dubois, 'did not wait for permission from Spain to send an astronomer to observe the transit from the South Sea.' The following account, taken from 'Cook's Voyages,' describes the preparations made for the journey :—

'It having been long before calculated that the planet Venus would pass over the sun's disc in 1769, a phenomenon of great importance to astronomy, and which had engaged the attention of men of science, it was judged that the most proper place for observing this phenomenon would be either at the Marquesas or at one of those islands to which Tasman had given the several appellations of Amsterdam, Rotterdam, and Middleburg, but which are now better known under the general name of the Friendly Islands. This being a matter of so much importance in the science of astronomy, the Royal Society, with that laudable zeal they have ever shown for its advancement, presented a memorial to his Majesty at the beginning of the previous year, requesting among other things that a vessel might be fitted out, at the expense of the Government, to convey proper persons to observe this transit at one of the places already mentioned. The petition being readily complied with, and orders having been given by the Admiralty to provide a vessel for that purpose, on April 3, Mr. Stephens, the Secretary to the Board, informed the Society that everything was progressing according to their wishes.

'Mr. Dalrymple was originally fixed upon to superintend this expedition : a man eminent in science, a member of the Royal Society, and who had already greatly distinguished himself respecting the geography of the Southern Ocean. As this gentleman had been regularly bred to the sea, he insisted (very properly too) on having a brevet commission, as captain of the vessel, before he would undertake the employment. Sir Edward Hawke (afterwards Lord Hawke, a naval officer, and not a civilian), who then presided at the Admiralty, violently opposed this measure; and being pressed on the subject, declared that nothing would induce him to give his sanction to such a commission.

'Both parties were inflexible, and it was therefore thought expedient to look out for some other person to conduct the expedition. Accordingly, Mr. Stephens, having recommended Lieutenant Cook, and this recommendation having been strengthened by the testimony of Sir Hugh Palliser, who was well acquainted with Cook's merit and abilities for the discharge of this office, he was appointed to this distinguished post by the Lords Commissioners, and promoted to the rank of Lieutenant of the Royal Navy on May 25, 1768. He was now, be it remembered, close upon forty years of age.

'This appointment having taken place, Sir Hugh Palliser was commissioned to provide a vessel adapted for such a voyage. After examining a great number then lying in the Thames, in conjunction with Cook, of whose judgment he entertained the highest opinion, they at last fixed upon the 'Endeavour,' a barque of 370 tons, which had been built for the coal-trade.

'In the interim, Captain Wallis having returned from his voyage round the world, and having signified to the Royal Society that Port Royal Harbour, in

King George's Island, now called Otaheite, would be the most convenient place for observing the transit, his opinion was adopted, and the observers were ordered to repair thither.

'Mr. Charles Green, the coadjutor of Dr. Bradley, the Astronomer Royal, was nominated to assist Captain Cook in conducting the astronomical part of the undertaking; and he was accompanied also by Joseph Banks, Esq. (afterwards Sir Joseph, the President of the Royal Society). This friend of science possessed at an early period of life an opulent fortune, and being zealous to apply it to the best ends, embarked on this tedious and hazardous enterprise, animated by the wish of improving himself and enlarging the bounds of knowledge. He took two draughtsmen with him, and had likewise a secretary and four servants in his retinue.

'Dr. Solander, an ingenious and learned Swede, who had been appointed one of the librarians in the British Museum, and who was particularly skilled as a disciple of Linnæus, and distinguished in his knowledge of natural history, likewise joined the expedition. Possessed with the enthusiasm with which Linnæus inspired his disciples, he braved danger in the prosecution of his favourite studies; and being a man of erudition and capability, he added no small *éclat* to the voyage in which he had embarked.

'Though the principal intention of this expedition was to observe the transit of Venus, it was thought proper to comprehend other objects as well. Captain

Cook was therefore directed, after he had accomplished his main business, to proceed in making further discoveries in the South Seas, which now began to be explored with uncommon resolution.'

The expedition sailed from Deptford on July 30, 1768, and on August 13 anchored in Plymouth Sound, from which after a few days' stay they proceeded to sea. It was not until April 10 that they saw Otaheite. 'On the 10th,' says the narrative, 'upon their looking out for the island to which they were destined they saw land ahead. The next morning it appeared very high and mountainous, and it was known to be King George the Third's Island, so named by Captain Wallis, but by the natives called Otaheite.'

In May they 'began to make preparations for observing the transit of Venus; and from the hints which Captain Cook had received from the Royal Society, he sent out two parties to make observations from different spots, that in case they failed at Otaheite they might succeed elsewhere. They employed themselves in preparing their instruments, and giving instructions in the use of them. On Thursday, June 1 (the next Saturday being the day of the transit), they sent the long-boat to Eimayo, having on board Mr. Gore, Mr. Monkhouse, and Mr. Sporing, a friend of Mr. Banks, each furnished with necessary instruments by Mr. Green. Mr. Banks and several of the Indians went out with this party. Others were despatched to find out a convenient spot at such a distance from their principal station as might suit their purpose.

Those who went to Eimayo in the long-boat, after rowing the best part of the night, by the help of some Indians on board a canoe which they hailed, found a proper situation for their observatory upon a rock, where they fixed their tents, and prepared the apparatus for the following day's observation. On Saturday, June 3, as soon as it was light, Mr. Banks left them to go to the island for fresh provisions. As he was trading with the natives who belonged to Tarras the king of the island arrived, with his sister, whose name was Nuna, in order to pay him a visit. . . . Mr. Banks returned to the observatory with his visitors, and showed them the transit of the planet Venus over the sun's disc, informing them that he and his companions had come from their own country solely to view it in that situation. Both the parties which were sent out made their observations with great success. They nevertheless differed in the accounts of the times of transits more than might have been imagined.' In Captain Cook's journal, the following account is given: 'The day proved as favourable to our purpose as we could wish; not a cloud was to be seen the whole day, and the air was perfectly clear; so that we had every advantage in observing the whole of the passage of the planet Venus over the sun's disc. We very distinctly saw an atmosphere, or dusky shade, round the body of the planet, which very much disturbed the times of the contact, particularly the two internal ones. It was nearly calm the whole day, and the thermometer, exposed to the sun about the middle of the day, rose to a degree of heat we have not before met with.'

Chappe was specially fortunate at St. Joseph. His observation has given rise to a good deal of controversy, with regard to its bearing on the question of the solar parallax. Powalky and others consider that Chappe's observation of the internal contact at egress was an observation of real contact, not apparent contact; Stone maintains the contrary. My attention was specially directed to this point by Newcomb, of Washington, U.S., and I must confess that Chappe's narrative seems to me unquestionably to bear the interpretation given to it by Powalky, with whom Newcomb agrees. Let the reader judge, remembering that real contact means the formation of the black drop or of the pear-shaped figure described at page 57; so that at total ingress real contact is later than apparent, while the reverse is the case at egress. Chappe writes as follows:—
'At the total ingress I observed very distinctly the second phenomenon, which had been noticed by the greater part of the observers in 1761. The edge of the disc of Venus lengthened itself, as if it had been attracted by the sun. I did not observe, for the instant of total ingress, the instant when the edge of Venus commenced to extend itself; but, not being able to doubt that this black point was not part of the opaque body of Venus, I observed the moment when it ended ('où il était à sa fin') in such sort that the total ingress could not have occurred earlier, though perhaps later by two or three seconds. The black point was

a little less dark than the rest of Venus; I think it is the same phenomenon which I had observed at Tobolsk in 1761. . . . At the second internal contact' (that is, internal contact at egress), 'the sun was undulating, as was Venus also, which rendered the observation very difficult. At this contact Venus elongated herself more considerably than in the morning, in approaching suddenly the edge of the sun.' It seems clear that Chappe here witnessed that sudden leap to the sun's edge at egress which is the counterpart of the sudden leap from the sun's edge at ingress; and that if the contact differed at all from the contact at ingress, it was in the fact that a longer leap was made, in other words, that he caught an earlier phase at ingress, which would correspond of course to a later phase at egress. As Chappe says himself that real contact at ingress might have been two or three seconds later, but certainly not earlier, we see that the contact he observed at egress corresponded even more closely with what he regarded as real contact,—that is, the moment of the leap by which the black drop is formed and broken. Yet Mr. Stone considers that at egress Chappe missed the real contact and observed the later phase of apparent contact.[1]

Le Gentil experienced in 1769 the culmination of

[1] Here and at pp. 90–92, I retract the views I expressed in my 'Sun,' and in reply to Prof. Newcomb's general criticism on my account of Stone's work. So soon as we met, and he described his objections in detail, I recognised their force. The Astronomical Society had, in fact, pronounced so decisively in favour of Stone's treatment of the transit of 1769, that I was not prepared to find errors so serious in it.

his misfortunes. With a persistent courage worthy of better success he determined, after his failure in 1761, to return to Pondicherry as soon as an opportunity presented itself, and to await there during eight years the transit of 1769. Dubois remarks that Le Gentil usefully employed those years in studying the astronomy of the Brahmins, on which subject he published an interesting work upon his return to France. But the object he had specially in view was unfortunately not attained. 'On June 3, 1769,' says Dubois, 'at the moment when this indefatigable observer was preparing to observe the transit, a vexatious cloud covered the sun, and caused the unhappy Le Gentil to lose the fruit of his patience and of his efforts.' Pondicherry would have been a useful station for observing the retarded egress, as we see from Plate V.

Pingré, who had observed the transit of 1761 at Rodriguez, was sent to observe the transit from a French station in the island of St. Domingo.

Although the observations made in 1769 were on the whole much more satisfactory than those which had been made in 1761, yet there was much to throw doubt on any determination of the sun's distance based even on the later transit. We have seen already that the peculiar distortion of Venus, illustrated in pp. 61–63 was presented in a marked degree in 1769; but even more unpromising was the observed difference in time between the moments of real and apparent contact. It is only necessary, as M. Dubois points out, to consider the difference recognised by those observers who noted the two phases, to see how largely the accuracy of the deduced solar distance must be affected by this cause.

Wales, at Hudson's Bay, using a telescope two feet long, magnifying 120 times, found a difference of 24 seconds between the real and apparent contacts at egress. Green, at Otaheite, found a difference of 40 seconds at ingress and 48 seconds at egress. Cook, at the same station, found the difference 60 seconds at ingress and 32 seconds at egress. Yet these two observers used two similar telescopes, magnifying 140 times. Maskelyne, at Greenwich, using a telescope magnifying 140 times, found the difference 52 seconds; while Horsley, at the same station, with an achromatic telescope, 10 feet in length, magnifying 50 times, found the difference to be 63 seconds. Maskelyne remarks that the difference was greater than he had expected, considering that the telescopes were all nearly of the same quality, except a reflector of six feet used by Hitchins. The superiority of this instrument appeared to Maskelyne to account for the difference of 26 seconds, by which interval Hitchins observed the internal contact earlier than Maskelyne. Hornsby, at Oxford, used an achromatic telescope of 7½ feet, magnifying ninety times, and found the difference to be 57½ seconds; while Schuckberg, also observing at Oxford, found a difference of 69 seconds between the real and apparent contacts. An unknown observer at Caen, using a very small telescope, found the enormous difference of fully 150 seconds! Wilke, at Stockholm, also using a very small telescope, estimated the difference at 48 seconds. Lastly, Euler, observing at Orsk, with a telescope 12 feet in length, noted for the instants of contact two epochs differing by 30 seconds.

When we consider these wide and widely varying differences among observers who observed both kinds of contacts, we cannot wonder if considerable differences of absolute time were noted between observations of the same contact by different observers either at the same stations or at stations near enough for instituting a comparison. Thus Le Monnier and De Chabert, at St. Hubert, noted instants of contact differing 36 seconds from each other; while between Duval le Roy and De Verdun, at Brest, there was a difference of 30 seconds.

It is well remarked by Dubois that observations of external contact at ingress can have no value. He adds that observations of external contact at egress are somewhat more reliable; but it must be very difficult to distinguish the moment when the solar limb resumes an exactly circular shape. Accordingly the fourteen exterior contacts noted by different observers could have no real value. Yet it is worthy of remark that the difference between moments of external contact observed at St. Petersburg by Mayer and Stahl amounted only to 27 seconds; while at Gurief the difference between two such observations amounted to 28 seconds. So that, as Newcomb has remarked of the observations made during the transit of Mercury in November 1868, it would seem as though the errors in the estimated instant of an external contact might be expected to be of the same order as those affecting the estimated instant of an internal contact.

Dubois tells us that upwards of two hundred memoirs were sent to the Academy of Sciences on the value of the solar parallax deducible from the observations made in 1769. How many were sent to the Royal Society I do not know; but probably as many as four hundred were sent to the different learned bodies of Europe.

A comparison of the results obtained by the most competent computers showed that the observations of 1769 were much more valuable on the whole than those of 1761; for, whereas the results obtained in 1761 ranged in value between 8″·5 and 10″·6, we find the following five results selected as those most carefully calculated on the basis of the observations of 1769 :—

De Lalande fixed the parallax at			8″·50
Fr. Hell	"	"	8·70
Hornsby	"	"	8·78
Euler	"	"	8·82
Pingré	"	"	8·88

The solar distances corresponding to the parallaxes 8″·50 and 8″·88 are respectively 96,162,840 miles and 92,049,650 miles.

It is somewhat singular that, notwithstanding the clearest evidence of a cause of uncertainty sufficing to account for such differences as the above table presents, Lalande and Pingré, who had obtained the most widely different results, were both quite confident of the accuracy of the values they had deduced. Lalande says in his memoir, that regarding the whole series of observations of 1769, the solar parallax is incontestably 8″·5; while Pingré says in reply, 'of two things one: either no result at all can be deduced from the transit of 1769, or it must be admitted that the value of the solar parallax is very close indeed to 8″·8 ('est à très-peu près de 8″·8).'

In his first memoir, in which the above tabulated value, 8″·82, was given, Euler had not taken into account the observations made at Otaheite by Green, and at St. Joseph, in California, by Chappe. Going over his work afresh, and introducing these observations, he deduced the parallax 8″·68. Dionis du Séjour, employing only observations of duration, and combining the transits of 1761 and 1769, deduced the value 8″·84. But in his 'Traité Analytique des Mouvements Apparents des Corps Célestes' he adopts as the final result of his calculations the solar parallax 8″·8128.

It is worthy of notice that when chief reliance was placed on observations made at Halleyan stations of the first class, the value of the parallax approached more nearly to that now recognised as probably the more correct. Thus, combining observations made at Otaheite with Father Hell's observations at Wardhuus, De Lalande obtained the value 8″·72, and yet larger values when Hell's observations were combined with other observations. Yet, as we have seen, De Lalande adopted 8″·5 as the best mean value of the solar parallax.

Doubts, indeed, were thrown upon Father Hell's observations, on account of corrections which had been made in his MS. notes of the phenomena (and partly, also, because of the known fact that he alone of all the observers of the transit recognised no distinction between real and apparent contacts). The idea that Hell's records were forged was thrown out by the Astronomer Royal. But such a suspicion need hardly be seriously considered. Not only is nothing known about Fr. Hell which for a moment justifies the supposition that he could be guilty of the act charged to him, but we know now that his observations accord better with the latest estimates of the parallax than those of other observers.

Encke in 1824 published an analysis of the observations of the transit of 1769, from which he deduced for the solar parallax the value 8″·6030. By combining the observations of both transits he deduced that value 8″·5776 (corresponding to a solar distance of 95,274,000 miles) which for more than a quarter of a century thereafter maintained its ground in treatises on astronomy.

But about the year 1850 it began to be recognised that the sun's distance had been over-estimated. Various methods of determining the solar parallax, inferior singly to the observation of transits of Venus, but collectively superior—and superior, moreover, because of the greater accuracy with which (owing to the improvement in instruments of precision) they could be applied—concurred in showing that the sun's distance was less than had been supposed by at least three millions of miles. The consideration of these methods in detail would occupy more space than is here convenient. The reader will find them fully described in the second chapter of my treatise on the sun. In this place let the following summary suffice:—

In 1845 Hansen announced that by a method based on observations of the moon's motions he had deduced the parallax 8″·9159, corresponding to a distance of 91,659,000 miles. Leverrier, from the careful study of the sun's apparent motions, as affected by the earth's monthly revolution around the common centre of gravity of herself and the moon, deduced a solar parallax of 8″·95, corresponding to a distance of 91,330,000 miles. Prof. Newcomb, of Washington, U.S., obtained by the same method the parallax 8″·84, distance 92,500,000 miles. From observations of Mars when at his nearest to the earth Prof. Newcomb deduced the parallax 8″·85, corresponding to a distance of 92,300,000 miles. Stone, formerly of Greenwich, obtained by this method the distance 91,400,000 miles; while Winnecke deduced the distance 91,200,000 miles. Foucault, measuring the velocity of light by means of a rapidly revolving mirror (a plan devised by Wheatstone), and comparing the value so obtained with that inferred from the observation of the eclipses of Jupiter's satellites and the aberration of light, deduced the solar parallax 8″·86, corresponding to a distance of 92,100,000 miles. From the study of those planetary perturbations which depend on the relative masses of the earth and the other planets Leverrier deduced the value 8″·859 for the parallax, or 92,110,000 miles for the sun's distance. It will be seen that the values thus obtained indicate a solar parallax of 8″·89, corresponding to a distance of about 91,950,000 miles. The limits of probable error are considerable, however, and we scarcely know more at present than that the solar parallax almost certainly lies between the values 8″·82 and 8″·96, corresponding to the distances 92,676,000 miles and 91,228,000 miles.

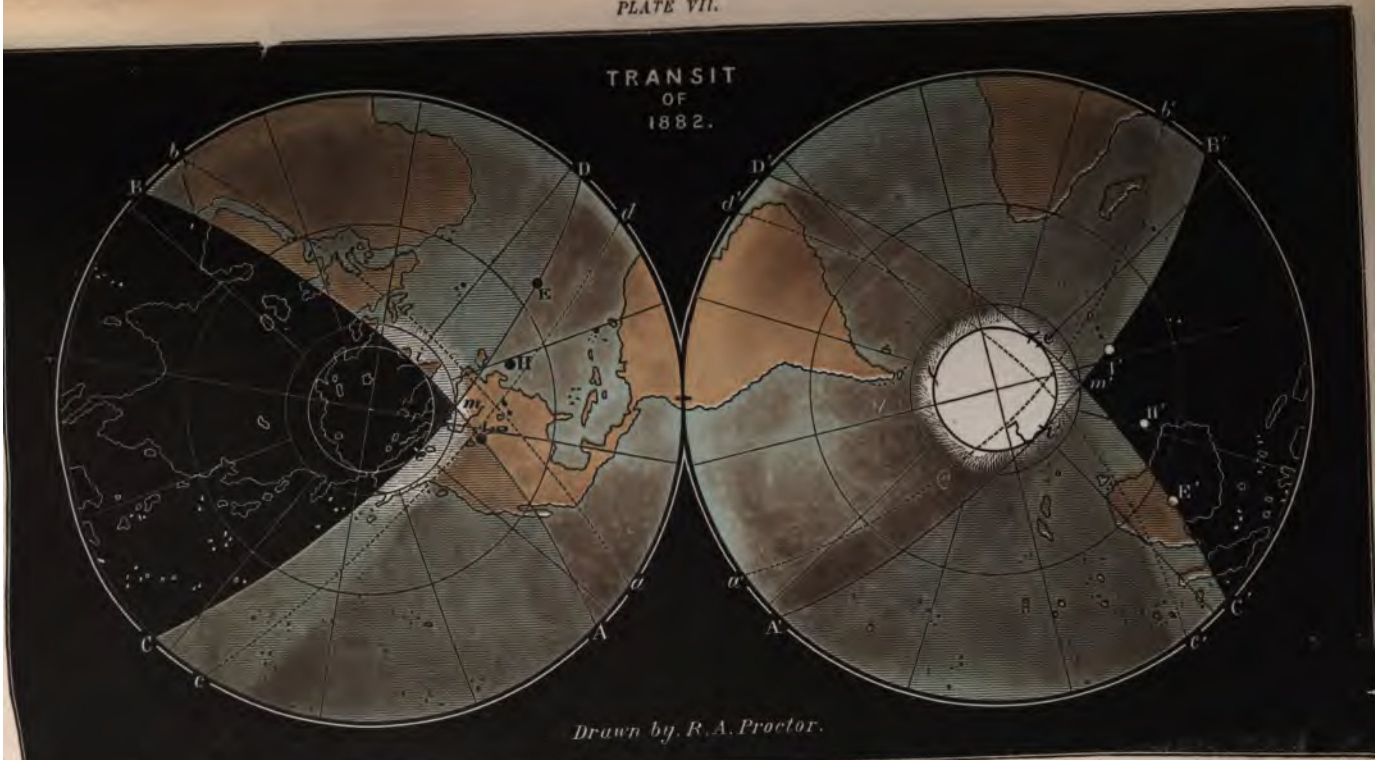
As soon as it became clearly recognised that Encke's estimate of the sun's distance from observations of the transit of 1769 was considerably in error, doubt necessarily fell upon the method itself which had till then been regarded as the most satisfactory for determining the sun's distance. Efforts, however, were made to restore the credit of the method by a re-examination of the observations made in 1769. These efforts have been regarded by many, especially in this country, as successful; but it must be confessed the investigation has shown us rather how the error crept in than how it can be avoided in future applications of the method. This will appear when we consider the nature of the researches by which astronomers have sought to restore the waning credit of the observations of 1769.

Powalky in 1864 discussed forty-four observations, ence varied greatly with the varying circumstances under which the observations were made, and *always largely exceeded seventeen seconds*, it seems quite impossible to adopt Mr. Stone's method as trustworthy. We cannot, therefore, wonder that Continental and American astronomers have, by common consent, declined to accept Mr. Stone's results as having much weight, or indeed as proving anything except what had already been ascertained—the fact, namely, that the observations made in 1769 afford but unsatisfactory evidence respecting the sun's distance.

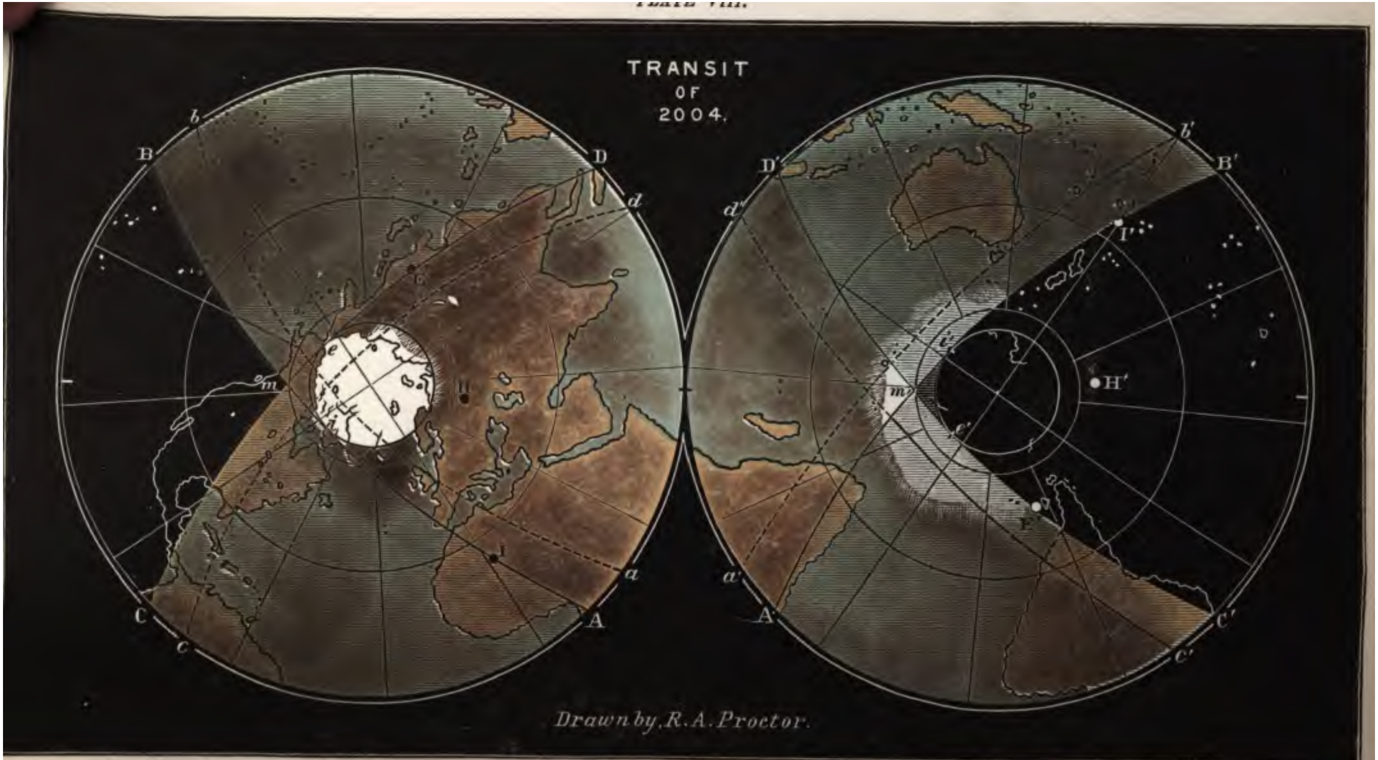
But the imperfect nature of the observations made in 1761 and 1769 can be sufficiently explained without attributing inferiority to the method of determining the sun's distance in pursuance of which the observations were made. It cannot be doubted that the measurement of the sun's distance resulting from those observations was more trustworthy than any which could have been obtained at that time by other methods. We have learned to apply other methods so much more accurately than they could have been applied in the last century, that they give better results than a superior method could then give. But it still remains probable that the method depending on the observation of Venus in transit *is* superior to other modes of determining the sun's distance; and that when this method is applied with the improved instruments of our time its superiority will be rendered manifest.

PLATE VI.

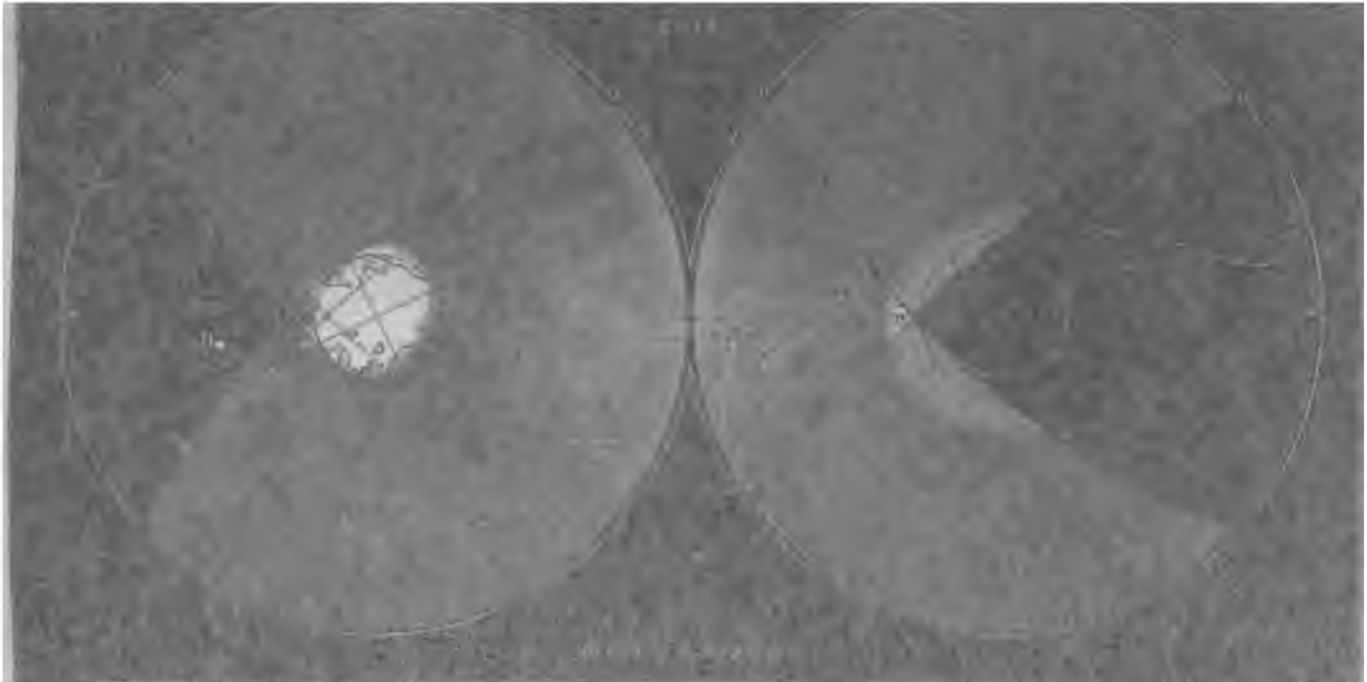
A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along $a\,b$, $a'\,b'$, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along $c\,d$, $c'\,d'$, sun 10° high at egress.



