

**THE ELEMENTS**  
**OF THE**  
**FOUR INNER PLANETS**  
**AND THE**  
**FUNDAMENTAL CONSTANTS OF ASTRONOMY**

**BY**

**SIMON NEWCOMB**

1835-1909

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**Supplement to the American Ephemeris and Nautical  
Almanac for 1897**

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**WASHINGTON**  
**GOVERNMENT PRINTING OFFICE**  
**1895**

## PREFACE.

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THE diversity in the adopted values of the elements and constants of astronomy is productive of inconvenience to all who are engaged in investigations based upon these quantities, and injurious to the precision and symmetry of much of our astronomical work. If any cases exist in which uniform and consistent values of all these quantities are embodied in an extended series of astronomical results, whether in the form of ephemerides or results of observations, they are the exception rather than the rule. The longer this diversity continues the greater the difficulties which astronomers of the future will meet in utilizing the work of our time.

On taking charge of the work of preparing the *American Ephemeris* in 1877 the writer was so strongly impressed with the inconvenience arising from this source that he deemed it advisable to devote all the force which he could spare to the work of deriving improved values of the fundamental elements and embodying them in new tables of the celestial motions. It was expected that the work could all be done in ten years. But a number of circumstances, not necessary to describe at present, prevented the fulfillment of this hope. Only now is the work complete so far as regards the fundamental constants and the elements of the planets from Mercury to Jupiter inclusive. The construction of tables of the four inner planets is now in progress, those of Jupiter and Saturn having already been completed by Mr. HILL. All these tables will be published as soon as possible, and the investigations on which they are based are intended, so far as it is practicable to condense them, to appear in subsequent volumes of the *Astronomical Papers of the American Ephemeris*. As it will take several years to bring out these volumes, it has been deemed advisable to publish in advance the present brief summary of the work.

The author feels that critical examination of this monograph may show in many points a want of consistency and continuity. The ground covered is so extensive, the material so diverse as well as voluminous, and the relations to be investigated so numerous, that no conclusion could be reached on one point which was not liable to be modified by subsequent decisions upon other points. The author trusts that the difficulties growing out of these features of the work, as well as those incident to the administration of an office not especially organized for the work, will afford a sufficient apology for any defects that may be noticed.

NAUTICAL ALMANAC OFFICE,

*U. S. Naval Observatory, January 7, 1895.*

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# ELEMENTS AND CONSTANTS.

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## CHAPTER 1.

### GENERAL OUTLINE OF THE WORK OF COMPARING THE OBSERVATIONS WITH THEORY.

1. In logical order, the first step in the work consists in the reduction of observed positions of the Sun and planets to a uniform equinox and system of declinations.

The adopted standard of Right Ascensions was that originally worked out in my paper on the Right Ascensions of the fundamental stars, found in an appendix to the *Washington Observations for 1870*, and extended to a fundamental system of time stars in the catalogue published in Vol. 1 of the *Astronomical Papers of the American Ephemeris*. This system coincides closely with that of the *Astronomische Gesellschaft* and the *Berliner Jahrbuch*, about the epoch 1870, but the centennial proper motion is greater by about  $0^s.08$ .

In Declinations, the adopted standard was that of BOSS, which has been used in the *American Ephemeris* since 1881, and on which is based the catalogue of zodiacal stars just referred to. But as Declinations generally are not immediately referred to fundamental stars, the method of reducing observations to this system in Declination was not entirely uniform.

#### *Observations used.*

2. The following is a general statement of the observations used, and the extent to which they were corrected, or re-reduced.

*Greenwich.*—Dr. AUWERS courteously supplied me with the results of his re-reduction of BRADLEY'S observations both of the Sun and planets. From the beginning of MASKYLENE'S work until 1835, the Greenwich observations were completely re-reduced, utilizing, so far as possible, AIRY'S reductions. The

data necessary for these observations were discussed in Prof. SAFFORD'S paper, Vol. II, pt. II, which paper was prepared for this purpose. In the case of the Greenwich observations from 1835 onward, it was deemed sufficient to apply constant corrections to the Right Ascensions, determined from time to time by comparisons of the adopted Right Ascensions with the standard ones. In the case of the Declinations, Boss's special tables were used, but in the later years it was judged sufficient to apply the constant correction necessary for reduction to Boss's standard.

*Palermo.*—PIAZZI'S observations of the Sun and Planets were completely re-reduced, the zero point of his instrument being determined from the observed Declinations.

*Paris.*—LEVERRIER'S reduction of the Paris observations from 1801 onward was made use of, applying the correction necessary to reduce the results to the adopted standard.

*Königsberg.*—BESSEL'S clock corrections were individually corrected by the new positions of the fundamental stars, so that practically the Right Ascensions may be considered as completely re-reduced.

In the case of the other observatories, it was deemed sufficient to determine, by a comparison of the adopted or of the concluded Right Ascensions and Declinations of the fundamental stars with the standard catalogue, what common corrections were necessary for reduction to the standard. When, however, the period was covered by Boss's tables, the correction which he gives as varying with the Declination was applied. After more mature consideration, I am inclined to think it would have been better to apply a constant correction to the Declinations in every case, except those where the change with the Declination was quite large.

Although these processes were somewhat heterogeneous, it is believed that the main object of referring the Declinations to a system of which the error would be a uniformly varying quantity was fairly well attained. The subsequent determination of this error both in Right Ascension and Declination is a necessary part of the work.

The following is a list of the observatories whose observations of the Sun and Planets were included in the work:

Greenwich .....	1750-1892
Palermo .....	1791-1813
Paris .....	1801-1889
Königsberg .....	1814-1845
Dorpat .....	1823-1838
Cambridge .....	1828-1844
Berlin .....	1838-1842
Oxford, Radcliffe .....	1840-1887
Pulkowa .....	1842-1875
Washington .....	1846-1891
Leiden .....	1863-1871
Strassburg .....	1884-1887
Cape of Good Hope .....	1884-1890

The number of the meridian observations of the Sun, and of the planets Mercury, Venus, and Mars, actually included in the work is approximately as follows:

The Sun .....	40, 176
Mercury .....	5, 421
Venus .....	12, 319
Mars .....	4 114
Total .....	<hr/> 62, 030

### *Semidiameters of Mercury and Venus.*

3. The reduction of the semidiameter of the planets was a point to which special attention was given. In the case of Mercury, the adopted semidiameter at distance unity was  $3''.34$ . The values adopted by the various observatories in reducing their observations varied so little from this that in cases where the original reductions were accepted no correction was applied for the difference. So, also, when the observers applied a correction for reducing the observed center of light to the actual center of the planet, no revision of this reduction was made. Such was supposed to be the case with the Paris observations. When the published Right Ascension was that of the center of light simply, a reduction to the true center was computed by the empirical formula used in the Washington observations. If we put  $i$  for the angle between the Earth and Sun as seen from the planet, then  $1 + \cos i$  will represent the fraction of

the apparent transverse diameter of the planet that is illuminated by the Sun. It was assumed that when the illumination was such that the thickness of the crescent approached zero, the point observed would be two-thirds of the way from the center of the planet to the limb, and that when the planet was dichotomized the center of observation would be five-twelfths of the way from the center to the limb. These conditions, with the added one that when the planet was fully illuminated the correction should vanish, suggested the employment of the formula

$$\text{Correction} = \text{semidiameter} \times \frac{(1 - \cos i)(5 + \cos i)}{12}$$

This correction was to be multiplied by the sine or cosine of the angle which the line of cusps made with the meridian to reduce it to Right Ascension and Declination respectively.

The correction being practically the same whenever the Earth and planet return to the same positions in anomaly, it is possible to embody it in a table of two arguments, one depending on the longitude of the Earth, the other on that of the planet. Actually, however, the table was arranged in a more convenient form, in which one argument is the date at which Mercury last passed perihelion, and the other, its mean anomaly. Owing to the importance which this correction may assume, a partial transcript of the table actually employed for the reduction in Right Ascension is given on the next page. Read horizontally, the numbers show the corrections of the argument through one revolution of the planet. Vertically, they may be regarded as giving the successive corrections corresponding to any one position of the planet, while the Earth goes through a complete revolution. The table as actually used extended to every  $10^\circ$ , but the values for every  $60^\circ$  of mean anomaly will suffice to show the general magnitude of the correction.

The correction to the Declination was embodied in a similar table, which it is not deemed necessary to print at present.

In the case of Venus, it seems scarcely possible to decide upon a value of the semidiameter, or a law of its apparent change, which should apply to all parts of the orbit. After a

careful examination of the data, it was decided to reduce all the observations with the semidiameter

$$\frac{8''.75}{\Delta} + 0''.20$$

when made with modern instruments, and to use a value  $0''.3$  greater in earlier observations. The actual reductions of all

*Correction for defective illumination of Mercury in R. A.*  
*Arguments: Date of perihelion passage at side, and mean anomaly "g" at top.*

