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Astronomers Mark Time: Discipline and the Personal Equation

Ah, I perceive you think me weak in the extreme, [said the astronomer] but you will never realize that an incident which filled but a degree in the circle of your thoughts covered the whole circumference of mine. No person can see exactly what and where another's horizon is.

Thomas Hardy, *Two on a Tower* (1882)

Astronomy. There could be nothing better. I defy the ingenuity of journalists to persuade their public that any given member of the proletariat can have a personal grievance against astronomy.

Joseph Conrad, *The Secret Agent: a Simple Tale* (1907)

The Argument

It is often assumed that all sciences travel the path of increasing precision and quantification. It is also assumed that such processes transcend the boundaries of rival scientific disciplines. The history of the personal equation has been cited as an example: the "personal equation" was the name given by astronomers after Bessel to the differences in measured transit times recorded by observers in the same situation. Later in the nineteenth century Wilhelm Wundt used this phenomenon as a type for his experiments on reaction times. For historians of psychology, this has been taken to be an exemplary case where quantified laboratory science rescued astronomy by showing that this was really a psychological phenomenon measurable only in complication experiments. This paper challenges this story. Astronomers neither ignored, nor despaired of, the personality problem. Instead, the managers of the great observatories developed a new chronometric regime of vigilant surveillance of subordinate observers. The astronomers' solution was thus intimately connected with social and material changes in their way of life: a division of labor in the observatories, a network of observing sites, a mechanization of observation. The paper documents these changes and then presents a study of one case where managers, amateurs, and psychologists clashed for authority over the personality problem. Measurement is given its meaning when situated in specific contexts of styles of work and institutions. Disciplines give meanings to values, and often resist attempts by others to redefine these meanings or to gain authority over measurement. Quantification is not a self-evident nor inevitable process in science's history, but possesses a remarkable cultural history of its own.

Psychology and the Personal Equation: A Simple Tale

The term "personal equation" appeared in the early nineteenth century as a label for the worrying fact that astronomers seemed to differ from each other in the times they recorded for transits. The difference varied with time and with the type of observation: for example, personal equations might differ for lunar as opposed to stellar transits. The personal equation possesses a history which is characteristic of all origin myths. In the paper Edwin Boring contributed to the Conference on the History of Quantification in the Sciences held in 1959, the personal equation took pride of place in an account of the progressive move to quantify reaction times, formally inaugurated by Wundt and Donders in the 1860s (Boring 1961). In 1796 the Astronomer Royal at Greenwich, Nevil Maskelyne, dismissed his assistant David Kinnebrook because of the difference of between one half and a whole second in the transit times measured by the two observers. This corresponded to a significant difference of about thirteen seconds of arc. The great German astronomer Friedrich Bessel, preoccupied with the reduction of the Greenwich catalogues of observations during the 1810s and 1820s, was then credited with the initiation of the term "personal equation" to characterize this difference, and with the inauguration of a systematic program to ascertain characteristic relative differences between pairs of astronomical observers. "No-one knew at the time," Boring explained, "why there should be these individual differences" (Boring 1961, 115–18; Maskelyne 1799–1800, 3:319; Bessel 1875–76, 3:300–304).[1]

Few further developments were sufficient, apparently, to give birth to a full-fledged quantitative research program on the personal equation. Boring referred to advances from within galvanic technology which allowed the construction of a chronoscope by Wheatstone in 1840 and of the chronograph by Harvard and Greenwich astronomers during the 1850s. Previously, the term "personal equation" had referred to specific differences between named pairs of observers; thus, between 1823 and 1834 Bessel worked to ascertain comparative differences between himself and leading astronomers in Germany and Russia. In 1820 Bessel worked with Walbeck at Königsberg and established that on average he was 1.041 seconds slower than his colleague; Walbeck visited Friedrich Wilhelm Struve at Dorpat in Estonia in 1821. By 1834 five different direct or indirect comparisons between Bessel and Struve had been made, and these demonstrated the significance of the personal factor (Boring 1950, 137; Stigler 1986, 240–42).

Personal visits by astronomers to the great observatories now took on a specific function in Bessel's program. Instruments such as the chronoscope and the chronograph enabled the measurement of absolute rather than relative personal equations.

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[1] "My Assistant, Mr. David Kinnebrook, having at this Time unfortunately commenced a vicious way of observing the times of the Transits too late, it will be necessary to make an allowance for those Errors where his Observations, distinguished with the initials D. K. of his name, are intermixed with mine, that is where one of us observed the Star or Stars, and the other the Sun or Planet whose Place was to be settled from them" (Maskelyne 1799–1800, 3:319). Compare the chronological context provided in Alkon 1982.

Standard methods of observing stellar transits in the early nineteenth century depended on coordinating the observed movement of the image across the wires of the micrometer and the audible sound of a standard clock – the "eye-and-ear method." The strategy adopted by Airy at Greenwich in the 1850s involved the combination of the motion of an "artificial star" whose speed of transit could be standardized, with a recording device in which the initiation of a galvanic signal could record the elapsed time as measured by the astronomer under test. Airy's recording device included a uniformly rotating drum and a powerful electromagnet to produce a calibrated trace; the "artificial star" was typically produced as a point image of a bright light (Airy 1856; Howse 1975, 45; Bennett 1980, 279; King 1976). The intervention of this complex electromagnetic technology between observer and image had profound implications for the status of astronomical observation. Furthermore, the technology of the personal equation represented the highest accomplishment of a program which quantified a network of collective astronomical observers (Duncombe 1945, 63–76). But historians of quantification in psychology tell a simpler tale.

Boring argued that "the discovery of the personal equation by the astronomers and their later success in measuring absolute personal equations led into both the complication experiment and the reaction experiment of the new scientific psychology." That is, the history of the personal equation is colonized by a teleological account of the emergence of "the experimental dynamic psychology of motivation." Wundt's speech before a gathering of astronomers at Speyer in 1861, together with the work of Donders and his student de Jaager on discrimination, during that decade, are interpreted as the psychological discovery that "the personal equation is measuring a psychological phenomenon." Historians of psychology make it seem inevitable, for example, that in 1865 de Jaager would see the registration techniques used at Utrecht Observatory as an obviously psychological problem (Boring 1961, 116; Boring 1950, 147–49; de Jaager 1970, 43–48). In fact, such a "discovery" represented a careful exercise in the expansionist policy of laboratory psychology. As Boring goes on to suggest, here quantification was the sign under which German experimental psychology seized upon whole ranges of practices developed within a wide variety of other disciplines. It is now familiar to historians of the sciences that disciplinary histories like that of quantitative experimental psychology are powerful resources in the establishment of boundaries around areas of inquiry. This disciplinary history places the personal equation within the prehistory of a psychology, and attributes a puzzled incomprehension to the teams of astronomers who so graciously, yet unwittingly, offered this psychology its finest chance for disciplinary growth.[2]

This simple tale makes the problem of astronomical "personality" look obviously psychological. No attention is paid to the ways astronomers handled this issue. Quantification is held to be inevitable and progressive. The incorporation of the

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[2] For quantification in psychology, see Bresson 1972. For critiques of Boring, see Brozek 1970; Danziger 1979; O'Donnell 1979.

personal equation into experimental psychology needs no explanation save that of the self-evident need to make psychology into a science. Finally, it is assumed that quantification is a universal good. Who could contest the increase in precise measurement promised by the transposition of instruments such as chronoscopes into the psychology laboratory? Those contemporaries who observed that the laboratories of the "new psychology" seemed more concerned with telegraphy than psychology were merely testifying to this advance. It is as though quantification were value-free. However, as shown in studies of William Thomson's Glasgow laboratory (Wise and Smith 1986; Wise 1988), the values which experimenters measure are the result of value-laden choices. Wundt, Titchener, or Cattell all organized their laboratories to enable particular parameters to be measured and to make those measurements meaningful. It is impossible to separate processes of quantification from the preferred work styles which sustain them (Kuhn 1961). In 1884, working at Wundt's Leipzig lab, Cattell attacked his master's chronoscope technique because it ignored variations in induction times in the magnet. Cattell designed a new "gravity chronoscope" which would escape this trouble and would "also enable me to carry on my experiments alone." Sales of the instrument helped diffuse reaction-time techniques in the United States, spreading new values of quantification with it (Sokal, Davis, and Merzbach 1976). Social technologies organize workers to make meaningful measurements; material technologies render specific phenomena measurable and exclude others from consideration; literary technologies are used to win the scientific community's assent to the significance of these actions. Such episodes indicate how these technologies work together. The formation of a *discipline* is simultaneously the process of organizing work to produce these values and the system of knowledge which gives the values their meaning (Shapin 1984).