A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along a b, a′ b′, sun 10° high at ingress.
… along c d, c′ d′, sun 10° high at egress.



A B and A' B' separate sunlit and darkened hemispheres at ingress; along *a b*, *a' b'*, sun 10° high at ingress.
C D and C' D' separate sunlit and darkened hemispheres at egress; along *c d*, *c' d'*, sun 10° high at egress.
I, I' are the Delislean poles for ingress; E, E', those for egress; H, H', the Halleyan poles.



A B and A' B' separate sunlit and darkened hemispheres at ingress; along a b, a' b', sun 10° high at ingress. C D and C' D' separate sunlit and darkened hemispheres at egress; along c d, c' d', sun 10° high at egress. I, I', are the Delislean poles for ingress; E, E', those for egress; H, H', the Halleyan poles.

PLATE VIII.

A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along $a\,b$, $a'\,b'$, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along $c\,d$, $c'\,d'$, sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan

CHAPTER IV.

OF TRANSITS AND THEIR CONDITIONS.

BEFORE we proceed to the consideration of the transits now approaching it will be desirable to enter on a more complete examination than heretofore of the general principles on which the determination of the sun's distance by observation of Venus in transit depends. To this subject the present chapter is therefore given. It deals with the various methods which are available for determining the sun's distance, the order in which transits recur, and lastly, the considerations on which the choice of stations will depend, in any given transit. These various points I wish to treat in an entirely popular manner, and therefore I shall leave out of account all those minor details which have to be considered in the complete discussion of the subject, referring the reader who may wish for a more thorough investigation of the matter to my 'Essays on Astronomy' and 'The Universe and the Coming Transits.'

First, then, let us consider the passage of Venus between the earth and the sun on the occasion of a transit, and see how the sun's distance may be inferred from the various appearances presented when the transit is viewed from different parts of the earth.

Let E E′ (fig. 13) be the earth, and V Venus passing between the earth and the sun (at S) on the course

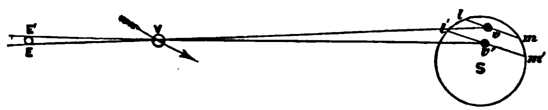


Fig. 13.—Illustrating the general principles on which the determination of the Sun's distance by transit observation depends.

shown by the arrow, so that at the moment indicated by the figure a transit is in progress. At this moment let us suppose that from a northern station E′ Venus is seen projected upon the sun's face at *v′*, while from a southern station E she is projected at *v* (*v* and *v′* marking the place of her *centre*). It is to be noted that true perspective being quite out of the question, I here for convenience suppose the circle S to represent the *disc* of the sun seen from E, so that in considering what follows the reader need not trouble himself about the curved nature of the sun's *surface.*

Now, the *proportions* of the solar system being well known ever since the Copernican theory was established —or rather, since Kepler's laws were discovered—we know that the distance of V from the sun bears to the distance of E from the sun the proportion of about 72 to 100; whence, immediately, we see that E V bears to V *v* the proportion of about 28 to 72, or 7 to 18. And manifestly the opening-out of the lines V E and V E′ at the earth is less than their opening-out at the sun in this same proportion of 7 to 18; so that, for instance, if the two stations E and E′ are 7,000 miles apart (meaning the distance in a straight line, and for simplicity assuming that V E and V E′ are equal lines symmetrically placed with respect to the earth's globe), then *v v′* is a distance of 18,000 miles. But such a determination as this, if justly and satisfactorily made, would in point of fact amount to a determination of the sun's size, and therefore of the sun's distance. Observe—the astronomer at E is supposed to have accurately determined the apparent position of Venus's centre at *v*, while the astronomer at E′ has accurately determined the apparent position of her centre at *v′*; thus they know what proportion *v v′* bears to the diameter of the disc S, that is, to the sun's diameter. Say, for instance, they find it to be the 47th part of this diameter. But they know also that *v v′* is 18,000 miles in length. So that the sun's diameter is 47 times 18,000 miles, or 846,000 miles.

So soon, however, as we know the real size of the sun we know his distance. We know how large he looks, and a globe of given size can only present a certain apparent size at a certain distance. For example, a globe one inch in diameter looks just as large as the sun at a distance of about 107⅓ inches, or a little less than 9 feet;[1] a globe two inches in diameter

[1] A halfpenny, which has a diameter of one inch, will be found to exactly conceal the sun when placed at a distance of 107⅓ inches, the sun being at about his mean distance—that is, the observation being made in March, April, September, or October.

must be set twice as far away to look just as large as the sun; a globe three inches in diameter thrice as far away; and so on. In brief, the sun (like any one of these globes when placed as described) lies at a distance 107⅓ times as great as his own diameter. So that multiplying 846,000 by 107⅓ we get for the sun's distance (as resulting from the observations imagined above) 90,160,000 miles.

The considerations just discussed form the basis of all the various methods for determining the sun's distance by transit observations. These methods are only so many contrivances for bringing out the true result as satisfactorily as possible, by eliminating the various possible sources of error.

We may call the method just sketched the *direct method*, because it depends on the simple observation of the place of Venus on the sun's face. I shall have occasion presently to discuss the method somewhat more in detail. Let it suffice, here, to notice that the method presents manifest difficulties. The two observers, at E and E′, are of course not in direct communication; yet it is essential that their observations should either be made exactly at the same time or that at least the exact difference of time should be known. Again, it is not an easy matter to measure the place of Venus on the sun's face with the accuracy that the method requires. For these reasons Halley was led, in anticipation of the transit of 1761, to devise another method.

Let us suppose, for simplicity, that the two stations at E and E′ are not shifted by the earth's rotation while the transit lasts. In this case the observer at E would see Venus traverse such a path as *l v m*, while the observer at E′ would see her traverse the parallel path *l′ v′ m′*. The time occupied by Venus in each case would of course be proportional to the apparent length of the lines *l m* and *l′ m′*; so that if the time were accurately noted by the two observers, the apparent lengths of these lines would be known; whence, of course, the simplest possible geometrical considerations would give the position of the two chords and the apparent distance *v v′* separating them from each other. This known, the sun's size and distance follow as in the direct method. Since the moment when Venus has just made her complete entry on the sun's face at ingress, and is just about to begin to leave his face at egress (in other words, the moments when her disc just touches the sun's edge on the inside), were supposed by Halley to be determinable with great accuracy, such a method as has just been described seemed to him admirably adapted for determining the sun's distance.

But clearly the difference of time in the imaginary case we have been dealing with, where the earth's rotation was neglected, will depend on the position of the chord of transit. Supposing Venus to traverse the centre of the sun's face, the two chords being equal in length, there would be no difference of time, while the difference would be great if the two chords were near the edge of the disc. In the latter case the method would be most successfully applicable, while in the former it would not be applicable at all. Now, we have seen that the transit of 1761, as calculated by Halley (see pp. 34 and 35), was nearly central. Nevertheless, owing to the rotation of the earth, a difference of duration would occur in the case of such a transit. In a general way this has been already shown in the note on pp. 34 and 35. But it is also easy to show that a displacement comparable with the $v\,v'$ of fig. 13 can be inferred from time observations applied as Halley suggested for the supposed conditions of the transit of 1761.

Let us suppose that in fig. 14 we are looking down upon the earth E and Venus V from the north, Venus

Fig. 14.—Illustrating the effect of the Earth's rotation on the motion of Venus in transit.

travelling (with respect to the earth)[1] in the direction shown by the arrow. Let us suppose I E to represent a chord of transit across the face of the sun S. Now, the earth is rotating in the direction *w e* E along the arc *w e*; and as a transit may last several hours,

[1] It is generally convenient to suppose the earth at rest, and Venus travelling only with the *excess of her motion* over the earth's motion around the sun.

a place which was at *w* when transit began (that is, when Venus appeared to be at I) would be carried by rotation to some point *e* by the time the transit ended (that is, when Venus appeared to be at E). In order to see the effect of such a rotation-shift on the apparent motion of Venus, let us take two lines, one from *w*, the other from *e*, through the centre of Venus (supposed at rest at V, near the middle of the transit) to the chord I E; we see that the line from he *earlier* position *w* passes to *v*, while the line from the *later* position *e* passes to *v′*. Thus the effect of the rotation of the earth during the time of transit, if considered alone, corresponds to a shifting forwards of Venus by the amount *v v′*. In other words, transit is shortened by the effect of rotation in direction *w e*. Suppose now another observer placed at the pole (whichever pole happened to be in sunlight at the time), so as not to be at all affected by rotation; or that, being placed near either pole, he were much less affected by rotation; or that, being placed on the side of the pole farthest from *e w*, the duration were *lengthened* through the effects of duration, instead of being shortened. Then there would arise on this account a difference of duration, which would lead to the determination of the sun's distance precisely as in the case before supposed. For in reality the result would be the determination of the apparent amount of the displacement *v v′* along the chord of transit, corresponding to the known displacement *w e* upon the earth; and the mere fact that both displacements are in an east-and-west direction does not render the observations less effective than those which in the former case gave the apparent displacement $v v'$ corresponding to the observers' displacement E E′, both displacements being on a north-and-south line.

In all ordinary cases, Halley's method depends partly on the distance of the observers measured in a north-and-south direction, and partly on the effect of rotation; and in the selection of stations both considerations have of course to be taken into account, the aim being to make the difference of duration as great, and therefore as exactly measurable, as possible. The considerations on which the selection of stations depends will be dealt with in a simple manner farther on.

We may conveniently call Halley's method the 'method of durations'—a name descriptive of the qualities of the method. But it certainly seems a mistake to limit the title 'Halley's method' to the case more particularly considered by him.[1] We may, therefore, use both names indifferently.

Halley's method requires the whole transit to be seen, or at least the beginning and end. Apart from other difficulties which this requirement introduces, the probability of favourable weather both at ingress or egress is manifestly less than the probability of

[1] An effort has of late been made to dismiss from use the title 'Halley's method,' which Sir J. Herschel and others had long used. I cannot see why Halley's name should thus be summarily dismissed from the position it has so long occupied.

favourable weather for a single observation only. It occurred to Delisle, when preparations were being made for the transit of 1761, that assuming Halley was right in supposing the moment of contact at ingress could be determined with great exactness, a single observation of the sort might be employed instead of two terminal observations.

It is clear that the observer who sees Venus traverse such a chord as $l' m'$ (fig. 13) will see the transit begin earlier than one who sees her traverse such a chord as $l m$, for l is a point more *advanced* than the point l'. Suppose now that each observer notes the exact moment of local time when the transit begins (internal contact), and that, knowing his exact longitude, each can change his local time into Greenwich time; then these two Greenwich epochs will differ by an interval corresponding to the amount by which l is in advance of l'. But this gives a geometrical relation whence the distance between the chords $l m$ and $l' m'$ can manifestly be determined, just as well as though the length of each chord were ascertained. Hence $v v'$ becomes known, and thus, as in the direct method, the sun's size and distance can be determined.

Similar remarks apply (*mutatis mutandis*) to the observation of egress. The method, whether applied at ingress or at egress, is called Delisle's method.[1]

The employment of photography to record the

[1] It is singular that Delisle's name, like Halley's, is not used in Sir G. Airy's programme for the transits of 1874 and 1882.

place of Venus on the sun's face at any particular instant need not detain us here, as it manifestly introduces no new astronomical relations.

And now let us consider how transits of Venus recur, in other words, how those opportunities are from time to time offered which admit of being utilised in the various ways above described.

Let us examine, first, how successive conjunctions of Venus are brought about.

Let the paths of Venus and the earth around the sun S (fig. 15) be represented by the circles V V′ and E E′; and let us suppose that Venus and the earth are,

Fig. 15.—Illustrating the conjunctions of the Earth and Venus.

in the first instance, in conjunction, as at V, E, so that S V E is a straight line. We need take no account, at this stage, of the slight eccentricity of the two orbits, and of the fact that they are not exactly in the same plane. Thus, we may be supposed to be looking directly down upon the moving planets, which, instead of travelling, as they actually do, with a slightly varying velocity, are supposed to travel with their mean or average motion.

Now, the simplest way of determining when and where the two planets will be again in conjunction is perhaps the following :—

Imagine that a straight pointer from the sun to Venus, extending to the earth's orbit, like the line S V E, is carried round S as a central pivot by the motion of the planet Venus. Then whenever this pointer comes up to the earth, the three bodies—sun, earth, and Venus—are in conjunction. Now, Venus travels with a mean motion of 96′ 7″·8 per day around the sun (completing a revolution in 224·701 days), while the earth travels with a mean motion of 59′ 8″·3 (completing a revolution in 365·257 days [1]); so that in each mean solar day Venus gains, on the average, 36′ 59″·5 upon the earth. This is the rate at which our imaginary pointer, starting from a position such as S V E, sweeps onwards from the advancing earth, so as to again reach the earth by overtaking it, just as the minute-hand of a clock, after being in conjunction with the hour-hand, passes on towards its next conjunction, with the *excess* of its motion over the hour-

[1] Sidereal revolution is here considered, not the tropical revolution which forms the year of seasons.

hand. We have only, then, to ask how long it will take the pointer, with its mean daily gain of 36′ 59″·5, to gain one complete circuit, to have the interval in time between successive conjunctions of the earth and Venus —in other words, there will be just as many days in this interval as the number of times that 36′ 59″·5 is contained in 360°, or, reducing both to seconds, as 2219·5 is contained in 1,296,000. The division is easily effected, and gives us 583·9 days.

Our Venus-carried pointer thus takes 583·9 days in overtaking the earth. This is more than a year by about 218·6 days, in which period, with her mean motion of 59′ 8″·3 per day, the earth travels round nearly 215½ degrees. Now, 216 degrees would be ⅗ths of a complete circuit. We see, then, that the next conjunction-line, S V′ E′, must be set almost exactly ⅗ths of the way round from S V E, or in the position $S V_1 E_1$; the next will have the position $S V_2 E_2$; the third will have the position $S V_3 E_3$; the fourth, the position $S V_4 E_4$; and the fifth will be close up to S V E, in the position $S V_5 E_5$, about 2½ degrees behind S V E.

Since the interval between each conjunction is about a year and three-fifths, the whole time occupied before the position $S V_5 E_5$ is reached by the conjunction-line will be five times 1⅗ years, or 8 years, less the short interval corresponding to the earth's motion over the arc $E_5 E$. We see, then, how it comes to pass that an interval of eight years brings round nearly the same circumstances as at the beginning of the interval, and why, therefore, when a transit has occurred, another may occur eight years later. A *second* interval of eight years, as we shall presently see, changes the conditions too largely (though they are still approximated to).

It may be mentioned in passing that since Venus gains one complete circuit on the earth between two successive conjunctions, and the earth goes nearly eight times round for the five conjunctions just considered, it follows that Venus goes nearly thirteen times round. In other words, thirteen revolutions of Venus are nearly equivalent to eight revolutions of the earth.

And now let us consider the effect of the inclination of the orbit of Venus to that of the earth, still, for the sake of simplicity, leaving out of account the slight eccentricity of the orbits.

If E E′, V V′ (fig. 16), represent the two orbits, and Æ be the place of the earth at the autumnal equinox, then the line E E′ represents the intersection of the two orbit-planes; and if, as before, we regard the plane of the paper as containing the orbit E E′, then the part V *v* V′ of the path of Venus is to be as regarded slightly above, the part V′ *v*′ V as slightly below, the plane of the paper. Accordingly, the end of the pointer which we have supposed Venus to carry round the sun, passes above the semicircle E *e* E′ and below the semicircle E′ *e*′ E. And supposing this pointer to be of the length S E, so that its end appreciably travels round E *e* E′ *e*′ (except for the displacement above and below the plane of this orbit), it is easy to calculate how much above or below the level E *e* E′ *e*′ the end of the pointer runs. When in the direction S E or S E′, of course the Venus-carried pointer has its extremity on the earth's path; when in direction S *v e* or S *v′ e′*,

Fig. 16.—Showing the parts (*p p′* and *q q′*) of the Earth's orbit where transits can occur.

at right angles to E E′, the end of the pointer is at its farthest from the plane E *e* E′ *e′*. The inclination of the orbit of Venus being about 3° 23½′, and the distance S *e* (the earth's distance from the sun) being about 91,430,000 miles, it is easily calculated that the extremity of the pointer passes above *e* and below *e′* at a distance of about 5,409,000 miles. At any other point, as P or P′, the end is above or below by an amount less than 5,409,000 miles in the same degree that P M or P′ M is less than *e* S or *e′* S (P M P′ being drawn square to E E′).

Now, it is clear that, for a transit to occur, a line from the sun's centre through Venus to the earth's orbit, at the time of a conjunction, must not pass more than a certain distance above or below the earth's orbit—that is, a conjunction must occur near the positions V E or V′ E′. And it is easy to determine roughly *how* near the earth must be to E or E′ at the time of conjunction, for a transit to occur. For let S V E, fig. 17, be our imaginary pointer at the time of

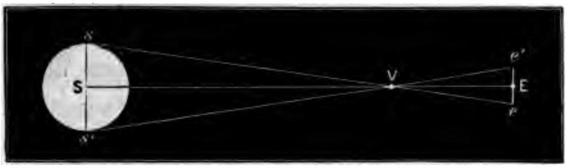


Fig. 17.—Illustrating the occurrence of transits.

a conjunction, and *s* V *e*, *s′* V′ *e′* lines touching the sun. Then it is manifest that if the earth be anywhere on the line *e′* E *e* at the time of conjunction, a line from the earth to Venus must meet the globe *s* S *s′*, or, in other words, there is a transit. But if the earth be above *e′* or below *e* at the moment of conjunction, there can be no transit. Now, S *s*, the sun's radius, is about 426,000 miles, and therefore E *e* and E′ *e′* are each less than 426,000 miles in the proportion in which V E is less than V S, or, roughly, as 277 to 723; so that E *e* and E *e′* are each equal to about 163,000 miles—a small distance compared with the actual range of the end of our Venus-carried pointer above and below the earth's orbit. And it is easily calculated[1] that the range on either side of E or E′ (fig. 16), within which a transit is possible, is represented by the arcs $p\,\text{E}\,p'$ and $q\,\text{E}\,q'$, each equal to about 3½ degrees.

Now, having found that the circuit of the earth's orbit has these two *transit-regions*, so to call them, it is not difficult to ascertain the general conditions under which the conjunction-line will fall from time to time upon one or other region.

Let it first be noted that the points E and E′ are at present those traversed by the earth on or about December 7 and June 6. The line E S E′ does not, however, bear a fixed position with respect to the point Æ, but the points E and E′ slowly shift forwards, that is, in the direction indicated by the arrow. The node of Venus's orbit shifts backwards with respect to the stellar sphere by about 20″·5 per annum; but as the point Æ shifts backwards annually by about 50″·1 (the precession of the equinoxes), it follows that the nodes V and V′, and therefore the points E and E′, advance with respect to Æ by about 29″·6 (the excess of 50″·1 over 20″·5) annually. Still, in dealing with the general question of the recurrence of transits, we must not regard the node of Venus as advancing by 29″·6 annually, but as receding by 20″·5; for in what

[1] We require to have

$$\frac{\text{E}p}{e\,\text{S}} = \frac{163{,}000}{5{,}409{,}000}$$

that is, the sine of the arc $\text{E}p = 163 \div 5409$. Whence $\text{E}p$ is an arc of about 1° 44′, and each of the arcs pp' and qq' about 3° 28′.

has hitherto been said about successive conjunctions of Venus and the earth, we have used the sidereal periods of both planets, and we cannot substitute the tropical year without making corresponding corrections.

We may regard the system of five conjunction-lines shown in fig. 15 as a spoked wheel, which slowly but continuously shifts backwards in such sort that any one spoke, S E, shifts to the position $S_5\,E_5$ in eight sidereal years *less* the time occupied by the earth in moving over E_5E, or about 2·449 days. This shift of position amounts to rather less than 2° 25′; but as the transit regions are themselves shifting backwards at the rate of 20″·5 annually, or about $2\frac{3}{4}'$ in eight years, we have the shift of the conjunction-lines, with reference to the transit regions, equal to about 2° 22′ in eight years.

Now let us suppose that the conjunction-line has at starting the position which it actually had on the occasion of the transit of the year 1631. Thus, let $p\,p'$ (fig. 18) represent what may be called the *December transit region*, and $q\,q'$ the *June transit region*, and let S V E, the first conjunction-line, fall so that E is the place of the earth on December 6.[1] The five next conjunction-lines have, as already shown, the positions $V_1\,E_1$, $V_2\,E_2$, $V_3\,E_3$, $V_4\,E_4$, and $V_5\,E_5$; and we see that E_5 being 2° 22′ from E, while $p\,p'$ is an arc of nearly $3\frac{1}{2}°$, E_5 falls within $p\,p'$, and there is again a transit, on or

[1] In the seventeenth century, but corresponding to her position on December 9 in the nineteenth century.

about December 4.[1] This corresponds to the transit of 1639. The next five conjunctions take place in due

Fig. 18.—Illustrating the regression of the conjunction-lines over a transit region ($p\,p'$).

order on the lines marked 6, 7, 8, 9, &c. We see, then, that there will be no December transits, that is, no conjunction within the arc $p\,p'$, until the gradual advance of the conjunction-line $E_2\,V_2$ has carried it by eight yearly steps to the transit region $p\,p'$. This manifestly requires as many eight-yearly intervals as $2^\circ\,22'$ is contained in the arc $E_1\,E_2$, or roughly the fifth part of the complete circuit; or, in other words, we must multiply $30\frac{3}{8}$ by 8 to obtain roughly the number

[1] In the seventeenth century, or about December 6 in the nineteenth.

of years. This gives 243 as the nearest whole number of years; and this, it will be noted, is the interval from the December transit of 1631 to the next December transit of 1874, or from the June transit of 1761 to the next June transit of 2004. But we see that while the conjunction-line $E_2 V_2$ is travelling by eight yearly steps to the transit region $p p'$, the conjunction-line $E_1 V_1$ will have travelled by similar steps to the position $E_4 V_4$, passing over the transit region $q q'$, and giving therefore two June transits in the middle of the period of 243 years.

And here, for the first time we have to note the effects of the slight eccentricity of the orbits of the earth and Venus. If the two paths were concentric circles their centre being the sun, the conjunction-lines would be distributed with perfect uniformity, so that the arcs $E E_2$, $E_2 E_4$, $E_4 E_1$, $E_1 E_3$, and $E_3 E_5$ would be exactly equal; but owing to the eccentricity of the orbits, and the consequent variation in the motions of both Venus and the earth, this uniformity does not hold. The five arcs just named, or others similarly formed from any other conjunction as a starting-point, are slightly different in length, being largest always when the earth's orbit approaches nearest to that of Venus, so that the angular motions of the two bodies around the sun differ least, and smallest where the orbits are farthest apart so that the angular motions of the two bodies differ most.[1]

[1] Anything like an exact discussion of the varying relative motions

At the present time, for instance, the conjunction lines have such positions as are indicated in fig. 19,

of Venus and the earth would be altogether out of place in a work of this nature. Let it suffice here to note the following values :—

	The Earth			Venus		
	°	′	″	°	′	″
Maximum daily motion . .	1	1	10	1	37	30
Mean " " . .	0	59	9	1	36	8
Minimum " " . .	0	57	11	1	34	52

Now, the perihelion of Venus is in longitude about 124¼°, the perihelion of the earth in longitude about 99½°, or nearly 25° behind. As the eccentricity of the earth's orbit is greatest, being nearly twice that of Venus's orbit (if measured in miles, still greater), we should not be far wrong in taking the earth's perihelion for the point of nearest approach to the orbit of Venus ; but inasmuch as opposite this point Venus is approaching perihelion, we somewhat diminish the longitude to obtain the actual point of nearest approach, which will be in about 70° of longitude, or at the place occupied by the earth on or about December 2. Here the daily motion of the earth is about 1° 0′ 56″, that of Venus about 1° 36′ 49″, the excess of the motion of Venus in longitude being therefore 35′ 44″. [When the earth is in longitude 90° her mean daily motion is about 1° 1′ 7·5″, that of Venus in the same longitude being about 1° 37′ 0·5″, an excess of 35′ 53″; so that the daily motions are not so nearly equal as in longitude 70°. In fact, it chances that the motions of Venus and the earth in conjunction are nearest to equality almost at the time corresponding to a December transit.] Now, at the opposite part of the two orbits, or in longitude about 250°, we have the earth's daily motion about 57° 29′, that of Venus 1° 35′ 19″, an excess of about 37′ 50″, or more by about 2′ 6″ than that in longitude 70°. It follows necessarily that successive conjunction-lines (after successive eight-yearly periods) fall nearer together in the June part of the orbits than in the December part. For the exact eight years which carry the earth from position E (fig. 15), to position E again soon after conjunction at E_5 with Venus at V_5, correspond to thirteen complete revolutions of Venus plus 0·955 days, wherever E may be. Now let v be the place reached by Venus when the earth is at E, then V v is the space traversed by Venus in 0·955 days. But V v also measures the gain of Venus on the earth, while the earth has been passing from E_5 to E. Now, in longitude 70°, Venus, being nearer her perihelion, moves faster than in longitude 250° ; hence on this account the arc V v will be greater for a December conjunction than for a June one. Since, then, the gain V v of Venus is greater at a where the eccentricities of the two orbits are properly shown, and the conjunction-lines are placed in longitude

December conjunction than at a June one, while yet it accrues at a less rate as we have seen above, it follows that it requires a longer time to accrue: in other words, the arc corresponding to $E_5 E$ requires a longer time, and if the earth moved uniformly would be a longer arc at a December conjunction than at a June one. But the earth is moving faster in December than in June; *à fortiori* therefore the arc corresponding to $E E_5$ will be greater for a December than for a June conjunction. Thus is explained the greater distance between the transit lines of a December pair than between the corresponding lines of a June pair. See Plate I. To further illustrate this, and also to make this reasoning more directly applicable to the subject matter of this chapter, I will now proceed to calculate the actual displacement of the conjunction-line in eight years, for the two transit regions respectively.

