THE IMPROBABLE PROGENITOR

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ABSTRACT

Twenty years after Maskelyne, the Astronomer Royal at Greenwich, fired an assistant whose timings of stellar transits across the meridian differed from his own by more than the accepted limit, a report of the incident caught F. W. Bessel's interest; he subsequently demonstrated that such individual differences in perception are a universal phenomenon and he attempted to discover their cause. The problem intrigued generations of astronomers, physiologists, and mental philosophers of an experimental bent. Eventually, attempts to solve it helped lead to the founding of the new science of psychology.

Astronomy and psychology are not ordinarily thought of as fields having much connection with one another, yet a series of events which began at the Royal Observatory Greenwich toward the end of the eighteenth century was to play a significant role in triggering and, in the early stages, guiding the gradual course of psychology's development as a science. The initial link in this chain of circumstances was forged in August, 1795, when the Rev. Nevil Maskelyne, D.D. (1732–1811), the fifth Astronomer Royal, first noticed a difference between his own measurements and those of an assistant in the timing of the transit of stars and planets across the meridian of the observatory. The taking of such stellar transits was one of the most important tasks of every observatory of the period; of the observations made at Greenwich between 1787 and 1798, almost eighty percent of those reported were of this type. On such transits depended the determination of the true time and the accurate calibration of clocks and of ships' chronometers. The accuracy of astronomical timings is also of contemporary relevance, as visual observers of lunar and planetary occultations and of other phenomena requiring timings still assume a degree of accuracy for such timings greater than probably exists.

The method then in use for the taking of transits was the "eye-and-ear" method of James Bradley, who from 1742 to 1762 had been Maskelyne's predecessor, but one, as Astronomer Royal. The instrument used in studying stellar transits was a telescope on an east-west axis and turning in the plane of the meridian. The astronomer's task was to determine the exact moment at which a star crossed the meridian. In Bradley's method, when a star is about to make its transit, the observer reads off the time from

FIG. 1—Rev. Nevil Maskelyne (1732–1811), 5th Astronomer Royal. Engraving by Edward Scriven.

a clock and continues to count the second-beats while watching the movement of the star. As the moving image approaches the central wire, he fixes firmly in mind its position at the first beat before it crosses the wire and its position at the first beat after; from the distances of these two points from the meridian, the astronomer estimates by eye the time of the crossing in tenths of seconds. The problem, therefore, is to determine the correct tenth of the relevant second. The method involves visually fixing the exact place of the star at the first beat, remembering this point, the fixing of the place at the second beat, remembering that, the comparison of the two positions and the expression of their relationship in tenths. Despite such complexities, it was assumed that the method was accurate to one- or two-tenths of a second. Maskelyne believed that even this minimal error could be reduced by averaging the measurements for the different wires in the focus. Any remaining inaccuracies were not thought to be due to human error: "The difficulty of attaining the desired exactness arises from various causes; sometimes from the great faintness of the object, sometimes from its over brightness, a tremor or undulation owing to a bad state of the air, or quick motion of the star through the field of the telescope occasioned by the great magnifying power, and sometimes from flying clouds" (Maskelyne 1799, p. 339).

One of the astronomical phenomena which interested eighteenth-century astronomers was the eclipsing of Jupiter's moons as they passed behind the planet. Maskelyne and his assistant, David Kinnebrook, using adjacent telescopes, attempted to record precisely the moment when an eclipse began or was complete. Maskelyne noticed, however, that their measurements were differing by more than the accepted error. In the third volume of the *Astronomical Observations* from Greenwich (Maskelyne 1799, p. 339), the Astronomer Royal reported on this difference and its eventual outcome.

> I think it necessary to mention that my Assistant, Mr. David Kinnebrook, who had observed the transits of the stars and planets very well, in agreement with me, all the year 1794, and for a great part of the present year, began, from the beginning of August last, to set them 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 8/10ths 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 reluctance, as he was a diligent and useful assistant to me in other respects, I parted with him .... The joint excellence of the clock, transit instrument, and method of observing, when properly attended to, have been the means of first discovering this error, and then ascertaining the quantity of it .... I cannot persuade myself that my late Assistant continued in the use of this excellent method of observing, but rather suppose he fell into some irregular and confused method of his own; as I do not see how he could have otherwise committed such gross errors. However, this unfortunate instance shews the necessity of first duly understanding, and then closely adhering to, this method of observing.

