

Constant differences: Friedrich Wilhelm Bessel, the concept of the observer in early nineteenth-century practical astronomy and the history of the personal equation

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Abstract. In 1823 the astronomer Friedrich Wilhelm Bessel gave notice of an observational error which is now known as the personal equation. Bessel, however, never used this phrase to characterize the finding that when noting the time of a certain event observers show a considerable ‘*involuntary* constant difference’. From this starting point the paper develops two arguments. First, these involuntary differences subverted the concept of the ‘observing observer’. What had previously been defined as a reference point of trust and precision turned into a source of an error that resisted any wilful intervention. Second, and contrary to later suggestions, Bessel’s findings did not initially lead to discussions and measures of permanent control. In everyday astronomical work the influence of such differences could be avoided by comparatively simple means. Taking this into account offers a new perspective both on the history of the personal equation and on the significance of Bessel’s findings. Whereas the former has to be read as the history of a rather particular reaction to the phenomenon of constant differences, the latter is connected with a rather fundamental transition in the epistemology of the observer.

The ‘constant difference’ and the ‘personal equation’

In autumn 1823 Friedrich Wilhelm Bessel (1784–1846), director of the University Observatory in Königsberg, published in the preface to the eighth volume of his *Astronomische Beobachtungen* some experiences with differences between two observers noting the time of a certain event.¹ In more recent terms, what Bessel’s seven-page report indicates is the personal equation. Indeed, since the end of the nineteenth century, with the first summary accounts of this issue, Bessel’s work has typically been acknowledged as the earliest exhaustive piece of work on the personal

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The writing of this paper has benefited first and foremost from several discussions with Jutta Schickore. Wolfgang R. Dick and Klaus-Dieter Herbst introduced me to the elements of nineteenth-century stellar astronomy. I am also grateful to Simon Schaffer and two anonymous *BJHS* referees for their help in clarifying my argument. Research for this paper was carried out under grants from the Deutsche Forschungsgemeinschaft (Bonn) and the Max Planck Society (Munich).

¹ *Astronomische Beobachtungen auf der Königlichen Universitäts-Sternwarte in Königsberg von F. W. Bessel, 8. Abtheilung vom 1. Januar bis 31. December 1822*, Königsberg, 1823, III–VIII.

equation.² Yet Bessel himself never used this phrase, speaking instead of a ‘constant difference’ between observers, of a ‘difference in the moments of observation’ or simply of a ‘difference’ in noting the time.³

At first glance this detail seems unimportant. Nevertheless, a more textured look at the circumstances connected to the introduction of the phrase ‘personal equation’ into astronomical discourse reveals a significant transformation. When the phrase first appeared in print in 1833, this was also the opening of a new chapter in the career of the differences first discussed by Bessel some ten years earlier. With his definition of the ‘personal equation’, the then head of the Greenwich Observatory, the Astronomer Royal John Pond (1767–1836), did not invent a new name for such differences but rather established a set of mathematical operations for determining them.⁴ As the name of a method to detect and calculate ‘the difference in the times of noting’,⁵ the personal equation marks the beginning of a process in which a puzzling phenomenon, though rarely discussed by the astronomical public, gained the status of an urgent observational problem requiring means of permanent control.

With Simon Schaffer’s 1988 article ‘Astronomers mark time’ and more recent studies by Jimena Canales, historical interest in the personal equation has focused on two important questions. Schaffer’s particular virtue was that he renewed historians’ interest in an issue that for many years figured only as the starting point of psychological measurement. Rejecting the standard account of the early historian of psychology Edwin G. Boring, who held that the personal equation formed one of the cornerstones of the emerging field of experimental psychology,⁶ Schaffer proposed that astronomy had gained control over the phenomenon well before psychologists even entered the scene. As he emphasized, psychological expertise was superfluous since any possible damage to astronomical precision could be excluded with the help of a ‘chronometric regime of vigilant surveillance of subordinate observers’.⁷ This sketch of the astronomical observatory as a carefully woven site of knowledge production successfully undermined the claims of psychologists. Jimena Canales, in turn, laid stress upon the transdisciplinary qualities of the personal equation. Partly revising

2 R. Radau, ‘Sur les Erreurs personnelles’, *Le Moniteur scientifique* (1865), 7, 977–85 and 1025–32; (1866), 8, 55–61, 97–102 and 207–17; E. C. Sanford, ‘Personal equation’, *American Journal of Psychology* (1888/9), 2, 3–38, 271–98 and 403–30.

3 *Astronomische Beobachtungen*, op. cit. (1), III; *Astronomische Beobachtungen auf der Königlichen Universitäts-Sternwarte zu Königsberg von F. W. Bessel*, 11. *Abtheilung vom 1. Januar bis 31. December 1825*, Königsberg, 1826, [III]; and *Astronomische Beobachtungen auf der Königlichen Universitäts-Sternwarte zu Königsberg von F. W. Bessel*, 18. *Abtheilung vom 1. Januar bis 31. December 1832*, Königsberg, 1836, III. Today’s readers might be misled by the fact that Bessel’s untitled report was reprinted in his *Abhandlungen*, edited in 1875–6, under the heading ‘Persönliche Gleichung bei Durchgangsbeobachtungen’; see F. W. Bessel, *Abhandlungen* (ed. R. Engelmann), 3 vols., Leipzig, 1875–6, iii, 300–3.

4 *Astronomical Observations Made at the Royal Observatory at Greenwich in the Year 1832, Part 5: Supplement*, London, 1833, p. iv.

5 *Astronomical Observations*, op. cit. (4), p. iv.

6 E. G. Boring, *A History of Experimental Psychology*, 2nd edn, New York, 1950 (first published 1929), 134–53; and *idem*, ‘The beginning and growth of measurement in psychology’, *Isis* (1961), 52, 238–57.

7 S. Schaffer, ‘Astronomers mark time: discipline and the personal equation’, *Science in Context* (1988), 2, 115–45, 117–19; for the quotation see 115.

Schaffer's account, she underlined the scientific productivity that was connected to the personal equation. Pointing to complex arrangements through which astronomers, physiologists and several representatives of an emerging experimental psychology developed the subject from the 1860s, Canales characterized the personal equation as a 'privileged locus for back-and-forth exchanges between the exact sciences and the sciences of man'.⁸

Both interest in the means of control by which practical astronomy determined and reduced the effects of the personal equation on astronomical data, and attention to the efforts of various disciplines in debating the causes of the personal equation, have considerably deepened our understanding of the nineteenth-century regime of astronomical observation. They have also, significantly, extended understanding of the fruitful scientific potential of such differences between observers. Nevertheless, both Schaffer and Canales concentrate on a period in which such differences, already subsumed under the name 'personal equation', were constituted as a problem of astronomy and quite amply discussed. Both Schaffer and Canales focus on the situation from the middle to the end of the nineteenth century, whereas the two decades between the publication of Bessel's report and the end of the 1830s, when determinations of the personal equation in Greenwich reached the stage of permanence, have not been considered in detail.

Focusing on the career of Bessel's findings before the 'personal equation' leads to new questions. One of them has already been mentioned: the question of how it came to pass that for at least a decade the phenomenon of 'constant difference' was neither ubiquitously understood nor even fully discussed as a pressing problem. The second, but of course logically prior, question is just how it happened that Bessel stumbled over such constant differences at all. In what follows it is argued that this was not at all self-evident. There was, rather, a strong belief that at the least an experienced observer could exclude such an error in advance by relying on the right mixture of method and self-control. It is indeed one characteristic of Bessel's inquiry that it initially resulted in an experience of deep scepticism both for him and equally for his colleagues. Contrary to all expectation, such differences resisted the remedy of training and increased attention. As Bessel wrote, one has to cope with an '*involuntary* constant difference'.⁹ Thus concentration on the two formative decades between the 1820s and the 1840s offers a new entrance both into the history of the personal equation and into the significance of Bessel's findings. The former will come to be seen as the history of a particular reaction to the phenomenon of constant differences, which, as reaction, is closely connected to a certain organization of the business of observation. In the latter case, we see a central transition in the concept of the astronomical observer and the epistemology of observation comes into focus.

Considered more broadly, Bessel's inquiry offers a good example of what Jutta Schickore has very recently characterized as a reflexive turn in early nineteenth-century

8 J. Canales, 'Sensational differences: individuality in observation, experimentation, and representation (France 1853–1895)', Ph.D. thesis AAT 3091521, Harvard University, Cambridge, MA, 2003, 9.

9 *Astronomische Beobachtungen*, op. cit. (1), III. Bessel's emphasis.

scientists' considerations of the conditions of experience. Pointing to efforts in a wide range of scientific enterprises, she maintains that in the 1820s and 1830s inquirers started to treat research tools as items for research.¹⁰ In contrast with earlier approaches in conducting such inquiries, the machinery of experimentation and observation finally emerged as immovable impediments: 'the limits and flaws of the instruments and methods became themselves the objects of sustained scrutiny'.¹¹ The challenge of Bessel's findings was indeed that the astronomer as observing observer, or observer 'in action', took shape as an unavoidable source of error. This experience marks a rupture both in the beliefs and in the practices connected to everyday astronomical work. I am interested in the conversion of an incident, initially qualified as irksome but failing to produce deep doubts about the basic understanding of the observer in action, into a puzzling phenomenon that called for fundamental reconsideration of the observing observer. Thence we see another change, into a pressing problem demanding extensive measures of control, which forced technological innovation and inspired scientific debates. Starting with the first point, the conversion of an irksome incident into a puzzling phenomenon, the paper particularly focuses on the unspoken assumption that differences in noting the time of a certain event posed a challenge from the very first moment such a case was noticed. Against this, I offer grounds to doubt whether Neville Maskelyne, Pond's predecessor as Astronomer Royal and the man traditionally honoured as being the first to point to a such difference, was even talking about the same issue as Bessel.