Attention to discipline suggests different questions about the personal equation. The new measures introduced by Bessel and his German colleagues into early nineteenth-century positional astronomy were accompanied by a new variable whose meaning was ill-defined. The chronometric techniques developed by Airy and his contemporary observatory managers were designed to answer this need. How did these technologies change the regime of the observatory? The personal equation directly calibrated the disciplined performance of the observer. As H. M. Collins has suggested, calibration "is the use of a surrogate signal to standardize an instrument" (Collins 1985, 100–106). The surrogate is supposed to have the same effects as the signal whose character is ill-defined and disputed. The status of calibration depends on the plausibility of this identity. If the identity can be accepted by members of the relevant group then the possible strategies which members can use to investigate and describe the troubled source are more closely constrained. This constraint is a social process in the organization of experimental work. Precisely this feature emerges inside nineteenth-century astronomy. The observer was part of the "instrument" to be calibrated. Artificial stars and galvanic clocks substituted eye-and-ear methods. The act of observation was destroyed and then painstakingly rebuilt through a range of surrogates for some notional "direct" experience. This rebuilding accompanied a process of social reorganization. The observatory became a factory, if not a "panopticon." "Mere" observers were relegated to the base of a hierarchy of management and vigilance, inspected by their superiors with as much concern as were the stars themselves. Observation was mechanized, and observers transformed into machine minders. At Greenwich, such workers clocked in, kept regular hours, and were supervised by an ever-watchful management. The same fate was meted out to the calculators. Division of labor demanded precise control over an increasing range of menials.

Intriguing aspects of these changes include the moral and social connotations of the observatory hierarchy. "Personality" could be disciplined through the right moral conduct of the workplace and right moral habits of the work force. The stratification of and collaboration between astronomical workers are highly comparable with the coherence of experimental cultures and styles of work traced in more modern large-scale laboratories (Pickering 1984; Pinch 1986; Galison 1987, 267–70; Galison 1988). A further question also emerges: astronomy displayed itself to its public as the science of the empiricist, the hero, and the solitary. At least part of its astonishing status in the nineteenth century relied upon the image of the nocturnal stargazer locked up in his tower. But astronomy's command demanded the interchangeability of observers, not their isolation. Because of factors such as personality, observers separated in space and time had to be calibrated with complex social and material technology. How did astronomy's spokesmen reflect this fact? The question of the observatory as workplace and that of the astronomer's public image connect the problem of personality with the wider issue of control. Who was to manage observation? Who could be trusted? The well-known troubles of professionals and amateurs intersect our concerns here.

The following sections of this paper seek to respond to these inquiries. Attention is paid to the organization of Greenwich and other observatories as the disciplinary technology of chronometry interacted with the astronomical workplace. Some remarks are made about the rival images of astronomical work offered to the nineteenth-century public. Last, the paper presents an episode of professional and amateur conflict in the 1890s in which the problem of personality figured large in rival claims to custody over astronomy's authority. It is at this point that the psychologists' claim for power over the personal equation reappears, not as an obvious and inevitable advance in understanding and precision, but as one among many contested moves to measure, control, and discipline the act of observation.

Disciplining the Observer

When Maskelyne died at Greenwich in 1811 his observatory staff contained but one assistant, a replacement for the unfortunate Kinnebrook. By the end of the century the staff had expanded to fifty-three, including ten assistants, six established and twenty-four supernumerary computers (Maunder 1900, 98, 137–39; Meadows 1975, 7–14). The method of recording transits had changed too. William Ellis, a retired worker at Greenwich, reminisced about the traditional eye-and-ear method, in force until 1854. Using the beats of a nearby pendulum clock as the image of the star crossed the wires of the eyepiece, "it was in all cases my custom to take the second off the clock as the object approached the first wire, count through the transit without looking at the clock, and invariably check the counting after passage at the last wire" (Ellis 1897, 313–14). Airy's new transit circle and his electric barrel-chronograph dictated a new regime. A detailed description was provided by Walter Maunder, assistant at Greenwich Observatory from 1873: a galvanic button was pressed to initiate the timing of the transit when the star's image crossed each of ten vertical wires in the field, and a separate signal was used to record the position of a moveable horizontal wire coinciding with the image's path. A second observer read seven circle microscopes penetrating the pier of the telescope to record declinations of arc. In total, measurement of right ascension, declination, air pressure, and temperature required twenty-two recorded numbers (Maunder 1900, 188–92; Howse 1975, 45–46; Meadows 1975, 35–38).

The coordination of self-registration, prompt and accurate reduction of observations, and a rigid timetable for assistants' work, including the practice of clocking on introduced by Airy for all his subordinates, promoted a severe disciplinary regime at Greenwich (Airy 1896, 203). Maunder recalled that under Airy's "remorseless sweating," assistants would not survive the strain past the age of forty-six. The computers were typically started as teenage boys, later dismissed at short notice at the age of twenty-three. Promotion was possible on obtaining a certificate of competence at observation. "The system of combining the labour of unattached computers with that of attached assistants tends materially to strengthen our powers," Airy noted, pointing out how division of labor aided the observatory's productivity. Such concerns were not solely Airy's prerogative. During the 1820s, the mathematician Charles Babbage worked hard to win government support for his efforts to mechanize the calculation of astronomical tables. In his analysis of political economy and machinery published in 1832, Babbage used the French tactics of dividing computation into a hierarchy of skilled and unskilled workers as his chief example of the virtues of control over a disciplined work force. Unskilled computers "were usually found more correct in their calculations than those who possessed a more extensive knowledge of the subject." Astronomical computation should ape "that of a skilful person about to construct a cotton- or silk-mill, or any similar establishment" (Babbage 1835, 191–96; Berg 1980, 182–91). At Greenwich, in the same period, Airy's immediate predecessor, John Pond, had already demanded "indefatigable, hard-working, and, above all, obedient drudges" as assistants and computers. Airy's criticism of Pond's regime was not its observational but its administrative failings. The ideal observatory became indistinguishable, according to Maunder, from a Whitehall office, with its ledgers full not of "income tax schedules" but stars (Airy 1896, 216; Laurie 1976; Maunder 1900, 100, 137, 140; Meadows 1975, 9–10).

For Airy, this "thoroughly Saxon man," as his Cambridge colleague James Stuart described him, moral discipline went hand in hand with quantitative discipline (Crowther 1968, 360). He told the Admiralty that only "a man of respectable rank in society" could be secure "against the danger of being corrupted by makers of chronometers and of losing authority over the subordinate assistants by having recourse to them for assistance in disgraceful difficulties." Cambridge men normally satisfied these requirements. Otherwise, lowly and worse-paid assistants, such as the veteran John Henry Bellville, would simply be praised for their "gentlemanly manners. . . . [These] things are important in an institution where, the salaries being generally low, we have difficulty in finding assistants above a very low rank." Manners were of the essence for Bellville's task. Between 1836 and 1856 he toured London clockmakers, checking their timepieces with a standardized chronometer (Meadows 1975, 2, 69). Airy's telegraphic system of time communication obviated the need for such tasks. When he developed connections between the Greenwich recorders and the railway telegraph network in the early 1850s, he boasted not merely of the links now available between observatories, but also that "that Royal Observatory is thus quietly contributing to the punctuality of business through a large portion of this busy country" (Airy 1896, 215–16; Bennett 1980, 282).