$g =$	$0^\circ$	$60^\circ$	$120^\circ$	$180^\circ$	$240^\circ$	$300^\circ$	$360^\circ$
	<i>s</i>	<i>s</i>	<i>s</i>	<i>s</i>	<i>s</i>	<i>s</i>	<i>s</i>
Jan. 0 ..	+.19	-.16	-.07	-.03	-.01	.00	+.03
10 ..	.16	-.18	-.09	-.04	-.01	.00	.02
20 ..	.14	-.21	-.11	-.05	-.02	.00	.02
30 ..	.12	-.19	-.13	-.06	-.03	.00	+.01
Feb. 9 ..	.10	-.17	-.15	-.08	-.04	-.01	.00
19 ..	.08	-----	-.18	-.10	-.05	-.01	.00
Mar. 1 ..	.06	+.16	-.21	-.12	-.06	-.02	.00
11 ..	.05	.16	-.24	-.15	-.08	-.03	.00
21 ..	.04	.15	-.26	-.18	-.10	-.04	.00
31 ..	.03	.14	-----	-.20	-.12	-.06	-.01
Apr. 10 ..	.02	.12	+.23	-.22	-.15	-.07	-.01
20 ..	.02	.10	.20	-----	-.18	-.09	-.01
30 ..	+.01	.08	.18	+.24	-.21	-.11	-.02
May 10 ..	.00	.06	.15	.22	-.17	-.13	-.03
20 ..	.00	.05	.12	.20	-.12	-.16	-.04
30 ..	.00	.04	.10	.17	-----	-.18	-.05
June 9 ..	.00	.03	.09	.14	+.18	-.20	-.06
19 ..	.00	.02	.07	.12	.16	-.20	-.07
29 ..	-.01	.01	.05	.09	.15	-.20	-.09
July 9 ..	-.01	.01	.04	.07	.13	-----	-.11
19 ..	-.01	+.01	.03	.05	.11	+.16	-.12
29 ..	-.02	.00	.02	.04	.09	.16	-.14
Aug. 8 ..	-.03	.00	.01	.03	.07	.16	-.16
18 ..	-.04	.00	.01	.03	.06	.14	-.18
28 ..	-.05	.00	+.01	.02	.05	.13	-----
Sept. 7 ..	-.06	.00	.00	.02	.04	.11	-----
17 ..	-.07	-.01	.00	+.01	.02	.09	-----
27 ..	-.09	-.01	.00	.00	.02	.07	+.20
Oct. 7 ..	-.11	-.02	.00	.00	+.01	.05	.18
17 ..	-.12	-.02	.00	.00	.00	.04	.16
27 ..	-.14	-.03	-.01	.00	.00	.03	.13
Nov. 6 ..	-.16	-.04	-.01	.00	.00	.02	.11
16 ..	-.18	-.06	-.01	.00	.00	+.01	.09
26 ..	-----	-.08	-.02	.00	.00	.00	.07
Dec. 6 ..	-----	-.10	-.03	-.01	.00	.00	.06
16 ..	-----	-.12	-.05	-.01	-.01	.00	.05
26 ..	+.20	-.15	-.06	-.02	-.01	.00	.04
Jan. 5 ..	+.18	-.17	-.08	-.03	-.01	.00	+.03

the principal series of observations were corrected to this value of the element in question.

Observations of the estimated center of Venus, when made more than one hundred days from superior conjunction, were rejected altogether; when made within that limit, the point observed was assumed to be the center of gravity of the illuminated portion of the disk, considered as a plane figure, and the necessary reduction to the center was always applied.

A similar correction was applied to observations of the estimated center of Mars. The Paris results, after 1830, and the later Greenwich and Washington results, are published with the reduction for center of light already applied, and in these cases the published corrections were not changed.

#### *Tabular places.*

4. The tabular elements of the planets adopted for correction were those of LEVERRIER'S tables. These tables having been continuously used in Astronomical Ephemerides since 1864, it was judged more convenient to adopt the theory on which they were based as the provisional one to be corrected than it was to construct a new provisional theory. As the tables in their original form are extremely cumbrous to use, the theory was partially reconstructed by making manuscript tables of the principal perturbations, which were, however, carried only to tenths of seconds. With these tables the places of the planets were computed for dates previous to 1864.

As places of the Sun were necessary not only for direct comparison with observations of the Sun, but also for the geocentric places of the planets, an ephemeris of the Sun's longitude and radius vector was prepared for the entire period 1750-1864 to every fifth day, the lunar perturbation being omitted and afterward applied for each date when required.

The method of deriving the final tabular places varied with circumstances. When there was no accurate ephemeris available for comparison, which was the case before 1830, it was necessary to compute a completely independent set of tabular geocentric places. Sometimes these places were computed for the moment of the individual observations, but more generally, when the observations occurred in groups, an ephemeris was

computed in order that the work might be checked by differences. After 1830 it was common to compute an ephemeris for intervals of three, five, or ten days, thus deriving the corrections necessary to reduce the published ephemerides of the *Berliner Jahrbuch* or of the *Nautical Almanac* to those derived from LEVERRIER'S tables.

Until this plan was mapped out, and work well in progress upon it, it was not noticed that the planetary masses adopted in LEVERRIER'S tables were so diverse that corrections to reduce the geocentric places to a uniform system of masses would be necessary. Although theoretically the necessary reductions were very simple, I can not but feel that the application of such corrections involves more or less doubt and uncertainty, and that it would have been better to have constructed provisional tables based on uniform masses quite independent of those of LEVERRIER.

In *Annales de l'Observatoire de Paris*, Vol. II, LEVERRIER gives the following values of the masses used by him as the basis of his provisional theory:

$$\text{Mercury} \dots \frac{1}{3\,000\,000} = .000\,000\,333 \dots$$

$$\text{Venus} \dots \frac{1}{401\,847} = .000\,002\,4885$$

$$\text{Earth} \dots \frac{1}{354\,936} = .000\,002\,8174$$

$$\text{Mars} \dots \frac{1}{2\,680\,337} = .000\,000\,373\,087$$

The following table shows the factors by which these masses were multiplied in the cases of the several planets in LEVERRIER'S final tables. They were controlled by induction from the numbers of the tables themselves, the result of which was found in all cases to agree with the statements in the introduction to the tables.

In the last line of the table is shown the factor used in the present provisional theory.

	Mercury.	Venus.	Earth.	Mars.
In tables of—				
The Sun -----	1	1.004	-----	0.895
Mercury -----	-----	1	1	-----
Venus -----	1	-----	1	1
Mars -----	-----	0.975	1.0026	-----
Present work -----	1	1	1	0.8657

As in the actual work the masses of Mercury and Venus were to be determined from the observed periodic perturbations which they produced, it was necessary that the perturbations produced by them should all be carefully reduced to the adopted standard. The reduction was less necessary in the case of Mars, but was carried through all the work relating to the Sun.

*Comparison of observations and tables.*

5. The result of each separate observation of each body was compared with the tabular result thus derived. The residuals were then taken and divided into groups. The interval between the extreme dates of each group was always taken so short that it could be presumed that the mean of all the residuals would be the correction for the mean of all the dates. The general rule was that the interval should not exceed four or five days in the case of Mercury, or six or eight days in that of Venus, and that not more than six or eight observations should be included in a single group. In taking these means, weights were assigned to the results of each observation founded on the discordance of its residuals. Then to each mean a weight was again assigned equal to the sum of the weights of the individual residuals when these were few in number, but not allowed to exceed a certain limit, how great soever might be the sum of the individual weights.

*Equations of condition.*

6. Each mean result thus derived formed the absolute term of an equation of condition for correcting the tabular elements. The number of these equations was as follows:

	Equations.
The Sun -----	11, 676
Mercury -----	3, 929
Venus -----	4, 849
Mars -----	1, 597

In forming the equations of condition from observations of the planets, I adopted the system suggested in the introduction to Vol. 1 of these publications, namely, the determination of the solar elements not only from observations of the Sun itself, but from observations of each of the planets. The reason for this course is quite simple and obvious. An observation of the position of a planet as seen from the Earth is the exact equivalent of an observation of the Earth as seen from a planet, and thus depends equally upon the elements of both orbits. Hence, whatever elements of the Earth's orbit could be determined by observations made from a planet can equally be determined by observations made upon the planet. A strong reason for proceeding upon this plan was found in the very large errors, both accidental and systematic, to which observations of the Sun are liable.

The advantages, however, have not proved relatively so great as were anticipated. The eccentricity and perihelion of the Earth's orbit come out in the solution of the normal equations as functions of those of the planetary orbit to so great an extent that their weight is much less than that which would correspond to independent determinations from the same number of observations. On the other hand, the determination of these elements from observations of the Sun proved to be much more consistent than was expected, thus indicating a high degree of precision.

The case is different with the Sun's mean longitude referred to the Stars. Here systematic and personal errors enter so largely that the results from Mercury and Venus appear to be rather more reliable than those from the Sun itself. In the case of these planets it fortunately happens that the weight of the result derived for the Sun's mean longitude is not materially diminished by the uncertainty of the corresponding element of the planet, the errors of the two mean longitudes being nearly separated in a series of observations equally distributed around the orbit.

The systematic errors in observations of the Sun rendered it unadvisable to determine the elements of the Earth's orbit from observations of the Sun by a single system of equations. The solar observations, therefore, were classified according to

the observatory where made, and divided into periods rarely exceeding eight years in length. The elements are separately derived from the observations of each period. This system has the advantage of eliminating to a large extent the injurious effect of systematic and personal error upon the eccentricity and perihelion of the Earth's orbit, and also enabling us to judge of the precision of the corrections to those elements by the discordance among separate results.

Meridian observations of the Sun and Planets are referred to the fundamental stars, while the Right Ascensions of the latter are referred to the equinox, the position of which has heretofore depended on observations of the Sun. The adopted position of the fundamental stars therefore comes in, to a certain extent, as the basis of the work, and the constant parts of their systematic corrections are among the results to be derived.

Thus, in the case of the equations pertaining to the three planets, the following corrections were introduced as unknown quantities:

Correction of the mass of Mercury or of Venus.

Corrections to the elements of the orbit of the planet observed.

Correction of the obliquity of the ecliptic.

Corrections to the Sun's mean longitude, eccentricity, and longitude of perihelion.

Common corrections to the adopted Right Ascensions and Declinations of the fundamental stars.

In the case of Mercury an adopted hypothetical correction of the ratio of the radius vector of the planet to that of the Earth was also included in the equations, although little doubt could be felt that the true value of such a quantity must be zero. The reason for introducing it will be explained hereafter.

#### *Determinations of the masses and secular variations.*

7. The secular variation of all the preceding elements, the mean distances excepted, was also introduced into the equations from observations of the planets. In addition to the above elements, the mass of Venus appeared in the equations

derived from observations of the Sun, Mercury, and Mars, and the mass of Mercury in the equations derived from observations of Venus. The coefficients of the masses, however, depended wholly upon the periodic perturbations.