Suppose a conjunction to occur on or about $\left\{\begin{matrix}\text{December 9}\\ \text{June 6}\end{matrix}\right\}$. Then in eight sidereal years from this conjunction the earth has gone eight times round, while Venus has gone round thirteen times *plus* her motion in 0·955 *d.* This motion takes place at the daily rate of $\left\{\begin{matrix}1^\circ\ 36'\ 45''\\ 1^\circ\ 35'\ 13''\end{matrix}\right\}$ and therefore places Venus in advance of the earth by $\left\{\begin{matrix}5544''\\ 5456''\end{matrix}\right\}$; and the daily gain of Venus, or $\left\{\begin{matrix}2147''\\ 2269''\end{matrix}\right\}$ is contained $\left\{\begin{matrix}2{\cdot}582\\ 2{\cdot}405\end{matrix}\right\}$ times in $\left\{\begin{matrix}5544''\\ 5456''\end{matrix}\right\}$. Therefore conjunction must have occurred $\left\{\begin{matrix}2{\cdot}582\ d\\ 2{\cdot}405\ d\end{matrix}\right\}$ earlier, or since the earth's daily motion is $\left\{\begin{matrix}3650''\\ 3444''\end{matrix}\right\}$ conjunction must have occurred $\left\{\begin{matrix}2^\circ\ 37'\ 26''\\ 2^\circ\ 18'\ \ 1''\end{matrix}\right\}$ in longitude behind the conjunction-line of the earlier transit. Diminishing each arc by $2\frac{3}{4}'$ for the change of the nodal line in eight years, we obtain a motion (with respect to the node) of about $\left\{\begin{matrix}2^\circ\ 35'\\ 2^\circ\ 15'\end{matrix}\right\}$, near enough for our present purpose.

In the above, no account is taken of perturbations of the motions of Venus and the earth by the other planets.

It will be convenient to add here a more exact calculation of the transit arcs $p\,p'$ and $q\,q'$, fig. 16. We may follow the same plan as at page 107.

3°, 77°, 155°, 225°, and 292°, which correspond, nearly enough for our purpose, to the inferior conjunctions of

We have,—the distance of Venus from sun at $\begin{Bmatrix}\text{ascending}\\ \text{descending}\end{Bmatrix}$ node is $\begin{Bmatrix}65,865,000\\ 66,394,000\end{Bmatrix}$ miles (where the earth's mean distance is taken as 91,430,000 miles), and the earth's distance in the same longitudes respectively is $\begin{Bmatrix}90,036,000\\ 92,817,000\end{Bmatrix}$ miles; so that the distance of the earth from Venus at conjunction is respectively $\begin{Bmatrix}24,171,000\\ 26,423,000\end{Bmatrix}$ miles; and diminishing the sun's radius (which here for greater exactitude we take at 426,450, its true value if sun's mean distance be 91,430,000 miles) in the ratio $\begin{Bmatrix}24,171,000 : 65,865,000\\ 26,423,000 : 66,394,000\end{Bmatrix}$, we obtain for the distance corresponding to E e and E e' fig. 17 the value $\begin{Bmatrix}156,500\\ 169,720\end{Bmatrix}$ miles; and it thence follows that

$$\begin{aligned} p\,\text{E} = \text{E}\,p' &= 156,500 \text{ cosec } (3^\circ\ 23\tfrac{1}{2}') = 2,645,300 \text{ miles} \\ q\,\text{E}' = \text{E}'\,q' &= 169,720 \text{ cosec } (3^\circ\ 23\tfrac{1}{2}') = 2,868,700 \text{ miles} \end{aligned}$$

while the arc-measure of p E, or $\frac{p\,\text{E}}{\text{E S}}$, is equal to 1° 41′, so that $p\,p'$ is an arc of 3° 22′;

and the arc-measure of q E′, or $\frac{q\,\text{E}'}{\text{E}'\,\text{S}}$, is equal to 1° 46′, so that $q\,q'$ is an arc of 3° 32′.

These values are for the centres of Venus and the earth. It would be easy, but is scarcely worth while, to calculate them for exterior or interior contact, and for the whole earth,—that is, to determine the arc $p\,p'$ or $q\,q'$ for the extreme cases where if any part of Venus be seen on the sun's disc from any part of the earth, a transit shall be considered to have taken place, or where no transit shall be considered to have taken place unless the whole of Venus be seen within the sun's disc even from the station which throws her farthest from the sun's centre at the moment of nearest approach. Into such niceties, however, we need not here enter, as they are merely questions of curiosity, and neither present any difficulty nor involve any important principle.

It will be seen that since at two successive conjunctions near December 7, the conjunction-lines are separated by 2° 37′ 26″ (or about 2° 35′ measuring from the node), while the transit arc is about 3° 22′ in range, whereas at two successive conjunctions near June 5, the conjunction-lines are separated by only 2° 18′ 1″ (or about 2° 15′ measuring

Venus on the dates, September 26, 1871, December 9, 1874, February 24, 1870, May 5, 1873, and July 14, 1876. Now it is clear that the conjunction-line V_5 E_5 is

Fig. 19.—Showing the actual position of conjunction-lines of the earth and Venus.

farther from the nodal line V′ E′ than is the conjunction-line V_3 E_3. In fact the longitude of V′, the node, is about $255\frac{1}{3}°$; and V_3 is only $30\frac{1}{2}°$ or so from the node, while V_5 is about $36\frac{1}{2}°$ from the node. Hence the conjunction-line V_5 E_5 will take longer, in marching up by eight yearly steps to V′, than the half of the period of 243 years, which is the time in which it comes up to the

from the node), and the transit arc has a range of 3° 32′, there is a much greater chance of a pair of transits when the conjunction-line is sweeping over the June transit-region, than when it is sweeping over the December transit-region.

intervals during which each passage will bring but a single transit. The series 8, 105½, 8, 121½, 8, &c. will then be modified into the series 113½, 129½, 113½, &c. But various other modifications occur in the course of long periods of time. Thus the triplet of intervals 105½, 8, 121½, in the complete series may be changed either into the pair 113½, 121½, or into the pair 105½, 129½; while the triplet 121½, 8, 105½, may be changed either into the pair 129½, 105½, or into 121½, 113½, according to circumstances.

So much for the order in which transits recur, either at the ascending node in December or at the descending node in June. Let us now consider how stations are selected for applying the various methods which are available.

Let S, fig. 20, represent the sun, and V Venus, the arrows showing the direction in which Venus

Fig. 20.—Illustrating Venus's shadow-cone.

and the earth are travelling around S. Let S V p represent the Venus-carried pointer of which we have already made frequent use, its extremity p being in the figure rather above the earth's orbit, and travelling onwards with the excess of Venus's motion, so as to overtake the earth. Now let a cone, having the centre

PLATE X.

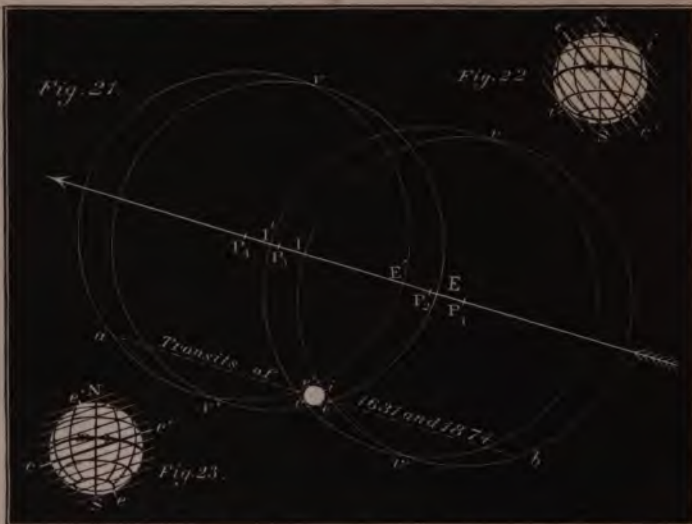


Fig. 22, Ingress 1631 and 1874. Fig. 23, Egress 1631 and 1874.

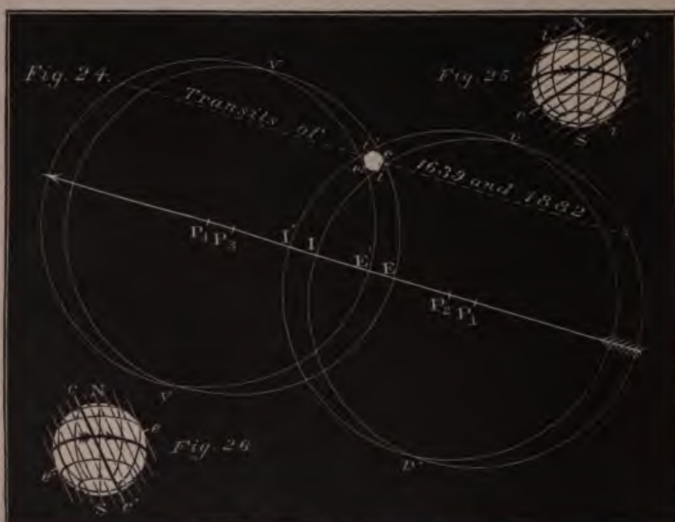


Fig. 25, Ingress 1639 and 1882. Fig. 26, Egress 1639 and 1882.

ILLUSTRATING PASSAGE OF VENUS'S SHADOW-CONE OVER EARTH IN 1631, 1639, 1874, AND 1882.

PLATE X.

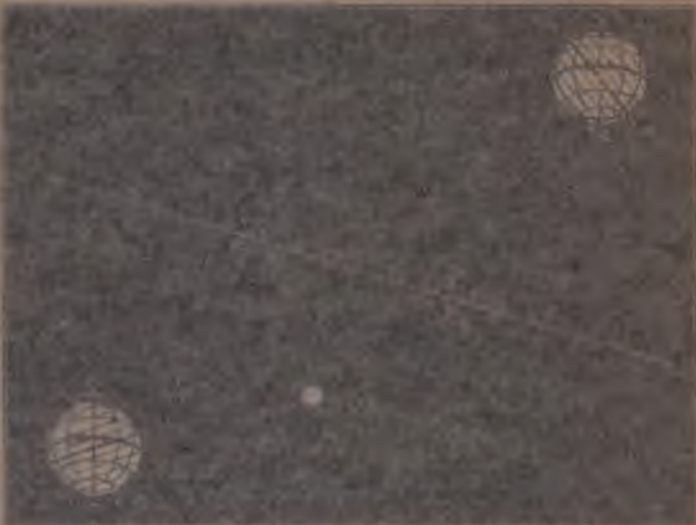


Fig. 23, Ingress 1631 and 1874. Fig. 24, Egress 1631 and 1874.

Fig. 25, Ingress 1639 and 1882. Fig. 26, Egress 1639 and 1882.

ILLUSTRATING PASSAGE OF VENUS'S SHADOW-CONE OVER EARTH IN 1631, 1639, 1874, AND 1882.

of Venus, V, for its vertex, and S V for its axis, be supposed to envelope the sun after the manner shown by the section *s* V *s′* in the figure, and let the prolongation of this cone beyond V, be *v* V *v′*, *v v′* being a circular section through *p*. Then we may regard this circular section (which corresponds to *e e′* in fig. 17) as travelling onwards like a gigantic wheel more than 300,000 miles in diameter, to overtake E; and if V is near enough to a node, then will this great circle pass athwart E in such sort that E will traverse a chord of the circle *v v′*. Let us try to picture such a passage. Suppose V to be near an ascending node so that the circle *v v′* as it overtakes E has a slight upward motion: also if we are looking from S towards E (and *v v′* were a real circular outline) we should see *v v′* moving from right to left to overtake E. It will be convenient to regard E as at rest so that we consider only the *excess* of the motion of *v v′* over that of the advancing earth.

In fig. 21, Plate X., *v* I *v′* represent the circle *v v′* of fig. 20 on an enlarged scale at the moment when the earth *e e′* is first touched at the point *i*. At this moment an observer at *i* will of course see the centre of Venus just crossing the edge of the sun. (This is manifest from fig. 20, where we see that a line drawn to V from any point, as *i*, fig. 21, on the surface of the cone *v* V *v′* will touch the globe *s s′*.) To an observer at *i* then, but to no one else on the earth *e e′*, transit will have begun (reference being always made to the centre of Venus).

and the motion of the centre of *v v′* along P_1 P_4 also unchanged, we manifestly modify the nature of the passage of the edge of *v v′* over the disc *e e′*.

The section *v v′* passing on, arrives at length at the position V *e* V′ touching the disc of the earth at *e*. At this moment the centre of Venus is seen, by the observer at *e*, on the edge of the sun; in other words, egress (of the centre of Venus) is taking place, and *e* is the station where egress is first seen. The section *v v′* passes on until it has the position V *e′* V′, when it is about to leave the earth finally, its last contact with the earth being at *e′*—where egress takes place latest. In the interval the edge of V V′ has been passing over the disc *e e′*, moving nearly parallel to itself: we have then *e* the *pole of accelerated egress* and *e′* the *pole of retarded egress.* As in the case of ingress, *e* is not exactly opposite to *e′* even on the *circle e e′*, while rotation has affected the *globe e e′*, so that *e* is still farther from being opposite to *e′* on the earth.

We may, however, in this as in the former case, regard (for a first approximation) *e* and *e′* as points on opposite extremities of a diameter of the earth, taking the moment intermediate between earliest and latest egress. With this assumption, the passage of the edge of the circle V V′ across the earth's face is illustrated by fig. 25 (Plate X.), which represents the disc *e e′* of fig. 21 on an enlarged scale, the edge of the circular shadow being shown in ten successive stages of its supposed uniform retreat. The earth is shown in the proper position for a December transit.

It need hardly be said that the face of the earth turned sunwards when the section *v v′* has advanced to the position V V′ is greatly changed from the face which had been turned sunwards when ingress was in progress. But the time of egress is easily calculable, like that of ingress, from the known motions of Venus and the earth; the face of the earth turned sunwards is also known; and all the circumstances of the passage of the edge of V V′ over the earth's face at egress are easily determined. In fact, all that was said respecting ingress is true, *mutatis mutandis*, in the case of egress.

The conditions represented in fig. 21 are actually those of the transit of 1874. The shadow cone of Venus passes slantingly upwards with reference to the earth, and the centre of the circular section *v v′* passes north of the earth. The earth passes, therefore, through the shadow section as along the dotted line, in the manner shown farther on in fig. 35. But the conditions of the transit of 1631 so nearly resembled those of the coming transit that fig. 21 conveniently illustrates both transits.

In order to more thoroughly master the above reasoning, the reader would do well to run over it again, using figs. 24, 25, and 26, instead of figs. 21, 22, and 23 respectively. Fig. 24, with its companion projections, illustrates the transit of 1882 (and approximately also the transit of 1639). See also fig. 35.

Then the reader can apply the explanation given above, with very slight changes, to the case of June transits, illustrated by fig. 27. Figs. 28 and 29 are the companion June projections of the earth for the earlier of a pair of June transits (as the transits of 1761 and 2004), while figs. 30 and 31 are the com-

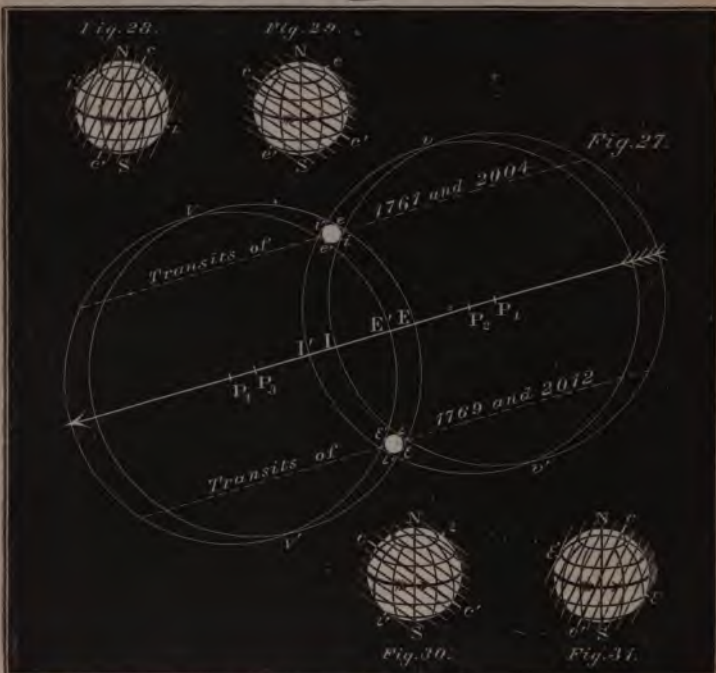


Fig. 27.—Illustrating the passage of Venus's shadow-cone over the earth during the transits of 1761, 1769, 2004, and 2012.

Fig. 28.—Ingress, 1761 and 2004. Fig. 29.—Egress, 1761 and 2004.

Fig. 30.—Ingress, 1769 and 2012. Fig. 31.—Egress, 1769 and 2012.

panion projections for the later of a pair of June transits (as the transits of 1769 and 2012). Moreover it chances, so nearly similar in position are the northern and southern transit chords for the four transits just

PLATE XI.

TRANSITS OF 1874 AND 1882.

ILLUSTRATING INTERNAL CONTACTS AND MID-TRANSIT, AND SHOWING RELATIVE DIMENSIONS OF THE DISCS OF VENUS AND THE SUN.

[illegible] time and the space [illegible] the [illegible] sufficient approximation for illustrative purposes [illegible] as in [illegible] Delisle's [illegible] any given [illegible]. Take [illegible] the [illegible] the Sun [illegible] at the beginning and end of the [illegible] precisely the same thing, the [illegible] of V[illegible] as seen from the earth [illegible] position [illegible] Plate X [illegible] of the [illegible] principle [illegible] corresponding [illegible] Having [illegible] end of [illegible] and [illegible] Plate XI [illegible] we [illegible] Plate XII [illegible] the end of the [illegible] XII [illegible] Plates [illegible] and XIII [illegible] earth [illegible] in Plate XI [illegible] the beginning [illegible] illustrate [illegible] present [illegible] ription.

Then the [illegible] of the points [illegible] and [illegible] fig. 21 [illegible] from the [illegible] fig. [illegible] and [illegible] in fig. [illegible] Plate [illegible]

[illegible]

PLATE XI.

TRANSITS OF 1874 AND 1882.

Illustrating internal contacts and mid-transit, and showing relative dimensions of the discs of Venus and the Sun.

named, that one and the same figure illustrates all four with sufficient approximation for illustrative purposes.

Now it is easy to see how Delislean stations are to be selected in any given case. Take the transit of 1874. We find first the hours at which the circle *v v′* (fig. 21) crosses the centre of the disc of the earth *i e i′ e′* at the beginning and end of the transit, or, which is precisely the same thing, the moment when the centre of Venus, as seen from the earth's centre, reaches the positions *b* and *b′*, Plate XI. This is in point of fact the 'calculation of the transit,' and depends on principles corresponding to those involved in the calculation of an eclipse. Having these two epochs of the beginning and end of transit, and also the positions of the transit chord *b b′* Plate XI., and *a b* of fig. 21 Plate X., we make a sun-view of the earth at the beginning of the transit as Plate XII., and another of the earth at the end of the transit as Plate XIII. (Plates XII. and XIII. really represent the aspect of the earth for the times of internal contact—illustrated in Plate XI.—at the beginning and end of transit, but they sufficiently illustrate the present description).

Then the positions of the points *i* and *i′* fig. 21, Plate X. are known at once, from the geometrical relations pictured in fig. 21,[1] and thus we have the poles of accelerated and retarded ingress placed as *i* and *i′* in fig. 22, Plate X., or as A and B in Plate

[1] Of course the vertical line N S in figs. 21, 24, and 27, represents north and south line of the earth's disc *i e i′ e′*.

XII., these positions being the same, it will be observed, as the positions of *i* and *i′* on the small disc *i e i′ e′* of fig. 21. The observers of the accelerated ingress must be near *i* on the illuminated hemisphere *i i′*, fig. 22, while the observers of retarded ingress must be near *i′*. The amount of acceleration or retardation will depend on the distance from *c c′*, a station at any point in any one of the parallels in fig. 22 having equal acceleration or retardation (according as the parallel is nearer *i* or *i′*). The parallel lines of the figure are of course circles on the globe of the earth; and *i* and *i′* are the poles of these circles.[1]

Again, the positions of the points *e* and *e′*, fig. 21, Plate X., are known; and thus we have the poles of accelerated and retarded egress placed as *e* and *e′* in fig. 23, Plate X., or as C and D in Plate XIII., these positions being the same as those of *e* and *e′* in the small disc *i e i′ e′* of fig. 21. The observers of the accelerated egress must be placed near *e* on the illuminated hemisphere *e e′* fig. 23, while the observers of retarded egress must be placed near *e′*. The amount of acceleration or retardation will depend on the distance from *c c′*, a station at any point on any one of the parallels of fig. 23 having equal acceleration or retardation (according as the parallel is nearer *e* or *e′*).

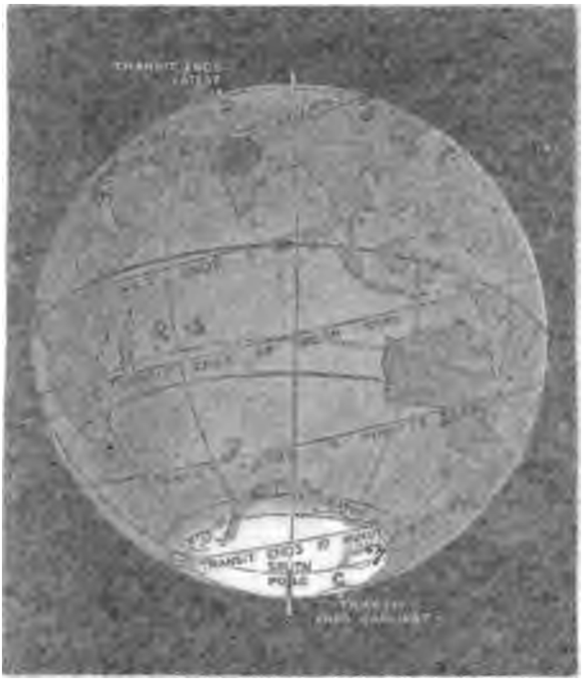
[1] The acceleration or retardation at a station for observing ingress manifestly varies as the distance of the station from the plane of the great circle having *i* and *i′* as poles; and similarly for retarded ingress, and for accelerated and retarded egress. Or in other words, the acceleration or retardation varies as the cosine of the arc-distances from *i* or *i′*, *e* or *e′*.

PLATE XII.

SUN-VIEW OF THE EARTH AT THE BEGINNING OF THE TRANSIT OF 1874.

1. Station at Hawaii.
2. „ „ Kerguelen Island.
3. „ „ Rodriguez.
4. „ „ New Zealand.
6. „ „ Nertschinsk.
7. „ (proposed only) at Possession Island.
8. „ „ Mauritius.
9. „ in North China.

PLATE XIII.



VIEW OF THE EARTH AT THE END OF THE TRANSIT OF 1874.

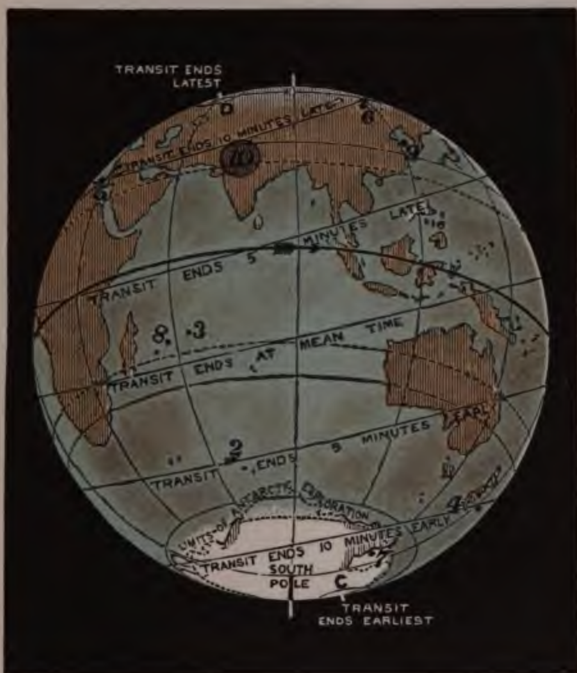
2. Station at Kerguelen Land.
3. „ „ Rodriguez.
4. „ in New Zealand.
5. „ at Alexandria.
6. „ „ Nertschinsk.
7. „ (proposed only) at Possession Island.
8. „ „ Mauritius.
9. „ in North China.
10. The North Indian Region (now occupied).

PLATE XII.

SUN-VIEW OF THE EARTH AT THE BEGINNING OF THE TRANSIT OF 1874.

1. Station at Hawaii.
2. ,, ,, Kerguelen Island.
3. ,, ,, Rodriguez.
4. ,, ,, New Zealand.
6. ,, ,, Nertschinsk.
7. ,, (proposed only) at Possession Island.
8. ,, ,, Mauritius.
9. ,, In North China.

PLATE XIII.



SUN-VIEW OF THE EARTH AT THE END OF THE TRANSIT OF 1874.

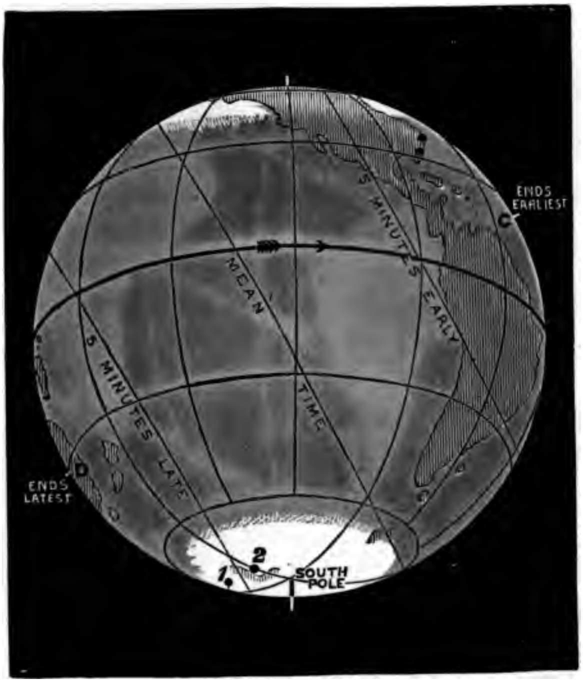
2. Station at Kerguelen Land.
3. ,, ,, Rodriguez.
4. ,, in New Zealand.
5. ,, at Alexandria.
6. ,, ,, Nertschinsk.
7. ,, (proposed only) at Possession Island.
8. ,, ,, Mauritius.
9. ,, in North China.
10. The North Indian Region (now occupied).

PLATE XIV.

SUN-VIEW OF THE EARTH AT THE BEGINNING OF THE TRANSIT OF 1882.

1. Sir G. Airy's proposed station at Repulse Bay.
2. " " on Possession Island.

PLATE XV.



Sun-view of the Earth at the End of the Transit of 1882.

1. Sir G. Airy's proposed station at Repulse Bay.
2. „ „ on Possession Island.

These parallel lines of the figure are circles on the globe of the earth, and *e* and *e'* are the poles of these circles.

Similar remarks apply to the case of the transit of 1882, illustrated by figs. 24, 25, and 26, Plate X., and by Plates XIV. and XV. The reader is recommended to go over the last three paragraphs afresh, using these last-named figures and plates.

It is easily seen that Delisle's method is applicable in every possible case. The poles of accelerated and retarded ingress and egress lie always (as *i*, *i'*, *e*, and *e'*) at the edge of the illuminated hemisphere, and stations can always be found near to these points, and in sunlight. Nor do the conditions of success depend at all upon the position of the chord of transit. We see indeed that in the case of a short chord as in fig. 21, the difference of time between ingress at *i* and *i'*, or egress at *e* and *e'*, is greater than in the case of a longer chord as in fig. 24; for $P_1 P_2$ and $P_3 P_4$ are greater in fig. 21 than in fig. 24. But it is easily seen that this advantage in the case of the shorter chord is counterbalanced by the slowness with which Venus crosses the sun's edge.[1] Of course the more slowly Vénus seems to cross the sun's edge the more difficult it is to determine the true moment of contact whether at ingress or egress, complicated as contact is by the phenomena of black-drop formation.

