Personal Equation in Astronomy*

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Introduction

In view of the great mass of literature which has been written about personal equation in astronomy, a history of this subject must be able to justify its appearance at this time. In the eyes of most astronomers, personal equation seems largely a problem of the past, which through the use of improved observational methods, has been almost entirely eliminated. In its early history this phenomenon was inextricably bound up with meridian transit work, and the successful efforts to reduce and control it in this field have led many people to ignore its still significant presence in other branches of astronomy.

The two most complete accounts of the history of personal equation have been written by psychologists, E. C. Sanford[29] and E. G. Boring,[30] but they both break off near the end of the last century. Again, these men have written of personal equation essentially from the point of view of its importance to the psychology of reaction time, and not from the standpoint of the astronomer. It has thus been deemed justifiable to review the early history of personal equation as viewed in the light of more recent psychological thought, and to continue forward to the present time the account of the research in meridan transit work and other fields of astronomy done on this question.

No attempt has been made to give a thorough review of the early history, since this has already been well presented by Sanford and Boring. The mere outline of this early period given here has been drawn largely from their accounts, augmented by a number of additions from other sources.

SECTION I

The Bradley Method and Personal Equation

Any history of personal equation in astronomy must necessarily begin with an account of David Kinnebrook at Greenwich in 1795. In the Greenwich *Astronomical Observations,* Volume 3,[1] Maskelyne, the astronomer royal, entered the following statement below the observations for July 31, 1795:

"My assistant, Mr. David Kinnebrook, having at this time unfortunately commenced a vicious way of observing the times of transits too late, it will be necessary to make an allowance for those errors where his observations . . . are intermixed with mine . . ." Further on in the same volume, Maskelyne continues:

*Note: The references in this paper will be given in chronological order at the end of the final installment.

"I think it necessary to mention that my assistant, Mr. David Kinnebrook . . . began from the beginning of August last, to set them [the transits] down half a second of time later than he should do, according to my observations; and in January of the succeeding year, 1796, he increased his error to 8/10 of a second. As he had unfortunately continued a considerable time in this error before I noticed it, and did not seem to me likely ever to get over it, and return to a right method of observing, therefore, though with great reluctance . . . I parted with him." Thus an obscure assistant at Greenwich Observatory was ushered out of a job and into a unique place in astronomical history; the first recorded instance of personal equation.

The method of observing used at this time was introduced by Bradley. In the field of the transit instrument was a series of evenly spaced vertical wires. A clock beating seconds was placed near the instrument and as a star crossed the field the observer noted the proportional distance of the star from the wire at the two clock beats immediately preceding and following the transit across each wire. The transits for each wire were put down to $0^s.1$, and while Maskelyne admitted that the time for each wire might be in error by $0^s.2$, he assumed that the mean of the wires in the field would be accurate to $0^s.1$.

With this assumed accuracy it can readily be seen why Kinnebrook's error of $0^s.8$ could not be tolerated. In using the Bradley method, Maskelyne cautioned, "If we are not quick in fixing the place of the star at the [clock] beat, we shall be apt to assign to it too backward a place in the telescope, and consequently to reckon on the time of the transit too great, as my late assistant did." With this explanation, Maskelyne dismissed the question of discrepancies between the results of different obervers.

The question of personal errors remained dormant from the time of Kinnebrook's dismissal until 1816 when von Lindenau mentioned the incident in a history of the Greenwich Observatory. Bessel, the astronomer at Königsberg, took note of this and at once became interested in the problem. It occurred to Bessel that once Kinnebrook had been informed of his "error," he must have tried to correct for it. His failure to do so convinced Bessel that it must have been involuntary. With this thought in mind Bessel set out to find his own personal equation.

In 1820 he compared himself with Walbeck at Königsberg. Two groups of five stars each were chosen and both men observed these groups alternately for five nights. The results revealed that Bessel observed, on the average, $1^s.041$ earlier than Walbeck. It was fortunate for astronomy that this difference was large for it urged Bessel on to further study of personal equation. When published in the *Königsberg Observations* for 1822 it aroused a great deal of discussion and introduced a distrust of the accuracy of the Bradley method that led astronomers to seek a more accurate method of observation.