Were it quite certain that the secular variations arise wholly from the masses of the known planets, the masses could of course be derived from these variations, and the latter would appear in the equations of condition only through the mass itself. On this hypothesis the secular variations would not appear in the equations, but only the masses. But it is well known that the perihelion of Mercury is subject to a secular variation which can not be accounted for by any admissible masses of the known disturbing planets. The same thing may well be true of the secular variations of the other elements. It is therefore necessary, in the absence of a known cause for such deviations, to derive the masses of the planets independently of the secular variations. In the case of Mars the mass is obtained with all necessary precision from the satellites. It is, however, different in the case of Mercury and Venus. Here no resource is left us but to determine them from the periodic inequalities. As the inequality produced by Venus in the Earth's longitude is rarely more than eight seconds, it might seem that the coefficient would be too small to obtain a sufficiently precise value of the mass. But in the case of observations upon the Sun, Mercury, and Mars the error of the determination of the mass in question may be almost indefinitely reduced by multiplication and extension of the observations without danger of systematic error.

To illustrate this, let us suppose the Sun's longitude to be determined with a meridian instrument only once a year, say at equal intervals of three hundred and sixty-five days. Let the longitudes thus observed be compared with an ephemeris in which the elements are affected with only slight errors. Leaving out of consideration the periodic perturbations produced by the planets, the comparison of the observed longitudes with the tabular ones through an entire century should be nearly constant. Any error affecting all the longitudes alike would appear as a constant. The errors of mean motion

would vary uniformly with the time. Thus the other elements would be nearly constant, and could be still more approximately represented by a slight apparent secular variation.

Now let the disturbing action of a planet, say Venus, be introduced. We should then have a series of deviations from the law of uniform increase, which would enable us to evaluate the mass of the planet. The value of this mass thus derived would not be affected by any systematic error common to all the observations, nor even by such an error which varied uniformly with the time. Nor would small errors in the adopted elements of the Sun have any effect upon the result.

If this would be the case for observations made only at a certain point of the orbit, *a fortiori* would it be the case for the observations made at various points of the orbit, since any tendency to a systematic effect of the errors of observation would thereby be ultimately eliminated.

Considerations almost identical apply to the case of observations upon either of the planets when we consider the action of the other planet upon the planet observed and upon the earth. But they do not apply to the case of the action of the earth itself upon the observed planet, or *vice versa*. For example, in the case of observations of Venus, we may suppose that all observations made when Venus is at a certain point of its relative orbit, near inferior conjunction, say one month before inferior conjunction, are affected with a certain error common to all observations made at that point of the orbit. Since the perturbations produced by the third planet will in the long run have all values, positive and negative, for these several observations, the systematic error in question will not affect the ultimate value of its mass. But the perturbations of Venus produced by the Earth, as well as those of the Earth produced by Venus, will not have all values in such a case, but only special ones dependent on the relative position. Hence, determinations of these masses might be affected by errors of the kind in question. We conclude, therefore, that the mass of the Earth can not be satisfactorily determined by the periodic perturbations which it produces in the motion of any planet, nor that of Venus by observations on Venus through its periodic perturbations of the Earth.

In the solution of the equations of condition the method of least squares has been used throughout, the arrangement of the work, the choice of quantities to be corrected, and the accuracy of the coefficients being so chosen as to minimize the great mechanical labor of making the necessary multiplications. The adoption of this method was necessary in order to separate, so far as possible, the various unknown quantities and show to what extent their values were interdependent. By no other method of combination could so large a number of unknown quantities have been separately determined in a way which would have been at all satisfactory. On the other hand, in combining the final results and deciding upon the values of the corrections to be adopted, the method has not always been applied, for reasons which will be developed in Chapter IV.

*Introduction of results of observations on transits of Venus and Mercury.*

8. In the case of Mercury and Venus the observed transits over the Sun give relations between the corrections to the elements more accurate than those ordinarily derivable from meridian observations. This is especially the case with Venus. The value of these observations is greatly increased by the fact that they are made when the planet is near inferior conjunction, and therefore nearest to the Earth, and in a point of the relative orbit where meridian observations are necessarily most uncertain. In the case of Venus the error of the heliocentric place will be more than doubled in the case of the geocentric place during a transit. As, however, the observation of a transit gives no one element, but only an equation of condition between the values of all the elements at the epoch, the only way of treating it is to introduce the result as such an equation, with its appropriate weight. The determination of the proper weight is a difficult matter. The systematic errors of meridian observations are such that the theoretical value of the weights assignable to so great a mass as we have discussed would be entirely illusory. In fact so great is the weight assignable to the observed transits of Venus that if we should regard the results of each transit as a condition to

be absolutely satisfied we should not be dangerously in error. I conclude, therefore, that there is more danger of assigning too small than too great a weight to these observations.

In order to determine what change was produced in the results by the use of the observed transits over the sun's disk, two separate solutions of the equations of condition for Mercury and Venus were made. In the one, termed solution A, the meridian observations alone were used; in the other, termed solution B, the combined equations formed by adding the normal equations derived from the transits to those given by the meridian observations were used.

In the case of solution A it was originally supposed that by using the mean epoch of all the observing in the case of each planet as that from which the time was to be reckoned, the normal equations for the secular variations would be almost completely separated from those for the corrections to the elements themselves. The separation would be complete were the observations at different epochs similarly distributed around the orbit. But, as a matter of fact, it was found that the accidental deviations from this symmetry were so considerable that the separation could not be regarded as complete. The solution was therefore made by successive approximations, the terms depending on the secular variations being in the first approximation dropped from the normal equations for the corrections to the elements, and afterwards included when approximately determined, and *vice versa*.

In the case of solution B, in which the transits were included, such a separation did not occur, and the equations were solved in the usual rigorous way for all the unknown quantities.

## CHAPTER II.

### DISCUSSION AND RESULTS OF OBSERVATIONS OF THE SUN.

#### *Treatment of the Right Ascensions.*

9. The meridian observations of the Sun have been treated on a system different in some points from that adopted in the case of the planets. It was possible to simplify the treatment by supposing that the small latitude of the Sun was always a definitely known quantity, so that when the observations were corrected for it the apparent motion of the Sun could be supposed to take place along the great circle of the ecliptic. This allowed the correction of the elements to depend on but two quantities—the obliquity of the ecliptic and the Sun's true longitude. Assuming the obliquity to be known, the longitude of the Sun could always be determined from an observation of its Right Ascension. An observed Right Ascension being compared with a tabular one, the residual gives rise to an equation of condition between the correction of the longitude,  $\lambda$ , of the obliquity,  $\epsilon$ , and of the Right Ascension of the Sun,  $\alpha$ :

$$d\alpha = \cos \epsilon \sec^2 \delta d\lambda - \frac{1}{2} \tan \epsilon \sin 2\alpha d\epsilon.$$

This equation may be used to express the error of the longitude in terms of the error of the obliquity and of the Right Ascension as follows:

$$\begin{aligned} \delta\lambda &= \sec \epsilon \cos^2 \delta \delta\alpha + \frac{1}{2} \tan \epsilon \sin 2\lambda d\epsilon \\ &= \sec \epsilon \cos^2 \delta \delta\alpha + 0.21 \sin 2\lambda d\epsilon \end{aligned}$$

The elements mainly to be determined from the observations in Right Ascension being the eccentricity and perihelion of the Earth's orbit, each of the coefficients of which go through a period in a year, the effect of the small term  $-0.21 \delta\epsilon \sin 2\lambda$  whose coefficient does not amount to  $0''.10$  after 1800, and has a period of half a year, will be practically without influence

on the result. The system was therefore adopted of deriving the residual in longitude directly from the residual in Right Ascension by the formula

$$\delta\lambda = F\delta\alpha$$

where

$$F = \cos^2 \delta \sec \varepsilon.$$

The residual  $\delta\lambda$  in true longitude is then to be expressed in terms of the residual  $\delta l''$  in mean longitude and of corrections to the eccentricity and to the longitude of the perigee relative to the Stars. In this expression the coefficient of the residual in mean longitude was always taken as unity, the value of the correction being so small in the case of LEVERRIER'S tables that no appreciable error would result from this supposition. Thus each residual in Right Ascension would give rise to an equation of condition of the form—

$$\delta l'' + P e'' \delta \pi'' + E \delta e'' = \delta\lambda = F \delta\alpha$$

We are here to regard  $\delta l''$  and  $\delta \pi''$  as corrections to the Right Ascensions relative to the clock stars, and not to the Sun's longitude or perigee simply. I shall therefore use the symbol  $c$  instead of  $\delta l''$  to express the relative correction hereafter.

#### *Treatment of the Declinations.*

10. The declination of the Sun in the case supposed is a function only of the longitude and obliquity. The equation for expressing the observed correction in Declination in terms of the corrections to these two quantities is

$$\Delta\delta = \sin \alpha \delta \varepsilon + \cos \alpha \sin \varepsilon \delta\lambda$$

Thus each observation of the Sun's Declination gives rise to an equation of condition of this form.

It is however to be supposed that the observations in Declination made at each observatory will be affected by a constant error. If the observations are truly reduced to the standard system of star places, this error will be that of the standard system. As a matter of fact, however, observations made in the daytime, especially on the Sun and at noon, are made under circumstances so different from night observations on

stars that we can not assume the error of the reduced declination to be necessarily the same as that of the star system. We must, therefore, in each case, regard the constant error in declination as something peculiar to the observatory and the instrument, which may or may not be worthy of subsequent discussion. Thus each residual in declination gives rise to an equation of condition,

$$\Delta\delta_0 + \cos \alpha \sin \epsilon \delta\lambda + \sin \alpha \delta\epsilon = \Delta\delta$$

$\Delta\delta$  being the excess of observed over tabular declination, and  $\Delta\delta_0$  the common error of all the measured declinations of any one series.

*Formation of the equations from Right Ascensions.*

11. The method of treating the observed Right Ascensions of the Sun was suggested by the fact that they are peculiarly liable to systematic and personal errors; the former likely to change with the seasons, and to be different for different instruments; and the latter to continue through the work of one observer. It is now well understood that the observed Right Ascensions of the mean of the Sun's two limbs relative to the fixed stars are affected by personal errors, no means of eliminating which have yet been tried. In a series of observations made by a single observer, under uniform conditions, this error would systematically affect only the relative mean of the Right Ascensions of the Sun and Stars, leaving the eccentricity and perigee derived from the observations substantially correct.