[1] Plate XI. shows us that from *b* to the centre of Venus's disc at *i* is greater than from *s* to the centre of her disc at *i'*. In fact this slowness of crossing corresponds *exactly* to the lengthening of the distances $P_1 P_2$ and $P_3 P_4$.

Delisle's method is then always applicable, and always under similar conditions. It is otherwise with Halley's method. Let us briefly consider this method in the approximate manner already used for Delisle's method.

To apply Halley's method both the beginning and end of transit must be seen. Now *i i'*, fig. 22, &c., enlarged into a sun-view of the earth as in Plate XII. (for the case of the transit of 1874), shows the face of the earth in sunlight when transit begins, while *e e'*, fig. 23, similarly enlarged, shows the face of the earth in sunlight when transit ends. We must select stations common to both these projections or sun-views of the earth in the case of any transit we are dealing with. For example, in the case of the transit of 1874, we see that such a station as 1, near A in Plate XII. (one of the Sandwich Islands), though excellent for observing the beginning of the transit by Delisle's method, cannot be employed for Halley's, because this station has already passed to the dark side (in other words, the sun has set there) before the end of the transit when the face of the earth pictured in Plate XIII. is turned sunwards. Again, the station marked 5 in Plate XIII. (Alexandria) is an excellent station for seeing the end of the transit by Delisle's method, but it cannot be employed for Halley's, because it is on the dark side of the earth (in other words, the sun has not risen), at the beginning of the transit, when the face of the earth pictured in Plate XII. is turned sunwards. But at any station in Australia, for example, the whole transit can be seen, and therefore Halley's method could be employed there so far as visibility of transit is concerned. It remains, however, to select among stations where both the beginning and end can be seen, those particular stations where the transit is considerably lengthened and shortened in duration compared with the mean transit (transit of Venus's centre supposed to be seen from the earth's centre). In the case of Delisle's method, whether applied to ingress or to egress, we had two poles (i and i' for ingress, e and e' for egress), and could estimate the value of any station at once by referring its position to these poles. We have now to inquire whether there are any corresponding Halleyan poles—a pole of lengthened duration, and another pole of shortened duration. I believe Encke was the first to point out that there are such poles and to give an analytical proof of the fact; but the following simple geometrical demonstration is, so far as I know, original.[1]

The parallel lines across the disc of the earth in fig. 22, represent circles on the earth having i, i' as

[1] Prof. Adams mentioned the fact that there are such poles, and indicated their position, at a meeting of the Astronomical Society at which I was present—March 1873, if I remember rightly. I submitted to him a day or two after the demonstration given in the text, and in his reply he remarked that the demonstration was the geometrical equivalent of the reasoning by which he had been led to recognise the existence and position of the Halleyan poles, as well as the law according to which the lengthening or shortening of the duration varies with distance from the poles. Subsequently I learned from M. Dubois' work, already often referred to here, that Encke had anticipated Adams in recognising these relations.

poles, and corresponding to times of ingress successively later and later, by equal intervals, as the parallel is farther and farther from *i*. Now if we suppose the globe of the earth rotated about an axis *i i'*, these parallel circles will still appear as parallel and equidistant straight lines. In fact, so long as the points *i* and *i'* are on the edge of the visible disc the parallel circles will appear as equidistant parallel lines. Similar remarks apply to the parallels in fig. 23; so long as *e* and *e'* are on the edge of the visible disc these parallel circles will appear as parallel and equidistant lines. Let us suppose, then, that the globe of the earth is so placed with respect to the observer that all four points *i*, *i'*, *e*, and *e'* are on the circumference of the visible disc. This is clearly possible, for *i i'* are extremities of one diameter, *e e'* those of another diameter of the sphere, and any two diameters must lie in one plane, which plane intersects the sphere in a great circle; so that we have only to place the sphere so that this great circle shall form its apparent outline, to have *i*, *i'*, *e*, and *e'* (the extremities of two diameters of this great circle) on the outline of the visible disc.

Now let fig. 32 represent on an enlarged scale the face of the globe thus brought into view, I_a and I_r corresponding to the points *i* and *i'*, while E_a and E_r correspond to the points *e* and *e'*. Also the number of parallels has been doubled to make the illustration more complete, the maximum acceleration and retardation at ingress and egress being divided into ten equal parts corresponding to the ten equal spaces on either side of the lines *e e′* and *c c′*. For convenience of explanation this maximum is regarded as 10 seconds, which is a little short of its value in the transit of 1874 and a little in excess of its value in the transit of 1882. Now consider any point *a* where a parallel of one system crosses a parallel of the other system. Since *a* lies on the fourth parallel from *c c′* towards I_a, the

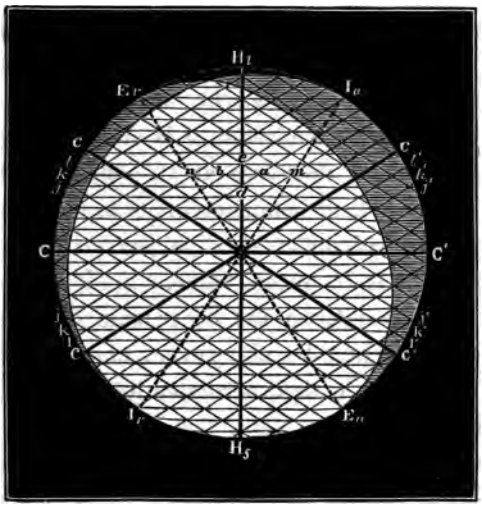


Fig. 32.—Illustrating the position of the Halleyan poles between the Delislean poles.

ingress is accelerated by 4 seconds, and since *a* lies on the third parallel from *c c′* towards E_r, the egress is retarded by 3 seconds. On the whole, therefore, the duration of the transit at *a* is 7 seconds greater than the mean. At the point *b* where the next parallel *from* I_a and the next *towards* E_r intersect, ingress is accelerated 3 seconds, and egress is retarded 4 seconds; so that the duration here, as at *a*, is 7 seconds greater than the mean. At the point *m* the ingress is accelerated 5 seconds, and egress retarded 2 seconds; so that at *m* also duration is lengthened 7 seconds. The same is the case at *n*. These points *m*, *a*, *b*, and *n*, lie manifestly on a straight line which produced either way to *k* and *k′* passes through other points of intersection of our two systems of parallels, at which points, by the method of reasoning already applied to *a*, *b*, *m*, and *n*, the duration is lengthened by the same number of seconds. At *e*, the lengthening is similarly shown to be 8 seconds, and the same at all points on the line *l e l′*. At *d*, the lengthening amounts to 6 seconds, and the same at all points on the line *j e j′*. Along C O C′ the duration has its mean value. And at any point on j j′, k k′, or l l′, on the other side of C O C, the transit is shortened by six, seven, or eight seconds, respectively. So with other cases for all points over the disc C O C′.

Now very little knowledge of geometry is required to show that these lines *j j′*, *k k′*, *l l′*, j j′, k k′, and l l′, and the other lines of fig. 32 similarly obtained, form a series of parallel equidistant lines; these lines being parallel to C O C′, the bisector of the angles E_r O I_r and I_a O E_a. These parallel lines are parallel circles on the sphere, having for poles the two points H_1 and H_2 the middle points of the arcs I E_r and I_r E_a. The farther one of the dotted parallels is from C O C′ towards H_1 the greater is the lengthening of the transit, and the farther such a parallel is from C O C towards H_s the greater is the shortening of the transit. The absolute maximum duration is at H_l, and the absolute minimum is at H_s. These points, then, are the Halleyan poles, and any one of the parallel circles having these points as poles indicates the position of stations at which the lengthening or shortening is in proportion to the distance of the plane of the circle from the plane of the great circle C O C′, towards H_l or H_s respectively.

But while the Halleyan poles and circles correspond thus geometrically with the Delislean poles and circles, there is one important difference. A Delislean pole for any phase is a point where the sun can actually be seen at that phase. But this is not necessarily the case with a Halleyan pole. The northern Halleyan pole for example, in the transit of 1874, is, by what has just been shown, the point midway between the A of Plate XII. and the D of Plate XIII. D being still on the darkened side of the earth at the time pictured in Plate XII., and A having passed to the darkened side at the time pictured in Plate XIII., it is manifest that the middle point of an arc from D to A must also lie on the darkened side at both these epochs; and therefore the northern Halleyan pole, though geometrically the point where transit lasts longest, is in reality a point where neither the beginning nor the end of transit can be seen. On the other hand, the southern Halleyan pole in 1874, is in sunlight throughout the whole transit, as we see by noting that the points B and C of Plates XII. and XIII. are themselves in sunlight throughout the transit, and that therefore the point midway between them must be so. The relations here described are those illustrated in fig. 32, where the dark lune I_a *c* I_r represents a part of the earth where the beginning of the transit is not seen, the dark lune E_r *c* E_a representing a part where the end is not seen, and H_l lying on a part where these lunes overlap, on which therefore neither the beginning nor end of the transit can be seen.

The reader will find no difficulty in making a corresponding construction to illustrate the transit of 1882. In fact, so far as the parallels are concerned, fig. 32 will represent the case of the transit of 1882 very nearly, for we see from Plates VI. and VII. that the distance between the two northern Delislean poles, and therefore between the two southern, is nearly the same in both transits—in other words, the arcs corresponding to I_a E_r, E_a I_r in fig. 32 are nearly right for the transit of 1882. But the darkened lunes must have the position they assume when fig. 32 is inverted; for we see from Plates XIV. and XV. that while the northern Halleyan pole is in sunlight in 1882, the southern is on the darkened hemisphere.

But besides that one Halleyan pole or the other is so placed that no part of the transit can be seen from it, the circumstances of different transits vary as respects the advantages offered by Halley's method.

For example, take H_s the accessible Halleyan pole in such a transit as that of 1874. We see that at this point the ingress is retarded and egress accelerated by the maximum acceleration or retardation, less only about 1⅗ tenths, so that the shortening is less than the sum of the maximum acceleration and retardation by only 3⅕ tenths of either. But if E_a and I_r were farther apart the shortening of the transit would not be so great. This is seen from fig. 33, which illustrates the

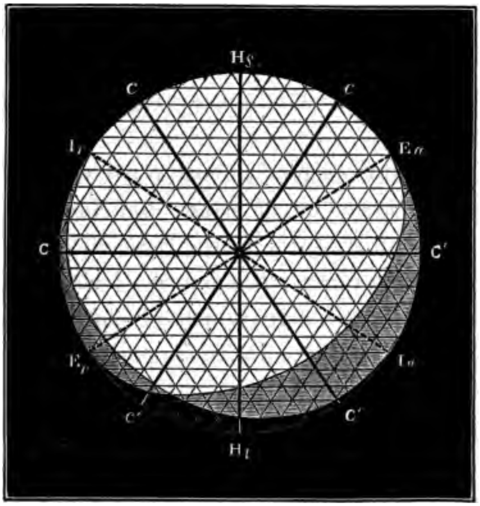


Fig. 33.—Illustrating a case unfavourable for Halley's method.

conditions of the transits of 1761 and 2004. Here E_a and I_r are farther apart than I_a and E_r in fig. 32; the southern Halleyan pole H_l is in this case the inaccessible one. We note first, that owing to the greater distance between I_a and E_r the point of nearest approach to the pole H_l is still a long way from that pole. Moreover, we see that at H_s in fig. 33, the retardation of ingress and the acceleration of egress are less than the maximum by 4½ tenths, so that the total shortening is less than the sum of the maximum acceleration and retardation by fully 9 tenths of either. If the interval in time corresponding to the space between successive parallels were 1 second, as we before for convenience assumed it, then in the case illustrated by fig. 32, the total shortening at the sunlit Halleyan pole H_s would amount to nearly 17 seconds, whereas in the case illustrated by fig. 33 the shortening at the sunlit Halleyan pole H_s amounts to little more than 11 seconds.

Thus, apart from geographical considerations, which may in some cases be of paramount importance, the applicability of Halley's method depends principally on the arc-distance between the two Delislean poles in the northern hemisphere, which of course is equal to the distance between the Delislean poles in the southern hemisphere.

This seen, it is easy to perceive that the first transit of a pair separated by eight years will be less suitable than the second.

First take a pair of December transits like those of 1874 and 1882—transits when Venus is at her ascending node. In this case the first transit always carries Venus north of the sun's centre, as along *b b′* in Plate XI., while the second carries her south of the sun's centre as along *s s′*; for this being her ascending node, and the second transit finding her, as already explained, less advanced in her orbit, she is beyond her ascending node at the first transit and behind that point at the second transit—that is, in north latitude in the former case, and in south latitude in the latter. Accordingly, the centre of Venus's shadow-cone passes north of the earth as in fig. 21 in the case of the earlier transit of a pair at the ascending node, and south of the earth as in fig. 24 in the case of the later transit of such a pair. Thus the first contact is in the north-eastern quadrant as at *i* in fig. 22, and the last in the north-western as at *e′* fig. 23; and the point *i* on the earth having been carried round by the earth's rotation to the darkened side and (remembering the position of the earth's axis) on a course giving it a greater distance from *e* than it would have if the rotation were round an axis N S, we have the two Delislean poles farther apart than they would be but for the inclination of the earth's axis. Or we might have deduced the same result by considering the two poles *i′*, fig. 22, and *e*, fig. 23; for we see that the motion of *i′* along its upward-bowed latitude-parallel is such as to give it a greater distance from *e* than it would have if the axis of rotation were N S. But in the case of the transit of 1882, we see that *i′*, fig. 25, the place of second contact, is brought by rotation *nearer* to *e*, the place of third contact,[1] than it would be if the rotation were around an axis N S; or we may infer the like by considering the relative motion of the northern Delislean poles *i* and *e′*.

[1] The reader should note that the effects here considered depend entirely on the tilt of the earth's axis.

The position of the axis of rotation is then unfavourable to the earlier transit of a pair occurring in December.

It will be easy for the student to apply similar reasoning to the case of transits occurring in June, as illustrated by figs. 27, 28, 29, 30, and 31; and it will be found in these cases also the rotation brings the Delislean poles (the northern pair or the southern pair) closer together, *cæteris paribus*, in the case of the later transit of a pair than in the case of an earlier transit.[1]

But another circumstance clearly affects the distance of the two northern, as of the two southern, Delislean poles. If the transit chord be short as in the case illustrated by fig. 21, the points *i* and *e′* (reference is now made to the small disc of fig. 21) will clearly be nearer together than where the transit chord is longer, as in the case illustrated by fig. 24; for the shorter the transit chord the greater is the angle enclosed between the intersecting arcs I *i* and E′ *e′*. Hence shortness of duration by tending to bring the two northern and the two southern Delislean poles close together renders a transit more favourable for the application of the method of duration.

To see, lastly, how geographical considerations enter into the discussion of this problem, compare Plates VI. and VII., illustrating the transits of 1874 and 1882. It will be seen that the northern or

[1] The same is proved in another way in 'The Universe and the Coming Transits,'—see also pp. 158, 159 of the present work; and in yet another way at pp. 38 and 39 of my treatise on the 'Sun.'

darkened Halleyan pole is nearly as far from the neighbouring sunlit region (for the whole transit) in the case of the former transit, as the southern darkened Halleyan pole is in the case of the latter transit. But the region where such approach has to be made in 1874 is altogether accessible, though doubtless bleak and cheerless during the northern winter prevailing there when transit occurs; whereas the sunlit region nearest to the southern Halleyan pole in 1882 is the inaccessible antarctic continent. Keeping away from that continent and within the space defined by the lines $a' b'$, $c' d'$, which show where the sun is ten degrees high at ingress or egress, there is absolutely no spot to be occupied which is near enough to H′ to be worth the trouble of journeying thither. In both cases the region around the sunlit Halleyan pole affords many good stations, though the transit of 1874 is not in this respect comparable with that of 1882; but the absolute absence of any southern station whatever in 1882 where the duration of transit is usefully lengthened, causes the method of durations to be wholly inapplicable on that occasion.

Thus far we have for simplicity considered the centre of Venus, or we may be said to have regarded Venus as a point. It is easy, however, to see what modifications are introduced when we take into account the fact that Venus is a globe. Thus, instead of a double cone, such as $s\, s'$ V $v\, v'$ in fig. 20, having the centre of Venus at its vertex, we must consider two double cones such as are shown in fig. 34, each enveloping both Venus and the sun, but one having its vertex outside the path of Venus (giving the shaded cone of the figure) and the other having its vertex within the path of Venus. It is manifest that any observer on the surface of the last-named cone, as for example, at *v′*, will see Venus touching the sun on the outside, *i.e.* in external contact; for the line of sight *v′* V *s′* touches both Venus and the sun, but *on opposite sides*, and is therefore directed to a point of contact *on opposite sides of which* the discs of Venus and the sun lie. On the other hand, an observer on the surface of the shaded cone, as on the prolongation of *s* V, will see

Fig. 34.—Illustrating internal and external contacts.

Venus just within the sun's disc, or in internal contact; for the line of sight touches both the sun and Venus *on the same side*, so that it is directed to a point of contact *on the same side of which* lie both the discs of Venus and of the sun.

We see then that taking the two concentric circular sections *v v′* fig. 34, we have only to substitute *external contact* and *internal contact* for what was said of the *passage of the centre of Venus* in the former case.

In order, however, to still further familiarise the student with these fundamental relations, I shall give an independent description of the relations presented in fig. 34, modifying into a more convenient form the explanation of the actual circumstances of the passage of the section of Venus's shadow-cone (for so the *v v′* of both figs. 20 and 34 may be regarded) over the less swiftly advancing earth.

If an observer were carried through the double cone shown in fig. 34 beyond Venus, he would see the following successive phenomena. When he came to the outer surface Venus would be in exterior contact; as he passed on to the inner surface Venus would enter more and more on the sun's disc, until when he reached the surface she would be in interior contact. Then as he travelled on through the inner cone Venus would seem to cross the sun's disc, and she would just touch it on the inside when our observer reached the surface of this inner region on his passage outwards. Next, as he passed onwards to the surface of the outer region, Venus would be seen crossing the edge of the sun's disc. And lastly, as he passed that surface he would again see Venus in exterior contact, the transit thereupon coming to an end.

During a transit of Venus the earth does actually pass in such a way through these regions; or rather these regions overtake and pass over the earth.

Since the cones overtake the earth in the direction shown by the arrows, we may consider that the earth passes through the cones in the contrary direction.

Suppose *v v′* (fig. 35) to represent the same section of the outer cone as *v v′* in fig. 34; V V′ the section of the inner cone; and E (fig. 35) the earth, as shown at E in fig. 34. Then $v\,v'$ is really moving towards the left; but we are to suppose that E is moving towards the right through $v\,v'$. Furthermore, if Venus is near an ascending node, as she will be during the approaching transits, we must suppose the earth to pass descendingly along such a course as E E′ through the region $v\,v'$. The actual course, both as respects posi-

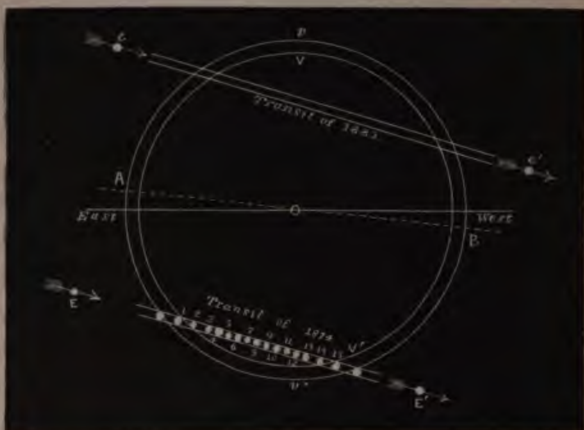


Fig. 35.—Illustrating internal and external contacts.

tion and direction, is determined from the calculated elements of the transit. With this calculation we need not here concern ourselves.[1] The figure shows

[1] As to the size of $v\,v'$ and V V′ compared with that of the earth, it is easily seen from fig. 34 that O v is greater and O V is less than the radius calculated in the note, p. 114 for the centre of Venus, by the radius of Venus increased in the proportion that the earth's distance from the sun exceeds the distance of Venus from the sun.

the course actually traversed by the earth in 1874 and 1882.

Now, taking the earth through *v v′* for the 1874 transit, let us consider the various critical points, so to speak, of her course. When she first touches the outer circle *v v′* external contact will have begun at that point of the earth which first reaches this circle. She passes on, falling more and more within *v v′*, until she is just wholly within. All this time external contact is taking place wherever the outline *v v′* intersects the earth's disc; at parts within that line Venus is seen partly within the sun's disc, and at parts outside of it external contact has not yet taken place. When the earth has passed wholly within the circle *v v′*, external contact has taken place at all parts of the visible hemisphere. But as at this time no part of the earth has reached the circle V V′,[1] internal contact has nowhere commenced. In other words, Venus is not yet fully upon the sun's disc as seen from any part of the earth.

Now, this part of the earth's motion is not illustrated in fig. 35, because external contacts and the passage of Venus across the sun's outline are not phases to which the observers of transits pay great attention. We now come to the important phases.

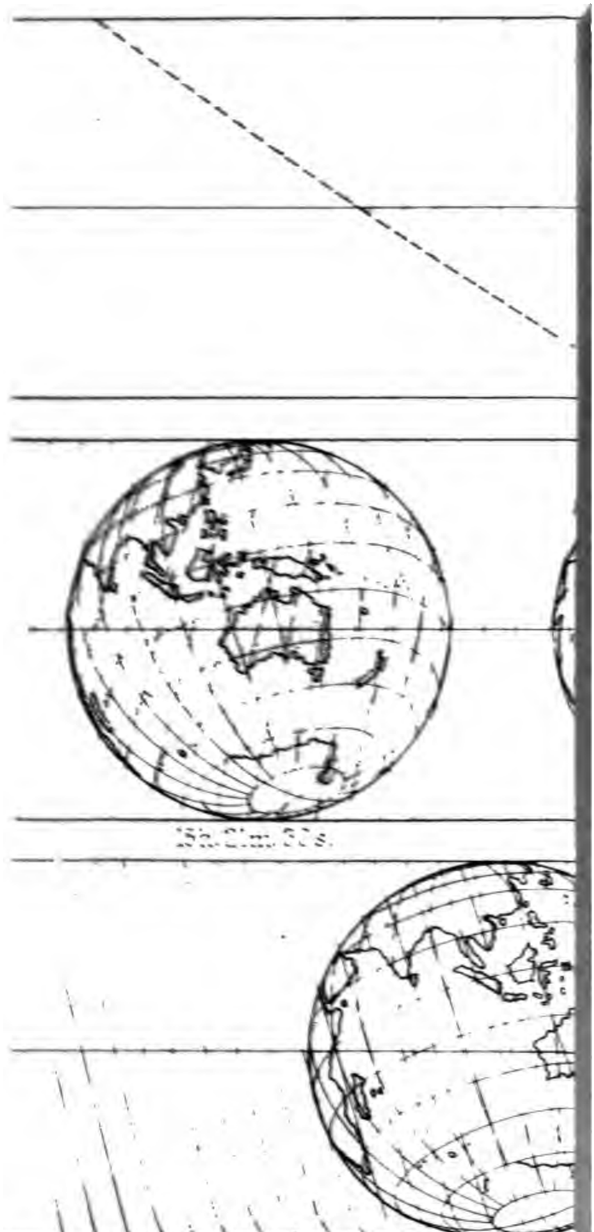
[1] The distance between the circles *v v′* and V V′ is obviously greater than the earth's diameter, if we consider how the two circles *v v′* and V V′ are obtained. For the diameter of Venus is very nearly equal to the earth's; so that the diverging lines from *s* or *s′* (fig. 34) are already separated at V by a distance nearly equal to the earth's diameter, and therefore at *v* or *v′* are wider apart.

When the earth just reaches the inner circle V V′, interior contact has just begun at the point on the earth which first touches this circle. Here, then, earliest of all, internal contact begins, and we have at this point the phenomenon called by astronomers *first internal contact most accelerated.* The earth is then in the position numbered 1 in fig. 35.

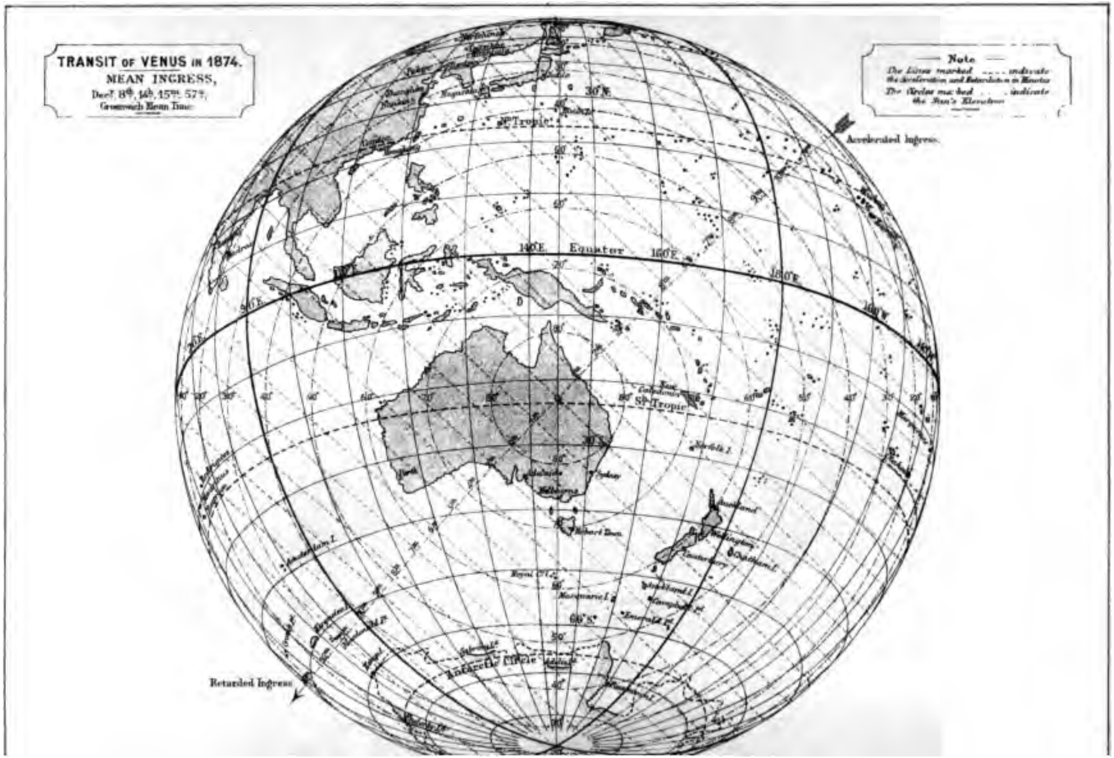
She passes on, the outline V V′ encroaching more and more over her face until she is wholly within this outline or in position 2. All this time internal contact is taking place wherever the outline V V′ intersects the earth's disc. At parts of the earth within that line internal contact has passed, or Venus is already fully upon the sun's disc. At parts of the earth outside that line Venus still breaks the outline of the sun's disc. When the earth is at 2, internal contact has taken place for all places on the earth's illuminated hemisphere. This contact takes place latest of all at that point on the earth's surface which at this moment touches V V′. It is here, then, that there occurs the phase which astronomers call *first internal contact most retarded.*

Then the earth passes onwards through the positions shown severally along her track in fig. 35.