Nothing new in Greenwich: Maskelyne's rapid answer

For the past century, accounts of the discovery and further development of what was then already called the 'personal equation' have usually started with a prologue set in the world's most renowned astronomical installation, the Royal Observatory in Greenwich. The story is repeatedly told of how during 1795 the Astronomer Royal noticed an increasing difference in time between the results of his own transit observations and those of his assistant David Kinnebrook (1772–1802). In a short message inserted into the third volume of the *Greenwich Observations* between the observations for July and August 1795, Maskelyne declared,

My Assistant, Mr. David Kinnebrook, having at this Time unfortunately commenced a vitious 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, from this Time to the 19th of January of the ensuing Year.¹²

10 J. Schickore, 'Ever-present impediments: exploring instruments and methods of microscopy', *Perspectives on Science* (2001), 9, 126–46.

11 Schickore, *op. cit.* (10), 140.

12 *Astronomical Observations Made at the Royal Observatory at Greenwich from 1787 to 1798 by the Rev. Nevil Maskelyne*, London, 1799, 319.

In order to understand the importance of this subject, one must bear in mind that transit observations, the measurement of the time at which a certain star passes the meridian of the place of observation, formed the principal concern and the quotidian routine of every astronomical installation at the end of the eighteenth century and long afterwards. From a strictly astronomical perspective, these observations provided the raw data for determining the right ascension, which together with the value of the declination set the coordinate of every star in the hemisphere. In addition, especially at Greenwich, transit observations played an important role in determining longitude at sea. The method of lunar distances which the Board of Longitude introduced as a standard for the Royal Navy in the 1760s was intimately related to a system of reference points that were easy to observe and precisely known. For this reason, every year the *Nautical Almanac*, produced under Maskelyne's guidance on behalf of the Board of Longitude, contained the probable positions of a number of fundamental stars based on regular observations at the Greenwich Observatory.¹³ Last but not least, transit observations served as a means of determining the rate of the observatories' clocks and were thus directly involved in setting the exact local time. Given the centrality of such observations, one can begin to appreciate the inconvenience or even threat posed by this difference of five- to eight-tenths of a second between Maskelyne's and Kinnebrook's observations.¹⁴ In angular measure this amount extends from seven to twelve seconds of arc, well beyond contemporary instrumental error that hovered around only one second of arc.¹⁵ In practical terms, a difference of one second in time is equivalent to a difference in longitude at the equator of about a quarter of a mile. The total amount certainly does not seem large in relation to the situation only some decades earlier, but was nevertheless sufficiently sizeable to call into question the status of astronomy as the most advanced scientific enterprise of the day.

Considering the important functions of transit observations, it is hardly surprising that their method received special attention. From the second half of the eighteenth century observations were usually made with a transit instrument arranged and fixed in the line of the local meridian. To determine a certain star's time of transit, the observer had to execute two principal operations. With his ear he had to follow the beat of a pendulum clock to provide the measure of time whilst simultaneously using his eye to follow the passage of the star over three, five, or sometimes even seven wires or spider's threads vertically arranged at equal distances in the telescope's eyepiece. The transit times, noted for every single wire, were then reduced to the transit time for the central wire, which should theoretically fall in the plane of the local meridian. Although this procedure was complicated enough, determining the exact moment of transit was the

13 D. Howse, *Greenwich Time and the Longitude*, London, 1997 (first published 1980), 65–78; D. S. Landes, *Revolution in Time: Clocks and the Making of the Modern World*, Cambridge, MA, 2000 (first published 1983), 160–6.

14 *Astronomical Observations*, op. cit. (12), 339.

15 A. Chapman, 'The accuracy of angular measuring instruments used in astronomy between 1500 and 1850', *Journal for the History of Astronomy* (1983), 14, 133–7, 135.

subject of yet further instructions. In the preface to the first volume of the *Greenwich Observations*, published in 1776, Maskelyne informed his readers,

My method of estimating fractions of a second in transit-observations is, by noting by my eye the proportion of the distances of the star from the wire at the beats of the clock immediately preceding and immediately following its passing the wire.¹⁶

The importance of this instruction becomes apparent if one considers that from Maskelyne's point of view a method of observing constituted much more than a set of appropriately chosen rules. In a second, longer message on the 'Kinnebrook affair' he emphasized, with respect to the causes of the noticed differences, that

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.¹⁷

The relation between error and method which Maskelyne indicated provides the key to his understanding of the whole affair. For the Astronomer Royal, the method of observing was one of the central issues that secured the trustworthiness of observations: only the 'right method', an expression he used some lines earlier, could guarantee the acuity of a set of observations and accordingly 'gross errors' first placed serious doubts on the behaviour of the observer.

Maskelyne's emphasis on method, in the sense of methodically carrying out a certain well-established set of observational operations, reveals that at the end of the eighteenth century natural philosophers had strong ideas on how a reputable observer should behave. In particular, the practice of the observer was a central issue of the 'system of recognitions',¹⁸ which regulated the reception of scientific observations. Training, skill and painstaking effort were key terms for evaluating both the observer and the value of his observations. Treatises such as Abbé Nollet's *Discours sur les dispositions et sur les qualités qu'il faut avoir pour faire du progrès dans l'étude de la physique expérimentale* (1753) or Jean Senebier's *L'Art d'observer* (1775), and handbook articles on 'observation' such as those in Sigaud de la Fond's *Dictionnaire de physique* (1781) and Gehler's *Physikalisches Wörterbuch* (1787), further developed the idea of the observer as the central authority and primary agent of reliability. Other writings, as for example the long paragraph 'De l'Usage des instruments & de la pratique des observations' in Volume Three of Joseph-Jérôme Lalande's *Astronomie* (1771), provided careful textual and pictorial clues to the right use of instruments. The observer typically formed the cornerstone of the regime to which the recognition of observations, thus the very possibility of discussing their truth, was subordinated. This regime relied mainly

16 N. Maskelyne, Preface, in *Astronomical Observations Made at the Royal Observatory at Greenwich from 1765 to 1774 by the Rev. Nevil Maskelyne*, London, 1776, pp. i–ix, p. v. The instruction remained the standard for Greenwich transit until the introduction of electromagnetic registering devices eighty years later.

17 *Astronomical Observations*, op. cit. (12), 339. It is noteworthy that Maskelyne here mentioned his predecessor James Bradley as the author of this observation method.

18 I am borrowing this term from Steven Shapin's characterization of the activities of the early Royal Society in *A Social History of Truth: Civility and Science in Seventeenth-Century England*, Chicago and London, 1994, 243–309.

on rules of observation and the will to perfect the means of observation to the highest possible degree, assuming that errors in observation, at least under ideal circumstances and with ideal instruments, could be prevented. In a very literal sense, this regime was one of instruction that attempted *a priori* to preclude any lapses, deceptions and mistakes with a catalogue of guidelines of what a good observer had to do and know.

Maskelyne's rapid solution can be easily explicated from this perspective. Discrepancies in observational results, which could not be attributed to unrecognized imperfections in the instruments, undoubtedly gave reasonable grounds for distrusting the observer. Moreover, the great discrepancy in experience between Kinnebrook and Maskelyne, the latter an astronomer who had held the most prestigious position in practical astronomy for more than thirty years and the former a young tyro who had been working in Greenwich for not more than two years, made it completely plausible that differences could only result from the assistant's deficient way of observing.¹⁹ But for the very same reason that the Astronomer Royal was able to place blame on his assistant, this incident in Greenwich cannot be linked directly to the history of the phenomenon that took form some decades later at the Königsberg observatory. George Canguilhem once suggested that before 'joining the end of one path to the beginning of another path so that they constitute one road, it is recommended to assure oneself that one deals with the same road'.²⁰ This, however, is not the case with Maskelyne and Bessel's understanding of what happened. For the Astronomer Royal the whole problem resulted from Kinnebrook's 'vicious way of observing the times of the Transits too late'. He thereby reduced an incident which, with its importance for local time and longitude, ultimately touched on the basis of two of the British Empire's most important commodities, to the explanation that a subject simply did not fulfil his task. For him the affair merely led to a long-standing fact: 'However, this unfortunate instance shews the necessity of first duly understanding, and then closely adhering, to this [i.e. the right] method of observing.'²¹

Maskelyne's conclusion did not suggest the existence of an urgent problem. It neither revealed an unknown nor invited colleagues to further discussion. His note in the *Astronomical Observations* did not pose a challenge to which Bessel simply had later to react. It reported in Maskelyne's own words an 'unfortunate instance' that resulted only in the dismissal of the unlucky Kinnebrook. The question is thus how to explain a transformation from what was initially just a 'disturbing incident' (from Maskelyne's perspective an easily understandable one) into the first case of a rather unexpected

19 For indications that the affair might have been provoked by an intrigue of Maskelyne see J. D. Mollon and A. J. Perkins, 'Errors of judgement at Greenwich in 1796', *Nature* (1996), 380, 101–2. A detailed picture of the tiresome everyday life of Maskelyne's assistants, mainly based on Kinnebrook's letters to his father, is provided by M. Croarken, 'Astronomical labourers: Maskelyne's assistants at the Royal Observatory, Greenwich, 1765–1811', *Notes and Records of the Royal Society of London* (2003), 57, 285–98.