Bessel made other determinations of personal equation during the next decade and the series with Struve is especially notable. In four comparisons between 1821 and 1834 the equation Bessel minus Struve was $-0^s.799$, $-1^s.021$, $-0^s.891$, and $-0^s.770$. Bessel recognized the variability of this personal equation, but, in accord with other astronomers of the time, agreed that greater accuracy could be achieved if transit observations were corrected for the differences between observers. During this same period Bessel found that his personal equation with respect to Argelander for transit work was $-1^s.223$, whereas for occultations the same relation was only $-0^s.222$. Bessel also found that by using a clock beating half-seconds, he observed transits about $0^s.05$ later than usual.

Other observatories followed Bessel's example and it seems certain that no longitude determination work has been done since that time that does not take personal equation into account. Robinson,[13] at the Armagh Observatory in Ireland in 1830, made a study of personal equation in observations of the first and second limbs of the sun and moon. At Altona, in 1833 and later, the equations Nehus — Wolfers $= +0^s.73$, and Peterson — Mädler $= +0^s.52$ were determined. In publishing the longitudes of Göttingen, Marburg, and Mannheim, in 1838, Gerling corrected the observations for the relative difference between himself and the other observers. Also in 1838, Airy began publishing in the *Greenwich Observations* the personal equations of his transit observers as determined from the clock corrections. This has furnished two long series of comparisons; first Main — Rogerson averaging $-0^s.7$, and Main — Henry averaging about $-0^s.06$.

In 1845 Lt. Maury[5] wrote of a test made for personal equation at the U.S. Naval Observatory which "consisted in casting, by a proper adjustment of the eyepiece, a sharp image of the sun and wires at the time of transit, upon a screen placed at a little distance from the telescope, and to require each of the four officers to observe and record in silence the transit of the Sun." This method avoids the necessity of dealing with the clock correction, as did the binocular eyepiece introduced at Greenwich in 1852.

These determinations and others in this period revealed that the personal equation was more variable than the instrumental constants and thus did not lend itself readily to the system of corrections applied to the constants. Faced with this problem, astronomers began looking for some method of transit observation that would diminish and control the personal equation.

It should be noted here that the personal equations found in the early part of this period are not definitive because of the method used in correcting instrumental constants. It was the custom at that time to adjust the transit instrument frequently so that the collimation, level, and azimuth factors equalled zero. In Volume 3 of the Greenwich *Astronomical Observations,* Maskelyne time and again notes with some amazement that he has found the collimation factor appreciably large after having just set it to zero several weeks before. J. M. Gilliss,[6] writing about his observations at Washington from 1838 to 1842 states ". . . it was not until the latter part of 1840 I became aware that the exact state of astronomical science demanded more than a simple record of the transits, after the errors of the instrument had been rectified."

Airy, while at Cambridge, seems to have been the first to realize the inconsistency of this method. He felt that the instrumental constants varied more than was suspected at the time and that even when the constants were set to zero, the instrument would "drift" off in a short time. Airy, therefore, advocated leaving the instrument uncorrected, and correcting the individual star reductions for the instrumental errors in much the same manner as it is done in transit circle work today. Airy's method of treating the instrumental constants did not acquire wide-spread use during that period, so that, while these early personal equations are purely relative, if the two men being compared worked on different nights their personal difference may also be influenced by changes in the constants.

Bessel, after establishing the fact that personal equation did exist in transit work, turned his attention to the cause of this difference. In the first half of the nineteenth century it was generally held that the time required for nervous impulse transmission was nil. Maskelyne had not recognized Kinnebrook's error as being personal equation and had blamed it on a misuse of the Bradley method. Bessel,[2] aware that this difference was personal, ascribed it to a psycho-physiological cause.

"If it is assumed that impressions on the eye and ear cannot be compared with each other in an instant, and that two observers use different times for carrying over the one impression upon the other, a difference originates; and there is a still greater difference if one goes over from seeing to hearing, and the other from hearing to seeing. That different kinds of observation are able to alter this difference need not seem strange, if one assumes as probable that an impression on one of two senses alone will be perceived either quite or nearly in the same instant that it happens, and that only the entrance of a second impression produces a disturbance which varies according to the differing nature of the latter." As the pioneer hypothesis of personal equation, Bessel's analysis shows keen insight, and he gives a hint here of the later widely-accepted psychological theory of prior entry.