As the earth passes out of the spaces V V′, *v v′*, similar phases occur in reverse order. We need note only the positions numbered severally 14 and 15. The first shows where the earth first reaches V V′, and the point on her surface which first touches V V′ is the place where occurs the phase called *second internal*

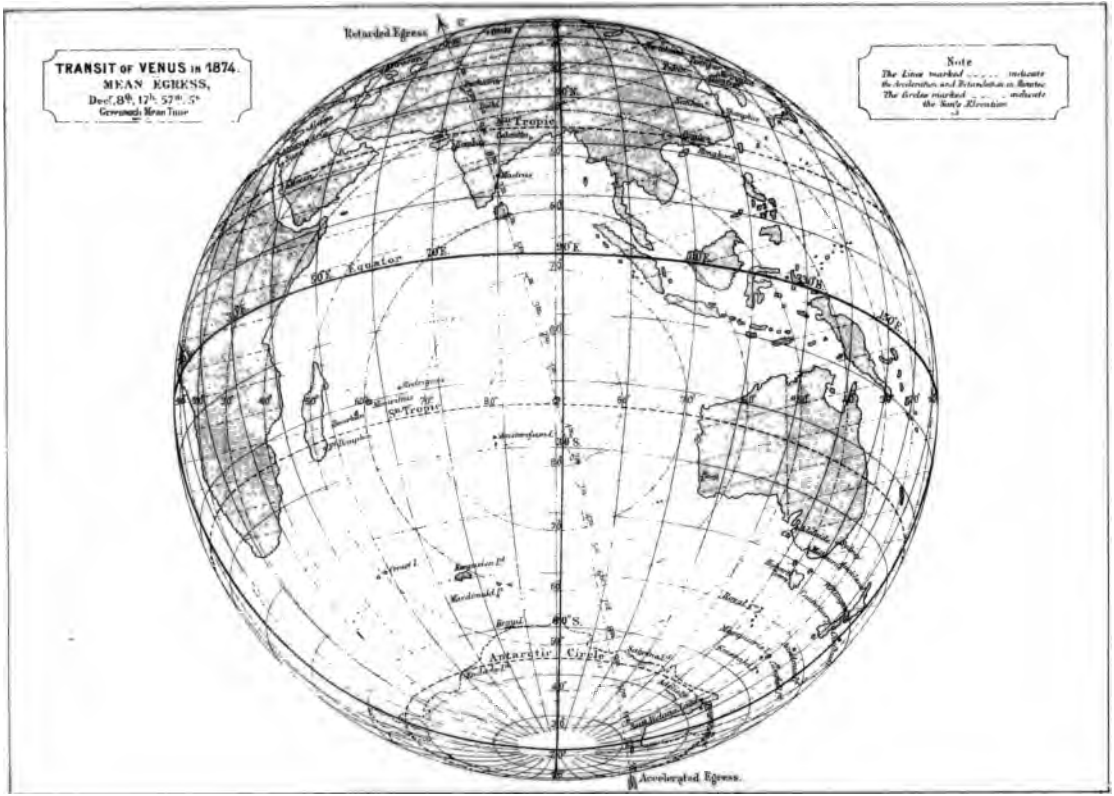


R.A. Proctor, del.



TRANSIT OF VENUS IN 1874.
MEAN INGRESS,
Decr. 8th, 14h, 15m, 57s,
Greenwich Mean Time
Note
Accelerated Ingress.
Retarded Ingress
Equator
140°E.
160°E.
180°E.
160°W.
N. Tropic
S. Tropic
Antarctic Circle
30°N.
60°S.
New Caledonia
Norfolk I.
Auckland
Wellington
Canterbury
Chatham I.
Hobart Town
Melbourne
Sydney
Adelaide
Perth
Amsterdam I.
Macquarie I.
Royal Co. I.

Pl. XVIII.


TRANSIT OF VENUS IN 1874.
MEAN EGRESS,
Decr. 8th, 17h. 57m. 5s
Greenwich Mean Time
Note
The Lines marked ...... indicate the Acceleration and Retardation in Minutes
The Circles marked ...... indicate the Sun's Elevation
Retarded Egress
Accelerated Egress.
Equator
Nn. Tropic
Sn. Tropic
Antarctic Circle
Kerguelen Id.
Macdonald Id.
Madras
Calcutta
Rodriguez
Mauritius
Amsterdam I.
Crozet I.

folding plate. The encroachment and the passing off not being strictly uniform,[1] these lines are not equidistant, nor are they strictly parallel or straight.

These two plates have been given merely to illustrate the exact constructions which can be applied to such projections, in order from them to ascertain the best stations for applying various methods.[2] They have been reduced by photolithography from two of the quarto plates illustrating 'The Universe and the Coming Transits.' For general descriptive illustration, Plates XII. and XIII. are more suitable, as simpler and clearer. Since at present I am not dealing with the special conditions of the coming transits, I need not here discuss the geographical details of these plates, or of plates XIV. and XV. illustrating the transits of 1882.

[1] The reason of this will be seen by a reference to fig. 35. Obviously the rate at which the earth's centre is approaching the centre of Venus (which rate really measures the rate of encroachment) diminishes during ingress, while for a like reason the rate of passing off increases during egress.

[2] Properly speaking Plates XVII. and XVIII. only represent the earth accurately for the moment when the outline of v v′ (fig. 35) crosses the earth's centre. Since, as we see by the cross-lines, no less than 25m. 6s. are occupied by the passage of the outline of v v′ over the earth's face, both at ingress and egress, the earth's rotation has to be considered. This, however, can very easily be done, since the latitude circles are shown, and the longitude circles are separated by ten degrees, corresponding to the earth's rotation in forty minutes. Thus from Plate XVII. we see that the cross-line marked 7m. on the right of the centre passes near Jeddo. But as the cross-line occupies this position seven minutes before it crosses the Earth's centre, we must put Jeddo back through an amount corresponding to seven minutes' rotation, or about one-sixth of the distance separating two longitude-circles in this neighbourhood.

But now, lastly, it remains to show how the actual progress of a transit as seen from the earth corresponds with the progress of the earth through Venus's shadow-cone as illustrated in figs. 34 and 35. For although the plan of dealing with the problem by considering the passage of the earth through these cones is, on the whole, the most convenient which can be adopted, and especially on this account, that it shows us directly what face of the earth is turned sunwards at the beginning or end or at any other stage of the transit, yet there is something artificial in this way of considering the subject. The student who wishes to know what can actually be seen from the earth seeks for something more than a description of what might be seen from the sun under certain imagined conditions.

A very simple consideration will enable us at once to transpose the relations illustrated in fig. 35 in such a way as to correspond to the actual transits across the solar disc.

Fig. 37.—Illustrating the connection between the passage of Venus over the sun's face, and the passage of the earth through Venus's shadow-cone.

Suppose S (fig. 37) the sun's centre, *s'* S *s* a diametral plane of the sun square to the line S V O, which forms the axis of the shadow-cones we have been dealing with (for simplicity taking the cone as in fig. 20). Thus *s s′* is a circle directly opposite to *v v′*, the planes of these circles being parallel. (The left-hand halves of the ovals *s′ s* and *v v′* are supposed to be the nearer.) Now imagine a straight line passing through V to the centre of the earth E on one side, and to the circular disc *s s′* on the other. Since the earth's centre carrying this line travels along E E′ athwart *v v′* on the path *i e*, such as is shown in fig. 34, passing slantingly downwards below the centre of *v v′*, it is clear that the other end of the line will travel along F F′, across *s s′*, on the path *a b*, moving slantingly upwards above the centre of *s s′*. If we looked at *v v′* from V, the motion of the earth would be from left to right along *i e*; and manifestly, if we looked at *s s′* from V, the motion along *a b* would also be from left to right. In other words, whereas *a b*, as a projection of *i e*, is inverted, it is not reversed right and left, provided we are supposed to view *v v′* and *s s′*, in turn, from the point V. The chord *a b*, then, so viewed, is a perfect *projection* of *i e*, inverted without being reverted right and left.

And clearly this principle of projection may be extended to all that is pictured in fig. 35, not only as respects motion along the transit chords of 1874 and 1882, but also as respects the sun-views of the earth supposed to be presented by the numbered discs, and actually presented on a much enlarged scale in Plate XVI. The circle *s s′*, of fig. 37, which represents the solar disc, is a perfect projection of *v v′* in this sense, that wherever an observer be supposed to be placed on the circle *v v′*, he would see the centre of Venus projected at a point of *s s′* corresponding to his own position, only inverted as respects north and south. And if we imagine a small figure of the earth properly placed on the chord *i e*, with correct pose of axis and rightly rotated, to correspond to the time at which the earth actually reaches that part of *i e*, then for every point on the sunlit-half of that small globe there will correspond a point on *s s′*. If, further, we imagine a straight line extending from V to this globe of the earth on one side and to *s s′* on the other, and that the former extremity is carried along all the outlines of continents and islands on the sunlit-half of the globe, the other extremity will describe on the disc *s s′* an inverted, but not reversed, picture of those continents and seas. Any point in this inverted picture will indicate the point on the sun's disc occupied by the centre of Venus, as supposed to be seen at the corresponding moment by an observer placed at the corresponding point of the earth's globe. So that when once we have constructed such a picture as Plate XVI., giving a series of sun-views of the earth during her passage through the sections *v v′*, V V′ (fig. 35), of the shadow-cones shown in fig. 34, we have at once the means of determining the apparent path of Venus's centre across the sun's disc for any station whatever upon the earth. In fact, Plate XVI., held up to the light, inverted, and looked at from behind, pictures the portion of the sun's disc traversed by Venus; the pictures

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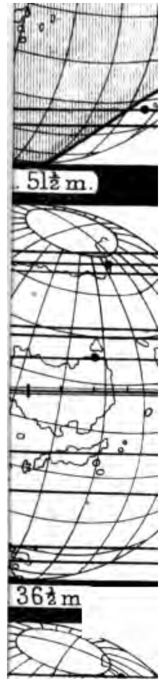

51½ m.
36½ m

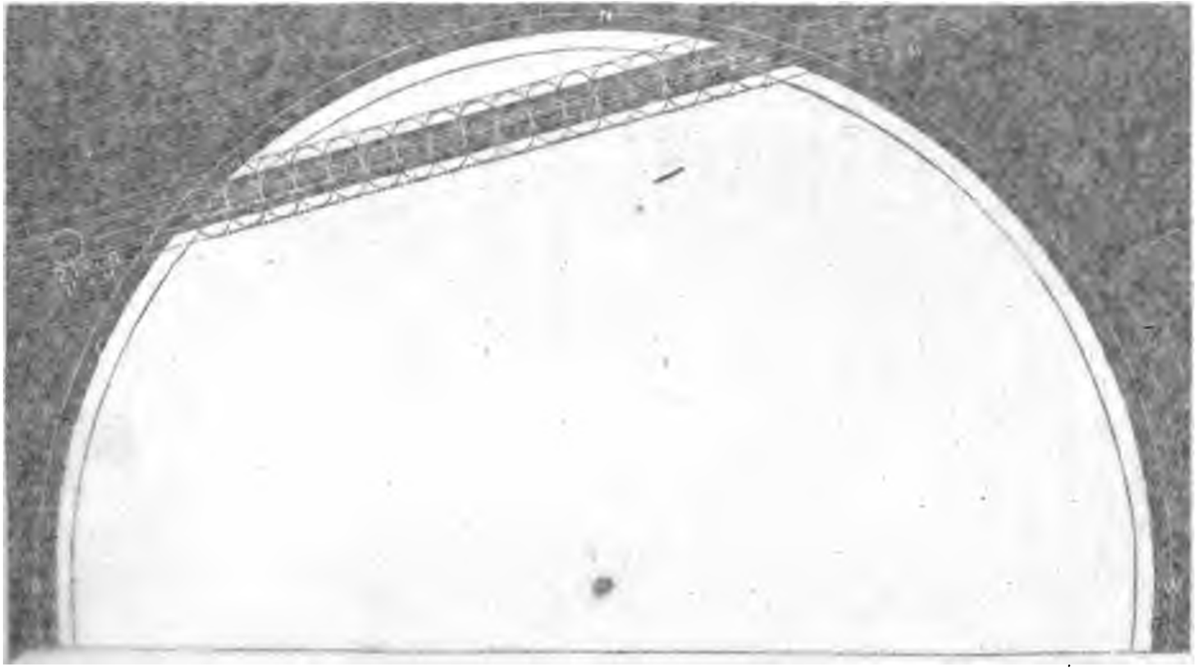
ı inverted without reversion are such pro-
I have been speaking of; and we have
down the place of any island or town in
ısive projections, and to connect the succes-
a line, to have the path of Venus's centre
ın's disc as viewed from that island or town
passage.
IX. has been constructed in the way here
ıly that I have thought it better to show
separated throughout by a quarter of an
d of having internal contacts (most accele-
most retarded) illustrated specially as in
Plate XX. is intended to explain more
meaning of Plate XIX. It shows the
lf of the sun's disc. Outside and inside
circles, one having a radius exceeding the
enus's semidiameter, and the other having
ss than the sun's by the same amount; so
Venus's centre crosses the outer circle her
touches the sun's on the outside, or she is
contact, while when her centre crosses the
her outline just touches the sun's on the
ıe is in internal contact. Parts of these
hown in Plate XIX. Across the disc five
s are drawn. The central one is the path
centre supposed to be viewed from the
the earth. The line next to the centre,
ws the path of Venus's centre supposed
ys so viewed from a southerly station as
wn as far as possible from the central

path on the *northern* side;[1] and the line next to the centre, below, shows the path of Venus's centre as supposed to be always so viewed from a southerly station as to be as far as possible from the path of the central path on the *southern* side. The lines next outside the two last-mentioned mark the boundaries of the track of Venus's *disc* as supposed to be seen from the centre of the earth. And lastly, the outside dotted lines mark the northern and southern boundaries of the tracks pursued by Venus's disc if so viewed that her centre would follow the tracks shown north and south respectively of the central path. *No part of Venus can be seen, from any part of the earth, outside these dotted lines.*

In Plate XIX., the tracks followed by Venus's centre as seen from twelve important stations, are marked in. The student can readily add, either on the plate itself or on a tracing from it, the transit path for any other station. It will be found a useful exercise to trace from Plate XIX. the central path and the outline of the sun's disc, and the path of any stations whether of the twelve dealt with in the plate or such others as the student may desire, and then having cut the picture thus formed into three parts by horizontal lines (where the black spaces fall in the plate) to connect them into one long strip corresponding to the transit band of Plate XX.

[1] There is no fixed point in the earth where this relation would hold. The observer would have to be placed at the point of the earth which just touches the southern transit-parallel in Plate XVI., and this is a point continually travelling backwards along a southern latitude parallel. A similar remark applies to the corresponding northerly positions.

PLATE XI.



SHOWING THE [illegible] ACROSS THE SUN'S DISC DURING THE TRANSIT OF 1874.

(The [illegible] may be regarded as a miniature of that shown in Plate XI.)

It remains only to be added that the process applied in the construction of Plates XVI. and XIX. to illustrate the transit of 1874, can easily be applied to any other transit. Take for instance the transit of 1882. Here a portion of the work has been already done, since Plates XIV. and XV. illustrate the beginning and end, with the position of the circles V V′ of fig. 35. A picture of the space enclosed between the transit chords for 1882 fig. 35, and the circle V V′ ought to be made on such a scale that the distance between the transit chords would equal the diameter of the discs in Plates XIV. and XV. Or, if that scale be too large, then figs. 38 and 39 may be used instead. A series of sun-views can readily be drawn on tracings of the meridians and parallels either of Plates XIV. and XV., or of figs. 38 and 39, corresponding to successive equal epochs (say fifteen minutes apart) all through the transit. These must be arranged in a row as in Plate XVI., in their proper order, and so posed that the central cross-lines (marked MEAN TIME in Plates XIV. and XV., and 0^m in figs. 38 and 39) may cross the track of central transit at the equal angles at which the circle V V′ crosses that track in fig. 35. Then will a picture corresponding to Plate XVI. have been constructed, except that internal contacts will not have been specially illustrated by projections corresponding to these contacts (as most accelerated and most retarded). The picture so drawn, if inverted and looked at from behind (or if inverted and viewed in a mirror), will correspond to Plate XIX., and enable the student to trace the path of Venus's centre as seen from any station whatever on that occasion.

Other transits may be illustrated with equal

TRANSIT OF 1882. (INGRESS.)

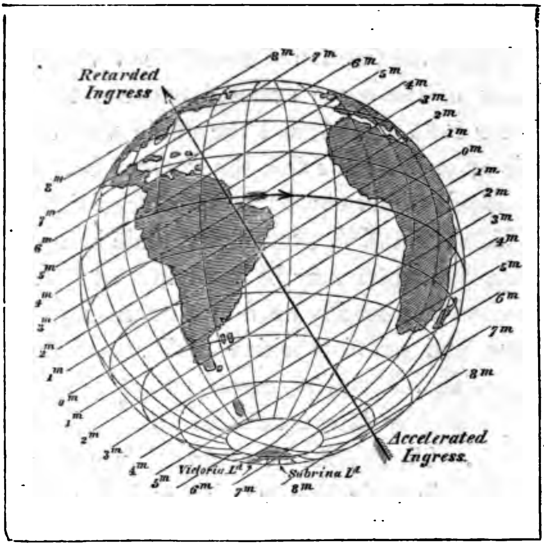
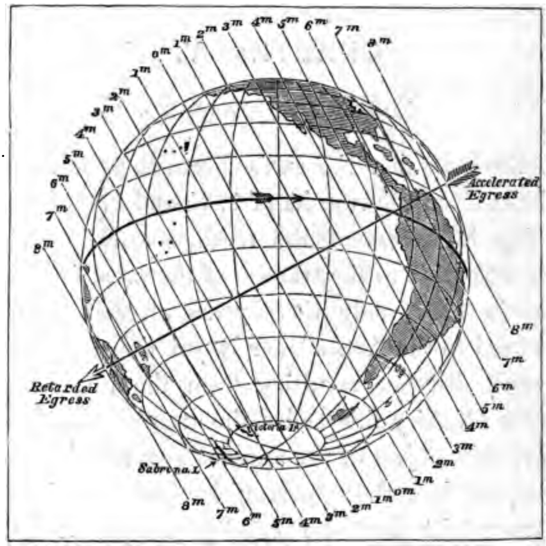


Fig. 38.—Illuminated side of the earth at ingress, Dec. 6, 2h. 15m. 56s. (Greenwich mean time.)

readiness. Nor need the details of the process be any further illustrated by examples, since any student who takes sufficient interest in these matters to attempt the projection of a transit in the manner here applied to the approaching transits, will have sufficiently examined the earlier portions of this chapter to be able to recognise clearly the relations involved in constructions of the kind.

The construction of such illustrative projections as lates II., III., &c. . . . IX., needs no explanation; r these are simply stereographic polar projections

TRANSIT OF 1882. (EGRESS.)



g. 39.—Illuminated side of the earth at egress, Dec. 6, 8h. 0m. 32s. (Greenwich mean time.)

f the earth, upon which various lines and points, btained by the methods already described, are laid own for convenience of study and reference.

CHAPTER V.

THE COMING TRANSITS.

THE discovery that the sun's distance, as determined by Encke from the transits of 1761 and 1769, was considerably in excess of the truth, naturally directed special attention to the transits of the present century. It was in 1857, only three years after Hansen had announced to the Astronomer Royal the correction in the sun's distance resulting from the lunar theory, that Sir G. Airy first called the attention of astronomers to the subject of the approaching transits, and to the inquiry how the opportunities presented by these transits might best be employed. In a lecture delivered before a meeting of the Astronomical Society in May 1857, he examined the various methods available for determining the sun's distance, and ascribing to the observation of Venus in transit the highest value, he considered in a general way the circumstances of the transits of 1874 and 1882. He pointed out that, *cæteris paribus,* the second transit of a pair is superior to the first for Halley's method; but unfortunately failed to observe that special circumstances may modify or even reverse this relation. Although

I have given one demonstration (in the preceding chapter) of the general law and of the fact that the coming transits present an exception to it, it will be well to show here the nature of Airy's reasoning :—

Let fig. 40 represent the face of the earth as supposed to be seen from the sun during a December transit, such as either of the approaching transits. Now, the earth during the transit is moving from right to left, or in the direction shown by the long arrow (the slant of the axis is for simplicity neglected). Her rotation shifts points on her surface in the way shown by the small arrow on the equator, the shift due to this cause being greatest on the equator. This motion manifestly takes place in a sense adverse to that of the earth's motion of revolution, everywhere except at stations on the shaded lune of the disc. Now, Venus transits with the excess of her motion of revolution over the earth's; and anything which tends to reduce the effects of the earth's motion of revolution, increases the excess of Venus's motion—or in other words, hastens Venus in her transit. So that at every point of the unshaded portion of the disc in fig. 40 Venus is hastened, more or less, by the effecta due to the earth's rotation. On the contrary, at every point on the shaded portion of the disc Venus is retarded in her transit.

These circumstances affect diversely the two transits of such a pair as we are now awaiting. If fig. 41 represents the sun's disc, the north point being uppermost, then the lines *a b*, *c d*, will represent chords of transit in 1874 (*a b* being the chord for a northern, *c d* being the chord for a southern station); and *a′ b′*, *c′ d′* will represent chords of transit in 1882 (*a′ b′* being the chord for a northern, *c′ d′* the chord for a southern station).

It is manifest that in 1874 the conditions affecting the duration of the transit as seen at a northern station

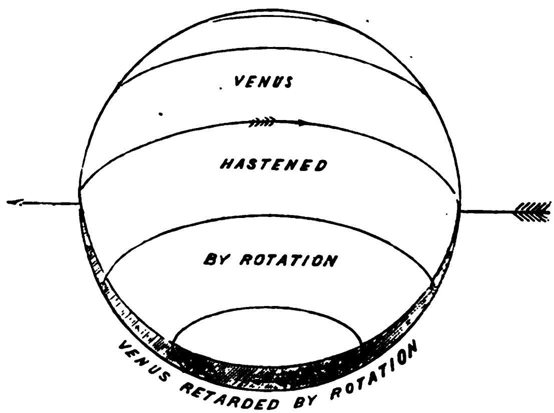


Fig. 40.—Illustrating the effect of the earth's rotation on the progress of a transit.

are adverse. The chord *a b* is longer, owing to the northerly latitude of the observer; but Venus is hastened on her course, and therefore the lengthening is not so great as it otherwise would be. We have then one favourable and one unfavourable condition, the latter to some degree cancelling the former. (In some transits of the kind the effect of rotation wholly cancels, or even more than cancels, the effect due to latitude.) The southern station, if taken where, throughout the transit, the observer is on the portion of the disc represented without shading in fig. 40, will give conspiring effects. The chord of transit *c d* will be shortened, and Venus will be hastened on her course. Hence we have for this station two favourable conditions. In all we have three favourable conditions and one unfavourable condition—so that if the con-

Fig. 41.—Illustrating the effect of the position of transit chords.

ditions are all equal in value we have a balance of only *two* favourable conditions.

On the other hand, in such a transit as that of 1882 we can theoretically secure four favourable conditions. We have at the northern station the shortened transit chord *a′ b′*, and a hastening of Venus—or two conspiring conditions. At a southern station we have the lengthened transit chord *c′ d′*, and by taking a station which throughout the transit lies on the shaded part of the disc (that is, an Antarctic station passing below the pole during the transit hours), we have Venus retarded on her transit path, or again we have two conspiring conditions. In all, then, we have *four* favourable conditions, or twice as many as we obtain for the balance of favourable conditions in 1874.

This is theoretically sound. Moreover, it is quite commonly the case that the effects due to rotation are equivalent to those due to latitude, and that therefore the adverse conditions at a station placed as the northern station in 1874 may be regarded as cancelling each other. In the transit of 1769, for example, the conspiring effects of rotation and latitude were nearly equal. The Astronomer Royal, in his 'Popular Astronomy' (published in 1848, be it noticed), justly assigns to rotation 10 minutes out of the observed maximum difference of duration, 22 minutes. It does not seem rash to infer that he had this result in his thoughts when, after mentioning that the best northern stations would probably not be occupied in 1874, he proceeded to remark (in 1857) that the '*observable difference*' *in the earlier transit would* '*probably not be half of that in* 1882.'

Although the observable difference in 1874 *is really half as great again as in* 1882, yet it mattered very little, at that early epoch, if any mistake of this sort crept into what claimed to be little more than a popular account of the general subject of transits. No one probably considered that the Astronomer Royal attached any weight to the details of his paper of 1857.

In fact, so roughly was the paper prepared that the time of mid-transit in 1874 was an hour wrong—an error not resulting from incorrectness of the tables, for the time of transit of 1882 was very nearly correct. In fact, the paper of 1857, accurate enough for its purpose, had not, and did not seem intended to have, any scientific weight.

But unfortunately, the Astronomer Royal, when next he dealt with the subject, seems to have regarded the transit of 1874 as *demonstrated* by his former rough paper to be unfit for the application of Halley's method. For, in 1864, he published a sufficiently accurate investigation of the transit of 1882, illustrated by projections (corresponding to those forming Plates VI. and VII.) well executed by Mr. H. Carpenter of Greenwich, and in this paper the transit of 1874 was not considered at all. In 1865, he again commented on the circumstances of the transit of 1882 without mentioning the earlier transit. When at length, in 1868, he published what purported to be a detailed description of the circumstances of the two transits, and of the duties not of English astronomers only, but of astronomers generally with respect to the transits, he remarked that Halley's method had been *shown* to fail totally in 1874.

It will serve, I think, to remove misconceptions if I quote here the remarks addressed to the scientific world by Sir George Airy in 1868 respecting the important transits of 1874 and 1882.

'On two occasions,' he writes, ('Monthly Notices,' 1857, May 8, and 1864, June 10) 'I have called the attention of the Society to the transits of Venus across the sun's disc, which will occur in the years 1874 and 1882; and have pointed out that, for determination of the difference between the sun's parallax and the parallax of Venus, the method by observation of the interval in time between ingress and egress at each of two stations at least, on nearly opposite parts of the earth (on which method, exclusively, reliance was placed in the treatment of the observations of the transit of Venus in 1769),[1] fails totally for the transit of 1874, and is embarrassed in 1882 with the difficulty of finding a proper station on the almost unknown Southern Continent.

'The publication of M. Le Verrier's new Tables of Venus, and of Mr. Hind's inferences from them as to the points of the sun's limb at which ingress and egress will take place in each transit (which inferences I have in part verified), has induced me again to examine the whole subject. And, without giving up the hope of using the observation of interval between ingress and egress at each of two stations in 1882, I have come to the conclusion (from all the information which has reached me) that it will be unsafe to trust exclusively to the chance of securing observations on the Southern

[1] As it has been said (as a correction of my own criticism of the above paper) that Sir G. Airy did not describe the 'method of durations' as failing totally, but only Halley's method, *meaning the method of durations as applied to a nearly central transit,* I invite special attention to his careful wording. The parenthesis removes all doubt as to his real meaning (for the transit of 1769 was far from central); though indeed without the parenthesis the meaning is unmistakable.

Continent; and that, while observations are by all means to be attempted in that manner, it is also very desirable to combine with them observations of the same phenomenon (at one time the ingress, at another time the egress), made at nearly opposite stations whose longitudes are *accurately* known, and recorded in *accurate* local time. This principle being once admitted, the transit of 1874 is, or may be, as good for observations of that class as the transit of 1882; and the selection of localities for the observations must be made with equal care for the two transits.'

He then explains how the maps which illustrate his paper were constructed, and proceeds to discuss the individual maps with reference to the selection of stations for observing the several phenomena. Plate VI. will serve as well as these maps to illustrate what follows:—

I.—'*Stations for observing the Ingress as accelerated by Parallax*'—*that is, Stations near* I (*Plate VI.*) *on the Illuminated side of* A B, *but not too near to* A B.[1]

'Owhyhee and the neighbouring islands are excel-

[1] If 10° be assumed as the lowest elevation at which useful observations can be made, then the stations must not lie within 10° of the circle A B. The arcs *a b*, *c d*, *a′ b′*, *c′ d′*, Plates VI. and VII., indicate this limit for the transits of 1874 and 1882. Thus a station for observing ingress accelerated by parallax (in 1874) should not be anywhere within the zone A B *b a*; so that the best point for observing accelerated ingress would be that point on *a b* which lies nearest to I. Similar remarks apply to observations of retarded ingress near I′, of accelerated egress near E′, and of retarded egress near E, the dotted curves *a′ b′*, *c′ d′*, and *c d* marking the limits outside which stations should be placed.