Historians of the Airy regime have rightly stressed the "factory mentality" which dominated the observatory (Chapman 1985). Its significance is not limited to Airy's astonishing disciplinary vigilance and moral rectitude. The connection between division of labor at the observatory, the self-registering instruments, and the electric telegraph was one aspect of a process by which networks of observatories were established, and by which these international networks began to be coordinated and allied with the powerful grid of commerce and empire (Headrick 1981). The great European observatories, such as Arago's at Paris and Quetelet's in Brussels, were key nodes of this grid. Both Arago and Quetelet were early managers of systems for minimizing the effect of personality. Arago sought to separate optical and auditory components of recording, and to make a trigger mechanism for marking elapsed time on the chronometer (Arago 1853). Quetelet, as we shall see, collaborated with Greenwich in assessing the personalities of his observers. Other model institutions of astronomical discipline included the Göttingen observatory under Gauss and Harding (1816), where personalities were first assessed in 1833, and Struve's headquarters at Pulkovo (1839), built on the Göttingen plan of telescope mounting and division of tasks, representing the center of a new program of precision stellar astronomy and geodesy whose aims included surveys of the Russian empire and, as one visitor put it, "to assist and encourage young astronomers . . . just as [Louis] Agassiz does or professes to do in Zoology at Cambridge [Mass.]" (Cowan 1912; Donnelly 1973, 57–74; Szanser 1972; Reingold 1964, 139).

The interests of training, business, and navigation were linked to those of the observatory managers and the state. Time was a commodity which Airy and his colleagues controlled and distributed. The controversial establishment of the Prime Meridian in 1884 was a mark of this authority. It has been argued that the anarchist assault on Greenwich Observatory, fictionalized by Conrad in 1907, "could not have picked a more graphic symbol of centralized political authority" (Howse 1980; Kern 1983, 11–16). Recent work on the connections between the power of the nineteenth-century laboratories, such as William Thomson's at Glasgow or Louis Pasteur's at Paris, and these networks of communication and dependence, help illuminate the observatories' role (Wise and Smith 1986; Latour 1984). Cannon's notion of "Humboldtian science," programs which combined arrays of precision measurement with aims for a science of the whole planet, is also a feature of the observatories' enterprise. But the enterprise affected both the external and the internal workings of observatory management. Astronomers' institutions provided the models for new earth sciences such as geomagnetism and meteorology. In 1845 John Herschel told the British Association for the Advancement of Science at Cambridge that "every astronomical observatory which publishes its observations becomes a nucleus for the formation around it of a school of exact practice" and that magnetism and meteorology must now form part of these practices. Airy's developing concerns with geomagnetism bound his institution more firmly to these networks and, in turn, prompted further differentiation of functions within the Greenwich system. Organizations such as Greenwich or Pulkovo divided their functions between departments of terrestrial and astronomical, chronometric and stellar affairs (Cannon 1978, 73–110; Herschel 1846, xxix; Cawood 1977).

Discipline and hierarchy inside the observatories went hand in hand with the formation of a disciplined world outside their walls. Latour argues that scientists make this external world susceptible to measurement by changing it to conform to the regime within their laboratories. Pasteur's heroic vaccine trials involved the transformation of a farm into a field station (Latour 1983). Astronomers faced the same task. This was the context for their construction of techniques to manage the personal equation. In assessments of the difference in observatory positions, in comparisons between observatories' data, and in celebrated international collaborations such as eclipse expeditions or the transit of Venus in 1876 (see figure 2), astronomers carried with them their disciplined regime, with its new complex technology. Herschel said that "a ship is an itinerant Observatory." Such mobile field stations had to be made susceptible to the management imposed in the metropolitan headquarters (Herschel 1846, xxxv).

With this technology the problems of personality were confronted and controlled. For Airy, a key link was the connection between Greenwich and Pulkovo, using Altona as an intermediate point. In 1844 he organized the dispatch across the North Sea of forty-two chronometers in the care of transit observers who "were twice interchanged, in order to eliminate not only their Personal Equation, but also the gradual change in Personal Equation" (Airy 1896, 166). Two years later Airy sought to enlist the German astronomer P. A. Hansen in his program of research into lunar motions. Airy commissioned a new altazimuth to confirm the superiority of Hansen's work to that of previously accepted data. As Meadows points out, such changes also changed the regime of observation at the observatory. Collaboration with the

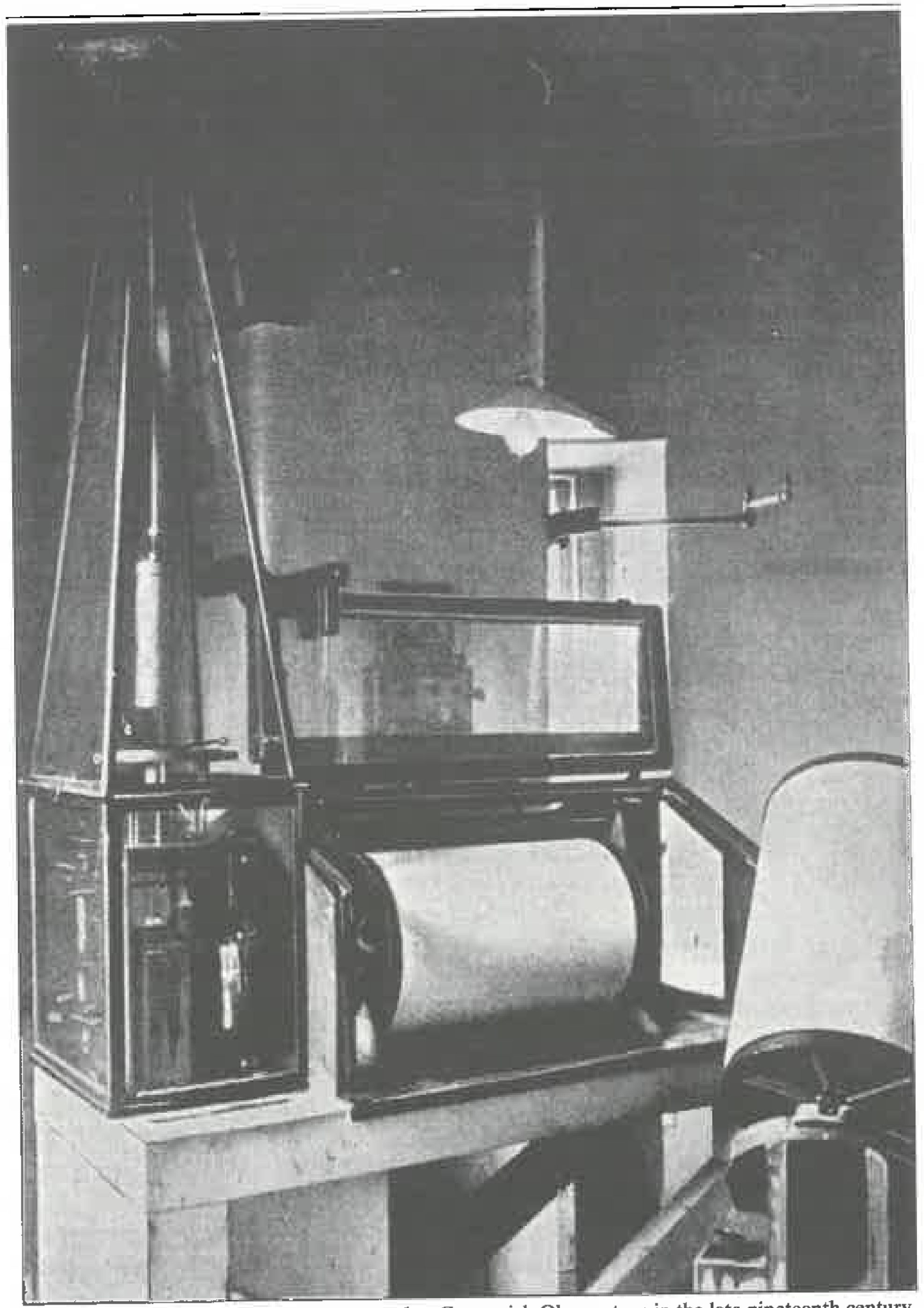

Figure 1. The barrel-chronograph as used at Greenwich Observatory in the late nineteenth century. (Source: Maunder 1900, 158.)

German astronomer proved justified and fruitful: the new figures also revealed that one of the Greenwich observers had a personality which differed for lunar and for stellar observations (Meadows 1975, 34–35).