The incident was forgotten by everyone (with, presumably, the exception of the unfortunate Kinnebrook) until it was mentioned in 1816 by von Lindenau in a history of the Royal Observatory. This reference attracted the attention of F. W. Bessel (1784–1846) at Königsberg. Intrigued by the report, Bessel asked himself why a well-trained and apparently conscientious observer, using good equipment and a method purported to be accurate to one- or two-tenths of a second, should have persisted in giving measurements so at variance with those of his superior. Bessel decided the discrep-

FIG. 2—Friedrich Wilhelm Bessel (1784–1846).

ancy must have been involuntary. If it were to occur in other than this isolated case, it would be of major significance for astronomers. Bessel sent to Greenwich for a copy of Maskelyne's complete observations and, after studying them, decided to test for differences among observers more experienced than Kinnebrook.

Bessel's first attempt was made in 1819 when he visited Baron von Lindenau and Johann Encke, Director of the Paris Observatory, but it turned out to be abortive when there was only one clear night during the fortnight they spent together! The next year at Königsberg, when he had an opportunity to compare his observations with those of Walbeck, the weather was more co-operative. Selecting ten stars, they alternately observed transits of five stars one night and five the next, for five nights. They discovered that Bessel always estimated the time the star crossed the meridian as earlier than Walbeck; the average difference was 1.041 seconds, with little variation. How could two observers differ by more than a second using a method that involved estimating fractional intervals between beats of a clock only one second apart? Yet Bessel reported: "We ended the observations with the conviction that it would be impossible for either to observe differently, even by only a single tenth of a second" (quoted in Sanford (1888)). Although these results have been questioned, and it has been suggested that they were due to an artifact of some observing procedure – as, for example, assigning the "heard beat" to the earlier or later of adjoining seconds – from one point of view it is irrelevant whether the result is a true difference or merely an artifact; the findings served to increase further Bessel's interest, and led him to continue his investigation of the phenomenon.

In 1823, Bessel had an opportunity to make further comparisons, this time with his former student, Friedrich Argelander (1799–1875), compiler of the formidable *Bonner Durchmusterung*. Departing from the more usual method, Bessel made the clock corrections while Argelander made the observations. The results were compared with those made on the same stars in 1821, when Bessel had done both the observing and the clock corrections. The difference between the two observations presented in what became the standard form, is $A - B = 1.223$ seconds. As a result of Bessel's presentation in this form of the various comparisons, the phenomenon came to be called the "personal equation".

Bessel was particularly anxious to compare his own observations with those of Friedrich Struve (1793–1864) of Dorpat who was more experienced in making stellar transits than either Walbeck or Argelander. As no opportunity presented itself to make direct comparisons, Bessel conceived the idea of determining a personal equation by means of a third observer. Both Walbeck and Argelander had compared themselves with Struve on visits to Dorpat, and these results were available. Using them, Bessel made the following calculations:

By direct comparison –

$W - B = 1.041$ s (1820), $A - B = 1.223$ s (1823),
$W - S = 0.242$ s (1821). $A - S = 0.202$ s (1823).

Hence, by subtraction –

$S - B = 0.799$ s. $S - B = 1.021$ s.

Eventually Bessel had data to permit him to make three such indirect comparisons and two direct ones with Struve. The various values of these personal equations varied from 0.044 s to 1.021 s. It had originally been Bessel's hope that he could use the personal equation as a means of correcting the observer error of any particular astronomer. Indeed, something of the sort seems to have occurred to Maskelyne (1799, p. 339) when he advised readers of the *Astronomical Observations* of the necessity of correcting for Kinnebrook's "vitious way of observing". "To reduce his transits to mine, subtract half a second from the beginning of August 1795 to the end of the year, and 8/10ths of a second in the month of January 1796." Disturbed, however, by the range of the personal equation, Bessel eventually concluded that these differences are variable, too variable in fact to make it possible to "calibrate the observer". But other astronomers were less pessimistic, and argued that the variability of the personal equation was not so large that correction was entirely useless.