With the Bradley method, if a star is approaching the micrometer wire and the observer goes from seeing to hearing, the observed transit time will be too great. Again, if the observer goes from hearing to seeing, the transit time will be too small. According to this explanation, Bessel and Maskelyne went from hearing to seeing, while Argelander and Struve went from seeing to hearing. It was only at this time that astronomers began to realize that the $0^s.8$ difference between Maskelyne and Kinnebrook might have been caused in part by Maskelyne's tendency to observe transits too early. This seems to be borne out by the discovery that Maskelyne observed transits $0^s.27$, $0^s.18$, and 0^s14 earlier than three of his other assistants.[41] H. P. Hollis[97] has even gone so far as to suggest that Maskelyne's perception of sound was impeded by old age and that therefore, he, rather than Kinnebrook, was responsible for the $0^s.8$ relative equation. In this connection it may be significant that Newcomb[10] found Maskelyne's catalogue to be systematically $0^s.128$ too small in right ascension.

Another cause for personal equation was suggested in 1830 by the astronomer Nicolai.[4] "In my opinion it can scarcely have its basis in any other causes than that a difference exists in different individuals for mental reflexes from external impressions of the eye and of the ear. Thus one comes to believe that in one individual the mental reflex from the eye, for example, occurs earlier than the mental reflex from the ear . . ." This idea was not received as favorably as Bessel's, but is indicative of the thought that was being given to the question of personal equation.

Thus, in the first half of the nineteenth century, the existence and variability of personal equation were established. And as the 1850's approached, astronomers turned their attention more and more toward the task of further controlling and diminishing this troublesome personal error.

SECTION II

The Key-and-Chronograph Method of Observing

The second period in the history of personal equation in astronomy begins about 1850 with the introduction of the key-and-chronograph method of observing meridian transits. It should not be inferred that the Bradley method was discarded at this time. Rather, it was continued in use for many years in conjunction with the chronograph method.

It had been realized from Bessel's analysis of the cause of personal error in the Bradley method, that if the observer were given one operation instead of two, this error might be diminished. In 1828, O. P. Repsold suggested a method wherein as a star crossed each wire of the transit micrometer, the observer pressed a key, thereby making a mark on an evenly moving tape. His device did not have a constant rate of motion, however, and he died before being able to perfect it. In 1843, Arago[10] arranged to have the observer at the telescope make a sharp audible signal at the instant the star was bisected by the micrometer wire. An assistant, standing at the clock, estimated the time of this sound between the two adjacent clock beats, and set down the transit time. Thus, each man had but one stimulus to respond to. Arago found that this greatly reduced the personal equation and as an improvement he arranged the second hand of the chronometer to make a mark on the dial when a trigger, held by the observer, was pulled. A photographic method suitable for sun transits was described by Faye[7] in 1849. Instantaneous exposures were made at regular intervals of the sun's limb and the micrometer wires, in this way eliminating the observer entirely. The method could not be used for star transits, however, due to lack of sufficient light, and so was not adopted.

The eventual development of the chronograph was due largely to a combination of the Morse printer and a clock wired to operate an electric circuit at each beat. Bache and Walker of the U. S. Coast and Geodetic Survey have been given most of the credit for developing the chronograph as we know it today.[12] While Bache undoubtedly coördinated and encouraged this work in the middle of the nineteenth century, the inventive ability of Dr. John Locke[8] of Cincinnati was responsible for much of its design.

Up to this time, clocks which transmitted their beats over electrical circuits had been devised by Steinheil, Bain, and Wheatstone, but they made no record and could only operate a Morse sounder or keep a distant clock in synchronism.[106] In longitude work these transmitting clocks were a decided advancement since the time at the primary station could be sent by wire to the secondary stations. With these clocks, however, if the transmitting line was broken even momentarily, the observers at the secondary longitude stations could not identify the beat being transmitted from the primary station when the circuit was resumed.

Locke's design of a chronograph with rotating drum and moving pen carriage was a partial answer to this problem. His second contribution was a transmitting clock which identified the beginning of each hour and minute by the omission of certain beats. With this chronograph, the signals from the key held by the observer were made as interruptions in the record of the clock beats. In using this apparatus in longitude work, the clock at the primary station would transmit its beats by wire to chronographs in each of the secondary stations. If the circuit was interrupted, when transmission was resumed the seconds being recorded at the secondary longitude stations could be identified by the omission of certain clock beats. A clock and chronograph designed by Locke were installed at the U.S. Naval Observatory in 1849. In 1854 a somewhat similar chronograph designed by Bond[9] was put in use at the Greenwich Observatory. The fundamental design of Locke's chronograph has remained unchanged since 1850, although later developments have produced the kymograph and various printing and photographic chronographs.

The introduction of the key-and-chronograph method of recording meridian transits made it possible to determine reliable absolute personal equations. It must be remembered that the personal equations prior to this time are mainly relative, that is, most of them were determined by comparing one observer's results with those of another observer. This method did not reveal the difference between each observer and the true time of transit.