Collaboration with other astronomers also led to the decisive move in the management of observers' personality: the use of the galvanic barrel chronograph, from 1854, to accompany the transit circle and the altazimuth (see figure 1). This was a technology Airy adopted from Bond, at Harvard, and Ormsby Mitchel, working at Cincinnati. Mitchel told Airy about his strategy for the establishment of "the observer's absolute personal equation, or what I shall term hereafter, the 'personality of the eye'." Mitchel wrote to Greenwich of his work on staff at Cincinnati, and simultaneously on a range of thirty other subjects, not astronomers, to ascertain the range and variation of these absolute personalities. "Unconscious anticipation" emerged as an almost universal tendency among his subjects; Mitchel prescribed both "special attention" and the adoption of his complex electromagnetic chronographic technique. Mitchel argued that "the large differences existing between observers . . . are not due to physiological constitution, but almost entirely to false habits of observation." These habits could be disciplined with the new technology. While the observatory at Cambridge University expressed interest in Mitchel's proposal, Airy followed suit enthusiastically (Mitchel 1858; Mitchel 1860, 195; Hetherington 1983, 75). He found that the Mitchel method needed further division of labor: new tasks included galvanic engineering, paper preparation, and conversion of paper traces into figures. But the size of the personal equations could be halved this way, and, using his observer Edwin Dunkin as the standard of personality, Airy was able to assemble a reliable tabulation of personal equations and their changes (Meadows 1975, 35; Airy 1896, 218, 222–23).

The problems were intimately linked with the new technologies used to build the international astronomical network. Greenwich was dotted with flat stones marking sites at which foreign astronomers had set up their transit instruments in order to compare their personalities with Greenwich staff (Maunder 1900, 175–77). Quetelet cited the Kinnebrook anecdote at some length when working with Sheepshanks on the junction between the observatories of Greenwich and Brussels in 1838. He wrote that these problems were a question of failings of astronomical training: "this circumstance is particularly marked in those who have not yet acquired a lengthy habit of observation." It was a problem of fatigue and of experience to be solved with the techniques of astronomical discipline (Quetelet 1843, 13). Remarks such as those show that while astronomers recognized that the personal equation was a problem of the observer's character, it was not to be handed over for psychological investigation. Consider two statements from the beginning and the end of the nineteenth century. Maskelyne describes his problems with Kinnebrook by detailing the beauties of his Graham clock and Bird transit instrument. He appeals to the highest agreed tribunal – the precision instrument makers of eighteenth-century London. He then describes the eye-and-ear method, established by his great predecessor Bradley. Finally, he defines what must be wrong: "I cannot persuade myself that my late Assistant continued in 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. . . . The great thing is to aim always at the truth, and avoid any partial method of observing" (Maskelyne 1799–1800, 3:339–400). At the end of the century, Maunder described Kinnebrook as "a martyr of science." The problem of personality was an aspect of human character, but it was *therefore* manageable by astronomical discipline:

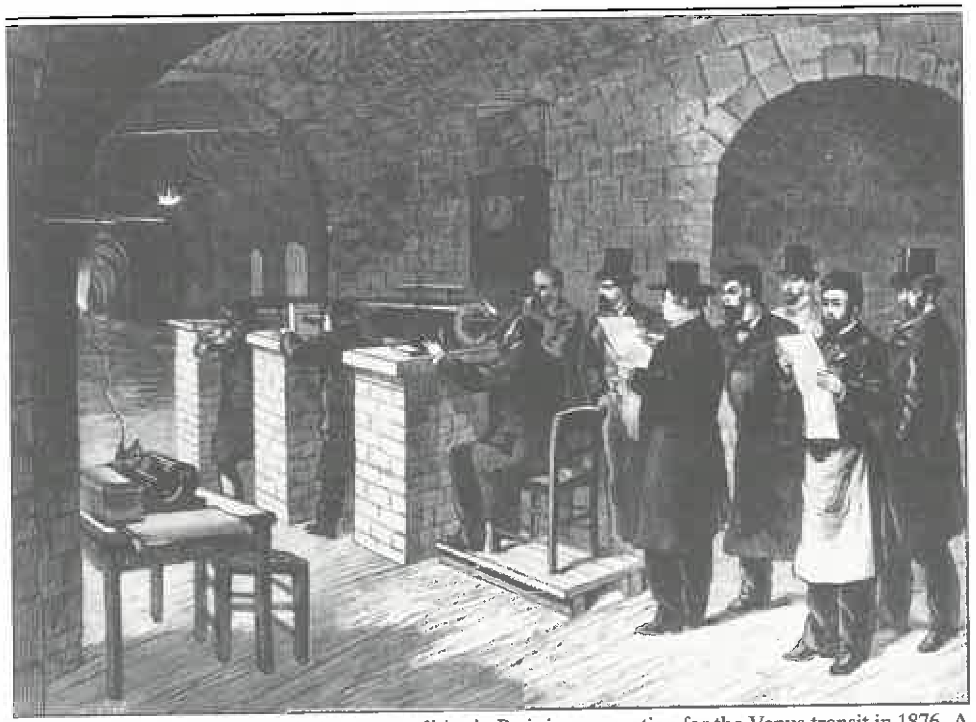

Figure 2. Tests of observers' relative personalities in Paris in preparation for the Venus transit in 1876. A model of the planet's transit across the Sun's face is 100 m away at the far end of the tunnel; a galvanic barrel recorder is at front left. Subjects were expected to depress the galvanic buttons when timing the apparent transit, yielding measures of their differences. (Source: *L'Illustration* 68:392.)

> There will be a constant difference between the eager, quick, impulsive man who habitually anticipates, as it were, the instant when he sees star and wire together, and the phlegmatic, slow-and-sure man who carefully waits till he is quite sure that the contact has taken place, and then deliberately and firmly records it. *These differences are so truly personal to the observer that it is quite possible to correct for them.* (Maunder 1900, 177; my emphasis.)

Between the time of Maskelyne and that of Maunder, the concept of the personal equation emerged. But this emergence did not involve a loss of authority by the astronomical community over the observer's personality. That authority became even more secure. It did involve a loss of the observer's authority within the discipline of astronomy. That loss was of immense importance for astronomers' styles of work and for their public image.

Between the Watchtower and the Workshop

Expense, status, and the evaluation of observers demarcated a relatively novel community of "technical astronomers." Their work was contrasted with that of the humble. Like other observatories, Lord Rosse's giant reflector, the "Leviathan" at Parsonstown in Ireland, received streams of fascinated sightseers. In 1854 Airy told a female amateur that "if a night is fine, it is wanted for [Rosse's] use or for the use of professional astronomers. If it is not fine, it is of no use to any body . . . the appropriation of the telescope on a fine night to any body but a technical astronomer is a misappropriation of an enormous capital of money and intellect which is invested in this unique instrument" (Airy 1896, 221–22). The manager of the Parsonstown reflectors, Thomas Romney Robinson, had been one of the earliest British astronomers to measure personalities, using the solar limb in 1830 as his standard observation (Robinson 1859, x–xii). When William Rowan Hamilton was contemplating taking up the Dublin astronomy chair, he told Robinson that he was unwilling to commit himself to the tasks of observation; Robinson replied that Airy loathed observation and Pond had abandoned the work of stargazing:

> It is not necessary for a man to observe, himself; he may render . . . most important services to Science by his calculations, and make his assistants observe for him. Schumacher [at Altona] observes very little himself, but is very accurate in superintending his assistants. . . . Bessel would be a first rate Professor of Astronomy, even though he never put his eye to a telescope. (Graves 1882–89, 1:432.)

The demarcation between the superintendent and the observers was often stresed. In his popular lectures aimed at raising funds for new American observatories, Mitchel argued that this demarcation was necessary because "the observer himself is but an imperfect and variable machine, utterly incapable of marking the exact moments required" (Mitchel 1860, 176). John Herschel agreed. "The abridgement of the merely mechanical work of such observatories by self-registering apparatus is a subject which cannot be too strongly insisted on," he told his audience at the British Association. "The merely mechanical part of the observer's duty" must be minimized in order to liberate the astronomer from error and degradation (Herschel 1846, xxxiv). Mitchel pointed out that the new chronometric techniques adopted at Cincinnati and now extended to Greenwich changed what was required of an accomplished observer. "While in the old method long practice was required to make an accomplished observer (the best of whom could not record more than the transits on seven wires) in the new method a few nights of practice give all desirable experience" (Mitchel 1860, 195). The problem of personality was a part of the problem of the social position of the observer. This problem mattered because the status of the

astronomical observer was a key item in astronomy's public appeal. Mitchel was one of many who tried to win public support, often of a tangible, financial kind, for the new regime embodied in published tables, precision instruments, and fine observatories. Good observation was rapid, complex, and expensive. Legitimacy for the new techniques was a prize worth winning.