Having accepted that there are individual differences in perception, the interest of astronomers turned to the question of why such differences should exist. Maskelyne's original assumption had been that Kinnebrook had been careless, "falling into some irregular and confused method of his own". Bessel's demonstration of the ubiquitousness of the phenomenon had shown the possible inadequacy of this explanation. Another possible explanation was in terms of physiological differences among observers; but as the nervous system was assumed to react instantaneously, or nearly so, the answer did not seem to lie in that direction.

Various other hypotheses concerning the cause of the phenomenon were proposed. Bessel was among those who favoured a psychological explanation. The measurement of stellar transits involved the incorporation of information from two different sense modalities, the visual and the auditory. He hypothesized that the varying results for different observers might be due to differences in the rates of assimilation of this diverse information. Adopting a theory of this sort, the Director of the Paris Observatory, D. F. Arago (1786–1853), predicted in 1842 that the personal equation could be reduced substantially by simplifying the task of the astronomer. It was found that when one observer was instructed to make a sharp noise at the instant the star crossed the meridian and a second observer was directed to record the fraction of a second at which the noise occurred, the differences declined substantially. Continuing this research, Arago used a stop watch to record the instant of the meridian passage and later designed a trigger or "key" which would make a mark on the dial of a chronometer when pulled by the observer at the moment of the transit. As a fraction of the second could later be determined, the problem became a simple motor reaction to a visual stimulus rather than the complex assimilation of visual and auditory data involved in Bradley's ear-and-eye method.

The technique for reducing the observed individual differences which came to be preferred involved the use of the chronograph. An early version was developed in 1828 by the German instrument maker, Repsold, but it was not until 1850 that a really useful model was developed in the United States. Very simply, the chronograph is a drum-shaped piece of apparatus with two pointers tracing parallel lines. One of these is connected with a timer so that the pointer draws a "jag" in the line every second. The other pointer is attached to a key which the observer taps at the moment of transit. As in Arago's method, all that is required of the observer is that he react to the appropriate visual stimulus. It was discovered at the Greenwich Observatory, which first used the apparatus in 1854, that with this instrument the personal equation was reduced to less than a tenth of a second, the level of precision which astronomers had originally considered acceptable. Technical advance continued, and by 1862, the Hipp Chronoscope was being used to measure the personal equation to a level of accuracy of 1/1000 of a second.

One important result of the invention of the chronograph was that it soon led to the measurement of the absolute personal equation. Bessel had attempted to correct for individual differences in perception by a comparison of two observers; his "personal equation" was, in fact, a *relative* measurement. With the development of electrical devices which could record the exact instant of a stellar transit, it became possible to compare an observer's measurements with the "true times"; in other words, to have an "absolute personal equation" based on a comparison of the actual time with an observer's scores based either on Bradley's method or on Arago's key. It could now be stated that astronomer *A* took 300 ms to respond to a visual stimulus, and astronomer *B* took 200 ms to respond to an auditory cue.

The discovery of the new automated techniques made the issue of individual differences in perception a less acute problem for astronomy. Yet the peak of interest in the personal equation was in the 1860's with the interest continuing until approximately 1890. One major area of investigation involved measuring the dependence of the personal equation upon differing astronomical conditions. Astronomers, and then physiologists, discovered that reaction time (as the phenomenon of the individual perceptual differences was now coming to be called) was not constant for one individual, but variable. Not only did reaction time change with stimulus conditions such as magnitude and speed of movement, but also as the

FIG. 3—Hermann von Helmholtz (1821–1894). Oil painting by L. Knaus in the National Library of Medicine, Bethesda, Maryland.

observer's state changed. The variety of relevant astronomical factors which appeared to be related to the phenomenon eventually resulted in a return to Bessel's early view that a psychological rather than an astronomical analysis of the situation was needed.

While astronomers had struggled to develop automated measuring techniques and to determine the range of astronomical measurements for which the personal equation was relevant, there was, simultaneously, a revival of interest in the earlier theory that the basis of observed perceptual differences among observers might be physiological. Was it possible that there *were* differences in the speed of neural transmission from one observer to the next? Hermann von Helmholtz (1821–1894) reintroduced this possibility for consideration.