On taking up the work it was also supposed that, owing to the different effect of the Sun's rays upon the instrument at different seasons, and the different circumstances under which observations were made, the Right Ascensions of the Sun would be affected by errors varying in a regular way through the year, but not wholly expressible as a term of single annual period. It was therefore deemed best to consider the observations possibly affected by an error of double period, having the form

$$x' \cos 2g + y' \sin 2g$$

The introduction of the coefficients  $x'$  and  $y'$  added two more terms to the equations of condition, which terms, however, did not express any astronomical fact, but only the possible errors of the observations.

An additional and very important element to be determined from the observed Right Ascensions was the mass of Venus. The question now arose whether, by a uniform series of observations, extending through some definite period, the corrections to the eccentricity and perigee and the coefficients  $x'$  and  $y'$  could be completely separated from the coefficients of the correction to the mass of Venus. Examination showed that from such a series of observations, extending through eight years, the mass of Venus could be determined irrespective of all systematic errors repeating themselves with the season, provided that the observations were equally distributed throughout the year, or even that an equal number were made at the same time through successive years. As neither of these conditions are practically fulfilled it was judged best to assume in the beginning that the systematic errors of an unknown kind repeated themselves at each season during an eight-year period, and that they could be expressed in the form

$$c + x \cos g + y \sin g + x' \cos 2g + y' \sin 2g$$

$x$  and  $y$  would appear as errors of eccentricity and perigee which could not be eliminated.

The quantities actually introduced as the unknown ones of the equations of condition were as follows:

$\mu'$ , the factor of correction of the mass of Venus;

$x$ , one-fifth the correction to the eccentricity;

$y$ , one-fifth the correction  $e'' \delta \pi''$ ;

$x', y'$ , one-tenth the coefficients expressing the supposed error of double period arising from all causes whatever;

$c$ , the constant correction to the Right Ascension of the Sun relative to the Stars.

The coefficient of  $c$  was supposed unity throughout. The reduction of the residual in Right Ascension to that in Longitude and the other factors were taken from a table like the following, of which the argument was the day of the year.

Separate tables were constructed for 1802 and 1850, but they were so nearly identical that no distinction need be made between them. Furthermore, the error introduced by supposing the mean anomaly to have the same value on the same day of every year is entirely unimportant.

*Table of coefficients for expressing errors of the Sun's Right Ascension in terms of errors of the elements of the Earth's orbit.*

		$\frac{da}{dl}$	$\frac{dl}{da}$	Coefficients of—			
				$x=0.2\delta e$	$y=0.2e\delta\pi$	$x'$	$y'$
Jan.	1.....	1.09	0.91	+ 0.1	—10.0	+ 0.1	+10.0
	11.....	1.07	0.93	1.8	9.8	3.5	9.4
	21.....	1.04	0.96	3.4	9.4	6.5	7.6
	31.....	1.01	0.98	5.0	8.7	8.7	5.0
Feb.	10.....	0.98	1.01	3.4	7.7	9.8	+ 1.8
	20.....	0.96	1.04	+ 7.6	— 6.5	+ 9.9	— 1.6
Mar.	2.....	0.94	1.06	8.6	5.1	8.7	4.9
	12.....	0.92	1.08	9.4	3.5	6.6	7.5
	22.....	0.92	1.08	9.8	1.9	3.7	9.3
Apr.	1.....	0.93	1.07	10.0	— 0.1	+ 0.3	10.0
	11.....	0.94	1.05	+ 9.9	+ 1.6	— 3.1	— 9.5
	21.....	0.96	1.03	9.5	3.2	6.1	7.9
May	1.....	0.99	1.01	8.8	4.8	8.4	5.4
	11.....	1.02	0.98	7.8	6.2	9.7	— 2.2
	21.....	1.05	0.95	6.6	7.5	9.9	1.2
	31.....	1.07	0.93	+ 5.3	+ 8.5	— 8.9	— 4.5
June	10.....	1.09	0.91	3.7	9.3	6.9	7.2
	20.....	1.10	0.91	2.1	9.8	4.1	9.1
	30.....	1.09	0.91	+ 0.4	10.0	— 0.7	10.0
July	10.....	1.08	0.93	— 1.3	9.9	+ 2.7	9.6
	20.....	1.05	0.95	— 3.0	+ 9.5	+ 5.8	— 8.2
	30.....	1.03	0.97	4.6	8.9	8.2	5.7
Aug.	9.....	1.00	1.00	6.1	8.0	9.6	+ 2.7
	19.....	0.97	1.03	7.3	6.8	10.0	— 0.8
	29.....	0.95	1.05	8.4	5.4	9.1	4.1
Sept.	8.....	0.93	1.07	— 9.2	+ 3.9	+ 7.2	— 6.9
	18.....	0.92	1.08	9.7	2.3	4.5	8.9
	28.....	0.92	1.08	10.0	+ 0.6	+ 1.2	9.9
Oct.	8.....	0.93	1.07	9.9	— 1.1	— 2.2	9.7
	18.....	0.95	1.05	9.6	2.8	5.4	8.4
	28.....	0.97	1.02	— 9.0	— 4.4	— 7.9	— 6.1
Nov.	7.....	1.00	0.99	8.1	5.9	9.5	— 3.1
	17.....	1.03	0.96	7.0	7.2	10.0	+ 0.3
	27.....	1.06	0.94	5.6	8.3	9.3	3.7
Dec.	7.....	1.08	0.92	4.1	9.1	7.5	6.6
	17.....	1.09	0.91	— 2.5	— 9.7	— 4.9	+ 8.7
	27.....	1.09	0.91	— 0.8	—10.0	— 1.6	+ 9.9

Finally, throughout the work the equations of condition were expressed only in entire numbers, the decimals being neglected. To lessen the number of equations of condition, the residuals were divided into groups generally covering from ten to fifteen days, the length of the group being determined by the condition that the perturbations of Venus must not change much during the period.

While the formation and solution of the equations of condition on this system were going on, it was found that the introduction of the assumed coefficients  $x'$  and  $y'$  was a refinement productive of little or no good result. In fact, the observations of the Sun proved to be much freer from annual sources of error than I had supposed, as will be seen by the tables of their results soon to be given. This is shown by the general consistency of the corrections to the eccentricity and perigee given by the work at the same or different observatories during different periods.

In marked contrast to this is the discordance among values of the correction  $c$  to the relative Right Ascensions of the Sun and Stars. This quantity it is that is affected by personal error and possibly by the effect of the Sun on the instrument. Under a perfect system of discussion it would be advisable to determine it separately for each observer. This however was practically impossible.

#### *Solution of the equations.*

12. For the purposes of forming and solving the normal equations, the equations of condition were divided into groups of generally from four to eight years, the exact lengths of which will be seen from the following exhibit of results. The equations for each period were solved on the supposition that the corrections were constant during the period. Thus every separate result is independent of every other, except so far as they may depend on the same instrument or the same observer at different times.

The first column shows the years through which the observations extend.

The second one shows to the nearest year the value of  $T$ —that is, the fraction of the century after 1850.

The third column shows the value of  $\mu'$ , or that factor which, being multiplied by the adopted mass of Venus, is to be applied as a correction to that mass, to obtain the value given by the observations.

All systematic errors arising from the instrument and the observer are so completely eliminated from the separate determinations of  $\mu'$  that they may be regarded as absolutely independent of each other, that is—as not affected by any common systematic error.

We have next the relative weight assigned to each value of  $\mu'$ , which is determined in the usual way from the solution, and is, therefore, on a different scale for different observatories.

Next is given the value of  $c$ , or the apparent correction to the Right Ascension of the Sun, relative to the assumed Right Ascensions of the Stars, as given by observations during the several periods and expressed in seconds of arc, followed by the weights assigned to the separate results.

The next two columns, the corrections to the solar eccentricity and to the longitude of the perigee, require no further explanation.

Respecting the weights ultimately assigned to these quantities, and to  $c$ , it is to be remarked that they are the result of judgment more than of computation. It is only possible to enumerate in a general way with some examples the considerations on which they are based.

In assigning the weight of  $c$  the number of observers engaged is an important factor in determining it. Other factors are the steadiness of the atmosphere and the adaptation of the instrument to this particular work. General consistency is an important factor in the assignment. In this respect the Cambridge observations are quite remarkable; if their excellence corresponds to their consistency they must be the best ones made.

It will be seen that PIAZZI's results are thrown out entirely. The wide range of his values of  $c$  led to the inquiry whether more consistent results would be obtained by taking shorter periods, but it was found that the values of  $c$  varied from time to time in such an irregular way that his instrument

must have been affected by some extraordinary cause of error, unless some mistake has been made in interpreting or treating the observations.

The Oxford values of  $c$  are unusually discordant. The presumption that this discordance arises mainly from the special personal equation in observations of the Sun, described on page 17, derives additional weight from the greater relative consistency of the values of  $\delta c''$  and  $e''\delta\pi''$ . I have therefore allowed the values of these quantities to receive a fair weight.

The value of  $c$  for Paris, 1866-'70, has received a much reduced weight, solely on account of its excessive value. It seems that the work of one observer who made many observations during this period was affected by an unusual systematic error.

*Results of observations of the Sun's Right Ascension.*

GREENWICH.