F. Kaiser of Leiden proposed a scheme in 1851 for the determination of the absolute personal equation involved in the Bradley method. In addition to the standard clock he used another clock which beat at a slightly different rate. Actuated by clockwork, an artificial star was moved across the telescope field. At the instant of coincidence of the star and the micrometer wire, the pendulum of the vernier clock was automatically released. An assistant then counted the beats until the two clocks coincided and from the known rates of the clocks, the true time of the transit could be derived. This, compared with the observed time of transit, gave the absolute personal equation. In 1858 Hartmann described a machine for finding the absolute personal equation with the Bradley method, but the device was too complicated to produce consistent results.

With the advent of the chronograph, more trustworthy machines for determining absolute personal equation were developed. Plantamour, Hirsch, Kaiser, and Eastman[21] are among those who devised machines in the 1860's and '70's. These early machines, generally speaking, consisted of a simulated star, moved horizontally by clockwork and focused on a translucent surface which was marked with several vertical lines in representation of transit micrometer wires. This simulated micrometer field was viewed through a short tube or with a low-power eyepiece. As the artificial star moved across the field, a pointer on the carriage moved over contacts which corresponded to the lines in the field. These contacts were recorded on a chronograph, as well as the contacts made by the observing key. The absolute personal equations found in this manner were applied to the star transit times observed with the meridian telescope.[34] A more complete description of these early machines will be found in a paper written by John L. E. Dreyer[22] in 1876.

After a period of experimentation, however, astronomers became dissatisfied with this procedure. It was deemed more accurate to determine the absolute personal equation with the same micrometer with which the real star transits were observed. In 1889 Wislicenus[31] experimented with a measuring device which was mounted inside the transit telescope. A concave mirror set inside the objective converged the rays from the field lamp, thus forming a spot which moved over the field as the eyepiece was moved. The eyepiece was driven by clockwork and contacts on the eyepiece slide were recorded on a chronograph. One advantage claimed was that the absolute personal equation could be determined in all positions of the instrument. The system of electrical contacts did not function properly at different declinations, however, thus invalidating the results.

The method finally developed for measuring personal equation with the transit telescope micrometer was to place the machine in one of the mark houses of the transit instrument and observe the simulated star as it crossed the field of the telescope. This use of transit telescope and personal equation machine in conjunction has been continued through the key-and-chronograph period up to the present time, at some observatories, for investigations in connection with the Repsold, or traveling-wire transit micrometer.

With the development of the chronograph and of reliable personal equation machines, it might seem that the problem of personal error in meridian transit astronomy had been solved, but experimentation in the latter half of the nineteenth century and early twentieth century proved otherwise. A plethora of material was written on the question during this period, and virtually any observational catalogue of the time provides an interesting discussion of personal equation.

When the chronograph was brought into use in astronomy no exact method of observing meridian transits was prescribed. As a result, although the difference was not recognized at first, two distinctly different methods developed; like Topsy, they just grew. Gould and Gill seem to have been the first astronomers to note that two methods existed. Gill[24] in discussing the observed positions of Mars comparison stars in 1878 found systematic differences in the results from various observatories. Seeking a cause for these differences he found that there were two distinct habits in recording transits with a chronograph. He noted that the majority of observers anticipated the transit so that the sound of the key tap coincided with the transit. Gould[18] pointed out, in *Transatlantic Longitude,* that this method introduced an error which varied with declination. He urged that all observers should not tap the key until the star was seen to be bisected. This, he said, would eliminate the variation introduced by changes in declination.

Newcomb[39] classified these two methods as follows: "In method A, which I believe to be the most common, the observer anticipates the passage of the star, so as to have his signal completed at the exact moment of crossing [the wire], much as if he were shooting at a [flying] bird. In the other method, which I shall call B, the observer waits until he actually sees the star on the thread, and then commences the nisus which terminates in the signal." Newcomb states that he prefers method A, which amounts essentially to this; that the observer fixes by habit a certain spatial interval such that when the star is this distance from the wire, prior to crossing, and the observer starts his reaction, the sound of the signal coincides with the bisection of the star by the wire. Assume that the observer chooses the size of this spatial interval while observing stars of 30° declination. His transit times for faster moving stars (closer to the equator) will then be late, and for slower stars (closer to the pole) will be early. This error, which varies as the secant of the declination, is a fault of method A, and is eliminated by method B.