The history of the personal equation is marked by astronomy's nineteenth-century legitimation crisis. In 1851 Mitchel staged a demonstration of the accuracy and efficiency of his galvanic chronometry when the American Association for the Advancement of Science visited Cincinnati. By enlisting allies such as the Harvard astronomer Benjamin Peirce he quelled criticism of the precision he claimed, and so helped the mechanization of observation (Hetherington 1983, 75–76). His lecture tours and displays at his own observatory had similar ends (McCormmach 1966). The simplicity and permanence of the astronomical enterprise, and the identity of all apparently variant individual observers, formed Mitchel's theme:

> Let it be remembered that the astronomer has ever lived, and never dies. The sentinel upon the watchtower is relieved from duty, but another takes his place, and the vigil is unbroken. No – the astronomer never dies. He commences his investigations on the hill-tops of Eden – he studies the stars through the long centuries of antideluvian [*sic*] life. The deluge sweeps from the earth its inhabitants, their cities, and their monuments; but when the storm is hushed, and the heavens shine forth in beauty, from the summit of Mount Ararat the astronomer resumes his endless vigils. (Mitchel 1850, 5.)

The establishment of a worldwide network of equally heroic observers was fuel for this encomium – Mitchel writes that "the watchtowers of science now cover the whole earth, and the sentinels never sleep" (Mitchel 1850, 21). Mitchel accompanied these images of the observer as eternal hero with the lessons he claimed pure observation now taught. Among these was the truth of the nebular hypothesis, the claim that this solar system originated from a condensing cloud of luminous fluid. These views drew massive public interest to Mitchel and his colleagues. They seemed relevant to terrestrial concerns with moral progress (Brooke 1977; Numbers 1977). When astronomy made news, lecturers like Mitchel could profit. But in the very same lectures astronomers described themselves as perfect sentinels, and yet noted the careful discipline which such sentinels imposed upon their subordinates to make their work count as precision astronomy.

Astronomers seemed to be isolated observers on their watchtowers, but they lived as managers of mathematical workshops. It seemed necessary to minimize the mechanization of observation in order to preserve the astronomer's empiricist prerogative and win public support. But this support was needed to fund the expensive technology upon which astronomy's standing depended.

Mitchel's ambivalent use of the imagery of empiricism and mechanization was shared by his colleagues. In September of 1847 the Harvard astronomer William Bond obtained an expensive German reflecting telescope, the twin of Struve's great instrument at Pulkovo. He also planned the introduction of the new technologies for checking personality. He had to persuade his audience that the cost of his reflectors and transit instruments was justified. Within days he was able to announce that his reflector had resolved the Orion nebula while Rosse's giant instrument had not. This nebula was widely held to be composed of truly nebulous fluid, so not resolvable into stars. Orion was a very well known object to Bond's public: its fluid character was often cited by lecturers such as Mitchel as good evidence for the truth of the nebular hypothesis. Today, the nebula is not held to be resolvable; it is possible that Bond was misled by seeing Orion against a background of faint stars. But when he told the Harvard president about the new telescope, Bond could "see no other way in which the public are to be made acquainted with its merits" save by announcing this resolution, however premature this news. Here is evidence of the relationship between astronomy's public image and the behavior of astronomical observers (Numbers 1977, 35–36; Jones 1968, 57–61).

Further evidence is provided by the career of one of the nebular hypothesis' staunchest disciples, John Pringle Nichol. In 1836 Nichol began to rebuild Glasgow Observatory with expensive German telescopes and clocks, then sought support from the city: "I have not the slightest doubt that if Astronomy were shown to them in its true character the Glasgow people could themselves build an observatory." It would become "one of the most magnificent establishments in the Empire" (Nichol 1836). The expense drove Nichol and his supporters to bankruptcy, compelling him to stage tours in Britain and the United States and to engage in a massive publishing campaign to recoup his losses, producing some of the century's most influential texts on popular astronomy and the nebular hypothesis. In winter 1847–1848 he met Mitchel in New York. Mitchel struck Nichol as "full of charlatanism, having just persuaded large audiences that between him and the honor of American astronomy was then an indissoluble alliance" (Nichol 1848). This was the reaction of a successful lecturer to his closest competitor. The significance of the relation between astonomy's popularizers and the mechanization of the observatories was made clear when Nichol lobbied the Treasury for finance. The government responded that Glasgow Observatory would be supported if Nichol agreed to join in Airy's Greenwich program of precision transit measurement and magnetic and meteorological recording. After Nichol's death in 1859 he was replaced by Robert Grant, a staunch disciple of Airy who soon made Glasgow a cog in the machine of precision technical measurement (Coutts 1909, 389–90).

This machine needed a history which differentiated itself from the mythical empiricism of the watchtower. Mitchel used the watchtower image to summon up the continuity of astronomical records and the invulnerability of the observatory to outside interest, whether in the form of political strife or more mundane perturbation. Even the architectural figure to which Mitchel made reference was becoming outdated. The last tower observatory was probably that built at Bogotá in 1802. Instead, the fashionable new model had a central dome in which the telescope took pride of place, situated firmly on massive piers and supported with rooms for the subsidiary staff of mechanics, calculators, and observers under the gaze of the observatory manager.[3] An 1810 English encyclopedia commented that "this description, a tower erected on an eminence, though it may apply to ancient observatories does not agree with modern practice, where firmness of foundation and a convenient disposition of instruments are considered of more importance." Such were the new layouts of the great German observatories such as that of Gauss at Göttingen or the astrophysical center at Potsdam (Donnelly 1973, 55, 147). Under the German hegemony of Bessel and his colleagues, astronomy recognized its own historical continuity and contemporary power as that provided by the enterprise of reduction of accumulated observation through the analysis of sources of error. Herschel held that this showed that "astronomy is a science peculiarly in unison with the German national character . . . that painstaking scrutiny which penetrates through all details, and will not be satisfied till perfection is reached" (Herschel [1829] 1857, 514).

Herschel was reporting to the Royal Astronomical Society on the award of medals to Bessel and to Heinrich Schumacher, who helped coordinate this program by editing the chief astronomical periodical of the time and disseminating Karl Gauss' methods for analysis and reduction, part of the basis for Bessel's program. This prompted Bessel's first efforts to construct a model of the relative personal equation – reduction necessarily involved the identification and elimination of a series of perturbations both physical (such as aberration, nutation, or atmospheric refraction) and personal (Herrmann 1976; Duncome 1945, 2–13; Stigler 1986, 140–48, 202–3). His colleague Wilhelm Olbers originally drew Bessel's attention to the data available from Greenwich, principally that of Bradley and Maskelyne. The result of the work Bessel pursued at Königsberg was issued in two great volumes, the *Fundamenta astronomiae* (1818) and the *Tabulae Regiomontanae* (1830) (Williams 1981; Erman 1852, 1:96–98).[4] Significantly, Herschel saw Bessel's work as "the perfection of astronomical book-keeping." For European astronomers Bessel's "great mass of scientific capital" provided both the practical and economic basis of their discipline. The personal equation was part of this new accountancy (Herschel [1829] 1857, 510). Hence the comment of Maunder in his popular history of Greenwich Observatory that for the astronomer "life, on the one side, approximates to that of the engineer; on the other, to that of the accountant" (Maunder 1900, 15).

The contrast between the observer and the accountant suggests an important connection between the personal equation and nineteenth-century debates on scientific method. Discipline stratified astronomy. Hierarchies of empiricism and theory, between the elite savants and the humble cultivators, posed thorny problems for the spokesmen of Victorian science, such as Airy, Herschel, and their Cambridge colleague William Whewell (Yeo 1981, 1986). For Whewell, celestial mechanics dominated the "permanent" sciences, and, as he wrote in December 1834, was "of course my pattern science" (Todhunter 1876, 2:199). In the section on "the

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[3] For a Renaissance observatory and the subtle connections between design and the vision of proper research, see Hannaway 1986, who brilliantly demonstrates the changing ideology of such structures.