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'Owhyhee and the neighbouring islands are excel-

[1] If 10° be assumed as the lowest elevation [illegible] can be made, then the stations must [illegible] [illegible] s *a b*, *c d*, *a′ b′*, *c′ d′*, Plates VI. and [illegible] [illegible] nsits of 1874 [illegible] [illegible] erated by parallax [illegible] [illegible] e A B *b a*; so that [illegible] [illegible] be that point [illegible] [illegible] to observations [illegible] [illegible] B′, and of [illegible] [illegible] *d* marking [illegible]

lent. The factor of parallax[1] is about 0·92, and the sun is at nearly two hours' elevation. There is English society at Woahoo. These islands are just within the tropics. For use of this station the absolute longitude must be accurately determined.

'At the Marquesas Islands the factor of parallax is 0·7, and the sun is nearly as high as at Woahoo. Our neighbours across the Channel have, from the time of Louis XIV., taken an honourable lead in scientific enterprise of every class. I trust that we may rely on them for accurate determination of longitude at Marquesas, and for accurate observation of the ingress in 1874.

'The desert Aleutian Islands can scarcely be recommended, although the factor 0·8 for the westernmost of them, where the sun is highest, is favourable. But it is very probable that the Russians will soon have established telegraphic communication with the mouth of the Amoor, by which its absolute longitude will be accurately determined; and though the factor is only 0·57, the sun is 15° high, and the station will be valuable.

'On the whole, if the British Government will undertake the accurate determination of longitude of Woahoo, and the careful observation of ingress there in 1874, we may consider that good provision is made for the accelerated ingress.'

[1] This expression indicates the acceleration or retardation at the station, regarding the maximum acceleration as unity.

II.—'*Stations for observing the Ingress as retarded by Parallax*'—*that is, Stations near* I′ (*Plate VI.*) *on the Illuminated side of* A′ B′, *but not too near to* A′ B′. *See note, p.* 163.

'The best station, as referred to the test of numbers, is Kerguelen's Island, where the factor is 0·91, and the sun is 25° high. This island is emphatically known as "The Island of Desolation." I know not whether its character is so repulsive, or its utility as a zero of longitude so small, as to make our nautical authorities unwilling to determine its longitude, and to station observers there in 1874. If these difficulties are not thought too great, it will be an excellent position. At Crozet's Islands the factor 0·98 is very favourable, but the sun is rather low (10° altitude).

'The next stations in order of merit are Rodriguez, Mauritius, and Bourbon. Mauritius possesses this claim, that it will be a fairly good station, though not so good as Bourbon, in 1882; in 1874 as well as in 1882, it has this disadvantage, that the sun will be low. If only one longitude can be determined in this chain of islands, it ought to be that of Mauritius; if two can be determined, they ought to be those of Rodriguez (for 1874) and Bourbon (for 1882).

'At Madras and Bombay the factors, 0·47 and 0·44, are small; but the value of either station does not depend entirely on its simple factor, but upon the sum of its factor with the factors at the stations under head I. These two observatories, with well-known longitudes, will prove very useful stations.

'With the assistance which we may hope to receive from the British Government, we may consider the observation of the retarded ingress as well secured.'

III.—'*Stations for observing the Egress as accelerated by Parallax*'—*that is, Stations near* E′ (*Plate VI.*) *on the Illuminated side of* C′ D′, *but not too near to* C′ D′. *See note, p.* 163.

'Excluding from consideration the Southern Continent as not to be entertained in our thought without the most absolute necessity, the stations in order of merit are the Auckland Islands, Canterbury, Wellington, and Auckland, in New Zealand (factors ranging from 0·83 to 0·77), Norfolk Island (0·66), Melbourne and Sydney (0·6). I omit Chatham Island, where the sun is rather low. The existence of the observatories at Melbourne and Sydney makes the observation of the accelerated egress almost secure, although, in confirmation, I should much desire to have one station at least on the New Zealand group.'

IV.—'*Stations for observing the Egress as retarded by Parallax*'—*that is, Stations near* E (*Plate VI.*), *on the Illuminated side of* C D, *but not too near to* C D. *See note, p.* 163.

'The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84; and we have, therefore, only to consider the elevation of the sun, leaving to the national Governments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the sun will be rather low, and at Moscow it will be on the horizon. We may, with the utmost confidence, leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends.[1] One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria; but, failing this, I trust that no efforts will be wanting to determine accurately its longitude—a longitude which was in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best

[1] It cannot but be manifest from the whole tone of this passage that the conditions of the transit for the scientific world, and not for British astronomers only, were intended to be presented in the Astronomer Royal's paper.

of all the stations for observation of the retarded egress.'[1]

TRANSIT OF VENUS, 1882, DECEMBER 6.

FIRST, BY THE METHOD OF ABSOLUTE LONGITUDES.

V.—'*Stations for observing the Ingress as accelerated by Parallax'—that is, Stations near* I′ *(Plate VII.), on the Illuminated side of* A′ B′, *but not too near to* A′ B′. *See note, p.* 163.

'Omitting for the present all allusion to the Southern Continent, it will be seen that the best station is Kerguelen's Island, its factor being 0·98, and the sun's elevation (12°) probably sufficient. This circumstance, in addition to its value as explained in the discussion of Plate II., renders it well worthy of attention. At Crozet's Islands the factor is 0·9, and the sun's elevation 23°; abstractedly it is preferable to Kerguelen's Island, but not in quite so great a degree as that in which Kerguelen's Island is superior in list II. The next in value are Bourbon and Mauritius, with factor about 0·78, the sun being higher at Bourbon. On comparing these qualifications with those remarked under head II., the reasons

[1] The absolute omission of the Indian stations here, though they had been mentioned among those useful for observing retarded ingress, is remarkable, but is readily understood when the Astronomer Royal's maps are examined. North India is nearer to E (Plate VI.) than Alexandria, and has a higher sun.

will be evident for my recommendation that either the longitude of Mauritius or the longitudes of Bourbon and Rodriguez should be determined.

'At the Cape of Good Hope the factor is about 0·62, and the observation there will be valuable.

'The satisfactory observation of the accelerated ingress requires, however, some longitude-determinations.'

VI.—'*Stations for observing the Ingress as retarded by Parallax'—that is, Stations near* I (*Plate VII.*), *on the Illuminated side of* A B, *but not too near* A B. *See note, p.* 163.

'Every city near the seaboard of the United States of America, and every important city of Canada, commands this phenomenon most favourably. The lowest factor is 0·95, and the smallest elevation of the sun is 12°. The utmost reliance may be placed on the zeal of our American brethren for observing the ingress. As great facility exists for determining the absolute longitude of any place within the range of American telegraphs (Harvard having been accurately referred to Greenwich), it is unnecessary to look further. Otherwise it might be remarked that Bermuda, Jamaica, and the West Indian Islands, and both sides of Central America, are excellent stations, but requiring determinations of longitude.'

VII.—'*Stations for observing the Egress as accelerated by Parallax*'—*that is, Stations near* E (*Plate VII.*), *on the Illuminated side of* C D, *but not too near* C D. *See note, p.* 163.

'All the American stations mentioned in the last paragraph, from Halifax to New Orleans, and Bermuda and the West Indian Islands, are well situated for this observation, the factors being near 0·85, and the sun's altitude varying from 4° at Halifax to 32° at New Orleans and Jamaica. The coast of South America also is favourable, from its union with the isthmus to the harbour of Rio de Janeiro. It is believed that efforts have been made for exact determination, in a nautical sense, of the longitude of Rio; it may now be desirable to give to that longitude the utmost accuracy.'

VIII.—'*Stations for observing the Egress as retarded by Parallax*'—*that is, Stations near* E′ (*Plate VII.*), *on the Illuminated side of* C′ D′, *but not too near* C′ D′. *See note, p.* 163.

'Omitting for the present the Southern Continent, this observation will be amply secured by the observatories of Sydney and Melbourne, where the factor is 0·96, and the sun's elevation 12° to 14°. If, however, the longitudes of the New Zealand stations can be ascertained, they, with factor 0·8 and sun's elevation 32°, will form a valuable addition.'

SECOND, BY THE METHOD OF INTERVAL BETWEEN INGRESS AND EGRESS.

'On comparing Plates VI. and VII., it will be seen that the North American localities supply, in a manner which leaves nothing to be desired, the demand for stations, at which the ingress is retarded and the egress accelerated, or the whole interval is diminished, by parallax.

'With these, it is necessary to combine one or more stations, at which the ingress is accelerated and the egress retarded, or the whole interval is increased, by parallax. On examining' Plates VI. and VII., 'it will be seen that the only possible method of responding to this demand is by the selection of stations on the Antarctic Continent, in which the observation will be made when the sun is nearly below the Pole.

'In so far as the coast of the Antarctic Continent follows nearly a parallel of latitude, the best position for a station is at 7^h east longitude. The factors would be, for ingress and egress, about 0·95 and 0·68. The sun would be, at each station, about three hours from the sub-polar meridian. But its elevation above the horizon would scarcely exceed 4°, and any alteration of the longitude, with the view of increasing the elevation at one phenomenon, would diminish it at the other.

'Advantage may, however, be taken of the deep southern inlet discovered by Sir James Ross, to the western side of which is given the name of South

Victoria. If a station can be established in latitude exceeding 72° S., it will be preferable, for observation of ingress, to the station in 7^{h} longitude, and if the expedition could be pushed on to an observing place in the neighbourhood of Mounts Erebus and Terror, that position would be greatly preferable. For observation of egress, it is manifestly far superior; the sun's altitude being about 27°. The factors for the two observations are respectively about 0·78 and 0·58.

'The decision on the choice to be made between these two stations, and the judgment on the facility, or even the possibility, of using either of them, must rest with persons who have had some familiarity with polar, and, if possible, with south polar voyages.'

'In partial correction of some small inaccuracies in these remarks, it may be observed that—

'The ingress, as viewed from the earth's centre, is always a few minutes earlier, and the egress always a few minutes later, than is supposed in the maps.'

'As affected by parallax, the phenomenon is always retarded with ascending sun and accelerated with descending sun.'

'As referred to apparent solar time, the phenomena are slightly retarded.'

'The only phenomena which are critically affected by these corrections are those' of Plates IV. and V., 'and in both the circumstances of solar elevation are rendered more favourable.'

'There may be a cause of uncertainty in the observation-elements on which M. Le Verrier's tables of Venus are founded, arising from the unsatisfactory form in which the observations of the planet were recorded in Bradley's time. At a critical inferior conjunction, when Venus was apparently a very large body, very deeply hollowed, it is impossible to say whether the limb was observed or whether an attempt was made to observe the centre. In my reductions of the planetary observations I inclined to the former idea. Subsequently M. Le Verrier adopted the latter. The tables are, at this time, very accurate, and it may prove that M. Le Verrier's interpretation was correct. Any uncertainty, however, of this kind makes it desirable to avoid observations of the ingress or egress very near to the horizon.'

This account of Sir G. Airy's treatment of the two transits would be incomplete without some description of his views as to the occupation of Antarctic stations, or without an account of the opinions advanced in support of his views by authorities whom he had invited to attend the meeting of the Astronomical Society before which the above programme was advanced.

In 1857 the Astronomer Royal's remarks were thus reported ('Monthly Notices' for May 1857, p. 216): 'The southern tract is a part of the Antarctic land discovered by Lieut. Wilkes, of the United States navy,[1] included between Sabrina Land and

[1] This Antarctic 'land' had, however, been *sailed over* by Ross ten

Repulse Bay. The Astronomer Royal is informed by General Sabine that December 6 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 would be extremely desirable that the country should be reconnoitred some years before the transit.' (The whole passage should be studied, but space will not permit me to quote any considerable portion of the passages I refer to here.) Again, in the same Report (p. 221): 'The Astronomer Royal argues that the future astronomical public will not be satisfied unless all practical use' (probably a mis-report for 'practicable use') 'is made of the transits of Venus in 1874 and 1882, and that for these the determination of some distant longitudes, and a reconnaissance of Wilkes's Land, must be effected within a few years.'

The next remarks of the Astronomer Royal on this subject appeared in the 'Monthly Notices' for June 1864, pp. 173–177. In this paper, after considering the circumstances of the transit of 1882 (leaving that of 1874 unmentioned), he proceeds to say: 'On the whole, I think it very desirable that a reconnaissance should be made of the points under consideration, and that it should not be long deferred. The first locality to be examined is that in 7^h east longitude, between Sabrina Land and Repulse Bay; and the points to be ascertained are—(1) whether the

years before; and in the later discussion of the subject Sabrina Land was substituted for Wilkes's supposed continent!

coast is accessible in December 6; (2) whether a latitude of 65° can be reached; (3) whether the sun can be observed' (under certain conditions which affect the problem unfavourably in 1882, but have no existence in 1874). 'Should the answer to the first or third questions be negative, then it would be proper to examine other portions of the South Continent, say in longitude not very different from 4^h west, but with no particular restriction except that of gaining the highest possible south latitude.'

The next reference to the subject appeared in the 'Monthly Notices' for May 1865, pp. 201–203. The paper bears the title, 'Letter from the Astronomer Royal to Sir R. I. Murchison, K.C.B., President of the Royal Geographical Society.' It runs thus:—

'I have learned, through the public papers, the tenor of late discussions at the Royal Geographical Society in reference to a proposal for an expedition towards the North Pole. I gather from these that the object proposed, as bearing on science, is not so much specific as general; that there is no single point of very great importance to be obtained, but a number of co-ordinate objects whose aggregate would be valuable. And I conclude that the field is still open for another proposal, which would give opportunity for the determination of various results, corresponding in kind and in importance to those of the proposed Northern Expedition, though in a different locality, and would also give information on a point of great importance to astronomy, which must be sought within a few years, and which it is desirable to obtain as early as possible.

'In the year 1882, on December 6, a transit of Venus over the sun's disc will occur; the most favourable of all phenomena for solution of the noble problem of determining the sun's distance from the earth, provided that proper stations for the observation can be found. (It will be remembered that it was for the same purpose that the most celebrated of all the British scientific expeditions, namely, that of Captain Cook to Otaheite in 1769, was undertaken. The British part of the enterprise was perfectly successful; but there have always been doubts of the accuracy of the corresponding observations in Lapland, which render a repetition of the observation very desirable.) In the "Monthly Notices of the Royal Astronomical Society" for June 10, 1864, I have very carefully discussed the circumstances of the coming transit, in reference to the selection of observation-stations. For the northern stations there will be no difficulty; they will be on the Atlantic seaboard of North America, or at Bermuda: all very favourable and very accessible. For the southern stations the selection is not so easy; the observation must be made on the Antarctic Continent; if proper localities can be found there, and if the circumstances of weather, &c., are favourable, the determination will be excellent; if those favourable circumstances do not hold, no use whatever can be made of the transit.'

Then follow certain sentences from the cited

'Monthly Notices,' bearing on the selection of southern stations, and including the passages which I have quoted above. The Astronomer Royal proceeds as follows:—

'The astronomical object of a southern expedition is, I trust, sufficiently explained in the sentences which I have quoted. In the event of such an expedition being undertaken, the precise determinations which I have indicated as bearing on the astronomical question must (from the nature of the case) take precedence of all others. But there would be no difficulty in combining with them any other inquiries, of geography, geology, hydrography, magnetism, meteorology, natural history, or any other subject for which the localities are suitable.

'And I have now to request that you will have the kindness to communicate these remarks to the Royal Geographical Society, and to take the sense of the Society on the question, whether it is not desirable, if other scientific bodies should co-operate, that a representation be made by the Royal Geographical Society to Her Majesty's Government on the advantage of making such a reconnaissance of the Southern Continent as I have proposed; primarily in the interest of astronomy (referring to my official responsibility for the importance of the examination at this special time); but conjointly with that, in the interests, perhaps ultimately more important, of geography and other sciences usually promoted by the Royal Geographical Society.'

In December 1868, notwithstanding the relatively unfavourable circumstances for applying this (Halley's) method to the transit of 1882, and the very favourable conditions under which Delisle's method can then be applied, the Astronomer Royal urged that only three stations should be occupied for Delisle's method in that year, the instruments of the five 1874 expeditions, 'thus set free from two stations,' being required at an observing station on the Southern Continent. He had now so far changed his mind as to the method of dealing with Antarctic difficulties, as to speak in the following terms: 'The choice of station being made,' he said, 'I would not recommend any reconnaissance, but I would propose that an expedition should go direct to the selected point in good time for the observation of the phenomenon. The season is early for South Polar expeditions, and any difficulties produced by ice would probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is a few days' observation to give clock-rate; then the clock times of the two phenomena will furnish all that is required. The first action to be undertaken by the Government,' he proceeds (and I invite special attention to the point), 'is to procure the stock of instruments, and this ought to be done without delay. An observing plant like that' (described in the earlier part of the same paper) 'is not to be obtained in haste, and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships and the selection of officers would probably require much less time.'

It appeared to the naval authorities who followed the Astronomer Royal in addressing the meeting, that the more certain course for achieving the desired result would consist in the preparation of an expedition to winter in Possession Island. I quote the following passages as bearing specially on the feasibility of such an expedition:—

Admiral (then Captain) Richards, Hydrographer to the Admiralty, said: 'My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that landing a party on Possession Island,' or one of the islands farther south, 'would be the most feasible course, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations. Doubtless a year passed in this region would be most profitably employed in adding to our knowledge of magnetism, and various other branches of physical science.'

Admiral Ommanney said, *inter alia*: 'I fully concur in all that has fallen from the Hydrographer to the Navy, and hope ere long to hear that operations are making for sending out to explore the Antarctic Seas.'

Commander J. A. Davis, who had accompanied Sir James Ross in that most gallant expedition during which Victoria Land was discovered, and who had himself landed at Possession Island, said that 'he believed there would be no difficulty whatever in again effecting a landing in the same place.' 'With regard to the period of the season at which the transit took place, it was to be remembered that the 6th of December was so early that no ships had ever reached the Antarctic Circle by that date; and as it would be necessary to arrange the instruments, &c. preparatory to the observation, he might say that the ships ought to be on the spot at least a month before. This would be the 6th of November, a date altogether out of the question; and as the ships could not winter in the South, the party would necessarily have to land the year before; but with good tents he had no doubt they could pass the winter very comfortably' (this, of course, and what follows, will not be taken strictly *au pied de la lettre*): 'they would have a pleasant prospect before them and plenty of penguins to live on. In comparison with Kerguelen Island and the Crozets,' he proceeded, 'the chances of observing the transit—meteorologically speaking—would be greatly in favour of South Victoria.'

Captain Toynbee also expressed an opinion strongly adverse to the meteorological chances at Prince Edward's Islands, the Crozets, and Kerguelen Land, since their neighbourhood is, he said, 'so far as my experience goes, subject to a great deal of thick weather.'

There were several points in the Astronomer Royal's communication to the Astronomical Society on this occasion which were calculated to attract the attention of those who had followed his former proceedings in connection with the transits of 1874 and 1882. Thus far he alone of all the leading astronomers had publicly dealt with the subject, and there was much in the tone of his preceding papers to suggest that in a sense he guaranteed a sufficient examination of the conditions of the transit to enable astronomers generally, not those of England alone, to await his announcement of what the different scientific nations might be expected to do, and to follow his instructions whensoever such announcement should be made. In the paper of December 1868 he still adopted this tone, while nevertheless it was apparent that he was not treating the subject in an exact manner. For instance, the statement in the very beginning of his paper that the method of duration had been *shown* to 'fail totally,' even if correct in itself—which subsequent examination showed not to be the case—was not in accordance with former papers, in which he had only expressed his opinion that *in all likelihood* it would not be advantageously applicable. Then secondly, the maps accompanying the paper were of the roughest possible description, insomuch that the shapes of the continents and oceans were barely recognisable: nor did these maps extend beyond the parts immediately adjacent to the points corresponding with I, I′, E, and E′ in Plates VI. and VII.; so that if by any possibility (which seemed at that time, however, incredible) Halley's method should be available in 1874, the maps could not have shown the fact, though this was precisely the sort of when he said that the difference of durations in the transit of 1874 would '*probably not be half of that in* 1882.' So that I confess it was with some surprise that I read a letter to the Astronomical Society (almost contemporaneous with the one addressed to myself), in which he stated that in 1857 he had 'fully considered' the application of Halley's method to the transit of 1874. An inference in 1857 that the difference of durations in 1874 *would probably not be half that in* 1882, and a belief in February 1869 that the difference in 1874 *may probably be greater than in* 1882, did not seem to me then, and does not seem to me now, to correspond with a 'full consideration' of the method depending on this difference of duration. The letter to the Astronomical Society just referred to is noteworthy, however, as mentioning a criterion which at that time Sir G. Airy adopted in comparing Delisle's and Halley's methods: 'I hope,' he says, 'the probable error of geometrical longitude *will not be more than one-half* of the probable error of ingress or egress.'[1]

[1] Subsequently when it became necessary to make out that Delisle's method was nearly equal in value to Halley's, Sir G. Airy and Mr. Stone insisted that the probable error of geographical longitude would be *less than one fourth* of the probable error of an observation of contact. This was done by reducing the estimate of the probable error in longitude determinations. All the time, the probable error of an observation of ingress or egress was inferred from the results of 1769, without any account being taken of the probable diminution of this error in consequence of the inquiries and experiments which have been made into its cause and nature. This is idle. If those inquiries and experiments are valueless, why should so much be said of them in the Greenwich Reports? If they are valuable, as we all hope and believe, why is the error which they are intended to reduce, treated as though it would probably be as great as ever?

Having now ascertained that the subject had not as yet been thoroughly dealt with, I resumed the investigation, and in May 1869 I submitted to the Astronomical Society a paper accompanied by six projections (from two of which Plates XVII. and XVIII. have been reduced by photolithography) illustrating the transit of 1874. This paper was published in the June number of the 'Monthly Notices.' I had by this time, to my regret, learned that my inquiries into the subject were distasteful to the Astronomer Royal, and therefore I avoided all mention even of his name in this paper; and where it was necessary to call due attention to the changed values of the various stations, I presented these in a tabular form,—Airy's values under head A, those of Puiseux under head B, and my own, which closely accorded in the main with those of Puiseux, under head C.

As some have supposed that I wished solely to substitute Halley's method for Delisle's (a change which would have been of little value), I deem it well to quote here from my paper of May 1869, the following summary of the conclusions therein demonstrated:—

1. '*The application of Delisle's method of absolute time differences.* The relative as well as the absolute values of many stations are affected. Some which had hitherto appeared unsuitable are found to be unobjectionable. Others which seemed good appear unfit. In other cases the relative values of two stations are so affected that the results of a comparison between them are directly reversed. Lastly, many stations

not hitherto thought of in connection with the transit are found to be well suited for the application of Delisle's method.

2. '*The comparison between Delisle's and Halley's methods.* Halley's method' (estimated by Sir G. Airy's own test) 'is found not merely to be applicable with advantage, which is all that can be said of it when central passages are considered, but to be superior to Delisle's—slightly, when reference is made only to such stations as had been hitherto dealt with, noticeably when Antarctic stations are made use of.

3. '*The comparison between the transits of* 1874 *and* 1882 *with reference to Halley's method.* This comparison shows that Halley's method may be applied much more advantageously to the transit of 1874 than to that of 1882.'[1]

The tables (which are given in abstract at the end of this volume) were followed by these remarks:—

'It will be seen, on a comparison of tables A, B, and C, that the effects of the change of phase are in some cases important. The coefficients of parallax are affected in several instances by more than 0·1 and in two cases by 0·22. In the cases of Crozet Island (Table II.) and Chatham Island (Table III.) solar elevations are so improved, that these stations, which would have to be rejected if central passage were con-

[1] Chapter IV. presents in pp. 128 *et seq.* an abstract of the reasoning by which the applicability of Halley's method in 1874 was demonstrated, and also indicates the places where the method can be most suitably applied. But the tables at the end of the book should be consulted, especially Table V.

sidered, are shown to be well suited for the observation of internal contacts. The diminution of all the coefficients in Table III., through the change of phase, has an important influence on the value of Delisle's method, so far as egress observations are concerned. It is important to notice, also, that under the heads C in Tables III. and IV. many stations not hitherto recognised as available are included among the best places for observing egress. The Indian stations in Table IV. seem too valuable to be neglected.[1] Peshawur is better even than Alexandria; Delhi is not inferior to the latter station (when solar elevation is considered as well as coefficient of parallax). Bombay, Calcutta, and Madras are also excellent. It may be noticed also that Bombay and Madras, which, when considered with reference to central passage, had seemed suitable places for the observation of retarded ingress, are found to have so poor a coefficient of parallax when reference is made to internal contacts, that it would seem useless to observe ingress there (so far at least as the application of Delisle's method is concerned).

'Of course, it will be impracticable for this country to send observers to more than a certain number of stations. But it is not unlikely that besides Russia, France, and England (the only countries specially concerned in the transit of 1874), other nations may

[1] Several times during the past year the mistake has been made of stating that I originally advocated North Indian stations for applying the photographic method. The above passage, written before this method had been thought of, contains my first reference to those stations.

care to take part in the solution of the noble problem of determining the sun's distance; and thus it seems advisable that all the stations where there will be any chance of obtaining useful observations, should be tabulated as nearly as possible according to their relative values.'

A discussion followed in which an attempt was made to show that Delisle's method was equal in value to Halley's. Even if this could have been proved, it would have been little to the purpose, since the question was not whether Halley's method was more or less favourably applicable than Delisle's, but whether it was applicable at all. This discussion was carried on in public. A private correspondence arose out of a letter which I addressed to Sir G. Airy, assuring him that my sole wish was to assist in securing what every astronomer agreed was desirable—the best possible utilisation of the opportunities available in 1874 and 1882. Sir G. Airy wrote me a letter, forwarding a copy to Admiral Manners, then President of the Astronomical Society, in which he complained that his paper of December 1868 had been treated as though it claimed to be an exact discussion of the conditions, due allowance not being made for his own statement that it was but a preliminary and comparatively rough investigation of the problem. This led me to believe (mistakenly, as afterwards appeared) that I had taken up the subject too hastily; for Sir G. Airy seemed not merely to promise a thorough analysis of the whole subject, but to imply that this was what had been intended all along, the suggestions made in December 1868 being merely provisional, and the actual arrangements to be proposed to Government depending on the promised thorough investigation.

Nothing could have been more satisfactory than the course thus indicated; and, accordingly, from that time (the summer of 1869) until the summer of 1872, I addressed no communication whatever on the subject of the controversy,[1] either to Sir G. Airy personally or to the Astronomical Society. Nothing, however, was done in this interval except to carry out the arrangements proposed in 1868. Accordingly, in 1872 I wrote to Sir G. Airy, recalling his attention to the promised investigation of the subject. I then learned for the first time that the old arrangements were still adhered to. On this, I made such protest as a student of astronomy, independent of official trammels, might properly (in my judgment then and now) address to the official astronomer to whom the charge of the matter had been left in accordance with ancient custom.

After this protest I allowed yet half a year more to elapse, and then, nothing having been done, it seemed time to take more earnest measures.[2]

[1] A paper of mine appeared in the 'Monthly Notices' for January 1870, in which the application of photography to the observation of the transit was dealt with; but this paper bore no reference to the questions which had been raised by me in 1869.

[2] In the interval events had taken place within the Astronomical Society to which I see Sir Edmund Beckett has thought it desirable to refer in the latest edition of his 'Astronomy without Mathematics.' Those events had no real importance, however, except for the *animus* shown.