The untested belief (common at the time) that the speed of light and the speed of neural transmission were comparable had led to a more or less out-of-hand rejection of the possibility of a physiological cause when Bessel had first searched for an explanation for individual perceptual differences. Helmholtz devised a method for determining the speed of conduction in motor nerves. Stimulating an exposed nerve at two varying distances from a muscle in a frog's leg, he measured the time it took the muscle to respond in each case. The difference in time could be used to determine the length of time it took for the impulse to be transmitted between the two points along the nerve. This method produced results which suggested that the speed of neural transmission – at least in the frog – was approximately 30 metres per second. Clearly, the old notion that "mental" processes were practically instantaneous, and their duration therefore unmeasurable, was untenable.

Recognizing the possibility of inter-species variability, Helmholtz extended his research to humans in 1850. His method was similar to that which he had used with animals with the exception that he now used the stimulation of cutaneous sense receptors rather than of exposed nerves. Instructing his human subjects to move a hand as soon as they felt a sensation, Helmholtz stimulated them on the toe and on the thigh. He then noted the difference in time for the motor response to occur and worked out the length of time it presumably took the impulse to travel between these two points. These were the earliest "reaction time" experiments as such but their similarity conceptually to the use of Arago's key in astronomy is apparent.

Unfortunately, the results Helmholtz obtained were inconsistent, with large differences appearing not only among subjects but within the trials of any one individual. As a result, Helmholtz abandoned the investigations; like the astronomers before and other investigators after him, he was disturbed to find such variations (particularly those which were intra-individual), considering them an impediment to precise experimentation. Luckily, the finding of the comparative slowness of neural transmission caught the interest of other scientists. Hirsch, the Swiss astronomer, working between 1861 and 1864 with the engineer, Hipp, who had just invented the famous Chronoscope, undertook to compare the speeds of reaction to varying types of stimuli. The investigators were particularly interested in comparing the different sensory modalities and the results they obtained appear to have been reasonably accurate. The average reaction times they reported for different subjects, and over different situations, were: 200 ms for visual stimuli; 150 ms for auditory; and, 140 ms for an electric shock to the hand.

In the late 1860's, the Dutch physiologist, Donders (1818–1889), took up the problem of reaction time and it was he who grasped the significance of the issue for psychology. If physical events such as a heart beat or, as Helmholtz had demonstrated, reflexive movement of the muscles take time, why should not the higher mental processes? If thought (or stimulation) and movement follow one another at an appreciable interval, then the body does not instantly obey the mind. It might be that psychological as well as neural processes occupy time.

The general problem was to determine the length of time which the various mental processes took. Donders' early work dealt with simple reaction time, the making of a predetermined response to a predetermined stimulus. An example of this most basic form of reaction experiment might involve the subject pressing a key when an auditory stimulus, such as a bell, sounded; the time between the sounding of the bell and the pressing of the key was simple reaction time. Excited by the possibility of being able to measure a psychological process, Donders quickly moved on to more complex processes. If the reaction was made more complex, could not the increased time be attributed to what was added to complicate the reaction?

Donders theorized that there are two significant variables in reaction-time situations – the stimulus variable and the response variable. The response required of the subject can vary in complexity; instead of merely making a motor reaction when the stimulus is perceived as in the simple reaction experiment, he might, for example, be required both to discriminate and choose among alternative movements. To examine the effects of these variables empirically, Donders devised what has become known as the "subtractive method". Beginning with a subject's times in a simple reaction experiment, complications are added, one by one. In Donders' most complex case, two stimuli were presented in random order; the task was to react with one response if stimulus *A* was presented first and with a different response if stimulus *B* was the first to appear. It was Donders' assumption that in complex reactions, one or more additional acts are *inserted* into the simple reaction and the time they require may be determined by *subtracting* the simple reaction time from that required for the more complex. Although there were serious methodological weaknesses in Donders' investigations, these first attempts to measure the speed of the higher mental processes were both ingenious and original. "Mental chronometry", as it was termed, was launched.