20 'Avant de mettre bout à bout deux parcours sur un chemin, il convient d'abord de s'assurer qu'il s'agit bien du même chemin'. G. Canguilhem, *Etudes d'histoire et de philosophie des sciences*, 7th edn, Paris, 1994 (first published 1968), 21.

21 *Astronomical Observations*, op. cit. (12), 339.

phenomenon. How did it happen that Maskelyne's message was not permanently forgotten, its temporary fate for twenty years?

In the autumn of 1823 Bessel remarked in a letter to his colleague Johann Franz Encke (1791–1865), then director of the Seeberg observatory, 'But how is it possible, that such a well-confirmed and clearly expressed experience like that of Maskelyne and Kinnebrook, was not taken into serious consideration for such a long time?'²² Bessel's surprise is obviously retrospective. His investigations alone transformed a virtually forgotten note on the unruly behaviour of an almost unknown observer into 'a well-confirmed and clearly expressed experience'. When Bessel first read about the incident in Greenwich he only saw a minor remark. In his short history of the Royal Observatory, published in 1816, Bernhard August von Lindenau (1779–1854), then director of the Seeberg observatory, included a list of all former assistant observers, adding after Kinnebrook's name the comment that, 'Maskelyne parted from this actually quite skilful assistant, because he fell into the habit of writing down the transits 0".5 to 0".8 too late.'²³ The sentence does not appear to provide more information than the fact itself. The reader is presented with a curiosity rather than a mature experience. Following Ludwik Fleck, this comment hardly counts as the 'weak hint of a resistance'²⁴ that might serve as a prelude for arriving at a completely different understanding of what had happened.²⁵ Tellingly, the incident is not marked with the expression that contemporary German astronomers reserved for truly troubling occurrences in those days: the word 'puzzle'. Nonetheless, the explicit discussion of such puzzles undoubtedly prevented the episode from sinking into oblivion.

Puzzles: the observer and observational errors, c.1815–20

In the first decades of the nineteenth century, practical astronomy experienced an impressively prosperous phase in the German-speaking countries, bringing remarkable institutional and technological progress and, above all, some genuine triumphs in theoretical and observational work.²⁶ The decades between 1810 and 1840 saw the

22 'Wie mag es aber wohl zugehen, daß eine so gut bestätigte und deutlich ausgesprochene Erfahrung als die von Maskelyne und Kinnebrook, so lange ohne ernsthaftige Berücksichtigung blieb?' F. W. Bessel to J. F. Encke, 2 October 1823, Bessel Papers, Archiv der Berlin-Brandenburgischen Akademie der Wissenschaften, Berlin (subsequently BP), Letter-Vol. 16, 160.

23 'Maskelyne trennte sich von diesem an sich geschickten Gehülften, weil er die Gewohnheit bekam, alle Meridian-Durchgänge 0".5 bis 0".8 zu spät anzugeben.' B. A. von Lindenau, 'Beiträge zur Geschichte der Greenwicher Sternwarte und derer in Großbritannien überhaupt', *Zeitschrift für Astronomie und verwandte Wissenschaften* (1816), 2, 199–244, 236. For this paper as the source of Bessel's first acquaintance with the incident see *Astronomische Beobachtungen*, op. cit. (1), III.

24 L. Fleck, 'Scientific observation and perception in general' (1935), in *Cognition and Fact: Materials on Ludwik Fleck* (ed. R. S. Cohen and T. Schnelle), Dordrecht, Boston, Lancaster and Tokyo, 1986, 59–78, 77.

25 As claimed in Boring, *A History*, op. cit. (6), 135 ff; and in G. P. Brooks and R. C. Brooks, 'The improbable progenitor', *Journal of the Royal Astronomical Society of Canada* (1979), 73, 9–23, 11 ff.

26 A comprehensive outline of the activities in the German lands during the eighteenth and nineteenth centuries is still lacking. For the Enlightenment see R. Baasner, *Das Lob der Sternkunst. Astronomie in der deutschen Aufklärung*, Göttingen, 1987. For a good actor's account see G. A. Jahn, *Geschichte der Astronomie vom Anfange des Neunzehnten Jahrhunderts bis zu Ende des Jahres 1842*, 2 vols., Leipzig, 1844.

astronomical community centred around such eminent figures as Carl Friedrich Gauss, Friedrich Wilhelm Struve, Heinrich Wilhelm Olbers and Heinrich Christian Schumacher, and others already mentioned, Johan Franz Encke and Friedrich Wilhelm Bessel. This was a collective configuration well on its way to challenging British and French dominance in practical astronomy. Yet the successful interaction of mathematical methods, observational skills, innovation in instrument-making and deeper insight into the investigation of observational errors ended in a paradoxical experience for the actors: general advance and common effort did not automatically lead to more agreement on details. It was in this context that German astronomers often resorted to the word ‘puzzling’. Consider this letter from Lindenau to Bessel in March 1818:

I am very eager to get the third volume of your Königsberg observations, and as soon as I have it in my hands I will give a minute notice of everything either for our journal [Lindenau’s *Zeitschrift für Astronomie und verwandte Wissenschaften*] or for the *Jenaische Litteratur Zeitung*, and on this occasion I will mention the three puzzling phenomena in practical astronomy:

1. the Greenwich latitude according to Pond and to Bradley
2. the results of the summer and the winter solstices
3. the difference of your determinations of declinations compared to Piazzi’s.

Of course I will argue only historically, but it seems necessary for me to focus astronomers’ attention once again on these peculiar phenomena and thereby perhaps finally to achieve a satisfying explanation after all.²⁷

The puzzles that Lindenau lists are neither the first nor the last of those familiar at the time. Some turned out to be misunderstandings; others occupied astronomers for years. In general, one must understand all of these puzzling phenomena as the by-products of a process now usually discussed under the heading of the ‘probabilistic revolution’.²⁸

The introduction of probability calculus represented a highly visible change in the practice of natural philosophy in evaluating observational data at the beginning of the nineteenth century. Nevertheless, only an increasing attention to constant errors²⁹ of

27 *‘Sehr begierig bin ich auf Ihren dritten Jahrgang Königsberger Beobachtungen, und ich werde sobald dieser in meinen Händen ist, von Sämmtlichen entweder in unserm Journal oder in der Jenaischen Litteratur Zeitung eine umstaendliche Anzeige davon geben, und bei dieser Gelegenheit, der drei räthselhaften Erscheinungen in der practischen Astronomie*

1. *der Greenwicher Breite nach Pond und Bradley*
2. *der Resultate aus den Sommer und Wintersolstitien*
3. *der Differenz ihrer Declinationsbestimmungen mit denen von Piazzi erwähnen.*

Natürlich werde ich dabei nur rein historisch verfabren, allein ich halte es für nothwendig, die Aufmerksamkeit der Astronomen wiederholt auf diese merkwürdigen Erscheinungen hinzuführen, und dadurch doch vielleicht endlich zu einer befriedigenden Erklärung zu gelangen.’ B. A. von Lindenau to F. W. Bessel, 10 March 1818, BP, No. 287, f. 199vs.

28 Z. G. Swijtink, ‘The objectification of observation: measurement and statistical methods in the nineteenth century’, in *The Probabilistic Revolution* (ed. L. Krüger, L. J. Daston and M. Heidelberger), 2 vols., Cambridge, MA, London, 1987, i, 261–85.

29 In the first decades of the nineteenth century the actors treated as constant errors both errors whose value was indeed always the same and those errors which followed a certain rule but for example decreased

observation allowed the full breakthrough in mathematical error analysis. Because probabilistic tools could be deployed only for discussions of data free of constant errors, scientists always had to shore up their edifice by relying on thorough inquiry and experimentation into such matters as instrument errors.³⁰ Much of what German astronomers noticed as ‘puzzling’ must be understood in this context. A common experience was that differences appeared between various sets of observations that exceeded reasonable probabilities but could not be attributed to a certain source of error. ‘Puzzling’ thus became a category not for pointing to complete confusion but rather the contrary. Puzzles took shape against a well-defined background of protocols of data evaluation. Such incidents eventually acquired the status of scientific objects in their own right. As Bessel stated in a letter to Gauss dating back to the summer of 1818,

We have only very recently reached the stage where we explore small errors or deviations outside the limits of probability with the same attention as previously afforded to the bigger ones; both point to a physical source (in nature itself, in the instruments or in the observer) and we only now consider the discovery of this source ... a *scientific* discovery as significant as a more conspicuous one may have been considered earlier.³¹

Here Bessel did not fail to underline his own merits, because it was primarily he who refined the search for such errors to a hitherto unknown subtlety.³² His recalculation of James Bradley’s observations in Greenwich, also published in 1818, provided a definitive example of his ‘distinctive and rigorous approach to the analysis of astronomical data’.³³ It is all the more interesting in this respect that in his letter to Gauss Bessel also mentioned the observer as one possible source for errors lying outside the limits given by probability. Of course astronomers then conceded that observers could commit ‘gross errors’ because of insufficient training (as in the case of poor Kinnebrook), or that they could be deceived by the mind’s interpretation of sense data, widely treated in such writings of the day as the article ‘Gesichtsbetrüge’ in Gehler’s *Physikalisches Wörterbuch*. It was also further conceded that anatomical features of the senses such as the blind spot of the eye could limit or bias perception, yet in all these cases it was thought that attention, self-control and instruments could help to avoid any contamination of the observational results. Bessel’s idea pointed to an alternative conception of

and increased in relation to changes in their causes (in recent terms, systematic errors). F. W. Bessel, in a letter to J. F. Encke, 28 January 1819, BP, Letter-Vol. 16, 82, pointed to this difference.