I accordingly resumed the discussion in the 'Spectator' for February 8. By a singular coincidence a powerful paper appeared in the 'Times' of February 13 supporting the views which I had advanced. This paper was commonly, and I believe correctly, attributed to Sir Edmund Beckett. (In his 'Astronomy without Mathematics' he mentions the rumour without contradicting it.) And many believed that the coincidence was not accidental—that is, that the nearly simultaneous appearance of the two papers had been planned beforehand. But this was not the case. Neither had Sir E. Beckett any prior knowledge of my intention, nor had I of his.

What followed strikingly illustrated at once the power of the press and the unwillingness of the official mind to move from a position once taken up. Aware of the latter peculiarity, I thought it desirable to bring my proposals forward in such a way that some point might be flatly refused, in order that essential matters might be yielded. It was not difficult to effect this. There was very little prospect at that late date that Antarctic stations would be occupied for the transit of 1874. Enderby Land and Possession Island, near South Victoria Land, were, geometrically, among the best southern stations for observing the transit; and if all had been trustworthy that was said of the last-named station by Admiralty officers in December 1868, this station would have been not only geometrically excellent but meteorologically preferable to most of the southern stations already provided for. It required, indeed, that a party should winter there; and I had pointed out two years before, in 'The Sun,' the fact (which Commodore Davis seemed to have forgotten) that when Ross landed there in 1847, the sailors were quickly compelled to retire by the abominable smell proceeding from the accumulations of guano. The proposal of an Antarctic station was therefore an excellent one to be refused, if, by allowing the official mind that luxury, concession might be obtained on other and more important points.

Matters befell as I anticipated as respects the refusal, though concessions were not at first made very willingly. 'On a review of the whole case,' said the Astronomer Royal in his reply, 'I decline to recommend that an expedition be sent to any station on the Antarctic continent.'

Per contra, no concession was made except the admission that North India ought to be occupied by a photographic party (not as a Delislean and Halleyan station). The use of Halley's method was opposed absolutely, on the ground (i.) that the Russians would probably not occupy Nertschinsk, the station in Siberia (marked 6 in Plates XII. and XIII.) which I had specially recommended, (ii.) that Puiseux had probably abandoned his ideas respecting the use of Halley's method (which ideas, said Airy, 'have not again been promulgated on the Continent'), and (iii.) that no other nations would care to provide for northern Halleyan stations. A few days later, Mr. Goschen, then Secretary of the Admiralty, said that even at those stations already provided for, where, as I had shown, longitudes could be readily noted, they *would* indeed be noted, but little reliance would be placed on them.[1]

An unfortunate *contretemps* diminished the effect of this official humility. Within a few days from Sir G. Airy's declaring his belief that other nations would not apply Halley's method—nay, that probably not even a solitary northern Halleyan station at Nertschinsk would be occupied—came news *that Russia proposed to occupy not only Nertschinsk, but ten other Halleyan stations in Siberia*; that America proposed to occupy three other northern Halleyan stations, Germany two others: and before long it was announced that France would occupy two other northern Halleyan stations.

But it was still possible that these energetic proceedings by other nations might be rendered useless by shortcomings on our part; for as yet no adequate provision had been made for southern Halleyan stations, and it was manifest that other nations were looking to England to take a large share in this part of the work. Eighteen northern Halleyan stations were provided for, and as yet only one first-class southern Halleyan station, Kerguelen's Land,—and that origi-

[1] Sir George Airy soon after wrote to me that there had been some misunderstanding here. I should conceive on the whole that there *had* been. It may perhaps be easily understood that Mr. Goschen, who, of course, had no technical familiarity with the subject, might have misapprehended some statement addressed to him by Sir G. Airy.

nally named without the least idea that it was a Halleyan station at all.

The time had come for very plain speaking. Be it noted that if Delisle's method succeeded at each of the selected stations, then the transit would yield very good results; but even then not so good as though Halley's method were extensively applied in addition. For every method successfully applied, and indeed every observation, reduces the probable error in the final result. But there was at this stage a risk that the operations would fail altogether.[1] If, as was and

[1] Very strangely, what I said in May and June 1873, about a great risk of failure, has been regarded as altogether inconsistent with what I have said during the present year about a great chance of success. Mr. Christie, (described in the 'Greenwich Reports' as 'the Astronomer Royal's confidential representative,') gravely took me to task in the 'Academy' for this alleged inconsistency; and even in an exceedingly fair-spirited review in the 'London Quarterly,' my fears in 1873 are thus referred to:—'This we think is where Mr. Proctor has erred; for he subsequently expresses at least comparative satisfaction with the final arrangements.' I will now give the very words in which in June 1873 I described the risk of failure in 'Fraser's Magazine':—'Let the following startling facts be noted in conclusion. If there is bad weather *either* in the Sandwich Isles on one side, or at the Mauritius group and Kerguelen Island on the other, Delisle's method applied to the beginning of the transit will fail totally. If there is bad weather *either* in the New Zealand Islands, or at the opposite northern stations, Delisle's method applied to the end of the transit will fail totally. There would remain, then, only the chances depending on the three methods which require that the whole transit should be seen. For these methods ample provision has been made in the northern hemisphere, by Russia, Germany, and America; so much so that England's neglect as regards her North Indian stations becomes of relatively small importance. *But*, in the southern hemisphere, Kerguelen Island is the only really well-placed station to be occupied for applying these methods, and at Kerguelen Island fine weather occurs on about one day in ten. There remain the Macdonald Islands, suggested (only) for occupation by

is possible, Delisle's method were to be frustrated by bad weather at the Sandwich Islands *or* at Kerguelen Island and neighbouring stations, then ingress observations would fail. If egress observations were to be frustrated by bad weather at New Zealand and neighbour-

Germany, but unlikely to be occupied except by a specially nautical nation. Yet the whole space between Kerguelen Island, Enderby Land, Possession Island, and Auckland Island, is suitable for the three methods (and also, be it noted as important, for Delisle's method). There are several islands scattered over this region, and probably many others which have not yet been discovered. It is most unfortunate that nothing has been done, during the four years which have passed since I noted these facts, to make reconnaissances over the whole of this region; but surely it will be even more unfortunate if no station is occupied in it. Of the duty of Great Britain in this matter I have spoken earnestly, because I feel warmly. Viewing the matter as an Englishman, I may say that I should feel concerned if this duty, neglected thus far by us, should be undertaken by America, the country to which, next after us, the duty belongs. But viewing the matter as a student of science, my great wish is to see due advantage taken of the great opportunity afforded by the approaching transit, without specially caring whether this country or another obtain more honour in accomplishing the task.'

Now if this warning be viewed in connection with the fact that instead of one first-class southern Halleyan station there will now be four, besides extra stations on Kerguelen Island (forty or fifty miles apart) and many new second-class stations, I think satisfaction now will be found to be altogether consistent with dissatisfaction in May and June 1873. For my own part, however, I am satisfied, rather as a student of science than as an Englishman, for of all the four really unpleasant stations in the Southern Seas, England occupies the one which is most conveniently accessible. But I was not careful to dwell on this point, nor should I do so now, were I not compelled by a charge of inconsistency, as singular as the dissatisfaction with which my satisfaction has been viewed in certain quarters. If I am asked to admit that my suggestions were very slowly adopted, and that even now we ought not to be altogether satisfied, I am ready to do so. General satisfaction at the removal of the great risk of failure is not inconsistent with the feeling that much more might have been done by England, and that since America *must* occupy the first place in the transit of 1882, we need not have suffered her to be so completely ahead of us in the transit of 1874.

ing stations, *or* at the opposite region, then egress observations would fail. In either case very imperfect results would be attained; but if both events were to happen, then no result at all would be achieved. Now there were eighteen northern Halleyan stations admirably suited to supply a third chance of success by Halley's method if they were but properly balanced in the southern hemisphere, but otherwise valueless. For although, besides being Halleyan, they were also excellent as subsidiary Delislean stations (each having a double chance, because either the beginning or the end would serve for that method), yet the multiplication of northern Delislean stations could not remove the chances of failure on account of the fewness of southern stations. In either respect, whether to balance the northern Halleyan stations as such, or to give new Delislean chances, nothing was at that time promised. Apart from all question of the choice of methods, there was no suitable provision for southern observation. Kerguelen's Land was the only first-class Halleyan southern station yet provided for, and none of the other southern stations could be regarded as high even in the second class. These others—Canterbury Island, Auckland Island, Mauritius, and Rodriguez, stood fairly well in the second class, and that was all that could be said. The only good station, Kerguelen's Land, was one at which all the meteorologists had said that bad weather was far more probable than fair weather. I had already pointed out, as geometrically suitable, Kemp Island, Crozet Island, Macdonald Island, the group of islands of which Campbell Island is the chief, St. Paul's Island, and several others of less value, yet well worth occupying if geographically suitable. But Admiral Richards, without mentioning these islands by name, had in the 'Times' described me as recommending the occupation of places which were 'little better than geographical myths.'

It began to appear as though, after all, nothing would be effected until too late, so that perhaps some time about the year 1877 or 1878 astronomers would be lamenting that the favourable opportunities presented by the transit of 1874 had not been properly utilised.

But at this critical stage a new force appeared on the field, and compelled the Admiralty to retreat from the position they had so bravely defended. The Board of Visitors at Greenwich met on June 7, 1873, and it was there proposed, by Professor Adams, and carried unanimously, that Professor Cayley, who in his capacity as President of the Astronomical Society was Chairman of the Board, should apply to Government 'for the means of organising parties of observers in the Southern Seas, with the view of finding additional localities in the sub-Antarctic regions for observing durations'—that is, for applying Halley's method.

Of course, no one expected that this application would be acceded to in full. The course pursued by Government had thus far been the usual one in cases where it is feared that a change of plan may be construed into an admission of error. Sir G. Airy had been asked to inform the Secretary to the Admiralty whether the Astronomer Royal's arrangements were well-considered; and Sir George Airy had replied indicating his entire approval of the Astronomer Royal's arrangements. Admiral Richards had expressed the opinion that the Hydrographer to the Admiralty was quite right in regarding as 'geographical myths' in 1873 places which in 1868 Admiral Richards had thought 'easily accessible.' 'Moreover' the suggestions made by Sir G. Airy and Admiral Richards in 1868 'had not been brought before the Admiralty in an official form'—that is, were not the suggestions of the Astronomer Royal and the Hydrographer to the Admiralty. It is perhaps open to question whether this way of inquiring into the matter was the one best calculated to elicit just opinions or to lead to satisfactory results.

Yet the Government could not remain altogether idle, when the leading astronomers of England requested that something might be done (for even Sir George Airy, in yet a third character, supported Prof. Adams' proposal). Fortunately, the 'Challenger' was voyaging in the Southern Seas, and could be employed to reconnoitre Kerguelen Land (a part of her original programme), and to get a distant view of Macdonald Island. The result, presented in combination with a storm or so, has been held to establish conclusively the inaccessibility of those 'geographical myths,' which Cook, Ross, and other Antarctic voyagers (some of them still living) perversely insisted on visiting.

But other nations have not been deterred by the dangers and difficulties which unquestionably have to be encountered in voyages to sub-Antarctic stations. America, inquiring among sealing captains, found that the Crozet Islands could be occupied, and determined to send an observing party to that first-class Halleyan station, as well as to occupy second-rate Halleyan stations in Tasmania, New Zealand, and Chatham Island. The French Government decided to occupy Campbell Island and St. Paul's Island (those inaccessible 'geographical myths'), besides a second-rate southern station at Caledonia Island. Thus already the first-class Halleyan stations had been quadrupled in number, and the second-class largely strengthened. Germany and America both proposed to occupy Macdonald Island, if possible, but found that this island (sometimes called Heard Island) really is almost, if not quite, inaccessible, though an attempt will be made to land a party there. Our own country sends a second observing party to Kerguelen Island (to be stationed fifty miles from the other), with instructions to occupy Macdonald Island, if possible.[1] If not, there will be four parties in different parts of Kerguelen Land; and as the island is large, and the different parties will employ, besides Delisle's and Halley's method, two different modes of photographing the transit, we may

[1] So the chief of the two English Kerguelen parties informed me. What I learned when in America from Mr. Chappell, the head of the sealing merchants in those seas, does not encourage me to believe that the attempt will succeed. I hope it will be made, however.

consider that the Kerguelen stations will be equivalent to at least two separate chances of success. Adding the first-class Halleyan stations, Crozet, Campbell, and St. Paul, and the second-class stations, Bourbon, Rodriguez, Mauritius, Hobart Town, Melbourne, Sydney, &c., as well as the first-class Delislean stations in the South, and remembering that all the southern stations are good Delislean stations, while several of the added Halleyan stations are doubly good Delislean stations because serving both for ingress and for egress, it must be admitted that the risk of failure to which I pointed in May 1873 has been as completely removed as circumstances allowed. It would be no exaggeration to say that *the chances of success have been quadrupled since then.*

I have omitted to mention (in fact, the change was made so quietly that I do not know where to place it) that the North Indian station Roorkee, which had been first described as only to be used for photography, is now provided for as a Delislean and Halleyan station.

About this time it was discovered:—

First, that when Sir G. Airy said the method of durations fails totally in 1874, he meant Halley's method as it would have been used if the transit of 1761 had been nearly central.[1] Secondly, that from the beginning he always meant to apply Halley's method in 1874, wherever it could be applied.[2] Thirdly,[3] that

[1] See 'Nature' for June 1874. Compare note, p. 162.

[2] See 'Naval Magazine' for October 1874.

[3] See 'Academy' for August 1874.

Halley's method is not *really* to be applied by English observers, since they are not instructed to *observe the duration* of transit, but to *observe the time when transit begins and ends.*

Majora canamus.

In the meantime, a new and most important auxiliary method of observing the transit—the photographic method—which may hereafter, even if not in the case of this year's transit, become the most trustworthy of all, had been suggested by Dr. De la Rue,—or rather, it may perhaps more fairly be said, that this method, first suggested by Faye, had been exhibited in a practicable form by De la Rue.

In December 1868 Dr. De la Rue read a paper before the Astronomical Society containing the following remarks, *inter alia*:—

'The conditions which transits of Venus offer for the determination of the relative position of the sun's and planet's centres are more advantageous than those presented by solar eclipses, inasmuch that it is far more easy to measure directly the distances between the centre of the disc of the sun and that of the image of the planet upon it, than it is to measure the distances between the peripheries of the sun and moon,[1] or the angular opening of the cusps[2] of the partially eclipsed sun. And in transits of Venus any error of observation would not affect the final result nearly so much as in solar eclipses; for example, in the transits

[1] *Phil. Trans.*, p. 383, Table I. [2] Ib. p. 55, Table III.

of 1874 and 1882, an error of 1″ in the measurement would, for the maximum displacement, give an error of only 0″·185 in the deduced solar parallax.

'Moreover, it may be observed that in photographic records it is by no means important to catch exactly the phases of contact, as two photographs obtained at a sufficient interval afford the means of calculating to a great degree of refinement, and of tracing, the path of the planet, which, for the conditions of the problem, may be considered to be a straight line between the two positions recorded.

'Nor is it in any way essential, as it is with eye-observations, that favourable conditions should exist for retarding the period of contact at one station and accelerating it at another, because the chords representing the planet's path can be derived from photographic records with as much accuracy under what would be considered unfavourable conditions as under favourable conditions for eye-observations, for the length of the chords need not be directly considered in determining the nearest approach of the sun's and planet's centres.

'During the duration of the transit, it would be possible, in a clear state of the atmosphere, to obtain a series of photographs at intervals of two or three minutes, and any or all of these would be available for comparison with the records obtained at all the stations selected.

'The epoch of each photographic record is determinable with the utmost accuracy: 1st, because the time of exposure is not more than the $\frac{1}{50}$th or the $\frac{1}{100}$th of a second; and 2nd, because the instantaneous slide, as it flashes before the secondary lens, affords an audible signal[1] by striking against a stop a small fraction of a second after it has shut off the image of the sun. This interval might be determined by experiment and taken into account.

'In the Kew Photoheliograph the solar disc would, at the epoch of the transit of 1874, have a semi-diameter of 1965·8-thousandths of an inch (a diameter of nearly four inches), Venus a semi-diameter of 63·33 of these units, and the parallax of Venus referred to the sun would be represented by 47·85 of these units; the maximum possible displacement being 95·7 units, or nearly $\frac{1}{10}$th of an inch. In 1882 the sun's semi-diameter would be 1964·9 units; that of Venus 63·31 units; the parallax of Venus referred to the sun 47·82 units; the maximum possible displacement 95·6 units.

'When the photographs have been secured, the measurements by means of the micrometer, which would have to be performed, consist in determinations of the sun's semi-diameter, in units of the arbitrary scale of thousandths of an inch, the angle of position of' different positions of the centre of Venus, and the corresponding distances of her centre from the centre of the sun. 'The measurements by means of the micrometer described in the "Phil. Trans." 1862, pp. 373–374, can be obtained to the $\frac{1}{2000}$th of an inch

[1] *Phil. Trans.*, p. 364.

(0″·25), and the position angles to one or two seconds of arc. For each photograph measurements made at different times are remarkably accordant; the greatest difference between the semi-diameter of the sun of the several eclipse pictures of 1860 was $\frac{9}{1000}$ths of an inch, or about 4″·5; but, on taking the mean of measurements of forty-five photographs by two different methods, the difference was only $\frac{1\cdot5}{1000}$ths, or about 0″·75. I am inclined to believe that the distance could be ascertained to within 1″ by means of a few pictures, and possibly to 0″·25, if a sufficient number of photographs were obtained.

'Fears have been expressed that the collodion, in drying, becomes distorted; experiments, however, in 1860–61 have demonstrated that the shrinkage is only in the direction of the thickness. But as, in the case of the solar parallax, no refinement of correction ought to be neglected, it would be quite easy to ascertain once more whether any distortion does take place, by taking photographs on glass plates on which lines about a quarter of an inch distant had been previously etched; the collodion, which should be rendered purposely contaminated with particles in suspension, should be poured on the ruled sides to avoid parallax. After all the operations of photography, the film would have to be examined from the back, and the position of certain impurities with reference to the ruled lines noted whilst the collodion was wet, and after it had dried.

'No difficulties exist in photographing a transit of

Venus; the operations are quite the same as those practised daily at the Kew Observatory; no strain on the nerves would occur, as in the anxiety consequent on the desire of rendering available every moment of the short duration of a solar eclipse. All the operations could be conducted with that calm so essential for such a problem as the determination of the solar parallax, and I feel confidence in recommending that timely steps should be taken to secure photographic records of the transits of Venus in 1874 and 1882.'

Three months later, Col. Tennant wrote a paper commenting on the practical details of Dr. De la Rue's plan, in the course of which he pointed out that 'if an observer at a separate telescope had a break circuit key he could at any moment photographically record a phenomenon he saw and the instant of its occurrence. An assistant at the heliograph would be needed to replace the shutter and insert the dark slides, unless the observations are far apart. By using a repeating slide and combining the measurements on one plate, very valuable normal relations of the sun and planet would be found, and observations might be made during the ingress or egress (possibly, too, of the black drop) in considerable numbers. There is an advantage in this mode of observing the transit of Venus to which Dr. De la Rue has not alluded.[1] If

[1] 'Dr. De la Rue has pointed out,' says Col. Tennant, 'the facility with which the nearest approach can be got from the photographs at any one station, but if photographs be taken at two stations at a time when Venus is in the plane which includes them both as well as the earth's centre, these will show the whole effect of parallax; and it is

accurate micrometrical observations can be made by means of photographic pictures, then the range of suitable stations can be greatly enlarged; for any two stations, 140° or 150° apart, can have the observations combined by choosing a suitable time, and, of course, by means of equations of condition, the observations of stations in all sorts of places could be used with their proper weight. All the photographs could be measured by the same micrometer, whose errors of all sorts could be very carefully examined.'

To this Dr. De la Rue replied as follows:—

'With respect to the localities to be selected, the employment of the photographic method of observing transits of Venus has, as I have already stated, the advantage of rendering us independent of conditions favourable or indeed essential for eye-observations; for a few photographs obtained at each station would afford the means of ascertaining the path of the planet and its position at any given moment; and hence the proposal to determine the position of the planet's centre in relation to the sun's centre really includes the particular case contemplated by Major Tennant in the foot-note appended to his paper; but it is possible that the sun might be obscured at the critical time, at one of the two stations selected.'

In December 1869 I read before the Astronomical Society a paper, from which the following passages are extracts:—

the positions at these instants, and not the nearest approaches to the sun's centre, which should be compared.'

'It is impossible to read Dr. De la Rüe's account of the results of careful measurement applied to photographs of the solar eclipses in 1860 and 1868 without recognising that we have in photography, as applied to the approaching transit of Venus, one of the most powerful available means of determining the sun's distance. Within the last few years, solar photography has made a progress which is very promising in regard to the future achievements of the science as an aid to exact astronomy. So that doubtless, in 1874, astronomers will apply photographic methods to the transits of that year with even greater success than we should now be prepared to anticipate. It has therefore seemed to me that the photographic observation of the coming transit merits at least as full a preliminary inquiry as either Halley's or Delisle's method of direct observation.

'The result of an inquiry directed to this end has led me to the conclusion that photographers of the approaching transit should adopt for their guidance considerations somewhat different from those which have hitherto been chiefly attended to.

'It is undoubtedly true, as Dr. De la Rue has pointed out, that the photographer of the transit can readily take a large number of pictures, and by combining these, can ascertain with great accuracy the path of Venus across the solar disc. And by comparing the paths thus deduced for different stations a satisfactory estimate can be formed of the solar parallax.

I do not wish to suggest any departure from this course of procedure.

'On the other hand, it is undoubtedly true, as Major Tennant has remarked, that the greatest effect of parallax will be obtained, for any two stations, when both stations—the earth's centre and the centre of Venus—are in one and the same plane. So far as those two stations are concerned, his remark is just, that it is the position of Venus at the instant when the stations are so situated, and not the nearest approach of Venus to the sun's centre, which should be compared. And further, Dr. De la Rue's comment on this, to the effect that his method in reality includes Major Tennant's, is also correct. In fact, there can be no doubt that the position of Venus at the particular instant referred to by Major Tennant can be far more exactly ascertained by a reference to the complete path of Venus for each station than from any attempt to secure nearly simultaneous photographic records at stations far removed from each other.

'But it appears to me that the method I am about to suggest, according to which the whole question will be reduced to the determination of a parallactic displacement of Venus on a line through the centre of the sun's disc, is the one by which the fullest assistance will be obtained from photography; while a source of error, which has not hitherto been specially considered, will be practically eliminated.

'It must be remembered that in the comparison ot photographic records, whether for the determination of the path of Venus across the sun's disc at a particular station, or for the comparison either of Venus's apparent position or of her path as seen from two different stations, the accuracy of the results will depend in part on the certainty with which two or more pictures may be brought into comparison by means of a fiducial line or set of lines. It seems certain that no method can be devised by which all chance of error from this source can be eliminated. The great point would, therefore, seem to be to render its effect as small as possible.

'Now, let us consider for a moment Major Tennant's proposition, as giving a convenient illustration of the

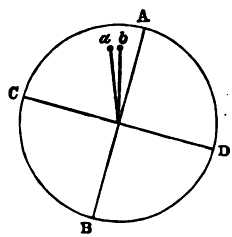

Fig. 42.—Illustrating the photographic and direct methods.

effects of any error either in the position of the fiducial lines, or in bringing those belonging to two pictures into exact correspondence. Let fig. 42 represent the result of a comparison between two photographs of the sun. A B and C D are fiducial cross-lines common to both pictures; *a* is the centre of Venus for one picture, *b* is her centre for the other; and on the exact measurement of *a b* depends the determination of the sun's parallax, so far at least as these two pictures are concerned. Now it is very obvious that if the lines A B, C D, for one picture, have not been brought into perfect correspondence with those belonging to the other, the distance *a b* will be correspondingly affected. In fact, it would appear that if the usual methods for making the correspondence as exact as possible are followed, almost as large an error would be introduced through this cause alone as by errors in the measurement of *a b*, since the two processes—the measurement of *a b* and the adjustment of the sets of cross-lines—depend on the very same circumstance, the nicety, namely, with which the eye and the judg-

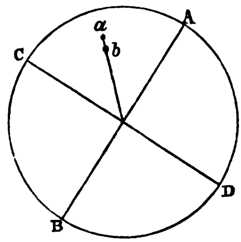

Fig. 43.—Illustrating the photographic and direct methods.

ment can estimate minute quantities of about the same relative dimensions.

'But now, if *a* and *b*, in place of having the position shown in fig. 42, were situated as in fig. 43, it is clear that the distance *a b* will not be appreciably affected by any small error in the adjustment of the fiducial lines.

'The object, therefore, which it seems most desirable to secure is that Venus, as seen from two different stations at a particular instant, should have a relative parallactic displacement towards the sun's centre, or as nearly towards the sun's centre as possible. This amounts to adding to Major Tennant's conditions this further one, that the sun's centre should be in the same plane with the two stations—or rather to making this condition a substitute for that one which requires that the earth's centre should be in the same plane with the two stations. For, as a rule, we must not expect to be able so to arrange matters that two convenient stations on the earth, as well as the centres of the earth, Venus, and the sun, should be in the same plane.

'Dr. De la Rue's remark, that by taking a series of pictures the position of Venus may be ascertained at any moment, is in reality quite as applicable to my suggestion as to Major Tennant's. In fact, were it not, we might despair of securing the desired object, since we have no reason for believing that astronomers are so certain as to the exact progress of the transit that the conditions could be secured by anticipatory instructions: whereas, by applying Dr. De la Rue's method, it will be possible, after the transit is past, to determine the position of Venus at the proper instant with any desired degree of accuracy. And further, it is very obvious that no error in the placing of the fiducial lines for pictures taken at the same stations can much affect the accuracy of the result, since the comparison of successive pictures, taken at the same station, does not directly involve the element of the solar parallax, as when we have to compare two pictures or paths determined at different stations.

'The object which Plate XVI. was originally intended to subserve was to determine what stations are most suitable for applying photography to the transit of 1874, on the principles above enunciated; though, as we have seen, the drawing presents also an instructive illustration of the whole character of the transit.'

In Chapter IV., in pp. 148–152, I showed how all the chief elements of the transit could be deduced by considering the motion of Venus relatively to a pair of cones, each enveloping the sun and the earth, but one having its vertex outside the earth, the other having its vertex between the earth and the sun. In the paper from which I have been quoting, after explaining the construction of Plate XVI., I proceeded as follows:—

'We have only to invert fig. 35, and look at it from behind, to see what sort of path Venus would seem to traverse upon the sun's disc, either with reference to the earth's centre, or to any point of the earth's surface supposed to be properly depicted upon the small discs 1–15 in fig. 35.

'It follows, therefore, that if we want to determine two stations at which at any instant Venus would appear to have a relative parallactic displacement towards the sun's centre, all that is required is that we select two stations which are on the same radial line from the common centre of the circular sections in fig. 36.

· The positions of those radial lines which cross the earth's track *c d* are exhibited in Plate XVI.

· Passing over pictures 1 and 2, we notice in picture 3 that Kerguelen's Land and Crozet Island, lying nearly on a line with certain of the Aleutian Islands, suggest that pictures taken at the former stations at the beginning of the transit could be advantageously compared with pictures simultaneously, or almost simultaneously, taken at a station on one of the easternmost of the Aleutians. In like manner pictures taken near Enderby Land could be advantageously compared with pictures taken at Woahoo. Projection 4 does not differ much from the preceding, but the cross-lines have assumed a less inclined position, and Kerguelen's Land could, at the epoch belonging to this picture, be better combined with a somewhat more westerly Aleutian island. Projection 5 exhibits the advantage of a photographic station at or near Yokohama. Probably such a station, combined with one in Crozet Island or Kerguelen's Land, would give (by pictures taken near the hour belonging to Projection 5) absolutely the best results which photography can give. The remaining projections suggest the following combinations of photographic records: Projection 6. Yokohama and Heard Island, Kerguelen's Land and a station in Manchooria; Crozet Island and Pekin; Cape of Good Hope and Nertschinsk.