Some critics argued that one cannot say with certainty that "one process exactly fills its assigned span of time and is instantly superseded by another process when it ceases" (Murphy and Kovach 1972, p. 109). But, despite such doubts, Donders' work showed that some of the variability of results which had been bothering investigators for decades was due to central psychological processes rather than to simple differences in the speed of neural conduction. To him must also be given credit for beginning the examination of the time relations of psychological processes. As Boring (1963, p. 147) pointed out, there was a group of scientists in Germany in the 1860's who were looking for psychological variables that are measurable. "It was not so much that measurement intruded itself upon psychology as that these new psychologists were seeking out psychic measureables and dragging them triumphantly into psychology." If psychology was to become scientific, it must measure its variables. With Donders' discovery of a means of apparently measuring the higher mental processes, a new era had begun.

Foremost among these new scientists was the ambitious Wilhelm Wundt who, to further his career, changed disciplines from physiology to philosophy because of the greater availability of senior posts in the latter field. Having been appointed to the important Chair of the Department of Philosophy at Leipzig, Wundt established, in 1879, a laboratory for the experimental study of the phenomena of mental philosophy. Ben-David and Collins (1966) have suggested that Wundt's reason for this unprecedented move was a desire to raise the status of his new discipline to that of the field he had left, the high repute of which was based largely on the success of its scientific methodology. Questions about the nature of perception and the processes of thought had, of course, been the concern of mental philosophers for many centuries. What Wundt was now suggesting was that these same problems should be tackled by new methods, specifically by experimentation. In his fusion of the problems of mental philosophy with the methods of the physiologist, Wundt took the final steps in the creation of a new and separate discipline, experimental psychology.

The greater part of the work done at the Leipzig laboratory during the first twenty years of its existence involved studies of sensation and perception. Progress was soon being made on three types of experimentation, each of which had its roots in the problem of the personal equation. All involved the measurement of time as an experimental variable.

Of the three, the "time sense" studies were the most straightforward.

FIG. 4—Wilhelm Max Wundt (1832–1920).

Wundt and his students devoted considerable energy to examining the ability of individuals to compare intervals of time under different conditions.

> In this field quite a little host of problems revealed themselves as eminently suitable for experimental treatment, i.e., the ability to reproduce intervals of varying length, the influence on the estimate of an interval of the stimuli that marked its beginning and its end, the comparison of 'filled' and 'unfilled' intervals, of intervals 'filled' in a variety of ways (by mental work, sensory stimuli, etc.) (Flugel 1945, p. 186).

These problems are obviously directly relevant to an understanding of the nature of the task involved in Bradley's ear-and-eye method of taking transit timings.

Of greater significance was a type of study based fairly directly on Bessel's early work. Borrowing a term from Johann Herbart, Wundt began work on the "complication experiment". By "complication", Herbart had meant any psychological event which involved combining information from more than one sensory modality. Bradley's ear-and-eye method involved such complication, as the task required input from both the visual and auditory modalities. Wundt's primary concern was with the amount of time required for different complications. In order to study these phenomena, he devised in 1861 a new piece of apparatus, "Wundt's Complication Clock". This mechanism utilized a pendulum which swung across a scale marked off in degrees; at a predetermined point in its arc, it produced a "click". The problem for the subject was to determine where the pendulum was on the scale at the moment of the click. Studies which utilized the Wundtian apparatus typically investigated the effects of two variables, the speed at which the pendulum moved and the number of stimuli which were presented simultaneously – extra sounds or even electric shocks being added to the sight of the moving pendulum and the sound of the clicks. The general findings were that when the pendulum moved at no more than moderate speeds, and when the accompanying stimulus pattern was simple, the subjects tended to indicate that the click had occurred at a point earlier on the scale than it had. As the speed of the movement increased, and with more complex accompanying stimuli, the click was perceived to occur later than it actually had. Once more the direct relevance of these findings is apparent to the problem originally posed to astronomers by the apparent inaccuracy of Bradley's method of determining meridian passage.