30 Swijtink, op. cit. (28), i, 274–7.

31 ‘Wir sind erst jetzt auf den Punct gekommen, kleinen Fehlern oder Abweichungen ausser den Grenzen der Wahrscheinlichkeit mit derselben Aufmerksamkeit nachzuspüren als früher grossen; beiden muss ein physischer Grund (in der Natur selbst, in den Instrumenten oder dem Beobachter) zugehören, und die Entdeckung dieses Grundes ... sehen wir erst jetzt für eine eben so bedeutende wissenschaftliche Entdeckung an als früher eine mehr augenfällige angesehen worden sein mag.’ F. W. Bessel to C. F. Gauss, 15 June 1818, in *Briefwechsel zwischen Gauss und Bessel* (ed. Veranlassung der Königlich Preussischen Akademie der Wissenschaften), Leipzig, 1880, 272 ff.; original emphasis.

32 K. M. Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics*, Ithaca and London, 1991, 66–8. See also K. Lawrynowicz, *Friedrich Wilhelm Bessel 1784–1846*, Basel, Boston and Berlin, 1995, 136–50.

33 Olesko, op. cit. (32), 66.

observer error. Together with the assumption that such errors fell outside the limits of probability, he suggested that they were due to a certain ‘physical source’ that could not be eliminated from the observational work and that their scale or impact had to be determined empirically and then taken into account in the same manner as ordinary instrumental error. Indeed, in the introduction to his catalogue of Bradley’s observations, he discussed a probable error of the observer in determining the time of transit.³⁴ But Bessel apparently did not engage in particular trials. Furthermore, he clearly considered the error of the observer to be forced by external circumstances, specifically the dependence of the velocity with which the star crosses the wires on the star’s declination and the magnification of the telescope’s eyepiece. This stands in contrast to his later findings, which pointed to the intrinsic, ‘involuntary’ conditions of the observer.

Contemporary debates show that the observer did not normally survive as one of the probable sources of error outside the limits of probability as suggested by Bessel. This of course does not exclude certain exceptions, such as the recording in 1804 of an ‘error of the sense [*Fehler des Sinnes*]’ by the geometer and astronomer Johann Friedrich Benzenberg in his *Versuche über die Umdrehung der Erde* (1804). This might have been a good example of a constant error of the observer. But there is a distinction between the act of publishing a finding and whether this finding becomes part of everyday routine. Benzenberg’s considerations made no such impact.³⁵ Quite the opposite was the case. A flat denial of any vague indication that the observing observer could serve as a source of constant error was entirely the norm. This remained true for the so-called ‘circles controversy’ from 1817, in which Bessel harshly rejected any attempt to link disputed determinations of declinations directly to the observing astronomers.³⁶ This continued to hold true, albeit with a slight but important qualification, in the following episode from among such astronomical puzzles.

The observer under discussion: skill, insight and the ‘possibility’ of a constant error

In the summer of 1818 differing sets of data once again attracted the attention of astronomers. The discussion began with Encke’s observation that the right ascensions

34 F. W. Bessel, *Fundamenta Astronomiae pro Anno MDCCLV deducta ex observationibus viri incomptabilis James Bradley in specula astronomica Grenovicensi per Annos 1750–1762 institutis*, Regiomonti, 1818, 8 ff.

35 Benzenberg’s voluminous study *Versuche über das Gesetz des Falls, über den Widerstand der Luft und über die Umdrehung der Erde, nebst der Geschichte aller früheren Versuche von Galiläi bis auf Guglielmini* was widely rejected in the German community of astronomers. This might be why little attention was paid to his discussion of the ‘error of the sense’. Bessel obviously did not learn about Benzenberg’s finding before 1824. Letters between Benzenberg and Bessel do not touch on this issue. In printed work apparently only the article ‘Beobachtung’ in the first volume of the new edition of Gehler’s *Physikalisches Wörterbuch*, published in 1825, then remarked on this matter.

36 ‘It must be clear for everyone who has examined the matter that here, at least insofar as you, Gauß, Pond ... are the quarreling parties, it is only a controversy over [meridian] circles, not observers’ – ‘*Daß hier, wenigstens in so fern Sie, Gauß, Pond ... die streitenden Parteien sind, nur ein Streit der Kreise, nicht der Beobachter, stattfindet, muß jedem klar sein der Einsicht besitzt.*’ F. W. Bessel to J. J. von Littrow, 18 November 1817, BP, No. 413.

of Polaris (the Pole Star), which followed from the determinations of the Seeberg observatory, differed markedly from Gauss's determinations in Göttingen. 'At the moment this deviation still seems to me entirely enigmatic',³⁷ Encke remarked in a letter to Bessel. Encke had good reasons for his alarmed tone. Because of the favourable circumstances of these observations, they usually served as the basis for examining the exact adjustment of the telescope in the meridian of the place of observation and thus as a simple means for detecting errors in the installation of the instrument.³⁸ Apart from noting the deviations between Gauss's and his own measurements, Encke was threatened by the very possibility that his observational work as a whole might be contaminated by an unrecognized error of the instrument. Half a year later the whole affair turned out to be mainly the result of misunderstandings and premature conclusions. The incident nonetheless provides strong evidence for the evaluation of the observer in this context.

When asked for his advice, Bessel first suggested that the influence of diurnal temperature variations on the instrument's installation might have caused the error.³⁹ Encke, however, denied this explanation and went on to complain about the 'phenomenon of the constant difference between the Polaris observations of various observatories'.⁴⁰ This sentence ultimately led Bessel to formulate a further source of possible error in January 1819. Having assured Encke that the recent differences were no longer cause for any serious concern, he went on to provide the following explanation:

By the way, you know my opinion concerning the possibility of a *periodic* difference [due to diurnal temperature changes and changes of temperature over the seasons]; a *constant* one, i.e. one which remains the same *for all seasons*, would have to originate from the observer, who observes the passage of the star over the wire ... either too late or too early. Indeed, I believe in the possibility of this error; nonetheless I believe that the observer is able to control himself so much that he can estimate the transits *correctly*; at least I have taken *great pains* to get into the habit of a practice, which I consider to be free of this error.⁴¹

It is revealing that even an astronomer such as Bessel, who always considered new sources of error, here reached almost the same conclusion as had Maskelyne some twenty years earlier. Of course, the 'possibility' of a constant error of the observer is foregrounded, but this only in order then immediately to negate its actual influence in the sentence that follows. Method, training and self-control, 'taking great pains',

37 'Mir ist diese Abweichung bis jetzt noch ganz räthselhaft.' J. F. Encke to F. W. Bessel, 12 July 1818, BP, Letter-Vol. 13, 60.

38 See F. W. Bessel, 'Tafeln für die scheinb. Oerter des Polarsterns', *Astronomisches Jahrbuch für das Jahr 1817*, Berlin, 1814, 197–206, 197.

39 F. W. Bessel to J. F. Encke, 26 October 1818, BP, Letter-Vol. 16, 68.

40 J. F. Encke to F. W. Bessel, 12 January 1819, BP, Letter-Vol. 13, 77.

41 'Uebrigens kennen Sie meine Meinung über die Möglichkeit eines periodischen Unterschiedes; ein constanter, d. i. durch alle Jahreszeiten sich gleich bleibender, müßte vom Beobachter herrühren, der den Durchgang durch den Faden, entweder ... zu früh oder zu spät angäbe. An die Möglichkeit dieses Fehlers glaube ich auch; allein ich glaube, daß der Beobachter es über sich gewinnen kann, die Durchgänge recht zu schätzen; wenigstens habe ich mir viele Mühe gegeben mir eine Praxis anzugewöhnen die ich für frei von diesem Fehler halte.' Bessel to Encke, op. cit. (29); original emphasis.

still seemed to guarantee that such an observer's error could be avoided in advance. That said, it must be conceded that Bessel's reflections indicated some discomfort concerning the right way to evaluate the observing observer. In particular, attributes such as skill or talent were now debated in very different terms from those of Maskelyne's time.

In the same year, 1819, Gauss developed a formula for calculating the error of the observer's eye and ear.⁴² Gauss's interest was stimulated by a discussion on the probable error of a single transit observation which the astronomer Joseph Johann von Littrow, relying on Bessel's observations, had published a year before.⁴³ The method chosen by Gauss included an important prerequisite. As Rudolf Wolf later suggested, Gauss seemingly determined the error of the eye and the ear by reference to the slight differences between observations of transit time via a single wire and the mean transit time resulting from the observations at all wires.⁴⁴ Isolating the most appropriate values for solving the resulting term was a laborious affair. Moreover, this was an approach that did not allow one to retrieve uniform errors that might affect every single case. One could not derive a truly constant observer error in noting the times of transit. That Gauss indeed did not believe in the possibility of such an error is confirmed by his reaction to Bessel's later findings (see the following section of this paper). At the time, however, Bessel's ideas on the observer 'in action' were much closer to those of Gauss than to the findings of his 1823 report. In his programme of error analysis, at least, he was by now evaluating the observing observer as one of the variables that probabilistically influenced the size of the errors with each observation.