[4] For the background to Bessel's work, see the excellent discussion in Williams 1981, chap. 4.

instruments and aids of astronomy during the Newtonian period" in Whewell's *History of the Inductive Sciences* (1837), he said collaborative precision was "a labour which is still going on, and in which there are differences of opinion on almost every point; but the amount of these differences is the strongest evidence of the certainty and exactness of those doctrines in which all agree." Airy figured in Whewell's story as the chief exponent of this ultimate precision and ultimate observational success; the personal equation was mentioned only in the third edition (1857), and then because Whewell claimed that the effects of the "errour" had now been palliated. The replacement of eye-and-ear methods by "the combination, and almost identification, of observation by touch with its record by galvanism" was worth registering in the *History*. This was because it had now been introduced at the ideal observatory, and because it added galvanic batteries to the list of necessary equipment for the ideal science (Whewell [1837] 1857, 2:221–23, 473–74).

Authorities such as Airy and Whewell were well known for their deep suspicion of a wide-ranging "Baconianism," if this were interpreted as a license for leveling technical skill. Whewell's addresses to the British Association from as early as 1833 made this all too clear: scientific discovery was the preserve of the proficient. At the Cambridge meeting of that year, Whewell's lecture nicely balanced a harsh condemnation of the hopes of mere cultivators to aid the progress of astronomy, "the queen of the sciences," "the labour of the most highly gifted portion of the species for 5000 years," with a consoling offer of certain tightly limited tasks suitable for the devotee. Whewell declared that untheorized fact collection was strictly useless, and that "even in subordinate contributors to science" the highly ordered work of the mathematically equipped supervisors was essential (Whewell 1834, iii, xxiii–xxiv; Morrell and Thackray 1981, 267–75). This methodological theme was closely connected with a specific political goal which Whewell and Airy sought to attain in 1832–1833, the public endowment of the program for the reduction of astronomical observations. Plans for the use of networks of variably trained observers immediately raised the problem of the personal equation (Cawood 1979). In aiming for a certain degree of government support, the Cambridge strategy needed to be nicely judged. Airy's "Report on the Progress of Astronomy during the Present Century" (1832) to the British Association for the Advancement of Science, included a bold critique of the common English model of astronomical observation. The career of the Association would be marked by its managers' commitment to close contacts with the elite of the European astronomical network. Arago, Quetelet, Schumacher, and Bessel achieved high status within the organization (Morrell and Thackray 1981, 328, 372–86). Airy, then director of the Cambridge Observatory, used his idolatrous admiration of Bessel's reduction of Bradley's observations in 1818. Airy harangued his audience by appealing to patriotism and to theory. Whereas "in England an observer conceives that he has done everything when he has made an observation," Airy claimed that "in foreign observatories" quantitative tabulation and reduction "are considered as deserving more of an astronomer's attention, and demanding greater exercise of his intellect, than the mere observation of a body on the wire of a telescope." Airy hinted that he hoped for major support for reduction of more Greenwich data (Airy 1832, 182–87).

Whewell picked up the hint the following year at Cambridge, quoted Airy in extenso, and then spelt out the implications for this view of astronomical observation. "The suggestions which the observations themselves supply, for change of plan or details, cannot in any other way be properly appreciated and acted upon . . . it cannot but grieve us to see so much skill, labour and zeal thus wasted." The campaign worked. Airy, Herschel, and Francis Baily successfully lobbied the Association and the government for finance for the reductions – in 1840–1841 a further demand for support for the reduction of the Greenwich lunar observations was equally triumphant. Substantial sums were won by Airy and his team, working through the Association, to reduce and publish Greenwich's, Paris', and South Africa's observations. This was a central part of the program to construct a worldwide astronomical network, but it also reinforced stratification within that enterprise (Whewell 1834, xx–xxi; Morrell and Thackray 1981, 509–12; Meadows 1975, 29–32).

By the end of the century it was possible to draw a sharp line between popular misunderstanding and professional discipline. Maunder satirized "the vivid and highly-coloured picture of the astronomer at his 'soul-entrancing work'." He contrasted the notion that "the fortunate astronomer sits at his telescope and *discovers* – always he *discovers,*" with the more accurate notion that "the professional astronomer has hardly anything to do with the show places of the sky." Maunder insisted that the search for comets and asteroids, the drawings of the surface of the planets, and spectroscopy were all "most interesting to the general public, and followed with much devotion by many amateur astronomers. For this reason it does not form an integral part of the program of our State observatory" (Maunder 1900, 13–15, 143). This startling claim was a highly polemical maneuver in the polity of late Victorian astronomy. Maunder was founder of the amateurs' British Astronomical Association and editor of their journal. Throughout the 1890s he firmly refuted reports of sightings of Martian "canals" and the possibility of Martian life. This was linked with Maunder's deep religious hostility to the plurality of worlds. He frequently stated that in Martian observations, as elsewhere, there was "a great divergency between the description of different observers" and that "we cannot assume that what we are able to discern is really the ultimate structure of the body we are examining" (Crowe 1986, 490, 505–6). The problem of reliable observation and the hierarchy of professional and amateur dominated the process by which the personal equation, as a problem resolvable inside the technical astronomy of the elite observatories, came to be debated by amateur observers and by professional experimental psychologists.

Amateur Imagination and Professional Skill: A Contest About Personality

The issue of professionalism in nineteenth-century astronomy has drawn considerable attention (Lankford 1979; 1981; Williams 1987). In the 1890s it involved the meaning of the personal equation. The debate on amateur astronomy reached furious levels during the work of the Devonshire Commission on Scientific Instruction and the Advancement of Science between 1870 and 1875, particularly in view of its recommendations for the establishment of new state-funded astrophysical observatories, along European lines, and separated from Airy's control. The Royal Astronomical Society was split in 1873 and again in 1881 on the issues of government support, amateur power, and the overwhelming authority of Airy's regime (Dreyer and Turner 1923, 173–78, 207–11; Meadows 1972, chap. 4). To understand the role of the personal equation it is necessary to emphasize striking features of astronomical culture in this period. Problems of perception were endemic. Struggles between amateurs and professionals on the merits of the large telescopes at the great observatories hinged on issues such as contrasting pictures of Saturn's spots (Alexander 1962, 201–3; Williams 1896; Lankford 1981a, 26). The more famous work of Schiaparelli, Lowell, and Maunder on the Martian canals also raised problems of differing perceptions of planetary features (Crowe 1986, chap. 10; Hetherington 1976; Burnett 1979; Hoyt 1976). Because of this problem, explicitly amateur organizations took care to police their members' observing practices. Accurate quantification became a mark of membership of such groups. Protagonists included Captain William Noble, a Sussex amateur, secretary of the Society for Opposing the Endowment of Research and first president of the British Astronomical Association, founded in 1890 to "afford a means of direction and organization in the work of observation to amateur astronomers" (Sellers and Doig 1948; Kidwell 1984, 538–39; Hollis 1904). Noble also figured prominently in the telescope-size disputes (Lankford 1981a, 21–22). Noble told his members in the British Astronomical Association to record the circumstances of observation as accurately as possible. Specialization was a necessity. Arthur Downing, Superintendent of the Nautical Almanac, told Noble's members that they should also attempt to include personal equations in their records (Wilkins 1976).

Finally, differentiations were preserved between the work at the great observatories – whether amateur or truly professional – and that of members of organizations such as the BAA and its predecessors. Noble and Denning were not unusual in their strenuous defense of amateurs' worth. They were untypical in the breadth of the charges they made against the professional hegemony. Noble made specialization the amateurs' virtue. His inaugural address endorsed the attack on large telescopes, and glanced curtly at the opulence of the great observatories. But he also indicated the large areas which disciplined amateurs could capture. Hence, when the amateur Stanley Williams was attacked for his claims about the appearance of the surface of Saturn in 1895, Noble leapt to his defense. Williams was "a skillful observer and not an imaginative man." In 1887 Agnes Clerke wrote, in the same tone of voice, that "a true eye and a faithful hand" were the sufficient and necessary condition of "good work in watching the heavens." The difference between skill and imagination was a vital issue for all analyses of the personal factor in observation (Noble 1878, 222, 329; Noble 1890, 1892, 1896; Clerke 1887, 6–7).