Years.	T	$\mu'$	$\tau$	$c$	$\tau$	$\delta c''$	$e''\delta\pi''$	$\tau$
1750-'62	— .94	— .027	20	+0.33	1.5	+0.04	—0.42	2
1765-'71	— .82	— .041	10	+0.37	0.5	—0.08	—0.64	1
1772-'78	— .75	— .022	10	+0.74	0.5	—0.16	—0.49	1
1779-'85	— .68	— .035	5	+2.89	0.2	—0.18	—0.73	0.5
1786-'92	— .61	— .037	8	+1.51	0.2	—0.12	—0.88	0
1793-'97	— .55	— .114	5	+1.87	0.2	—0.22	—1.27	0
1798-'02	— .50	+ .060	5	+1.02	0.2	—0.42	—1.15	0
1803-'06	— .45	— .002	5	+0.27	0.2	—0.03	—1.03	0
1807-'10	— .41	— .068	5	—0.34	0.2	—0.32	—1.12	0
1811-'14	— .37	— .095	3	—3.33	0.2	+0.17	—1.08	0
1815-'18	— .33	— .052	6	—1.99	0.5	—0.12	—0.34	0
1819-'22	— .29	+ .010	6	—0.51	1	+0.22	—0.19	1
1823-'26	— .25	— .054	6	—1.08	1	+0.05	—0.17	1
1827-'30	— .21	— .045	6	—0.42	1	—0.09	—0.75	1
1831-'34	— .17	+ .016	7	+0.76	1	+0.04	—0.27	1
1835-'38	— .13	+ .020	8	+1.16	1	+0.26	+0.06	2
1839-'42	— .09	+ .061	8	+0.84	1	+0.32	+0.10	2
1843-'46	— .05	— .008	8	+0.15	2	+0.25	+0.22	2
1847-'50	— .01	— .045	8	—0.10	2	+0.28	+0.02	3
1851-'54	+ .03	+ .024	8	+0.40	3	+0.22	+0.02	3
1855-'58	+ .07	— .032	9	+0.36	3	+0.15	+0.02	3
1859-'62	+ .11	— .043	9	—0.02	3	+0.25	+0.22	4
1863-'66	+ .15	— .016	8	+0.31	3	+0.23	—0.05	4
1867-'70	+ .19	+ .031	8	+0.35	3	+0.33	—0.10	4
1871-'74	+ .23	+ .021	8	+0.12	3	+0.24	+0.05	4
1875-'78	+ .27	— .008	8	—0.12	3	+0.26	+0.06	4
1879-'82	+ .31	+ .017	8	—0.05	3	+0.21	+0.14	4
1883-'88	+ .36	+ .001	13	—0.20	3	+0.18	+0.07	4
1889-'92	+ .41	— .025	8	—0.44	2	+0.24	+0.11	3

*Results of observations of the Sun's Right Ascension—Continued.*

## PARIS.

Years.	T	$\mu'$	$\tau_0$	$\epsilon$	$\tau_0$	$\delta\epsilon''$	$\epsilon''\delta\pi''$	$\tau_0$
1801-'07	— .46	— .025	14	—1.78	0.5	+0.08	—0.23	1
1808-'15	— .38	+ .015	17	—0.65	0.5	—0.01	+0.12	1
1816-'22	— .31	— .050	14	+0.18	0.5	—0.13	+0.32	1
1823-'29	— .24	— .050	10	+0.01	0.5	—0.31	—0.02	1
1837-'44	— .09	— .034	19	+0.33	1	—0.04	+0.10	1.5
1845-'52	+ .01	+ .009	15	+0.10	1	+0.04	+0.10	1.5
1853-'59	+ .06	+ .014	15	+0.66	1	—0.04	+0.32	2
1860-'65	+ .13	+ .003	10	+0.38	1	+0.07	+0.26	2
1866-'70	+ .18	.000	7	+2.29	0.3	+0.13	+0.40	2
1871-'79	+ .25	+ .048	11	—0.26	1	—0.06	+0.22	2
1880-'89	+ .35	+ .002	14	+0.44	1	+0.24	+0.03	2

## PALERMO.

1791-'96	— .56	— .079	0	—0.07	0	—0.06	—0.85	0
1797-'01	— .51	— .116	0	—2.33	0	—0.29	—0.28	0
1802-'05	— .46	— .001	0	—3.11	0	—0.05	—0.76	0
1806-'12	— .41	+ .243	0	+5.92	0	—1.17	+1.55	0

## CAMBRIDGE.

1828-'34	— .21	+ .007	16	—0.13	2	+0.08	+0.12	4
1835-'40	— .12	— .033	14	—0.18	2	+0.06	—0.06	4
1842-'47	— .05	— .026	9	—0.21	2	+0.08	—0.12	4
1850-'58	+ .04	— .024	20	—0.11	2	+0.17	—0.04	4

## WASHINGTON.

1846-'52	— .01	— .038	5	—0.85	2	+0.20	0.00	3
1861-'65	+ .13	— .038	8	—0.53	4	+0.01	0.00	5
1866-'73	+ .20	— .004	13	—0.22	4	+0.18	—0.03	6
1874-'81	+ .28	— .033	12	—0.45	4	+0.07	—0.16	5
1882-'91	+ .37	— .002	17	—0.79	4	+0.07	—0.07	5

## KÖNIGSBERG.

1816-'23	— .30	+ .002	13	+0.30	1	+0.07	—0.28	3
1824-'30	— .23	— .006	12	+0.02	1	—0.16	+0.11	3
1831-'38	— .15	— .021	15	+0.23	1	—0.12	+0.03	3
1839-'45	— .08	— .021	12	+0.77	1	+0.08	+0.20	3

Results of observations of the Sun's Right Ascension—Continued.

OXFORD.

Years.	T	$\mu'$	w	$c$	w	$\delta e''$	$e''\delta\pi''$	w
1840-'49	— .05	— .043	12	+2.49	0.3	+0.24	—0.17	2
1860-'68	+ .14	+ .042	13	+1.96	0.3	+0.08	—0.13	2
1869-'76	+ .23	+ .054	15	+0.92	0.3	+0.20	—0.04	2
1880-'87	+ .34	— .014	9	—0.31	0.3	+0.27	+0.64	2

PULKOWA.

1842-'50	— .04	+ .047	11	+1.20	1	—0.12	+0.20	3
1861-'70	+ .16	+ .002	10	—0.40	1	+0.05	+0.28	3

DORPAT.

1823-'30	— .23	+ .021	9	+0.36	1	—0.12	—0.22	2
1831-'38	— .15	+ .008	6	+0.45	1	+0.02	+0.03	2

CAPE OF GOOD HOPE.

1884-'90	+ .37	— .026	12	—0.36	3	+0.02	+0.01	4
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STRASSBURG.

1883-'88	+ .36	— .014	12	—1.65	2	+0.23	+0.09	3
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The mass of Venus.

13. The mean results for the mass of Venus given by the work at the several observatories are shown as follows:

The probable error, where given at all, is that derived from the discordance of the separate individual results at the particular observatory. In some cases there are only one or two results; here no probable error could be assigned.

$w'$  is the sum of the weights of the result at each separate observatory, as given by the equations of condition. Were all the observations of equal accuracy, these would be the weights to be assigned to the separate results. Such not be-

ing the case, we choose for the actual weights certain numbers, founded partly on a compromise between the mean errors following each result or upon the values of  $w'$ , partly on a judgment of the accuracy of the observations.

*Values of  $\mu'$  for the mass of Venus.*

	$\mu'$	$W'$	$w$
Greenwich .....	$-.015 \pm .006$	226	11
Paris .....	$-.007 \pm .009$	146	5
Königsberg .....	$-.012 \pm .010$	52	3
Cambridge .....	$-.018 \pm .009$	59	6
Dorpat .....	$+.016$	15	1
Pulkowa .....	$+.025$	21	1
Oxford .....	$+.014 \pm .023$	49	1
Washington .....	$-.018 \pm .009$	55	4
Cape .....	$-.026$	12	1
Strassburg .....	$-.014$	12	1

Using the weights in the last column, we have for the mean result

$$\mu = -.0118 \pm .0034.$$

The mean error  $\pm .0034$  is that given by the discordance of the separate results of the preceding table.

*Corrections of relative Right Ascensions.*

14. The true values of the remaining quantities  $c$ ,  $\delta e''$ , and  $e''\delta\pi''$  are to be regarded as increasing uniformly with the time and therefore of the form

$$x + Ty.$$

Here  $T$  is the time, and in the treatment of these particular equations it is counted from 1850 in units of one century, so that  $x$  is the value of the correction at this mean epoch.

The quantity designated by  $c$  is the same which, elsewhere in this discussion, is represented by  $\delta l + \alpha$ , so that

$$c = \delta l'' + \alpha$$

I shall, however, for convenience, continue to use the designation  $c$ , or  $x + Ty$ .

As the observations at Greenwich and Paris extend over longer periods than at any other observatories, I shall first solve them separately. The totality of the Greenwich observations give for  $c$  the following normal equations and solution:

$$43.4 x + 1.65 y = + 4''.23$$

$$1.65 + 4.24 = - 1''.25$$

$$x = + 0''.11$$

$$y = - 0''.34$$

Those at Paris give the equations and solution

$$8.3 x + 0.04 y = + 1''.22$$

$$0.04 + 0.48 = + 0''.77$$

$$x = + 0''.14$$

$$y = + 1''.59$$

If we combine all the other results into a single set of normal equations, we have

$$40.2 x + 4.26 y = - 10''.84$$

$$4.26 + 2.20 = - 3''.98$$

$$x = - 0''.10$$

$$y = - 1''.62$$

It will be seen that the results for  $y$ , the secular motion, are markedly discordant. Indeed, if we refer to the exhibit of results, p. 23, we shall see that the values of  $c$  are much more discordant than those of the other two quantities. To obtain a definite value, founded on all the observations of the Sun's Right Ascension, I do not see that any better result can be obtained than that found from a general solution of the combined normal equations. The equations and their solution are as follows:

$$91.9 x + 5.95 y = - 5''.39$$

$$5.95 + 6.92 = - 4''.46$$

$$x = - 0''.02$$

$$y = - 0''.63$$

or

$$\delta l'' + \alpha = - 0''.02 - 0''.63 T$$

*Corrections to the solar eccentricity and perigee.*

15. I have already mentioned the remarkable consistency of the corrections to these elements given by the results at different observatories and at different epochs. The eccentricity is more consistent than the perigee. One cause for this, the consideration of which will throw some light on the relative merits of the observations, is that the error of Right Ascension depending on the Declination of the object observed affects the eccentricity less than the perigee. It is well known, from a comparison of the results, that the systematic differences in the Right Ascensions of different star catalogues vary somewhat with the Declination. Now, since the Sun's Declination goes through an annual period, it follows that this error will produce a systematic effect on both the eccentricity and the perigee. But the effect will be much larger in the case of the latter element than in the case of the former, because of the nearness of the perigee to the winter solstice, the difference being only some  $10^\circ$  or  $12^\circ$ . Consequently the extreme coefficients in the correction to the eccentricity have nearly the same values, with opposite signs, for the same Declinations in different seasons of the year. But it is different with the perigee. The coefficient of this quantity is negative from October until March, when the Sun is in south Declination, attaining its maximum value about January 1; while it is positive during the remaining months when the Sun's Declination is north, attaining its maximum value about July 1. A systematic difference in the errors of Right Ascension will therefore produce its full effect on the longitude of the perigee, while its effect on the eccentricity will be but slight.