Confirmation of this personal error with declination came from many sources. C. H. Hins,[72] in a discussion of zone work at Amsterdam involving three observers, found that two observers registered stars of high declination too soon, and stars of low declination too late, relative to the mean of a zone. This change in the personal equation between declinations 10° and 50° was from $0^s.011$ to $0^s.058$ for one observer, and from $0^s.014$ to $0^s.071$ for the other. The third observer had no change in personal equation with declination. Hins found that the first two observers used method A, while the third used method B. In the 1903-1911 program of the nine-inch transit circle at Washington,[70] the final corrections deduced for this personal error were Littell $-0^s.134$ secant declination, and Morgan $-0^s.032$ secant declination. A similar error varying with declination was also found in the 1913-1926 program of this instrument.[89] Other astronomers, however, using method B, found no change in personal equation with declination. M. Gonnessiat[36] of the Lyons Observatory, for example, conducted tests for this phenomenon in 1890-91, but could find no evidence of a declination equation.

In his discussion of the Mars comparison stars, Gill also suggests the idea of a variation of personal equation with magnitude in method A. An observer who anticipates, Gill states, ". . . will be nearly certain to press [the key] too soon for very bright and too late for very faint stars, because the larger disk and the rings of a bright star will make it appear closer to the web than the small sharp disk of a faint star at the same angular distance from the web." He implies that this magnitude error would not occur with method B, but Newcomb disagrees with Gill on this point.

Numerous instances of this magnitude error have been published. Bakhuyzen's experiments at Leiden in 1878 confirmed that faint stars were observed roughly $0^s.05$ later than bright ones.[22] In a comparison of photographic plates with the Cambridge Catalogue of 1875, Turner[43] found that 8.0 magnitude stars were observed $0^s.10$ later than 5.0 magnitude stars. Hinks[40] found a magnitude equation in the Cambridge Zone Catalogue of 1896, and in the Berlin zone work of the same year, an equation of $-0^s.007$ per magnitude that increased linearly with brightness was noted. In 1891 Christie[33] described a method of using screens over the objective of the Greenwich transit circle, stating that screened stars were observed later than unscreened stars. Tucker,[45] in discussing the 1893-96 program at the Lick Observatory, found that each faint star apparently transited $0^s.009$ later than a star one magnitude brighter. In 1900 a similar magnitude effect was found in the work of the six-inch and nine-inch transit circles at Washington. One of the most thorough determinations of magnitude equation for correcting an observational program has been made by Küstner;[60] the investigation also apparently being the first to include the color effect of the stars.

It must be assumed that Newcomb was correct in believing that the magnitude error would be present in both methods A and B, since no published results have been found to indicate that method B eliminated this personal error. In fact, Lewis Boss,[48] experimenting at the Dudley Observatory about 1900, found that he had a magnitude equation of the same order with either method A or B.

Astronomers also discovered that in key-and-chronograph observations, whether method A or B, the direction of the star's motion introduced a change in the personal equation. C. Wolf at Paris, and Wagner at Pulkowa, noted this phenomenon about 1870, and in later transit circle observational programs many references are made to it. In an 1875-77 program of differential longitudes in India, Captain Campbell[23] found this same effect and called it ". . . a novel feature of Personal Equation." His north — south difference amounted to $+0^s.077$.

This personal error due to the direction of motion of the stars is termed a head north minus head south difference since as the instrument is changed from a setting south of the zenith, to one to the north, the observer has to change his position from head north to head south. In the 1893-96 program at the Lick Observatory, Tucker[45] found a north minus south difference in right ascension of $+0^s.030$ and in declination of $-0''.05$. Again, in the six-inch transit circle program of 1900-01, Updegraff[54] noted a head north minus head south value in right ascension of $+0^s.028$ and in declination of $+0''.41$. Gill seems to have been the first astronomer to analyze this personal equation as a bisection error. As early as 1864, C. Wolf had discovered that he was unable to judge the middle point between two dots, but at the time this was believed due to some physiological fault of his eye. It soon became evident, however, that other observers had bisection errors also, probably due mainly to faulty judgment.

The remedy for this personal error was the reversing prism eyepiece. A reversing eyepiece was used experimentally in 1884 at Greenwich. On fine nights two clock stars were observed with reversed motion, but all the other observing was done with a non-reversing eyepiece. The reversing eyepiece has been used occasionally at both Greenwich and Cape since that time. In 1910 a reversing eyepiece was added to the six-inch transit circle at Washington, and a few years later, to the nine-inch. Since that time it has been the regular practice at Washington to make half of each observation with the prism direct and the other half with it reversed.