In a lecture on his new catalogue of Maskelyne's fundamental stars delivered to the members of the Berlin Academy of Science in June 1819, Bessel declared that ultimately neither the observer's experience nor the size of his instruments, following the then prevalent belief that the acuity of the instruments increased with their size, were decisive in determining the merits of a specific series of observations. For him, trust and reliability depended merely on the thoroughness by which the observer considered all possible errors: constant errors, as well as those emerging by chance. According to Bessel the 'errors of astronomical observation' formed 'two classes':

[T]he one contains the actual errors of observation, which are dependent on innumerable accidental causes and therefore can be considered to follow the general propositions of the calculus of probability; the other deals with ones that are provoked by constantly acting causes and which are to be ascribed to the deviation of the instruments from their mathematical ideal or from their manner of treatment. The first can be identified from the deviations of the observations between themselves, and their probable value as well as their influence on the determinations can be estimated by the rules which Laplace, Gauss and I have given; the second contaminate the observations only by certain laws and disappear as soon as

42 C. F. Gauss to W. Olbers, [c. 19 May 1819], in W. Olbers, *Sein Leben und seine Werke* (ed. C. Schilling), 2 vols., Berlin, 1894–1909, ii, Part 1, 726. See also C. F. Gauss to F. B. G. Nicolai, 30 January 1820, in *Briefe von C. F. Gauss an B. Nicolai. Zu Carl Friedrich Gauss' hundertjährigem Geburtstage* (ed. W. Valentiner), Karlsruhe, 1877, 8.

43 J. J. von Littrow, 'Ueber die Beobachtungen am Mittagsrohre', *Zeitschrift für Astronomie und verwandte Wissenschaften* (1818), 5, 9–24.

44 R. Wolf, *Handbuch der Astronomie, ihrer Geschichte und Litteratur*, 2 vols., Zürich, 1890–2, ii, 110 ff.

one discovers them. The former will be diminished through the *skill* of the observer and the quality of the instrument; the latter through the *insight* of the observer and rigour in the investigation of the instrument and in the method of observation. Both [classes of error] are independent of each other; deriving the amount of one from that of the other is not allowed; while the former can be small the latter can be big and vice versa.⁴⁵

In this context, the observer appears in a double role. While his ‘insight’ has influence on the analysis of the errors due to ‘constantly acting causes’, his ‘skill’ is mentioned as one of the factors that can diminish or increase the amount of the errors due to ‘accidental causes’. The skilful, experienced and watchful observer of Maskelyne’s time is now split into two individuals: a talented investigator who dedicates all of his genius to the exhaustive work of error analysis after the observations have been made, and a part of the observational machinery which stands as one of the variables producing accidental error in a set of observations. A skilful observer might be able to reduce the amount of this error, yet even the most talented and trained observer cannot make the error disappear entirely: the very act of observation was framed as an unavoidable source for accidental errors. But only a few days after his lecture to the Berlin Academy at the end of June 1819 an enterprise commenced that was in obvious discordance with this newly emerging probabilistic approach to the observer’s talents.

From 26 June the journal of the Seeberg observatory for the year 1819 offered the results for six transit observations that deserve particular attention: ‘Prof. Bessel’ observed the transit of the first star, ‘v. Lind[enau]’ the second and ‘Encke’ the third, and the last three transits repeat this same sequence.⁴⁶ Four years later in Bessel’s 1823 report the peculiar correlation between names and observations emerged as the first step to an inquiry into differences by noting the times of transits made by various observers.⁴⁷ Inclement weather prevented Bessel and his colleagues from continuing these observations, yet the plan itself tended to blur or complicate the picture of the twofold character of the observer laid out in the Academy lecture. In the latter case the observer appears as a person either whose skill influences the amount of accidental errors or whose insight permits the detection of constant errors. Now, however, the observer appears in a third role, namely as a person who does not reflect on constant

45 ‘[D]ie eine enthält die eigentlichen Beobachtungsfehler, die von unzähligen zufälligen Ursachen abhängen und deshalb den allgemeinen Sätzen der Wahrscheinlichkeitsrechnung folgend angesehen werden können; die andere begreift die von beständig einwirkenden Ursachen herrührenden, der Abweichung der Instrumente von ihrer mathematischen Idee, oder ihrer Behandlungsart zuzuschreibenden. Die ersteren können aus den Abweichungen der Beobachtungen unter sich erkannt, und sowohl ihre wahrscheinliche Größe, als die ihres Einflusses auf die Bestimmungen, nach den Vorschriften geschätzt werden, die Laplace, Gauss und ich gegeben haben; die letzteren stören die Beobachtungen nur nach gewissen Gesetzen und verschwinden sobald man diese erkennt. Jene werden verkleinert durch die Geschicklichkeit des Beobachters und die Güte des Instruments; diese durch die Einsicht des Beobachters und die Strenge der Prüfung des Instruments und der Beobachtungsart. Beide sind voneinander unabhängig; ein Schluß von der Größe der einen auf die der anderen ist unstatthaft; während jene klein sind, können diese groß seyn, oder umgekehrt.’ F. W. Bessel, ‘Bestimmung der geraden Aufsteigungen der 36 Maskelyne’schen Fundamentalsterne für 1815, auf Königsberger Beobachtungen gegründet’, *Abhandlungen der Königlichen Akademie der Wissenschaften in Berlin. Mathematische Classe* (1818/19), Berlin, 1820, 19–36, 19; original emphasis.

46 Beobachtungen Sternwarte Seeberg 1818–21, Forschungs- und Landesbibliothek Gotha, Manuscripts division, Chart. B 2190(7), f. 41vs.

47 *Astronomische Beobachtungen*, op. cit. (1), IV.

errors but simply causes them in the course of the act of observation. The sources are frustratingly silent on the reasons motivating this step. But the very fact that Bessel started to explore the possibility of such an error is telling enough. It reveals how, in the course of the debate on observational errors, the picture of the observer 'in action' had become fuzzy. The terms and the consequences of understanding the observer's conceivable errors were no longer self-evident.

Persisting surprise: *involuntary* differences

After numerous joint observations with a number of observers, it had in retrospect become quite inconceivable to Bessel that nobody had previously stumbled across the fact to which he now drew attention. At the very beginning of his 1823 report he left no doubt that Maskelyne would already have reached the same conclusions had he discussed the Kinnebrook case sufficiently thoroughly:

Assuming that with a good transit instrument both the stars and the threads appear completely distinct, that the motion [of the star] in one second with a magnification of 60 to 80 times is already very big, that the observer usually feels he is conscious of one or at best two tenths of a second: then the difference between Maskelyne and his assistant seems almost unbelievable; if one considers further, that Mr Kinnebrook must have wished to observe the transits earlier again, in order to approximate what was considered entirely correct, then one can no longer doubt, that an *involuntary* constant difference can take place between two observers, which clearly exceeds the limits of the accidental uncertainty ...⁴⁸

But what appears here to be the most natural inference was in fact inseparably bound up with the course of Bessel's own experiences. Before the inquiry we have only a scant indication that he really calculated with what he would later find. Even in his January 1819 letter to Encke he proposed a probable constant error of the observer rather as a thought experiment than as a truly conceivable option. At least in his own case he rejected the possibility of such an error. It is precisely this shift between the initial premises and the final assumptions that characterizes Bessel's enterprise. In contrast to Maskelyne's remarks, his findings demonstrated something new, showing up against expectation, which prior to this point no one had adequately considered.

It took more than a year for Bessel to follow up his first fruitless inquiries at the Seeberg observatory. In the winter of 1820–1 the young astronomer Johan Henrik Walbeck (1793–1822) of Åbo (today Turku in Finland) stayed with him in Königsberg for several months. Bessel's colleague and friend Friedrich Georg Wilhelm Struve (1793–1864) introduced Walbeck with the following words: 'With pleasure he would

48 'Wenn man bedenkt, daß sowohl die Sterne, als die Fäden, in guten Mittags-Fernröhren vollkommen deutlich erscheinen, daß die Bewegung in einer Secunde, schon bey einer 60 bis 80 mahligen Vergrößerung sehr groß ist, daß der Beobachter gewöhnlich bis auf ein, höchstens zwey Zehnthelle einer Secunde sicher zu seyn glaubt: so erscheint der Unterschied zwischen Maskelyne und seinem Gehülffen beynabe ungläublich; wenn man ferner erwägt, daß Herr Kinnebrook den Wunsch gehabt haben muß, die Durchgänge wieder früher zu beobachten, um sich dem zu nähern, was für ganz richtig gehalten wurde, so kann man nicht mehr zweifeln, daß zwischen zwey Beobachtern ein unwillkürlicher beständiger Unterschied Statt finden kann, welcher die Grenzen der zufälligen Unsicherheit weit überschreitet ...' *Astronomische Beobachtungen*, op. cit. (1), III; original emphasis.

like to help you with all kinds of calculations and reductions; and you would find in him a skilful computer and trigonometrer. He wishes above all to learn about practical astronomy from you, where he still deserves some closer instruction.⁴⁹ The status as a kind of trainee in observing is obviously the subtext of Struve's recommendation of interest. Bessel and Walbeck's subsequent joint observations appear to reproduce the Maskelyne–Kinnebrook situation of twenty-five years earlier. Two observers, one very experienced and the other merely a beginner, observed together. Bessel observed the transits of certain stars on the first day and Walbeck the next, while Walbeck first observed the transits of further stars, which Bessel then observed the following day.⁵⁰ The result of this procedure was that one could obtain two independent determinations of clock rate, which, in the case of a difference and all other circumstances being equal, could immediately prove that the two observers did not note the moment of the star's transit correspondingly. This was just the scenario that had once caused Maskelyne to focus on the unlucky Kinnebrook. Two sets of Greenwich observations led to two determinations of the rate of the clock, which, as we know, differed by more than half a second.⁵¹ The very same situation now happened with Bessel and Walbeck. Each of their four joint observations, including eighty observations in total, led to an even greater difference of about one second in time. In his report Bessel concluded 'that Walbeck always observed significantly later than me, namely ... in the mean 1."041, a result which can hardly be doubtful for some hundredths of a second'.⁵²

Bessel's further reaction reveals that his findings had definitively destroyed the older conception of the astronomical observer as the skilful master of an art. Several weeks after the comparisons had finished Bessel described his sheer amazement at the 'very peculiar phenomenon' in a letter to Johann Georg Tralles (1763–1822), secretary of the mathematical section of the Berlin Academy:

That difference is all the more astonishing, because usually with a strong magnification [as was used in this case] one can safely believe oneself to be conscious of a single tenth. What kind of consequences this experience might have is still obscure for me; but it seems for sure that they [the differences] will not disappear for all obs. [observers or observations].⁵³

This fact was apparently so surprising to Bessel that he repeated it in virtually the same terms in letters to Encke and Struve, as well as to his first mentor and fatherly friend

49 'Gerne würde er bey Rechnungen und Reductionen jeder Art behülflich sein, und Sie würden in ihm einen gewandten Rechner und Trigonometrer finden. Er hofft von Ihnen zumal in Bezug auf practische Astronomie zu lernen, wo ihm manche nähere Anweisung noch wohl thut.' F. G. W. Struve to F. W. Bessel, 15 September 1820, BP, Letter-Vol. 12, 134 ff.