In this connection, the very large negative values of the correction to the perigee during the period when the old Greenwich transit instrument was in use are quite remarkable. The progressive change in the value of  $e$  is also remarkable in this connection. It is to be remarked that the new transit was mounted in 1816, but account was not taken of this fact in grouping the equations. Hence it is only from the year 1819 that the results of the table are derived wholly from observations with the new instrument. The anomaly alluded to is

then seen to disappear. The fact that the abnormally large corrections in  $c$  are positive before 1800 and negative after it, while  $e''\delta\pi''$  is abnormally negative through the doubtful period 1765–1815, complicates the theory of these errors. I have not been able to consider them in detail, but have simply rejected the results for  $\delta e''$  and  $e''\delta\pi''$  from 1786 to 1818, having given them a gradually diminishing weight from BRADLEY'S observations to the first epoch.

As in the case of  $c$ , I have made a solution for Greenwich alone, Paris alone, the other observatories combined, and all combined. The results are shown as follows:

1. From Greenwich observations:

$$\begin{aligned} 54.5x + 2.73y &= + 11''.14; - 0''.88 \\ 2.73 + 5.72 &= + 1''.82; + 2''.69 \\ x &= + 0''.19; - 0''.04 \\ y &= + 0''.22; + 0''.49 \end{aligned}$$

2. From Paris observations:

$$\begin{aligned} 17.0x + 0.39y &= + 0''.30; + 2''.95 \\ 0.39 + 0.99 &= + 0''.29; + 0''.33 \\ x &= + 0''.01; + 0''.17 \\ y &= + 0''.29; + 0''.27 \end{aligned}$$

3. The equations and results from all the other modern observations are—

$$\begin{aligned} 77.0x + 4.99y &= + 5''.58; + 0''.35 \\ 4.99 + 3.68 &= + 1''.09; + 0''.40 \\ x &= + 0''.06; 0''.00 \\ y &= + 0''.22; + 0''.05 \end{aligned}$$

4. Finally, if we combine all the equations, we have—

$$\begin{aligned} & \delta e'' \qquad e'' \delta \pi'' \\ 148.5x + 8.11y &= + 17''.02; + 2''.42 \\ 8.1 + 10.39 &= + 3''.20; + 3''.42 \\ x &= + 0''.10; \quad 0''.00 \\ y &= + 0''.23; + 0''.33 \end{aligned}$$

In the case of the eccentricity the general accordance is quite satisfactory, and for the perigee it is much better than in the case  $c$ , the relative Right Ascension.

*Results of observed declinations of the Sun.*

16. The Sun's absolute longitude can be found only from observations of his declination, because this longitude is referred to the equinox, which is defined only by the Sun's crossing of the equator.

The corrections to the eccentricity and perigee, as just found, are so slight that they may be neglected in determining the correction of the absolute longitude from that of the declination. Thus, as already stated, the unknown quantities of the equations given by the declinations are the corrections of the mean longitude  $l''$ , and of the obliquity  $\epsilon$ , and a constant  $\Delta\delta$ , peculiar to each observatory, of which we take no further account. The equation of condition given by each observation or group of observations is

$$\Delta\delta + A \sin \epsilon \delta l'' + B \delta \epsilon = d\delta$$

where  $d\delta$  is the excess of the observed over the tabular declination, and

$$A = \operatorname{cosec} \epsilon \frac{d\delta}{d\lambda} = \cos \alpha$$

$$B = \frac{d\epsilon}{d\delta} = \sin \alpha$$

The equations are grouped and solved for periods, as in the case of the Right Ascensions, with the results shown in the following table:

*Results of observations of the Sun's Declination.*

GREENWICH.

Years.	T	$\delta''$	$w$	$\delta\epsilon$	$w$	$\Delta\delta$	$\delta'\epsilon$	$w$
		//		//		//	//	
1753-'57	— .95	+0.78	1	—0.34	1	—2.43	—0.34	1
1758-'62	— .90	+1.50	1	—1.81	1	—1.94	—1.81	1
1765-'70	— .82	—0.23	1	—0.95	0.5	+0.20	—0.95	0.5
1771-'78	— .75	+0.48	1	—0.93	0.5	+1.25	—0.93	0.5
1779-'85	— .68	+1.23	1	—1.09	0.5	—0.99	—1.09	0.5
1786-'91	— .61	+0.48	1	—0.50	0.3	+0.15	—0.50	0.3
1792-'97	— .55	+1.12	1	—0.70	0.2	—0.35	—0.70	0.2
1798-'03	— .49	+0.41	1	—1.02	0.1	—0.10	—1.02	0.1
1804-'10	— .43	+0.18	1	—1.41	0.1	—0.84	—1.41	0.1
1812-'16	— .36	—0.15	3	—0.53	3	+0.48	—0.53	3
1817-'22	— .30	—0.41	3	+0.03	3	+0.40	+0.03	3
1823-'28	— .24	+0.43	3	—0.10	3	+0.08	—0.10	3
1829-'34	— .18	—0.08	3	+0.21	3	+0.25	+0.21	3
1835-'40	— .12	—0.12	3	—0.20	3	+0.37	—0.13	3
1841-'46	— .6	+0.21	3	+0.13	3	+0.47	+0.12	4
1847-'52	0	+0.25	4	0.00	4	—0.24	—0.15	4
1853-'58	+ .6	+0.55	5	+0.18	5	—0.26	—0.35	5
1859-'64	+ .12	+0.03	5	+0.28	5	—0.46	+0.12	5
1865-'70	+ .18	—0.23	5	—0.15	5	+0.05	—0.36	5
1871-'76	+ .24	—0.15	5	+0.26	5	+0.16	—0.16	5
1877-'82	+ .30	—0.90	5	+0.22	5	+0.34	+0.08	5
1883-'88	+ .36	—0.27	5	+0.33	5	—0.14	+0.02	5
1889-'92	+ .41	—0.05	3	+0.19	3	+0.13	—0.07	3

PARIS.

1800-'03	— .48	+0.01	1	—1.93	1	—0.45	-----	-----
1804-'07	— .44	+0.70	1	+0.82	1	—2.02	-----	-----
1808-'10	— .41	+2.66	1	+1.60	1	—0.95	-----	-----
1811-'15	— .37	—0.92	1	—1.20	1	—1.18	-----	-----
1816-'21	— .31	+0.58	1	+1.68	1	—1.42	-----	-----
1822-'28	— .25	+1.09	3	+0.39	3	—0.01	-----	-----
1837-'42	— .10	+0.79	3	—0.15	3	+0.40	-----	-----
1843-'48	— .4	+0.43	3	—0.03	3	+0.19	-----	-----
1849-'54	+ .2	+1.19	2	—0.01	2	+1.34	-----	-----
1855-'60	+ .8	+0.35	3	—0.02	3	+1.22	-----	-----
1861-'66	+ .14	+1.35	3	0.00	3	+0.12	-----	-----
1867-'72	+ .20	+0.31	2	—0.67	2	+0.10	-----	-----
1873-'77	+ .25	—0.59	2	+0.04	2	+1.01	-----	-----
1878-'83	+ .31	—0.09	2	—0.32	2	+0.58	-----	-----
1884-'89	+ .37	—0.80	2	+0.32	2	+0.78	-----	-----

*Results of observations of the Sun's Declination—Continued.*

PALERMO.

Years.	T	$\delta''$	W	$\delta_{\epsilon}$	W	$\Delta\delta$	$\delta'_{\epsilon}$	W
		//		//		//	//	
1791-'03	— .53	—1.46	0	—0.95	-----	+0.78	—0.95	0.4
1804-'13	— .41	+1.70	0	—0.52	-----	+0.42	—0.52	0.4

CAMBRIDGE.

1833-'38	— .14	—0.21	2	—0.33	-----	+0.59	—0.54	1
1839-'44	— .08	+0.31	2	—0.20	-----	+0.29	—0.41	1
1847-'53	00	+0.21	2	+0.31	-----	—0.32	+0.10	1
1854-'58	+ .06	—0.15	2	+0.34	-----	—0.42	+0.13	1

WASHINGTON.

1846-'49	— .02	—0.28	4	—0.73	-----	—0.47	—0.81	2
1861-'66	+ .14	—0.11	4	—0.43	-----	—0.45	—0.25	2
1867-'72	+ .20	+0.74	4	—0.39	-----	+0.28	—0.51	2
1873-'78	+ .26	—0.58	4	—0.32	-----	+0.10	—0.45	2
1879-'84	+ .32	—0.31	4	—0.60	-----	—0.35	—0.72	2
1885-'91	+ .38	—0.02	4	—0.05	-----	—0.20	—0.18	2

KÖNIGSBERG.

1815-----	— .35	-----	-----	-----	-----	-----	—1.07	0.5
1820-'23	— .28	—0.14	2	—0.22	-----	—0.59	—0.47	1
1824-'27	— .24	+0.65	2	+0.49	-----	—0.60	+0.24	1
1828-'31	— .20	+1.08	2	+0.09	-----	—0.64	—0.16	1
1832-'34	— .17	—0.72	2	—0.15	-----	—1.32	—0.40	1
1837-'44	— .09	—0.66	2	—0.62	-----	—2.24	—0.87	1

OXFORD.