In the 1890s this issue became closely connected with the battles about the status of amateur observers. This was principally due to the work of the American astronomers Simon Newcomb and Truman Safford. The clash points to wider social implications of this process of refined measurement. Newcomb was Airy's most loyal disciple and imitator and was influential in the transformation of state-supported astronomy in the United States. He worked at the U.S. Naval Observatory and from 1877 was director of the Nautical Almanac Office. In the 1860s, Newcomb planned a radical reorganization in order to generate exhaustive planetary tables – this proposal necessarily involved the repetition of reductions of data from Greenwich, Paris, and Bessel's work at Königsberg (Plotkin 1978). This would generate fresh values for the reduction constants and for personal equations of the observers in America, France, Britain, and Germany. Newcomb's program was not completed until the mid-1890s, when discussions were initiated on a possible standardization of these constants. But as early as 1867, Newcomb suggested that the personal equation would vary with stellar magnitude because of variations in the time in which star images would reach a crosswire (Newcomb 1867). In the later nineteenth century this problem became urgent. Photography seemed to offer a new technical approach to the problem of the personal equation. Astronomers at Harvard and Cambridge were discussing the advantages and difficulties of transforming the personnel and practice of observatory work to accommodate this new resource. Lankford has pointed to the important social factors at play when, after 1885, astronomers at Lick were compelled to contemplate the adoption of photographic techniques (Lankford 1983). Debates about amateur work, the new institutional links between national observatories, and developing work in the new domain of experimental psychology all referred to the problem Newcomb had posed.[5]

Newcomb's proposals for publication of reduction constants to accompany tables of fundamental stars and to be adopted by the main observatory network were publicly aired at a Paris conference in 1896 and widely debated the following year. Some complained that at this meeting "the people who voted were not those chiefly concerned . . . [since] the people most affected by a change in the constants of the Nautical Almanacs are not those who make the Almanacs but those who use them." But Newcomb received influential endorsement from Arthur Downing, his opposite number at Greenwich and an important spokesman for disciplined observation and work on the personal equation within the BAA (Downing 1897, 1878). Downing's closest English ally was Robert Ball, astronomy professor at Cambridge. In 1897 Ball became president of the Royal Astronomical Society (Ball 1915). He immediately organized a presentation at the Society of a lengthy paper by his young assistant Arthur Hinks. Hinks was concerned with the problems of astronomical photography and their applications to Newcomb's program. His paper involved an examination of the

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[5] For the importance of the American effort to use stellar photography, see the long paper by the Yerkes observer F. L. O. Wadsworth, who explicitly states that "there are two general advantages which photographic methods possess," "accuracy and sensitiveness in the delineation of detail, combined with freedom from 'personal error' in the results obtained" (Wadsworth 1897, 335).

Newcombe personal equation and its variation with stellar magnitude (Hinks 1897). It was presented at a meeting in April, 1897 chiefly concerned with photography and personality. A South African amateur, Alexander Roberts, argued that there was an important personality effect in the judgment of brightness of pairs of adjacent stars. Roberts was convinced that his was a "subjective phenomenon" and not due to his deficient instruments nor to some feature such as astigmatism (Roberts 1897).

This was troubling, since it would increase the number of spurious variables and would subvert the standard magnitude scale developed by Argelander at Bonn, and then enforced by the weighty authority of the German-dominated International Astronomical Union (Argelander 1870; Herrmann and Hamel 1975). Downing commented that at the Paris conference the previous year, delegates had been unable to agree on whether such a correction should be built into the tables, but "decided not to introduce a correction of this kind owing to the uncertainty of its amount." He asked Roberts for a quantitative estimate of the effect. The Savilean professor of astronomy at Oxford, Herbert Turner, praised Roberts as an exemplary amateur: "if a professional astronomer were asked to give an example of real and unselfish astronomical work, he could not select a much better instance than the present one – the devotion of several years to the examination of one small point, small but nevertheless vitally affecting the accuracy of observations" (Royal Astronomical Society 1897, 186–88). Noble was drawn to comment by Roberts' work. He said that "there must be some physiological effect at the bottom of this, having its origin in the eye." He pointed out shrewdly that such effects had never been given "a satisfactory explanation in any book on astronomy that I have ever seen," and he regaled the Society with anecdotes about his experiences watching balloon flights. Hinks' paper, which followed, only emphasized the troubles of interpreting this set of effects and of showing how professional astronomy would manage them (Royal Astronomical Society 1897, 187–88).

Hinks' work had been undertaken on Ball's direct prompting. In accordance with German techniques, Hinks compared data from the Cambridge Zone Catalogue, completed the previous year, and the International Union's observations. Hinks also used Argelander's magnitude scale, though, as he pointed out, quite apart from the problems raised by Roberts and by Newcomb "in view of the later developments of the work the adoption here of the Berlin magnitudes is to be regretted." Purely on the basis of this Berlin-Cambridge comparison, however, Hinks was convinced that Newcomb was right and that "the observations are affected by a personal equation depending on magnitude" (Hinks 1897). However, Hinks had other data at his disposal – a table of comparative positions of twenty-six stars in the Cambridge catalogue and in photographs taken at Oxford under Turner's supervision. Turner, a veteran of Cambridge and Greenwich, was a keen advocate, with Hinks, of astrophotography. He "came to a determination to see what could be done to eliminate or determine personality by a photographic method," devoted the resources of the Oxford Observatory to this end, and heard from Cambridge that Hinks was planning a similar strategy. Turner's photographs convinced Hinks that his initial results were

right: astrophotography would enable astronomers to control the personal equation to an unprecedented extent. Others concurred: "photography may really supersede the eye even in such a delicate operation." Hinks' technique was straightforward: if the Newcomb effect existed, then the error in the horizontal coordinate of the center of each star's plate would increase with magnitude, while the equivalent variable for the star in the Cambridge catalogue would decrease. Hinks reported that the correlation was detectable, and went on to recommend the more widespread use of and publication of such plates (Hinks 1897, 477).

Noble again was skeptical: "anyone who has observed as many thousand transits as I have will know how much more accurately that of a small star can be observed than of a large one." He showed on the blackboard that this would affect the judgment for transits of large stars, and drew approval from the Greenwich veteran William Ellis. Noble had no deep commitment to the program Newcomb, Ball, Turner, and Hinks proposed. He was unimpressed by suggestions like Downing's that "directors of observatories should investigate this matter and make whatever determinations they can." His interventions persistently raised the issues of the disciplinary character of astronomy, and, above all, of its interest in the subjectivity of observation (Royal Astronomical Society 1897, 189–91, 226–28; Ellis 1897, 314). Noble received more resources for his case when the Society published another paper on the personal equation the next month, a paper which drew considerable public attention. It was by Truman Safford, a veteran of the U.S. Nautical Almanac and an astronomer at Williams College in Massachusetts. Safford's paper represented a dramatic attempt to subordinate what had been issues within astronomy to the expertise of the German experimental psychologists, Wundt in particular. Newcomb was attacked directly. Safford wrote that he was unconvinced by Newcomb's analysis of the personal equation of magnitude:

> It did not require a long search to find a second opinion (different) from Professor Newcomb's in the writings of the experimental psychologists. These investigators, who are men of scientific note in their respective countries, trace their intellectual lineage back to Bessel and other astronomers, and now claim that astronomers will do well to study their methods and results. To see whether their claims are well founded was the object of a long and rather intricate study. (Safford 1897; 1897a.)[6]

Safford's intricate study dealt body blows to received astronomical practice. He drew on data from Bessel and from the Greenwich catalogues, including both observers who were "skillful and experienced" and those who were not. Safford found the traditional method wanting. But he also indicated some worrying implications for the observatory reliance on their new galvanic recorder. He said that averaging eye-and-ear observations giving each observer equal weight "might be seriously in error"; chronographs were no more reliable in coping with the personal equation than were the old eye-and-ear methods; most importantly, in view of Newcomb's catalogue

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[6] Safford cited work by Mark Pattison, James Sully, Cattell, and Lange. For this work in the United States see Popplestone and McPherson 1980.

proposals, "without some process which shall pretty thoroughly eliminate these differences, no definite standard can easily be fixed; or in other words no standard catalogue . . . is practicable, and the need of such a catalogue is apparent." Safford's prescription was simple: Wundt's complication and reaction experiments were the key to solving astronomy's dilemmas.

> Thus we see that the riddles which Bessel brought forward, but did not completely solve, have given rise to, and have been greatly helped towards, a more complete solution by the rise of a new branch of sciences – experimental psychology – which is now pursued in many of the most prominent universities of the world as a new department of study, and it is now possible to combine in a certain degree the study of the human mind with that of the physical universe, and thus to contribute towards the restoration of the unity of philosophical study. (Safford 1897, 504, 506–7, 510, 514.)