50 *Astronomische Beobachtungen*, op. cit. (1), IV.

51 *Astronomical Observations*, op. cit. (12), 339.

52 'Walbeck [beobachtete] stets bedeutend später als ich, nämlich ... im Mittel 1",041, welches Resultat kaum einige Hunderttheile einer Secunde zweifelhaft seyn kann.' *Astronomische Beobachtungen*, op. cit. (1), V.

53 'Jener Unterschied ist desto mehr zu verwundern, da man sich bei einer starken Vergrößerung, gewöhnlich des einzelnen Zehnthels sicher bewußt zu sein glaubt. Was diese Erfahrung für Folgen haben kann, ist mir noch dunkel; aber es scheint gewiß daß sie nicht bei allen Beob. [Beobachtern oder Beobachtungen] verschwinden.' F. W. Bessel to J. G. Tralles, 11 February 1821, BP, No. 384, f. 56.

Olbers. It might indeed be possible to ‘safely believe oneself to be conscious of a single tenth’ but ultimately a difference of one second remained.⁵⁴

Considering Bessel’s persistent surprise, it appears that the first result of his inquiry was less a fact of nature than a disturbing discordance between what he thought he knew and the knowledge revealed by his observations. Although both Bessel and Walbeck seemed conscious of their activity and even increased their ‘attention to the true moment of transit’ throughout the course of their observations,⁵⁵ they were nevertheless unable to minimize the amount of the determined difference. This outcome provoked real perplexity. Contrary to Bessel’s assumption in January 1819, it now became more than likely that the observer ‘in action’ was not ‘able to control himself so far’ that he could avoid constant errors. On the contrary, it became evident that he was able to commit errors which could not be overcome by sheer human will and which resisted any effort on the part of the observing subject. Three years later this experience had completely lost its astonishing, unbelievable character. At the beginning of his report, cited above, Bessel introduced the possibility of ‘an *involuntary* constant difference’ as the last and most convincing explanation, not only for the discrepancies between Kinnebrook and his master Maskelyne. Yet at the very moment that he experienced an initial surprise, in his letter to Olbers Bessel characteristically resorted to a very well-known phrase. He concluded that all this was ‘extremely puzzling’.⁵⁶

Several remarks in Bessel’s correspondence show that German astronomers reacted to his findings with mixed feelings. Only Gauss directly rejected the significance of the comparison, claiming that Walbeck was not very good at observing stars. He believed that ‘the differences always must remain very small’ for ‘experienced observers’.⁵⁷ Bessel, however, had already found proof to the contrary. In February 1821, while on his way home to Åbo, Walbeck had made further comparisons with Struve, who was the director of Russia’s Imperial Observatory in Dorpat (today Tartu in Estonia). These comparisons revealed that Walbeck observed the transits between two and three tenths of a second later than Struve.⁵⁸ Although this amount was much smaller than that obtained between Walbeck and Bessel, it substantiated the possibility of constant differences between observers and, above all, it indirectly confirmed the considerable difference of seven-tenths of a second between Struve’s and Bessel’s observations, which had resulted from a single comparison in Königsberg in autumn 1820.⁵⁹

For the moment, the outcome of all of the joint observations taken together seemed to provide evidence for a certain regularity of the entire phenomenon. It now became

54 F. W. Bessel to J. F. Encke, [February 1821], Encke Papers, Archiv der Berlin-Brandenburgischen Akademie der Wissenschaften, Berlin, No. 145; F. W. Bessel to F. G. W. Struve, 20 January 1821, BP, Letter-Vol. 14, 130; F. W. Bessel to W. Olbers, 8 February 1821, in *Briefwechsel zwischen W. Olbers und F. W. Bessel* (ed. A. Ermann), 2 vols., Leipzig, 1852, ii, 185 ff.

55 *Astronomische Beobachtungen*, op. cit. (1), V.

56 F. W. Bessel to W. Olbers, 8 February 1821, in *Briefwechsel zwischen W. Olbers und F. W. Bessel* (ed. A. Ermann), 2 vols., Leipzig, 1852, ii, 186.

57 C. F. Gauss to W. Olbers, 18 March 1821, in W. Olbers, *Sein Leben und seine Werke* (ed. C. Schilling), 2 vols., Berlin, 1894–1909, ii, 92.

58 J. H. Walbeck to F. W. Bessel, 11 February 1821, BP, No. 393, f. 1.

59 *Astronomische Beobachtungen*, op. cit. (1), VI.

plausible that a significant difference could appear even between two very experienced observers such as Bessel and Struve, whose ‘acuity’, according to Gauss, was ‘incomparable’.⁶⁰ In addition, the agreement revealed by the results of all three comparisons between Bessel, Struve and Walbeck strengthened the idea that the differences, however ‘puzzling’ they might be, followed a regular pattern. Even observers who shared quite different experiences adopted the view that a completely new situation had been created. Immediately before Walbeck’s visit, Struve had conducted a comparison with his assistant Karl Friedrich Knorre (1801–83), which had not led to any noteworthy results.⁶¹ Yet this ‘failure’ did not cast doubt on the object under inquiry. Pointing to Bessel’s experiences, Struve also concluded that the ‘constant difference in determining the transits of approximately 1" in time between you [Bessel] and Walbeck is indeed highly peculiar’.⁶²

What had initially appeared with all the conditions of scepticism, then, was ultimately becoming an indisputable fact, one which could be expected to be reproducible in the future. After making a new set of comparisons with his assistant Friedrich Wilhelm Argelander (1799–1875) in the spring of 1823, Bessel anticipated thus:

Now things look like this: Argelander observes 1s, 22 later than I, Walbeck 1s, 02; you observe 0s, 22 earlier than Walbeck, thus 0s, 80 later than I. Therefore your difference with Argelander must be 0s, 42, which might be directly confirmed on the occasion of Argelander’s travel [to Dorpat], even though its correctness can hardly be doubted, because all observations converge harmoniously.⁶³

Bessel proved, however, to be wrong. No single result agreed with the corresponding earlier results. Moreover, extending the comparisons to other observation tasks and engaging in a slight application of experiment to the inquiry produced even more disagreement.

The life of a phenomenon: experimentalization and incoherency

‘This makes my mind stand still’.⁶⁴ This desperate outburst by Argelander characterizes perfectly the situation that the actors had to cope with in July 1823. The state of affairs was brought about by two moves. On the one hand, Bessel turned to a second type of astronomical observation, namely the ‘covering’ and ‘reappearance’ of stars, planets and the Sun. We would now call this the observation of eclipses. On the other hand, the

60 C. F. Gauss to F. B. G. Nicolai, 22 July 1820, in *Briefe von C. F. Gauss an B. Nicolai*, op. cit. (42), 26.

61 *Observationes astronomicas, institutas in specula Universitatis caesariae Dorpatensis*, Vol. 3: *Observationes annorum 1820 et 1821*, Dorpat, 1823, L.

62 ‘Die constante Differenz der Antritte nahe 1" Zeit zwischen Ihnen und Walbeck ist doch sehr sonderbar.’ F. G. W. Struve to F. W. Bessel, 30 January/11 February 1821, BP, Letter-Vol. 12, 138.

63 ‘Die Sache steht nun so: Argelander beobachtet 1", 22 später als ich, Walbeck 1", 02 später; Sie beobachteten 0", 22 früher als Walbeck, also 0", 80 später als ich. Sie müssten also mit Argelander eine Differenz von 0", 42 haben, welche sich, bei Argelanders Reise [nach Dorpat], direct wird bestätigen lassen, wenn auch kaum an ihrer Richtigkeit gezweifelt werden kann, da alle Beobachtungen vortrefflich harmoniren.’ F. W. Bessel to F. G. W. Struve, 21 May 1823, BP, Letter-Vol. 14, 189 ff.

64 ‘Mein Verstand steht dabei still.’ F. W. A. Argelander to F. W. Bessel, 19 July 1823, BP, Letter-Vol. 17, 2.

whole enterprise moved to a more experimentalist working style, introducing new types of set-up and varying certain circumstances of the observations.