Although complication studies as such were to remain only of secondary interest for psychologists, they were to form a "determining influence upon Wundt's psychological system" (Kirsch 1976, p. 123), and, indirectly through him, upon scientific psychology more generally. Kirsch argues that these studies, related to the attempt to understand the personal equation, "became the first purely psychological experiment". Although the astronomers had begun the study of the personal equation, their main concern was in nullifying its influence. "Wundt, on the other hand, used the complication experiment to measure a purely psychological process: the accommodation of attention" (Kirsch 1976, p. 123).

The third offspring of the personal equation to attract the interest of the Leipzig experimentalists was the study of "psychological time", or to use Exner's better-known name, reaction time. The reaction experiment, unlike the time-interval or the complication studies, was to become an important and persistent area of concern for experimental psychologists. The measurement of the duration of mental processes, or psychometry, was its central concern. The reaction-time experiment came to comprise "nearly half the researches undertaken in the Leipzic [*sic*] laboratory" (Cattell 1888, p. 45). Flugel (1945, p. 186) has suggested that "in the early days it appeared to be the greatest triumph of the new psychology".

Following in the path of reaction experiments established by Donders, Wundt devised an experimental situation in which two or more stimuli may be presented and a single motor response is to be made – but not until the "correct" stimulus has been presented. He termed this the "d-reaction", and believed that this task differed from the simple reaction only by the insertion of a need for discrimination; in the simple reaction the subject signals as soon as he becomes aware of a stimulus, but in this case he signals when he recognizes or identifies the stimulus. It seemed to Wundt that this was the same situation as that of the astronomical observer whose task was to signal when he saw the star right on the meridian.

Beginning with Donders' pioneering studies, the assumption had been made that as psychological processes change from the very simple to the more complex, new processes are added to the old, and the time required by the additional activities are merely added on top of that needed for the simple reaction. The earlier studies from Leipzig had similarly employed this logic, working this time from the more complex down to the simpler. This subtractive procedure was called into question, however, due to the large variations found in such measurements. Psychologists were no happier with individual differences than had been the astronomers, and like them, considered the variations as "errors" rather than as true individual differences to be considered as a topic of interest in their own right. A serious blow to the entire approach came in 1891, when Külpe demonstrated that the underlying theory of the subtractive procedure was not really justified, "since when the conditions of reaction are complicated, there is no simple addition of one mental process, the other process remaining unchanged. Rather, the whole conscious task is changed throughout its course" (Flugel 1945, pp. 187–8). If there is a change in the task required of the subject, for example if he is to judge rather than to perceive, there is a change in the observer's attitude and in the entire ensuing conscious process.

Despite understandable disappointment, the foundations of the new experimental discipline had been so firmly grounded by this time that the collapse of one area of investigation, even a favourite one, did not seriously threaten either the separate existence of psychology or its continuing development. One of the waves of the future was apparent almost immediately, when James Cattell, one of the small army of North Americans studying psychology in Germany, persuaded a reluctant Wundt to permit him to do his doctoral research on the problem of individual differences in reaction time. Instead of seeing such differences as errors which reduced the precision with which events could be observed, Cattell believed that they were the rule rather than the exception, and, indeed, perhaps even the most significant feature of many experiments. Cattell's interest in individual variability was reinforced by spending the year 1887 at Cambridge which afforded him the opportunity for contact with Sir Francis Galton who was interested in every sort of difference among people, from sex differences to those of age and race. Returning to the United States, Cattell published in 1890 the important article, "Mental Tests and Measurements", apparently using the term "mental test" for the first time; this article sparked an interest which was to become a dominant concern of North American psychologists and which has begun to subside only within the past two decades.

Conclusion. Improbable as it may seem, the firing in 1795 of an obscure astronomical assistant at the Royal Observatory at Greenwich was to precipitate a series of events of which the eventual outcome was to be the founding of a new scientific discipline. In an attempt to explain why two observers had differed in their timed measurements of stellar transits and, more generally, why there should exist individual differences in perception, a succession of astronomers, physiologists and finally experimental psychologists had tackled the problem. None of this was to help the unfortunate Kinnebrook who seems never to have been heard of again, and a totally satisfactory answer to the original question has even yet to be given. But the tens of thousands of psychologists now involved in widely varying fields of research and application all owe a debt to astronomy for its role in the formation of their discipline.

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