Safford's forceful appeal for the unification of psychology with astronomy challenged the boundaries of an entrenched discipline. Elite commentators pointed acidly to the fact that his paper had been "noticed at some length" by "nearly all the more popular magazines." It was also suggested that the paper "did not seem to contain anything very new or important." Traditional astronomical methods could cope with personality – the editors of *The Observatory,* who included Herbert Turner, politely corrected what they felt were some oversights in Safford's paper. Most importantly, it was argued that "it will be almost a pity if we manage to get rid of the personal equation by the photographic method, as some of us hope, for it will remove a link between the professional astronomer and the dilettante (this is a politer term than "astronomical public"; I wish I had thought of it before) in the shape of a topic at once important and intelligible to both" (Turner 1897, 331–32, 387–88). Noble immediately responded to this point. He wrote that Safford's work was valuable because it indicated an endemic and commonly shared problem which was not to be solved by any privileged technology alone. He accused "the rich man's table of the subsidized public observatory" of condescension to the amateur, and, most tellingly, argued that the problem of personality was much more significant than that now being debated by Newcomb and his supporters:

> I, in my weak-minded way, would humbly venture to submit that it is at least equal to that of . . . the measurement of the plates taken for the Astrographic Catalogue, which appears to be the King Charles's Head of every Report issued from certain observatories for some time past, but concerning which a very large proportion indeed of the mere "astronomical public" do not, in the emphatic phrase of the penultimate Duke of Bedford, "care one twopenny d---." (Noble 1897.)

The elite astronomers answered Noble swiftly. He was consoled with the thought that "some of those who know most are amateurs." Turner and his coeditors went on to argue that it was necessary for the protagonists of the Newcomb program to publicize their efforts to solve the problem astronomically: "it is just because the star-catalogue maker is subsidized from the public purse that he feels that these details, some of which relate to matters of controversy, should be submitted to public criticism before their final adoption." However, Turner also drew a sharp line between amateur astronomy ("which certainly has the same name but otherwise is quite distinct") and the "celestial surveying" which, crucially, was pursued by "state-supported establishments which exist and endure independently of any man's life." Men like Noble were praiseworthy but subordinate. And though patronized by the observatories, there were faults on the amateur side too: the controversy on the personal equation was believed to demonstrate this truth (Turner 1897, 353, 359–60).[7] Noble moderated his attack the following month, but Safford intervened in the broadening debate to reaffirm his commitment to an alliance between astronomer and experimental psychologist. He referred to major philosophy journals, such as *Mind* and Wundt's own *Philosophische Studien.* He warned astronomers in general that they had "failed to grasp with sufficient clearness the great difference between the psychical process of observation by the eye-and-ear and the chronographic method" (Safford 1897a, 387; Noble 1897a).

According to Safford, the reaction-time experiments of Wundt and his students Max Friedrich (1880) and Ludwig Lange (1888) provided an answer to this problem. Lange and his master claimed that such times were longer when attention was paid to the sensory stimulus (such as the motion of the star or the sound of the clock) as opposed to the muscular response (such as the pressure on the galvanic button). The Wundtians used a subtractive procedure to suggest that this difference was due to the time for "apperception": Safford evidently supposed that such phenomena were relevant to galvanic chronometry (Boring 1950, 148). Within a decade these procedures were under attack from Wundt's student Oswald Külpe, who denied that subtraction methods could work since the mental states in situations of sensory and muscular attention were quite different (Behrens 1980, 207–208; Woodward 1982, 187). No important astronomical techniques pursued Safford's suggestions. Instead, fresh commitment to astrophotographic techniques and the massive prestige of the Newcomb program obviated much of Safford's assault. Boring followed Safford's view: he claimed that "Bessel really set the problem as a psychological problem" and that personality was "destined to become the property of the new physiological psychology" (Boring 1950, 134, 142). As we have seen, this destiny was far from obvious to the contemporaries of Safford and Wundt. Professional managers such as Turner or Newcomb saw the problem as a safe part of their observatories' program. Noble perceived it as an opportunity to goad the grandees of state astronomy. The clash of interests shows the situated, context-bound value of quantification and precision. It questions the assumption that such moves are self-evident to the members of highly differentiated scientific communities.

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[7] Compare the splendid satirical remarks on "visits of the dilettante public to observatories," ibid., 361.

Conclusion: "The Wardens of Our Faculties"

When Wundt and Donders began to investigate the personal equation as a problem of reaction time and complication they were not choosing an embarrassing failing of nineteenth-century astronomy. They were using the instrumentation and data of the most self-confident disciplinary technology of the age. This discipline was embodied in the precision galvanic chronometry of the observatories which had mechanized observation and subjected it to detailed surveillance. The material technology of time-keeping and the social technology of the astronomical workshops provided psychologists with excellent models for scrutiny of the individual. Astronomical subjects were a special sort of individual, involved in specific social relations with their superiors. Several features of the personal equation are connected with this astronomical individualism. Before the appearance of chronoscopes and reaction times in the psychology labs, precision astronomy was already normative for the disciplines of the individual. The observatory manager Quetelet's "social physics" and his concept of the "mean man" drew heavily on the metaphor of statistics as celestial mechanics, and of variation between individuals as variation between observations. This was so despite the glaring disanalogies between the treatment of individuals in precision astronomy, where many observations of a putatively unique object were produced, and that of the plans of Quetelet and his converts among the statists in Britain, where single observations of many individuals were analyzed (Quetelet 1842, 9; Porter 1985; Buck 1981; Stigler 1986, chap. 5). Augustin Cournot, an important critic of many of the statists' assumptions, added an appendix to Herschel's astronomy textbook on the exemplary pattern for these new sciences of the social: "Just as observational astronomy is the model for the sciences of observation, theoretical astronomy the model of scientific theories, so stellar statistics must one day serve as a model for all other approaches" (Herschel 1835, 562). Such imagery was a commonplace among political economists and statists alike. The precision techniques for handling error developed by Gauss, Laplace, Bessel, or Airy accompanied an image of astronomy as the right discipline for treating individual subjects and their differences (Porter 1986, 95–96; Stigler 1986, chap. 4).

Reassessments of quantification in both the observatory and the psychology lab are implied by this story. Wundt found that observatory technology provided resources he needed to measure individual difference. The astronomical managers linked time measurement with the surveillance of individuals' performance of simple tasks (Wundt [1873–1911] 1902; 3:359–88). There were important connections between Wundtian scrutiny of individual behavior and themes of conservative liberalism in the Wilhelmine regime. Wundt aimed at a "conquest of individualism" which would recognize "the value of individual personality without thereby giving up the independent value of the moral community" (Wise 1983, 108; Van Hoorn and Verhave 1980; Woodward 1982). The technology of astronomical personality embodied connections between a disciplined moral community and subjects' individual capacity. Similar connections might be pursued in the formation of Wundt's experimental technologies at the Berlin physiology laboratory under Du Bois-Reymond, where his work of the late 1850s first used precision measures of muscular reactions and where he learnt his early politics (Wundt 1920, 143–46; Ungerer 1980; Lenoir 1986). Measurement only has meaning as part of a pattern of work in the laboratory. Different regimes – Airy's, Du Bois-Reymond's, Wundt's – generate and sustain different measures. The political economy of the great nineteenth-century observatories indicates the social and material basis of an influential set of values. The observatory network helped establish constant measure of time and position. It simultaneously helped make powerful technologies for disciplining the individual.[8] Boring wrote that "refinement is never at an end in scientific measurement" (1950, 141). Such refinement was traced just as much in the work of Josiah Wedgwood or Andrew Ure, the practitioners of precision measurement, factory discipline, "clocking on," and "time sheets," as it was at Greenwich or Königsberg. Networks of the workshop, the laboratory and the observatory became inseparable. Observatories increasingly relied on precision engineering and engineers disciplined their work habits. These were Wordsworth's "Wardens of our Faculties / and Stewards of our Labour, watchful men / and skilful in the usury of time" (Thompson 1967). The argument developed here depends on the specificity of this refinement and of the discipline which makes it possible.

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[8] Compare Latour 1987, 251, on "metrology": "Time is not universal; every day it is made slightly more so by the extension of an international network that ties together, through visible and tangible linkages, each of all the reference clocks of the world."

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