Projection 7. Kerguelen's Land and Tsitsikar; Crozet Island and Nertschinsk; Cape Town and a station west of Lake Baikal. Projection 8. Kerguelen's Land and Nertschinsk; Cape Town and Peshawur, or Roorkee; Repulse Bay or neighbourhood and Yokohama; Perth (Australia) and Yokohama. Projection 9. Yokohama and Perth; Enderby Land and Nertschinsk; Crozet Island and Calcutta; Cape Town and Bombay. Projection 10. Kerguelen's Land and Calcutta; Crozet Island and Peshawur; Cape Town and Teheran. Projection 11. Kerguelen's Land and Madras; Crozet Island and Peshawur; Cape Town and Aden. Projection 12. Kerguelen's Land and Peshawur; Crozet Island and Teheran. Projection 13. A New Zealand station and Yokohama; Hobart Town and a station near the mouth of the Amoor.

'From this list we see that Kerguelen's Land and Crozet Island, Peshawur and other Indian stations, and stations in Siberia, are those which give the most favourable opportunities for the application of the photographic method.'

The considerations described in the preceding passage were those followed in the selection of stations.

So far as preliminary inquiries are concerned, it remains only to be mentioned that most of the nations proposing to take part in the observations have made experiments on the phenomena of contact by means of an artificial transit. Although the results obtained at Greenwich are not altogether so satisfactory as they might be, owing to the short distance from the ob-

[illegible] in which the [illegible] lines have been placed. [illegible] presented in the [illegible] marked peculiarities [illegible] when I studied the phenomena [illegible] —

1. [illegible] considerable experience for an observer [illegible] the definite changes of appearance [illegible]

2. When two observers describe a particular phase which they are well determined to observe this phase [illegible] by each are generally [illegible] within a fraction of a second.

3. The successive phases of an ingress or egress [illegible] each other sometimes rapidly, at [illegible] so that in some cases all the [illegible] within three seconds, in other [illegible] the same series of phases is completed in ten seconds.

4. The time at which any particular phase is observed [illegible] with the aperture of the telescope. When a telescope of good definition is used, [illegible] the time of any phase at ingress is earlier than with an instrument of less perfect definition.

[illegible] such as these must necessarily serve [illegible] observations, especially [illegible] measure the distances [illegible] process, but will cause [illegible] to be obtained photographically by an ingenious arrangement contrived by Janssen. I think it not at all unlikely that the probable error of a contact may thus be made at least as small as the probable error of longitude determination. In this case the value of Halley's method will be *largely* superior to that of Delisle.

I turn next to the consideration of the plans of the various nations taking part in the observation of the transit of next December. Before reading what follows, the student will find it well to examine Plates VI. and VII. successively, after the manner indicated at p. 48, for the transit of 1761, and at p. 70 for that of 1769. Plates XII., XIII., and XVI. should also be examined.

To America the pride of place must in fairness be conceded. She might reasonably have contented herself with but slight efforts on the occasion of this transit (because the transit of 1882 will fall pre-eminently to her share). But the American Government has voted a sum (30,000*l.*) twice as great as that which has been granted by the British Government[1] for the purpose. Then, as I have already mentioned, America has undertaken the most difficult of all the tasks which the proper observation of the transit rendered necessary—I mean the occupation of the Crozets. Moreover, the preliminary investigation of the conditions of the transit by American astronomers is altogether excellent.

[1] It must be noted, however, that our Government unhesitatingly granted all that the Astronomer Royal asked, so that it would be altogether unfair to accuse the British Government of stinginess in the matter.

I have before me as I write the series of charts published by the American Commission appointed to investigate the circumstances of the transit. These consist of four finely-executed stereographic charts showing that hemisphere (and a fringe beyond) on the earth which is turned sunwards at the time of (1) ingress exterior contact, (2) ingress interior contact, (3) egress interior contact, and (4) egress exterior contact. On these charts are marked two series of curves, one carried through points where the contact occurs at the same instant, and the other carried through points where the contact occurs at the same part of the sun's limb. After a careful study of each chart (a study as careful as that which I gave to the Astronomer-Royal's charts in 1869), I am able to pronounce them singularly accurate for the degree of approximation which the authors claim.[1]

The American astronomers are disposed to rely chiefly on the photographic method, *applied at stations where the whole transit can be seen.* The condition italicised is of some importance as indicating a

[1] There is a very elaborate investigation of the error actually arising from the use of circles for the time-curves in the projection, to represent curves which are not in reality circles of the terrestrial sphere. The maximum error is found not to be more than 12′, which, says the author of the paper, 'having regard to the scale on which the charts have been constructed, may be considered as within the unavoidable errors produced by imperfection of drawing.' The maximum error is four times as great when the curvature of Venus's shadow-cone is altogether neglected in the usual way, according to which the resulting time-error is always of one sign. In the American charts the error is so distributed as to be positive or negative according to circumstances.

distinction between two possible photographic methods, one corresponding to Delisle's, the other to Halley's. The English arrangements, for example, include the application of photography at the Sandwich Islands, where only the beginning of the transit can be seen, and at Alexandria, where only the end can be seen. Such photographs can, of course, only have value when the absolute time at which each is taken is accurately known. On the other hand, the American stations are so selected that photographs can be taken throughout the whole continuance of the transit, and these, by indicating the chords of transit, will have a value independent of the exact determination of absolute time; so that, in fact, the original error of the Astronomer Royal (I mean the particular error relating to Halley's method) would, if not corrected, have affected the application of the photographic as well as other methods. Fortunately, the American astronomers have not been misled by it,[1] and at all the stations they propose to occupy, the whole transit will be visible, as will presently be more particularly noted.

The plan they adopt for photographing the sun differs essentially from that which European astronomers propose to employ. 'For the purpose of

[1] At the same time I must remark, that it seems to me altogether proper that the Delislean stations in question should be occupied by photographers. What I have all along insisted upon has been, the necessity of employing every available method, and occupying every available station; and it would have been a matter to be regretted, had any one of the regions originally suggested by Sir G. Airy been neglected.

obtaining an enlarged image on the photographic plate,' writes Professor Hilgard, of Washington, (describing the ordinary method), 'the image of the sun, after being formed in the focus of the telescope, is enlarged by a lens or camera to the desired size, the photoheliographs, as they are called, being thus enlarged to a diameter of about four inches. This plan has been adopted for the photographic apparatus to be used by the British, German, and Russian parties commissioned to observe the transit of Venus. A different plan has, however, been adopted for the American parties, with the view of avoiding some difficulties to which the former method may be thought subject. These are conceived to reside in the fact that not only all imperfections in the focal image are thus enlarged, but that the optical imperfections of the camera are superadded. To avoid this objection it was deemed best to make the telescope so long that the image formed in its principal focus would need no further enlargement. Here another difficulty presented itself. The telescope must be forty feet in length in order to give an image four inches in diameter. Such a telescope, pointed at the sun, would scarcely be manageable. Hence the plan was devised, which Professor Winlock was the first to put into practical operation. It consists in fixing the long telescope in a horizontal position, and reflecting the sun's rays into the object-glass by means of a plane glass mirror, moved by clockwork, so as to throw the image of the sun continually into the telescope. This need not be done with great precision, since, as has already been said, the time of exposure is exceedingly small, and the mirror can at any time be adjusted. It is obvious that, in this arrangement, as much depends upon the perfect figure of the mirror, as in the other upon that of the enlarging lens; but it is, doubtless, an advantage that different methods should be employed, so long as a sufficient number of stations are occupied to give an independent result for the sun's distance from observations by each method alone, since such only can be considered as strictly comparable. This condition is amply fulfilled by the abundant provision made by the American Government for the observation of the important event in prospect.'

I may remark, however, that Professor Newcomb, with whom I had the pleasure of a conversation relative to the subject, attaches very great importance to the advantages of the American method. He remarked that by employing this method the astronomer is enabled to measure the distance of Venus from the sun's centre with an exact knowledge of the value of the deduced distance, because, the focal length of the telescope being known, the value of any distance indicated in the focal image is at once determined. All that is necessary, then, is to determine the centre of the solar image, which can be safely done by measurements made from the limb. Manifestly no photographic effects affecting the position of the limb in the photograph could appreciably affect the determination of the centre even though such effects were not absolutely uniform all round. But in the ordinary method of photographing the determination of the arc-distance of Venus from the centre is not reliable (in a problem of such extreme delicacy), because the *estimated* dimensions of the solar image could not be *accurately* determined, while the observed dimensions, being determined from the photographic limb of the sun, would be affected more or less by photographic irradiation. No apparent sharpness of the limb can render certain the fact that the limb in the photographic image corresponds to the true solar limb. I must confess that Professor Newcomb's reasoning seems to me irresistible. It will be observed that it does not depend on practical or technical knowledge of photography, since the photographic irradiation demonstrably exists, and is demonstrably variable in amount. In a conversation with Dr. H. Draper, of New York, whose experience in those matters is well known to be unsurpassed, I found Professor Newcomb's doubts fully confirmed. It is true that Dr. Rutherfurd, whose great practical experience in solar photography is unquestioned, agrees with his eminent British rival in such work, Dr. De la Rue. But then it is to be remembered that both Rutherfurd and De la Rue view the matter as photographers, while Newcomb and Draper view it chiefly from an astronomical standpoint, and in this case the astronomical, not the photographic relations, are chiefly in question. We do not want handsome solar pictures, but pictures which can be confidently measured; and certainly the plan adopted by

American astronomers is that which best meets this requirement. I may add that a very eminent American astronomer, speaking to me on this subject, made this strong remark, 'I regard the photographic method adopted by the British astronomers as involving a mistake as fatal as Airy's original mistake would have been if uncorrected.' [1]

The programme of the American expeditions is as follows:—

Eight parties take the field, three in the northern hemisphere, 'where the meteorological conditions are supposed to be somewhat more favourable than at corresponding stations in the southern hemisphere,' where there will be five. The three northern stations will be (1) at Vladivostok, in Siberia; (2) at Tien-tsin, in China; and (3) at some as yet undetermined place in Japan. Originally, in response to the strongly expressed wish of the Astronomer Royal that two Delislean stations should be occupied, one on the Sandwich Islands group, and another at Tahiti, the Americans thought of occupying Owhyhee, rejecting as disadvantageous the suggested Tahitian station. But since then the idea of having any Delislean stations has been abandoned, and, as just mentioned, the five remaining stations are all to be in the southern Halleyan region. 'The "Swatara,"' says an American paper, 'the vessel which is to convey the various

[1] Lord Lindsay, it is to be noted, will employ the same method as the American astronomers, after carefully testing with Mr. Ranyard, in a series of photographic experiments, the reliability of the two methods.

southern observing parties to their stations,' sailed from New York during the first week of June, and is 'to lay in provisions at Cape Town, as well as a supply of hens for the sake of their eggs, wherewith to albumenise the photograph plates. Then a party will, weather permitting, be left at the Crozet and Kerguelen Islands. As in frequent conditions of the wind access to the Crozet Islands is impossible, enough provisions will be left with the observers, and possible prisoners, to last them a whole year. From Kerguelen the vessel will sail to Hobart Town, thence to Bluff Harbour, in New Zealand, and thence to Chatham Island, the last southern point of observation, which is either uninhabited or else inhabited by cannibals. Here the 'Swatara' will remain till the transit is over, and will then, the possible cannibals allowing, revisit the various stations to take up the different parties, supposing them to be found.' 'Each station will be provided with four principal instruments: The photographic telescope just described, with a 5-inch object-glass corrected for the actinic rays, and forty feet local length; a telescope of five inches' aperture and eight feet local length, equatorially mounted for the observation of contacts; a transit instrument for the determination of time and geographical position; and an astronomical clock. The telescopes, both visual and photographic, have been ordered from the well-known firm of Alvan, Clark, and Sons, who have just completed and mounted at Washington the greatest refracting telescope in the world. Although the photographic method is mainly relied on, the eye-observations of ingress and egress are not to be neglected, and it is proposed to supplement them by measuring the distances of the cusps while the planet is entering the sun's disc and leaving it.' This last point I regard as one of extreme importance, as will be gathered from my remarks on the subject in 'The Sun,' and in the Monthly Notices of the Astronomical Society, vol. xxx. p. 46 *et seq.*

While all the American stations, as well northern as southern, are Halleyan, the English stations originally selected, were all intended to be used solely as Delislean stations. In fact, as already seen, in dealing with those among them which really are Halleyan as well as Delislean, Airy failed to notice that both ingress and egress can be observed.

Nine stations have been provided for, eight by the Home Government, and one by the Indian Government. Originally five stations were to have been occupied.

It is well to note, first, that ample provision has been made for the application of Delisle's method. No less than three stations will be occupied in the group of the Sandwich Islands, where Captain Tupman (the head of the entire enterprise) will be stationed.[1] Here photography will be applied specially

[1] In an excellent article (though not quite free from mistakes) in the 'London Quarterly Review,' the writer justly speaks of Captain Tupman as the 'moving spirit and master of the whole enterprise.' The success of the English expeditions will be chiefly due to the zeal and energy of Captain Tupman.

to the determination of the moment of ingress, by a contrivance of Janssen's (improved by De la Rue), enabling the photographer to take sixty successive pictures of the ingress, at the rate of one per second. Under Captain Tupman's command will be Lieutenants Ramsden and Noble, and Messrs. Johnson, Forbes, and Barnacle. The observation of accelerated ingress has been well provided for, especially as some of the Halleyan stations in Japan and the north-east of Asia are excellent for this phase also.

Retarded ingress will be observed at Kerguelen's Land and Rodriguez. According to the published statements there will be two stations on Kerguelen's Land, but Fr. Perry, who is chief in this region, has power to assign one party to Heard Island if a landing shall be found to be practicable. The three stations here are all Halleyan as well as Delislean, the whole transit being most favourably visible. It is well, therefore, to note that ample provision has been made for applying Halley's method, as well as for photographing the whole progress of the transit. The observers under Fr. Perry will be Fr. Sidgreaves, Lieutenants Corbet, Goodridge, and Coke, and Mr. J. B. Smith. At Rodriguez, Lieutenants Neate and Hoggan, and Mr. C. E. Burton, will be the observers.

Accelerated egress will be observed at Christchurch, New Zealand, by Major Palmer and Lieutenants Darwin and Crawford. This station, like the stations for observing retarded ingress, is Halleyan also, and is now well provided for as a station for observing the whole transit.

Retarded egress will be observed at Alexandria by Captains Browne and Abney, and Mr. S. Hunter.

The transit will be observed at Roorkee, in North India by Colonel Tennant. The whole transit will be observed at that station, and photography employed. Mr. Pogson will observe the transit at Madras. At Colaba, Mr. Chambers will observe the transit with such instruments as were left there by the observers of the late Indian eclipse.

The total cost of the British expeditions, exclusive of the Indian station, will be about 15,000*l.*

Lord Lindsay's station at Mauritius must be mentioned in this connection. The work done there will probably be quite as reliable as that done at any other station, and the photographic preparations are, on the whole, more complete than those adopted anywhere else.

It may be mentioned, also, that Colonel Campbell will proceed to Thebes on a private expedition, working with the Egyptian party as a volunteer.

German astronomers occupy five southern Halleyan stations, one of these being either the desolate Heard Island, or Kerguelen Land (if, as is feared, Heard Island cannot be occupied). Their original purpose was to occupy one station in the north, viz. at Chefoo, in China, one in the Auckland Islands, and Macdonald Island, besides a photographic station in Persia. They will rely considerably on the 'direct method' of observation.

The following account of the plans of Russia, France, and Holland is taken from the excellent article in the 'London Quarterly Review,' referred to in the note at p. 193:—

'The Russians naturally occupy their own Siberian stations. It is possible that some little service may be rendered at Kazan, Nicolaief, Charkof, Odessa, and even Moscow. But apart from these there are to be twenty-six stations; but of these only the following will be supplied with a complete equipment of observers and instruments, viz. Wladivostock, Port Possiet, Lake Hanka, Nertschinsk, Xhita, Kiachta, Tashkent, Port Perofski, Fort Uralsk, Aschura-deh, and Erivan. These stations will be furnished with astronomers who are prepared by work with the artificial transit, and who are furnished with excellent equatorials with clockwork motion, a heliometer, or a photographic apparatus. The other stations are to be provided with good observing telescopes, and the remainder merely with small instruments. At eleven of the stations both ingress and egress will be seen, so that the Halleyan method may be employed; and at the remainder of the stations they will be chiefly concerned with retarded egress. M. Struve also has visited this country, as well as others, that comparisons might be made and greater accuracy secured.

'The stations chosen by the French are Campbell and St. Paul's Islands, Noumea, Pekin, Yokohama, and Saigon. M. Janssen goes to the Yokohama station, and this station in connection with St. Paul will be almost perfect for the Halleyan method. B great care is to be taken in the finding of longitudes; so that if only ingress or egress can be observed, the Delislean method may be employed. It is also an important matter that the French photographs will be taken by the Daguerreotype process, ensuring delicacy and avoiding the difficulties possible to the shrinkage of the film employed in other methods. The parties at St. Paul's and Campbell Islands—placed as they will be on islands of desolation—are furnished with fuel and provisions for six months. Originally the sum granted was 300,000 francs; but this is to be considerably augmented, and there may certainly be excellent results anticipated from this national effort.

'Finally, the Dutch are sending out an admirably equipped expedition to the island of Réunion. It is to be provided with a very fine heliometer and a photo-heliograph by Dallmeyer, like those used by England; and two excellent refractors for observation. They will also be furnished with meteorological instruments and all apparatus necessary for finding longitude and time.'

So much for the preparations on foot for the transit now near at hand. As regards the transit of 1882, it would be premature to speak. It is to be hoped that what has happened in the case of the transit of the present year will serve in some sense as a warning to astronomers not to place implicit reliance on the opinions of any astronomer, however deservedly eminent, and also to prevent any unduly hasty expression of opinion by persons whose official position would cause the admission of error to be unpleasant. Adding to this consideration the fact that a large amount of practical experience in the value of the various methods of observation will probably be acquired during the approaching transit, we may well hope that in 1882 even more valuable observations will be made. It needs but a short study of the sun-views forming Plates XIV. and XV., or, preferably in some respects, figs. 37 and 38, and of the stereographic projection Plate VII., to see that American astronomers will have to take by far the most important share in the work of observation in the northern hemisphere. May it be permitted to me, however, to hope that England, by well-considered expeditions to southern stations, will remove any doubts that other nations may have entertained as to her zeal for science?

I may note here that while Halley's method fails totally for the transit of 1882, a method which, in some degree, would take its place may be applied with considerable advantage if stations in Patagonia, Tierra del Fuego, the Falkland Isles, or, better still, the sub-Antarctic islands directly south of Cape Horn, could be reached. I refer to a method which may be called the mid-transit method, and which consists in the determination (preferably by photography) of the distance of Venus from the sun, near the middle of the transit, at two stations where the difference of her distance from the sun's centre will be the greatest possible. It will be manifest to the student, if he considers what I have said in pp. 209, 210, that the advantages claimed for stations on a radial line through the centre of Venus's shadow-cone (o in fig. 36) culminate when the earth is near the centre of her chord of passage through the shadow-section v v′. Wherever convenient stations exist for advantageously photographing the whole chord of transit, it would of course be absurd to select a station only advantageous for the middle of the chord; but in the transit of 1882 the whole chord cannot be very advantageously photographed at southern stations; and there will be a decided advantage in securing mid-transit photographs (as well, of course, as photographs of the beginning and end, and series of photographs for the whole transit where practicable).

However, for the present we need not inquire very closely into the conditions of the transit of 1882. Probably, long before that transit, the observations of the remarkable opposition of Mars in 1877, referred to in my 'Sun' at p. 25, will have given results scarcely less trustworthy than those obtained during the approaching transit.

It is not probable, judging from what Sir G. C. Lewis has shown respecting centenarians, that any of my readers will witness the transits of 2004 or 2012. Nevertheless, it may be interesting to know the circumstances of those transits and the regions of the earth where they will be wholly or partially visible. Mr. Hind, Superintendent of the Nautical Almanac, has calculated the elements of the two transits of the beginning of the twenty-first century. His results are thus presented in the Notices of the Astronomical Society for February 1872, p. 184:—[1]

Transit of 2004.

Greenwich Mean Time of Conjunction in Right Ascension = June $7^d\ 20^h\ 51^m\ 28^s{\cdot}8$.

For the centre of the Earth.

	d	h	m	s		°
First external contact	June 7	17	3	43	at	115·0
First internal contact	„	17	22	35	„	118·0
Second internal contact	„	23	5	40	„	214·6
Second external contact	„	23	24	32	„	218·5

The angles are reckoned from N. towards E. for the direct image.

At Greenwich the entire transit will be visible.

Transit of 2012.

Greenwich Mean Time of Conjunction in Right Ascension = June $5^d\ 13^h\ 4^m\ 44^s{\cdot}3$.

For the centre of the Earth.

	d	h	m	s		°
First external contact	June 5	10	22	11	at	40·3
First internal contact	„	10	39	56	„	37·8
Second internal contact	„	16	42	6	„	293·1
Second external contact	„	17	0	0	„	290·5

At Greenwich the egress only will be visible, the sun rising at $15^h\ 46^m$.

[1] M. Le Verrier's Tables of the sun and Venus represent so closely the motions of the earth and Venus in their orbits, that there can be little reason to doubt that the Tables will be as sensibly perfect in 2—0 as they are at the present time.

These results are illustrated by the projections forming Plates VIII. and IX., and by the transit chords shown in Plate I. (the Frontispiece). The reader who has followed the explanations of Plates IV. and V. will have no difficulty in understanding the plates illustrating the transits of 2004 and 2012.

We cannot doubt that when the transits of 2004 and 2012 are approaching, astronomers will look back with interest on the operations conducted during the present 'transit-season;' and although in those times in all probability the determination of the sun's distance by other methods—by studying the moon's motions, by measuring the flight of light, by estimating the planets' weight from their mutual perturbations, and so on, will far surpass in accuracy those now obtained by such methods, yet we may reasonably believe that great weight will even then be attached to the determinations obtained during the approaching transits. I think the astronomers of the first years of the twenty-first century, looking back over the long transitless period which will then have passed, will understand the anxiety of astronomers in our own time to utilise to the full whatever opportunities the coming transits may afford; and I venture to hope that should there then be found, among old volumes on their book-stalls, the essays and charts by which I have endeavoured to aid in securing that end (perhaps even this little book in which I record the history of the matter), they will not be disposed to judge over-

TRANSIT OF 1874.

TABLE III.—*Places where Egress is accelerated.*

Station	Sun's elevation	Accele-ration in minutes
	deg.	m.
South Victoria Ld. (Possession Ilnd.)	25·0	11·4
Adélie Land . .	34·0	10·6
Campbell Island .	26·0	10·3
Emerald . .	30·0	10·3
Macquarie Island .	32·0	9·8
Chatham Island .	16·0	9·8
Canterbury (N.Z.)	22·3	9·3
Wellington . .	20·0	9·2
Sabrina Land .	43·0	9·2
Enderby Land .	39·0	8·5
Royal Co. Island .	42·0	8·5
Auckland . .	19·2	8·5
Kemp Island . .	31·0	7·6
Hobart Town .	40·0	7·6
Melbourne . .	43·0	6·6
Sydney . . .	37·2	6·6
Adelaide . .	47·8	5·8
Kerguelen Land .	57·1	5·0
Crozet Island .	47·5	4·2
Perth (Australia) .	66·2	3·6

TABLE IV.—*Places where Egress is retarded.*

Station	Sun's elevation	Retar-dation in minutes
	deg.	m.
Orsk . . .	12·5	11·8
Omsk . .	11·5	11·7
Astracan . .	12·0	11·6
Aleppo . .	14·6	10·5
Peshawur . .	31·5	10·3
Alexandria .	14·0	10·0
Suez . . .	16·1	9·8
Nertchinsk .	10·1	9·8
Delhi . .	38·0	9·4
Tsitsikar . .	12·0	8·7
Bombay . .	45·0	8·5
Pekin . .	21·0	8·6
Kirin-Oula .	14·0	8·4
Tientsin . .	17·1	8·4
Calcutta . .	45·3	8·2
Aden . .	30·0	7·8
Nankin . .	27·0	7·6
Madras . .	52·0	7·4
Shanghai . .	26·0	7·2
Canton . .	37·0	6·6
Hongkong . .	37·0	6·5

TABLE V.—*Difference in the Duration of the Transit of Venus in 1874, at 14 Northern and 13 Southern Stations.*

Northern Stations	Southern Stations												
	Kemp Island	Crozet Island	Macdonald Island	Kerguelen Land	Emerald Island	Macquarie Land	Campbell Island	Royal Co. Island	Auckland Island	Hobart Town	Canterbury (N.Z.) and Bourbon Island	Melbourne and Mauritius	Adelaide and Rodriguez
	m	m	m	m	m	m	m	m	m	m	m	m	m
Nertchinsk	34·2	32·4	32·3	32·2	29·3	29·1	28·7	28·6	28·0	26·0	25·7	24·4	23·9
Tsitsikar	32·9	31·1	31·0	30·9	28·0	27·8	27·4	27·3	26·7	24·7	24·4	23·1	22·6
Kirin-Oula	32·7	30·9	30·8	30·7	27·8	27·6	27·2	27·1	26·5	24·5	24·2	22·9	22·4
Tientsin	32·0	30·2	30·1	30·0	27·1	26·9	26·5	26·4	25·8	23·8	23·5	22·2	21·5
Jeddo	31·8	30·0	29·9	29·8	26·9	26·7	26·3	26·2	25·6	23·6	23·3	22·0	21·5
Pekin	31·5	29·7	29·6	29·5	26·6	26·4	26·0	25·9	25·3	23·3	23·0	21·7	21·2
Tchefoo	31·0	29·2	29·1	29·0	26·1	25·9	25·5	25·4	24·8	22·8	22·5	21·2	20·7
Nagasaki	30·5	28·7	28·6	28·5	25·6	25·4	25·0	24·9	24·3	22·3	22·0	20·7	20·2
Bonin Is.	30·0	28·2	28·1	28·0	25·1	24·9	24·5	24·4	23·8	21·8	21·5	20·2	19·7
Nankin	29·8	28·0	27·9	27·8	24·9	24·7	24·3	24·2	23·6	21·6	21·3	20·0	19·5
Canton	26·8	25·0	24·9	24·8	21·9	21·7	21·3	21·2	20·6	18·6	18·3	17·0	16·5
Hongkong	26·7	24·9	24·8	24·7	21·8	21·6	21·2	21·1	20·5	18·5	18·2	16·9	16·4
Peshawur	26·6	24·8	24·7	24·6	21·7	21·5	21·1	21·0	20·4	18·4	18·1	16·8	16·3
Delhi	25·5	23·7	23·6	23·5	20·6	20·4	20·0	19·9	19·3	17·3	17·0	15·7	15·2

TABLE [illegible]

[illegible]

Parallax.	Mean Distance in miles.	Difference for 0"·01 in miles.
8·0	1?2,171,?91	126.142
8·1	100,922,560	123.661
8·2	99,680,390	120.097
8·3	98,480,120	117.240
8·4	97,307,830	114.478
8·5	96,?62,840	111.830
8·6	95,144,540	109.244
8·7	94,?12,200	106.766
8·8	92,884,540	104.362
8·9	91,840,920	102.045
9·0	90,820,470	99.805
9·1	89,822,450	97.632
9·2	88,846,2?0	95.533
9·3	87,89?,780	93.504
9·4	86,955,740	91.530
9·5	86,040,440	89.624
9·6	85,144,200	87.780
9·7	84,266,400	85.986
9·8	83,406,540	84.248
9·9	82,564,060	82.564
10·0	81,738,420	

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