1840-'45	— .07	+0.79	2	+0.42	-----	+0.67	+0.22	0.2
1846-'51	— .01	+0.35	2	+0.40	-----	+0.89	+0.20	0.2
1861-'66	+ .14	+0.36	2	—0.81	-----	+0.10	—1.01	0.2
1867-'72	+ .20	—0.16	2	—0.24	-----	+0.29	—0.44	0.2
1873-'76	+ .25	—0.38	2	—0.33	-----	+0.29	—0.53	0.2
1880-'83	+ .32	—0.43	2	+0.12	-----	—0.17	—0.08	0.2
1884-'87	+ .36	—0.24	2	+0.23	-----	—0.19	+0.03	0.2

*Results of observations of the Sun's Declination—Continued.*

## PULKOWA.

Years.	T	$\delta l''$	W	$\delta \epsilon$	W	$\Delta \delta$	$\delta' \epsilon$	W
		''		''		''	''	
1842-'45	— .06	+0.82	2	—0.35	-----	—0.01	—0.35	1
1846-'49	— .02	—0.10	2	—0.48	-----	+0.07	—0.48	1
1861-'65	+ .13	—0.53	2	—0.48	-----	—0.30	—0.48	1
1866-'70	+ .18	+0.27	2	—0.31	-----	—0.38	—0.31	1

## DORPAT.

1823-'28	— .24	+0.99	2	—1.26	-----	+0.59	—1.41	1
1829-'32	— .19	+0.99	2	—0.76	-----	+1.34	—0.91	1
1833-'38	— .14	+1.00	2	—0.63	-----	+1.34	—0.78	1

## CAPE OF GOOD HOPE.

1884-'87	+ .36	—0.51	4	+0.05	-----	+0.11	—0.07	2
1888-'90	+ .39	—0.84	4	+0.09	-----	+0.19	—0.21	2

## STRASBURG.

1884-'88	+ .36	—0.57	4	—0.05	-----	—0.77	+0.12	2
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## LEIDEN.

186-'69	+ .17	+0.14	4	—0.01	-----	+0.27	—0.24	2
1870-'76	+ .23	—0.23	4	—0.06	-----	—0.04	—0.29	2

*Correction to the Sun's absolute longitude*

17. So far as mere instrumental measurement is concerned, the correction  $\delta \epsilon$  should be determined with greater precision than  $\delta l''$  in the ratio 5:2, because the errors in declination have to be divided by the factor  $\sin \epsilon = 0.40$ , in order to form  $\delta l''$ . Allowing for this large increase in the source of error, the values of  $\delta l''$  are more accordant than those of  $\delta \epsilon$ . This is what we should expect. The values of the former quantity depend mainly upon the comparison of observations made

near the opposite equinoxes, when the sun has the same declination, and when the season is not greatly different. Indeed, if the season changed exactly with the sun's declination, all effects of annual change of temperature would be completely eliminated from  $\delta l''$ , as would also in any case any constant error which is a function simply of the Sun's Declination. It is therefore to be expected that the actual probable error of this quantity will conform more nearly to that determined from the residuals than in the case of the other.

For these reasons the value of  $\delta l''$  does not give rise to much discussion. The general result from all the observatories is, for  $\delta l''$ , when developed in the form  $x + y T$ .

$$x = + 0''.05$$

$$y = - 0''.97.$$

*Obliquity of the ecliptic.*

18. The determination of the obliquity rests upon an essentially different basis from that of the absolute longitude, in that it depends upon actual differences of measured Declinations, which differences are still further complicated by the fact that they are necessarily made at opposite seasons. A more detailed discussion of them is therefore necessary, and some modification may have to be made in the separate results as adopted. The following special circumstances affecting the observations are to be taken into consideration:

The BRADLEY Greenwich results for 1753-'62, are derived from a manuscript communicated by Dr. AUWERS, containing the results of his very careful reduction of BRADLEY's observed Declinations of the Sun, which were compared with HANSEN'S tables. The corrections were reduced to those of LEVERRIER'S tables by being computed at intervals sufficiently short to permit of the reduction being interpolated with all necessary precision. No reduction was applied either on account of the constant error of the Declinations determined by Dr. AUWERS himself, nor for reduction to the BOSS system of standard Declinations. Hence arises the large value of  $\Delta\delta$  given by these Declinations. Consequently the value of  $\delta\epsilon$  is

that given immediately by the instrument, on the system of reduction adopted by Dr. AUWERS, in which I have supposed that the Pulkowa refractions were used.

From 1765 to 1816 the Greenwich observations were made with the imperfect quadrant, the Declinations of which are subjected to an error which is not constant. The necessary corrections are derived by SAFFORD in Vol. II of the *Astronomical Papers*. The corrections are those necessary to reduce to BOSS's system, and they vary with the Declination. Hence the arc on which the obliquity depends is not that measured with the instrument itself, but that so corrected as to reproduce as nearly as may be the standard Declinations.

From 1812 onward the two mural circles were used. Up to 1830 no correction except the constant one derived by SAFFORD was applied to the Declinations as measured with these instruments. Hence the arc of obliquity is that measured with the instrument itself without being corrected by the standard stars.

After 1830 the Declinations were corrected by the tables for Greenwich given in BOSS's paper. These corrections vary somewhat with the Declination, and they are different also for different periods. Hence we have here a period during which the instrumental differences of Declination were corrected to reduce them to the standard star-system.

If the standard system were subject to no further error than a constant one, common to all Declinations within the zodiac, which common correction would be subject to a uniform change with the time, this system would doubtless be the best one to adopt in order to obtain the secular variation in the obliquity of the ecliptic. But, as a matter of fact, the standard Declinations are simply the mean results of Declinations measured with different instruments. It is, therefore, a question whether we shall get any better results by applying reductions to a standard system than we should get by simply taking the mean of the instrumental results, because the system is itself only a mean of such results. It is true that the standard system depends on more instruments than the obliquity, though not on better ones; but it is also to be considered that the reductions in the case of the Sun may be different from those

in the case of the stars, owing to the very different conditions in which the observations are made.

Another troublesome point arises from the refraction used in the reductions. The effect of refraction is always to make the measured obliquity less than the actual one; the correction to the obliquity on account of refraction is therefore a positive quantity, which is a minimum for an observatory at the equator and increase equally towards each pole. Some values of the obliquity were derived from BESSEL'S refractions of the *Tabulæ Regiomontanæ*, and others from the Pulkowa tables. Since the secular variation of the obliquity is more important than the absolute value of the quantity, it is essential that the standard to which all determinations of the obliquity are reduced should be as nearly as possible the same, and therefore that the same refraction should be used. But in reductions to standard star places we meet with the additional complication that the differences in the constant of refraction might be wholly or partially eliminated by the reductions to a standard system. It would therefore be a difficult question how far we should modify the values of  $\delta\epsilon$  on account of the use of different tables of refraction.

To avoid all these difficulties I have judged it best to make the obliquity depend mainly upon absolute measures, the reductions being made with the Pulkowa refractions.

*Effect of refraction on the obliquity.*

19. The determination of the average or most probable effect on the obliquity produced by using the Pulkowa refractions, instead of those of the *Tabulæ Regiomontanæ*, is easily determined. We divide the ecliptic into a number of equal arcs throughout the year, and by equations of condition express differences of refraction in terms of differences of Declination, and hence differences of obliquity. We thus find that at certain latitudes where observations were made, and where BESSEL'S refractions were used in the reduction, the following corrections are necessary to reduce the obliquity to the ones given by the Pulkowa refractions:

Pulkowa;	$\varphi = 59^{\circ}.8$ ; $\Delta\epsilon = - 0''.325$
Greenwich;	$\varphi = 51^{\circ}.5$ ; $\Delta\epsilon = - 0''.20$
Washington;	$\varphi = 38^{\circ}.9$ ; $\Delta\epsilon = - 0''.125$

Hence I conclude that for

Dorpat;  $\Delta\epsilon = - 0''.29$

Königsberg;  $\Delta\epsilon = - 0''.26$

Cambridge;  $\Delta\epsilon = - 0''.21$

Cape Town;  $\Delta\epsilon = - 0''.12$

The corrections to the obliquity thus derived, depending mainly on direct instrumental measurement, and reduced to the Pulkowa refractions, are designated as  $\delta'\epsilon$ . The results for this quantity are given in the last column of the several tables.

In the case of BRADLEY'S Greenwich results, I have taken as  $\delta'\epsilon$  Dr. AUWERS'S results unchanged, assuming in the absence of any specific statement that he has used the Paltowa refraction tables.

In the case of MASKYLENE'S observations, I have, by exception, used them as reduced to the standard star-system, because we have no other results at these times, and the error of his instrument is so strongly shown that it would not do to use the results unchanged. It will be seen, however, that small weights are assigned, and that the weights diminish towards the end of the series.

In the case of the Greenwich observations from 1812 to about 1834, no change has to be made, as the results are generally or always purely instrumental, and Pulkowa refractions are used in SAFFORD'S work.

From 1835 onward I have depended mainly on certain corrected Greenwich reductions. First, for  $\delta'\epsilon$ , I have used the results given by Mr. CHRISTIE in his very valuable paper on the Greenwich Declinations, in M. R. A. S., Vol. XLV, where the Declinations from 1836 to 1879 are reduced on a uniform system. Later, I have adopted the corrected results given in Appendix III to the Greenwich observations for 1887. In each case the result has been reduced to the Pulkowa refractions.

The Paris results rest on a different basis from the others, in that the zero point of the instrument depends wholly upon LEVERRIER'S Declinations of the stars, and I fear it was not always accurately determined. Observations near the winter solstice are mostly referred to one set of stars; those near the

summer to another set, the error of which may be systematically different. Certain it is that the results during the early years were very discordant. The weights as given in the table are those assigned *a priori*, without sufficient reference to the discordance of the older results. I have felt constrained to evade a decision as to their treatment by entirely omitting their results in the final discussion.