The first result of these changes was that Argelander and Bessel noticed that their differences in observing transits and in observing ‘coverings’ and ‘reappearances’ varied in time over an entire second.⁶⁵ Argelander and Struve, by contrast, were unable to find any difference for observations of the second kind, whereas for the first they tracked a difference in which Argelander’s transits came two-tenths of a second later than Struve’s.⁶⁶ There was thus no evident, regular relationship between the differences in observing transits and those in observing ‘coverings’ and ‘reappearances’. Different pairs of observers reached different results. A series of observations on the clock’s influence in measuring the time of transit caused additional confusion. When Bessel resorted to using a pendulum clock with a beat of a half-second instead of a second, he likewise observed the moments of transit half a second later, whereas Argelander and Struve could not find such variations.⁶⁷

The tendency towards using new types of set-up that allowed an increase in the total number of observations and simultaneously a reduction of the observational task to its basic elements also created complications. Because, presumably, eclipses are rare astronomical events, in most of the one hundred observations of ‘coverings’ and ‘reappearances’ that they carried out Bessel and Argelander used a rather simple arrangement consisting of a black spot covered by a sheet of paper.⁶⁸ Struve introduced more refined devices for this kind of observation. First he used a lamp whose light could be rapidly covered by a screen, then later one of the latest technological innovations of the time, Gauss’s heliotrope, a mirror which focused the sunlight into a strong single beam used in signalling.⁶⁹ At first glance, the outcome of this step towards experimentalization seemed rather favourable. The results of the comparisons showed considerable agreement and were not perceptibly affected by variations of the set-up. Nevertheless the introduction of these new means also meant that a new source of error had to be considered. In fact, some of the unpublished experiences from Bessel’s comparisons with Walbeck had already shown that differences between observers could vary with the set-up. Observations of a swinging pendulum, introduced as a substitute for transit observations, produced results with a difference considerably smaller than the difference determined from directly observing transits.⁷⁰

But matters were far more complex. The most striking feature of this enterprise was no doubt the experience that transit differences for a certain pair of observers did not appear to remain constant over the time that elapsed between the two sets of joint observations, those made in the winter of 1820–1 and those in spring 1823. These

65 See the results in *Astronomische Beobachtungen*, op. cit. (3), 8. Abtheilung, V. and VI.

66 *Astronomische Beobachtungen*, op. cit. (1), VI and VII.

67 *Astronomische Beobachtungen*, op. cit. (1), VII.

68 F. W. A. Argelander to F. G. W. Struve, 28 April 1823. Communication of Dr Wolfgang R. Dick (Potsdam).

69 F. G. W. Struve to F. W. Bessel, [July 1823], BP, Letter-Vol. 12, 221 ff. Also see *Observationes astronomicas, institutas in specula Universitatis caesareae Dorpatensis*, Vol. 4 (=N. S. Vol. 1): *Observationes annorum 1822 et 1823*, Dorpat, 1825, XLVII–L.

70 F. W. Bessel to J. G. Tralles, 11 February 1821, BP, No. 384, 56 ff.

results completely frustrated Bessel's expectation that the comparison between Argelander and Struve would confirm the regularity, which had shown up in the earlier comparisons with Walbeck. The difference in transits between Argelander's and Struve's observations was smaller than Bessel predicted. He was forced to conclude that the difference between Struve and himself had also changed.⁷¹ The proper word for such an outcome is already familiar: 'After this, I am very eager to learn how you will solve these puzzles',⁷² Struve remarked in a letter to Bessel in July 1823. He received an answer some weeks later:

Well then, my dear friend, we vary with respect to ourselves – the same happened to Maskelyne and Kinnebrook; over a few years we must differ 1s, 20, if the maximum has not yet been reached. – Now, who is observing *properly* and who poorly N.N. may know; I certainly do not observe properly, but two out of three persons – Struve, Walbeck, Argelander – don't observe properly either. One has as much to speak for it as the other – hence no one can count on absolute times.⁷³

Bessel's discussion of proper observing indicates that he still believed in the one true path. But this ideal had become unattainable for him: 'no one can count on absolute times'. Rather, the differences between observers themselves turned out to be dependent on time, specifically on the time that had elapsed between the observations. This experience carried significant consequences. It is interesting to note that in this context Bessel referred to the Maskelyne–Kinnebrook episode for the first time ever in his correspondence. Set in the context of more recent experiences, what had once been a curiosity in the history of the Greenwich Observatory now appeared to be one more case that confirmed the conclusions of similar observations.

At first glance, the picture offered by the outcome of Bessel's investigation in summer 1823 matches the concept of experimenting as a 'complication', developed by Gaston Bachelard in *Le nouvel Esprit scientifique* (1934). Rejecting the traditional understanding of experimental results 'as *perturbations* of a general law',⁷⁴ Bachelard claimed that unruly 'experimental facts' must in direct contrast be seen as offering a chance to recognize the research object in its full complexity. As Bachelard assumes in his often quoted key sentence, 'There are no simple phenomena; every phenomenon is a fabric of relations.'⁷⁵ This is an accurate description of what happened in the course of Bessel's further inquiries. The simple regularity displayed in the first set of comparisons in 1820–1 subsequently dispersed into ever more differences of

71 Cf. *Astronomische Beobachtungen*, op. cit. (1), VI.

72 'Begierig bin ich, ob Sie hiernach die Räthsel lösen werden.' F. G. W. Struve to F. W. Bessel, [July 1823], BP, Letter-Vol. 12, 223.

73 'Also, mein theurer Freund, wir variieren unter uns selbst! – das ist dasselbe was Maskelyne und Kinnebrook arrivirt ist; über ein paar Jahre müssen wir 1, "2 abweichen, wenn das Maximum nicht schon erreicht ist. – Wer nun Recht beobachtet, wer unrecht, das mag N.N. wissen; ich beobachte gewiß nicht recht, aber zwei von dreien – Struve, Walbeck, Argelander – beobachten auch nicht recht. Der eine hat eben so viel für sich als der andere – also ist auf absolute Zeiten nicht zu rechnen.' F. W. Bessel to F. G. W. Struve, 26 July 1823, BP, Letter-Vol. 14, 197; original emphasis.

74 Gaston Bachelard, *The New Scientific Spirit* (tr. A. Goldhammer), Boston, 1984 (first published 1934), 149; original emphasis.

75 Bachelard, op. cit. (74), 147.

differences; the phenomenon was linked to further circumstances and became associated with ever increasing numbers of set-ups and tasks of observation. But contrary to Bachelard's suggestion, the observed complexity did not lead to a deeper understanding of the phenomenon's characteristics, at least if one assumes that a deeper understanding leads to more comprehensive knowledge. Complexity in this case was almost identical with incoherence and, as regards the experimenter, with a loss of control. This did not imply that Bessel's findings automatically gained permanence in the form of a problem.

'Cold tradition': avoiding further discussions

Bessel's 1823 report at first had little impact. Four very brief communications, all published in 1824 and only one in an astronomical context,⁷⁶ comprised a rather weak response to his findings, which might presumably have been of utmost importance for practical astronomy. Additional notices may perhaps be uncovered in the literature, though those found resulted from a comprehensive survey. But this would not detract from an image of a limited presence of the report in scientific discourse in immediately subsequent years. In the German-speaking context, Bessel's conclusions were first discussed in public in autumn 1829, when his colleague Friedrich Nicolai presented a paper to the Heidelberg meeting of the Naturforscherversammlung on possible reasons for differences between observers.⁷⁷ Outside the German astronomical community, even more time elapsed before references to Bessel's report emerged in print.⁷⁸

Bessel's report undoubtedly failed initially to receive extensive public discussion. Edwin G. Boring's assumption that the publication of this report almost at once put practical astronomy on a state of alert therefore seems all the more remarkable. In his standard reference work, *A History of Experimental Psychology* (1935), Boring concluded that it was 'fortunate that the difference [between Bessel and Walbeck] was so large, for it stimulated Bessel to further work, and, when published in 1822 [*sic*], it attracted immediate attention'.⁷⁹ Subsequent historical contributions never again directly challenged the supposition that Bessel's findings must have directly provoked

76 [J. E. Bode], 'Einige astronomische Beobachtungen, Nachrichten und Bemerkungen', *Astronomisches Jahrbuch für das Jahr 1827*, Berlin, 1824, 203–8, 208; [Anonymous], 'Curious astronomical fact', *Philosophical Magazine and Journal* (1824), 63, 230–2; [E. de Billy], 'Fait curieux en astronomie', *Bulletin des sciences mathématiques, astronomiques, physiques et chimiques* (1824), 2, 111–3; [Anonymous], *Edinburgh Journal of Science* (1824), 1, 178 ff.

77 F. B. G. Nicolai, 'Über die bei den einzelnen Individuen statt findende Verschiedenheit des geistigen Reflexes der äußern Eindrücke auf die Organe des Gesichts und Gehörs', *Isis* (1830), 23, 678–81.

78 The first reference in the British context seems to be Francis Baily's 'Report on the new Standard Scale of the Royal Astronomical Society', *Memoirs of the Royal Astronomical Society* (1836), 9, 35–184, 92 (thanks to Jimena Canales), whereas in French, Bessel's paper apparently was first discussed in Alphonse Quetelet and Richard Sheepshanks's report on the difference of longitude between the observatories in Brussels and Greenwich (*Nouveaux Mémoires de l'Académie royale des sciences et belles-lettres de Bruxelles* (1843), 16, 3–18, 10).