That an observer's personal equation for the sun and moon might differ from that for stars was first indicated by Robinson,[13] of the Armagh Observatory, in 1830. He found the probable error of an observation, by the Bradley method, of the first limb of the sun to be larger than that for the second limb. He later concluded that this was due to personal equation, and that the cause was irradiation, since the error could be reduced by strong field illumination. Dunkin,[20] in discussing the Greenwich sun observations from 1864 to 1873, noted that each observer had a decided personality, differing for each limb of the sun. Dunkin[17] said this difference in personality for each limb also applied to observations of the moon. Stroobant,[46] in experiments with a personal equation machine in 1891, found his personal equation was larger for the first limb of discs representing the moon, Jupiter, Mars, Saturn, and Uranus, than it was for the second limb. A reversing prism eyepiece was not used in any of the above observations, so the bisection error is present. Later work at Washington, where a reversing eyepiece was used, gave smaller differences between limbs one and two, but the personality for the sun was still found to differ from that for the stars.

Scale error, which had been present in eye-and-ear observations, appeared also in the key-and-chronograph method. This is an error in estimating fractions of an interval, with the result that the fractions do not appear with equal frequency. Scale error arises from improper judgment on the part of the observer and may cause a systematic error in transit observations. In the Bradley method the scale error affects the observer's estimate of the distance of the star from the micrometer wire at each clock beat. With the key-and-chronograph method, the scale error enters in reading up the chronograph sheets.[100] It should be noted that in taking the mean results of several observers, their scale errors will not necessarily cancel out.[75]

Aside from the variations of personal equation with the object, (sun or star), and with the speed, direction of motion, and magnitude of the stars, experimenters in the latter part of the nineteenth century asserted that the illumination of the reticle, the position of the observer, and the type of observing key all exerted some influence. Improved field lighting alleviated the first mentioned effect, and the last two still lack confirmation. One of the most complete discussions and mathematical analyses of the types of personal equation evident in an observational program is given by H. R. Morgan in *U. S. Naval Observatory Publications,* Volume XIII, 1933, which yields further information on the personal differences mentioned above.

The key-and-chronograph method of observing reduced the personal equation and the probable error incident to the Bradley method, but it increased the accuracy of observations to the point where other types of personal error became evident. As it affected the true transit time, the absolute personal equation with method B measured about $0^s.1$, while with method A it was even less. But, as Campos Rodrigues,[50] of the Royal Observatory at Lisbon, pointed out in 1902, method B was best for observing since it gave the smallest probable error in the means of clock corrections. He stated that beginners usually tried method A because it made their personal equations small, but that this anticipation method increased their probable errors.

Despite this disadvantage, however, the majority of observers seem to have continued using method A in this period. The examples of Dunkin, Ellis, and Henry at Greenwich illustrate the decrease in the probable error of the chronograph method over the Bradley method.[29]

	Henry	Dunkin	Ellis
Eye and Ear	$\pm 0^s.112$	$\pm 0^s.062$	$\pm 0^s.069$
Chronograph	$\pm 0^s.058$	$\pm 0^s.048$	$\pm 0^s.053$

The discovery of so many additional forms of personal equation at this time led astronomers to believe that there might be many more as yet undiscovered, and made it seem impractical, if not impossible, to correct for so many variants. At a time when meridian transit astronomers were trying to increase the accuracy of their observations, the discovery of so many "unknown" equations cast some doubt on the whole structure of fundamental astronomy.

In 1828 Airy[3] had stated the main fault of the Bradley method as follows: ". . . I wish to express the feeling, of which the observer is sometimes painfully conscious, that he cannot, by any effort, bring the eye and ear to act together in the manner which is necessary for observing a transit." Now Newcomb expressed the observer's dissatisfaction with the chronograph method by pointing out that when the atmosphere was at all disturbed the stars seemed to jump over each wire, instead of crossing them at an even rate. It became more and more evident that a still better method of observing star transits was needed, and at the end of the nineteenth century, in response to this need, the new Repsold traveling-wire micrometer was being perfected.

It might be noted here that after 1880 very little theorizing was done by astronomers as to the fundamental causes of the various types of personal error. The astronomers continued to measure and correct for these quantities, but those few astronomers who continued trying to unlock the riddle of why personal error existed were regarded as mere amateurs by the sage professionals. These professional astronomers realized that the question of the psycho-physiological basis for personal error had been taken out of their hands. For in the latter half of the nineteenth century, a new science had come into existence; the science of experimental psychology.

(To be continued)