In the case of some other observatories it was difficult to determine exactly what refractions had been used in each special case and what reductions should be made. I have, however, determined the corrections in the best way I was able.

A precise determination of the secular change in the obliquity is of more importance for our present object than a precise determination of its amount. Hence a series of observations extending through a long period of time, and made on a uniform system, has an advantage over a number of isolated values, in that any constant error with which it may be affected will be eliminated from the secular variation. Possible constant differences between the determinations of the various observatories at different epochs will vitiate the secular variation, but the probable amount of this error may be diminished by using a number of separate determinations, such as are presented in the preceding table. In the Greenwich transit circle we have a very uniform series, extending over a period of forty years, but giving results systematically different from other determinations. This series gives for the correction to the obliquity:

Transit Circle, 1847-'91:

$$\delta'\epsilon = -0''.11 \pm 0''.06 + (0''.21 \pm 0''.46)T \quad . \quad . \quad (a)$$

Here, in view of the uniformity of method and reduction, we may regard the mean error of the centennial variation from the discordance alone as a fair approximation to the probable mean error. It will be seen that I have here included four years (1847-'50) of the Mural Circle results.

Continuing the Greenwich series backward, the question arises whether we can regard the results of the mural circle from 1812 to 1850 as comparable with those of the transit circle.

There is certainly nothing in the table to indicate any systematic difference. From the combination of the two we have—

M. C. and T. C., 1812–50:

$$\delta'\epsilon = -0''.08 \pm 0''.05 + (+0''.14 \pm 0''.23) T (1850) \dots (b)$$

Here the mean error is naturally smaller than in the case of the transit circle alone, but is now more subject to possible systematic difference between the two instruments.

If we now go back to BRADLEY, we meet with the very difficult question, whether we should regard his results as best comparable with the modern Greenwich observations, or with modern observations in general. If we assume that the difference between the Greenwich and other modern results is due to any cause which has remained unchanged since BRADLEY, we should reach one conclusion; otherwise, we should reach the other. The result of combining all Greenwich observations, with the weights as assigned, is—

$$\delta'\epsilon = -0''.11 + 0''.50 T \dots \dots \dots (c)$$

In this combination I have used the weak results of MASKELYNE, with the small weights assigned, although they depend wholly upon the standard declinations of stars. In view of the discordance between BRADLEY'S two results, this seems the only admissible course.

Next in the length of time which they include come the Paris observations, of which the results, with the weights assigned, are—

$$\delta\epsilon = +0''.01 - 0''.36 T$$

I give this result in order that nothing may be omitted. Undue weight has probably been assigned to the earlier determinations; in any case the method of deriving it from the original observations is so objectionable that no further use is made of it. A satisfactory discussion of the observations would require a complete redetermination of the zero points of the instrument from fundamental stars.

If we omit the Greenwich, Paris, and Palermo results, and combine all the others into a single set of equations of condition, we have the equations and results:

$$36.9x + 0.26y = -14''.37$$

$$0.26 + 1.88 = +1''.01$$

$$x = -0''.39$$

$$y = +0''.59$$

Here  $x$  is the value of  $\delta'\epsilon$  for 1860, and  $y$  its centennial variation. Transferring the epoch to 1850, as usual, the result is—

$$\delta'\epsilon = -0''.45 + 0''.59T \quad . \quad . \quad . \quad . \quad (d)$$

No reliable mean error can be computed, owing to systematic errors. In view of these, one mode of treatment would be to form equations of condition in which a possible systematic error at each observatory would appear as one of the unknown quantities. By this process we should get the same result for the secular variation as if we made an independent determination from the work of each observatory. At most of the observatories the period through which the observations are made, with one instrument and on an unchanged plan, is too short to render such a course advisable.

As a last combination, we shall combine the earlier Greenwich results, up to 1810, with Palermo and with all the modern results except Paris, first dividing the weights of the Greenwich results by 2. We then have the equations—

$$39.8x - 1.82y = -17''.12$$

$$-1.8 + 3.47 = +2''.99$$

$$x = -0''.40$$

$$y = +0''.65 \quad . \quad . \quad . \quad . \quad . \quad (e)$$

*Concluded results for the obliquity.*

20. The data on which these various results for the obliquity rest show the following noteworthy features:

(1) That the correction given by the modern Greenwich instruments, mural and transit circles, is markedly greater

than that given by other modern observations. This may be most plausibly attributed to the atmospheric conditions within the observing room.

(2) The minuteness of the change of the correction given by these instruments during nearly eighty years. To this circumstance is due the smallness of the centennial variation,  $0''.50$ , found from the totality of the Greenwich observations. A comparison of BRADLEY with the mean of the T. C. results only would have given a change of  $0''.97$  in 117 years, or a centennial change of about  $0''.80$ .

The long period, uniformity of plan, and systematic deviation of the modern Greenwich observations lead me to consider them as forming a series distinct from all others. We have therefore the following two completely independent determinations of the centennial variation:

(1) Modern Greenwich results:  $\dot{\gamma} = + 0''.14 \pm 0''.23$

(2) All other results  $+ 0''.65$

To the latter no reliable mean error can be assigned. To judge its reliability we may compare it with the results (a), (c), and (d)—

Greenwich T. C., alone,  $+ 0''.21 \pm 0''.46$

Greenwich observations in general,  $+ 0''.50$

Miscellaneous modern observations,  $+ 0''.59$

We may, it would seem, fairly give double weight to the result (2), thus obtaining, as the definite result from observations of the Sun alone:

Correction to LEVERRIER'S centennial variation of the obliquity of the ecliptic ( $- 47''.594$ )

$$+ 0''.48 \pm 0''.30$$

the mean error being an estimate from the general discordance of the data.

For the constant part of the correction I take—

$$\delta\epsilon(1850) = - 0''.30$$

*Summary and comparison of results.*

21. From what precedes we have the following as the values of the unknown quantities, and of their secular variations, as given by observations of the Sun alone.

	Value for 1850.	Cent. var.
$\delta e'' = + 0''.10 \pm 0''.03$		$+ 0''.23 \pm 0''.10$
$e''(\delta\pi'' + \alpha) = 0''.00 \pm 0''.07$		$+ 0''.33 \pm 0''.12$
$\delta l'' + \alpha = - 0''.02$		$- 0''.63$
$\delta l'' = + 0''.05 \pm 0''.12$		$- 0''.97 \pm 0''.23$
$\delta\epsilon = - 0''.30 \pm 0''.15$		$+ 0''.48 \pm 0''.30$
$\alpha = - 0''.07$		$+ 0''.34$

No estimate of the probable errors of these quantities would be useful which did not take account of the systematic differences between the results of different observatories. We have therefore formed the mean outstanding residual corrections given by the several observatories, as shown in the tables which follow. Originally the scale of weights used for the Greenwich observations did not correspond to that for the other observatories; they were, therefore, divided by 2. As used below, however, the change has been made in the case of  $\delta l''$  by multiplying all the weights of the other observatories by 2, and, in the case of  $\delta\epsilon$ , by dividing the Greenwich weights by 2.

The correction to the obliquity depends solely on  $\delta'\epsilon$ ; but the comparison has also been made with the values of  $\delta\epsilon$ , which, it will be remarked, differ from the others in that account is taken of the supposed variation of the systematic correction with the declination. It is noteworthy that the results are somewhat more accordant when this correction is omitted and purely instrumental errors are used for the obliquity.

The mean errors given in the preceding summary of results are derived from the discordances in question, and may be regarded as substantially real.

No use was made of the Paris results for  $\delta l''$  and  $\delta\epsilon$  for the reason that they depend on declinations referred to star

places which may be affected by differences in different Right Ascensions. They are, however, retained in the table to show the amounts of outstanding discordance.

*Outstanding mean residual corrections to quantities depending on the Sun's Right Ascension.*

	$\delta e''$	$e''\delta\pi''$	$\Sigma w$
Greenwich	+ 0''.09	-- 0''.03	54.5
Paris	- 0''.09	+ 0''.17	17
Cambridge	+ 0''.02	0''.00	16
Washington	- 0''.05	- 0''.12	24
Königsberg	- 0''.08	+ 0''.08	12
Oxford	+ 0''.06	+ 0''.02	8
Pulkowa	- 0''.15	+ 0''.22	6
Dorpat	- 0''.10	- 0''.03	4
Cape	-- 0''.16	- 0''.11	4
Strassburg	+ 0''.05	- 0''.03	3
Mean errors for			
weight unity	$\epsilon_1 = \pm 0''.34$	$\pm 0''.39$	
Mean error of $x$	$\pm 0''.03$	$\pm 0''.03$	
Mean error of $y$	$\pm 0''.10$	$\pm 0''.12$	

*Outstanding mean residual corrections to quantities depending on the Sun's Declination.*

	$\delta l''$	$w$	$\delta\epsilon$	$w$	$\delta'\epsilon$
Greenwich	- 0''.06	64	+ 0''.31	29.6	+ 0''.17
Paris	+ 0''.45	0	+ 0''.31	0	
Palermo	- 0''.39	0	- 0''.20	0.8	- 0''.20
Cambridge	- 0''.05	8	+ 0''.35	4	+ 0''.14
Washington	+ 0''.07	24	- 0''.22	12	- 0''.29
Königsberg	- 0''.20	10	+ 0''.31	5.5	0''.00
Oxford	+ 0''.14	14	+ 0''.19	1.4	- 0''.01
Pulkowa	+ 0''.12	8	- 0''.13	4	- 0''.13
Dorpat	+ 0''.75	6	- 0''.49	3	- 0''.64
Cape	- 0''.35	8	+ 0''.10	4	- 0''.02
Leiden	+ 0''.10	8	+ 0''.17	2	- 0''.06
Strassburg	- 0''.26	4	+ 0''.08	4	+ 0''.25
$\epsilon$ for weight unity	$\pm 0''.81$		$\pm 0''.74$		$\pm 0''.60$