79 Boring, *A History of Experimental Psychology*, op. cit. (6), 136. See also R. L. Duncombe, 'Personal equation in astronomy', *Popular Astronomy* (1945), 53, 2–13, 63–76, and 110–21, 3.

an explicit, highly visible reaction in the astronomical community. Indeed, only one participant in the following century of discussion even became sceptical. Focusing on the few comparisons in the period directly after the publication of Bessel's findings, the psychologist Edmund C. Sanford supposed as early as 1888 that at first the whole issue had not 'received much consideration in actual practice'.⁸⁰ Sanford's diagnosis seems all the more justified when supported by the fact that most of the few determinations before the early 1830s did not originate from astronomical work.⁸¹ Nonetheless, Sanford drew the wrong conclusion. Neither the almost complete absence of regular means of control nor the minor presence of Bessel's report in the literature of the day implies that his findings did not have direct consequences. Sanford may perhaps have been misled by the fact that the measures only left sparse traces in written and printed records. A few footnotes, a new column added to the tables of observations and two or three sentences are all that testify to the initial reaction to Bessel's findings.

Positional astronomy, to which the measurements of practical astronomy mostly belong, is the science of the relations between all heavenly bodies. Determination of a star's place in the celestial sphere was then always based on measurements that included the relation of one object to another. In the case of transit observations, this meant that it was not the absolute time of transit but rather the time difference between the transit of the observed star and that of a second star, or very often the solar transit, that provided the basis for further calculation of the star's right ascension.⁸² The same was true of the more practical use of transit observations in regulating observatories' clocks. In order to determine the degree to which a certain clock ran faster or slower over the course of one day, the actual running time of the clock was compared to two consecutive transits of one and the same star, or a number of stars, called 'clock-stars', which would theoretically comprise the time span of exactly twenty-four hours. Thus one could easily avoid the whole issue of constant differences between observers affecting the business of practical astronomy. As one of Bessel's colleagues already remarked in spring 1821, the phenomenon

cannot have any influence on the precision of the results of the observations, because it has to be assumed that a single observer will estimate the transits of *all* stars and of the borders of the Sun and of the Moon at the *same* quantity of difference from the true value. But experiment shows that in observatories where two observers are working, one observer must not be allowed to take the place of the other in one and the same series of observations ...⁸³

80 Sanford, *op. cit.* (2), 16 ff.

81 From the twelve determinations which I could identify between 1823 and 1832, only four formed part of regular astronomical measurements; See C. Hoffmann, *Unter Beobachtung: Naturforschung in der Zeit der Sinnesapparate*, Göttingen, 2006, 187.

82 See, for example, W. Pearson, *An Introduction to Practical Astronomy: Containing Descriptions of the Various Instruments, that Have Been Usefully Employed in Determining the Places of the Heavenly Bodies, with an Account of the Methods of Adjusting and Using Them*, 2 vols., London, 1824–9, ii, paragraphs LXII–III.

83 '[A]uf die Genauigkeit der Beobachtungsergebnisse kann sie [die Differenz], glaube ich, keinen Einfluß haben, indem anzunehmen ist, daß der eine Beobachter die Appulse aller Sterne und der Sonnen- und Mondränder um eine gleiche Größe zu der wahren verschieden schätzen wird. Der Versuch zeigt aber, daß auf Sternwarten, wo zwei Beobachter sind, der eine den anderen in einer und derselben Beobachtungsreihe nicht ablösen darf ...' F. B. G. Nicolai to F. W. Bessel, 4 April 1821, BP, No. 312, f. 16; original emphasis.

In other words, given that one and the same observer always noticed the transits too late or too early for the same amount of time, the resulting error would always affect every result in the same way and, consequently, could not influence the determination of the time difference between two transits. Separating the observations of different observers therefore provided a very simple means for avoiding any problems. Bessel concluded his 1823 report precisely with this advice. As he emphasized, the phenomenon entailed the ‘utmost harmful influence if the transit observations of two observers at one observatory were mixed’. For those readers who did not immediately grasp the implications of his words, he added that ‘at the observatory here [Königsberg], all observations of this kind are made by myself’.⁸⁴

Bessel’s final statement indicates why his findings could never acquire the status of a problem for some astronomers. Only observatories where more than one astronomer observed regularly were potentially in danger. This was rather exceptional, at least in the current German milieu. For example, even Gauss in Göttingen only started employing an assistant observer in the early 1830s and the few observatories that employed one or more assistants, such as Königsberg or Dorpat, strictly followed Bessel’s advice: each observation task was assigned to one specific observer. This procedure, however, never became the topic of discussion. Indeed, it only becomes apparent from prefaces and footnotes to the printed observations of the Dorpat and Königsberg observatories, which meticulously recorded each change of the observer from the mid-1820s.

A different way of coping with the phenomenon, to which Bessel had alluded, can be identified in the printed records of the Observatoire royale in Paris, where four observers were already at work by the 1820s. Comparing the tables of the transit observations for the year 1826 with those for the preceding year, it becomes apparent that a new column was added. Entitled ‘*Observateurs*’, every single observation in this column was marked with the initial of the observer.⁸⁵ Although the editors did not comment on this measure, it is likely that these initials were intended to enable the reader to relate only those observations that were made by one and the same observer. This assumption is supported by a similar change at the Greenwich Observatory. In the introduction to the printed observations for the second quarter of 1825, the Astronomer Royal James Pond remarked,

In consequence of a new regulation, the assistants have been directed to annex the initials of their names to their respective Observations whenever Mr. Pond is absent from the Observatory, or not immediately superintending the Observations himself. With respect to the Transit Observations, however, it may be proper to observe, that they are at present entirely entrusted to the care of Mr. Taylor, the first assistant.⁸⁶

Although subsequent volumes of the *Greenwich Astronomical Observations* prove that, contrary to Pond’s instruction, almost every observer was engaged willy-nilly in

⁸⁴ *Astronomische Beobachtungen*, op. cit. (1), VIII.

⁸⁵ *Observations astronomiques faites à l’Observatoire royal de Paris publiées par le Bureau des longitudes*, Vol. 2, Paris, 1838.

⁸⁶ *Astronomical Observations Made at the Royal Observatory at Greenwich for the Year 1825*, Part 2: April to June 1825, London, 1825, 7.

transit observations, marking the observations with the observer's name has to be understood, as in the case of Paris, as equivalent to the measures taken by the German astronomers. Different issues, of course, are raised by the facts that Pond's order was insufficient and more generally that neither the Greenwich observations nor the records of the *Observatoire royale* provide any evidence as to whether the daily work of calculation and reduction did indeed take the possibility of a constant difference between observers into account.

The astronomers' early answers to Bessel's findings shared one common feature: they simultaneously paid tribute to the new phenomenon and silenced it. Characteristically, the various reactions either aimed at avoiding any potential impact of the phenomenon or simply added a new column to the printed observations. A permanent control of such differences or a broader methodological discussion did not take place. Even the measures taken were never directly linked to the phenomenon to which they obviously responded. So at the outset Bessel's findings found only a passive presence in the knowledge of practical astronomy. They became part of a 'cold tradition'.⁸⁷ They were preserved in the form of undiscussed practices. The main consequence of this tradition was that the phenomenon's further analysis was profoundly limited. Instead of leading to an increase in research, it was, at best, sufficient for the astronomers to organize the observatory business adequately, and thereafter the phenomenon disappeared from the list of topics begging for attention.

Returning to Sanford's diagnosis, one can now understand why he missed the specific detail through which Bessel's findings initially affected astronomical practice. First, the means developed by Bessel and others tend to efface themselves; second, those means were not such as to attract the attention of anyone interested in an issue called the 'personal equation'. From his point of view, Sanford was indeed perfectly right. The career of the personal equation did not start before the regular determination of constant differences between observers was first introduced at the Greenwich Observatory. However, as preceding paragraphs have shown, determinations of that kind by no means accounted for the whole range of possible reactions to the phenomenon at stake. The personal equation was merely one means among others, introduced comparatively late. Rather than an answer to an urgent problem, it was a reaction to a self-generated mess.

'Hot tradition': labour division as problem

In 'Astronomers mark time' Simon Schaffer suggests that from the beginning, in reaction to what he calls the 'worrying fact' of the 'personal equation',⁸⁸ practical astronomy mainly aimed at disciplining the observer by technological means and by a rigid organization of the workplace. The changing situation in Greenwich between 1800 and 1900, which he uses as a reference point for his argument, fits quite well with

⁸⁷ With the terms 'hot' and 'cold' tradition I refer to the concept of hot and cold memory in Jan Assmann's widely discussed study *Das kulturelle Gedächtnis. Schrift, Erinnerung und politische Identität in frühen Hochkulturen*, München, 1992, 68–70.

⁸⁸ Schaffer, *op. cit.* (7), 117.

this picture of the observatory as a ‘factory’, if not as a ‘panopticon’.⁸⁹ During the decades between 1835 and 1881, when George Biddell Airy (1801–92) was in charge as Astronomer Royal, the installation’s agenda was dominated by issues of control implemented at the level of both instruments and management styles. Airy assigned each of the diverse observational tasks to certain members of his staff, then continued with the yearly determination of the personal equation of all employees from the beginning of the 1840s, and ultimately introduced electromagnetic apparatus to register transit observations in 1853. Observers were increasingly transformed ‘into machine minders’ who executed a highly specialized and regulated activity.⁹⁰ Although the Greenwich situation was somewhat unusual due to the size of the observatory and the tremendous public interest in its work as one of the cornerstones of the British Empire, similar trends can be seen in many additional astronomical installations of that epoch. That said, at second glance Schaffer’s outline might produce a misunderstanding. In his article, Bessel’s findings appear as something that self-evidently demanded measures of control. The prerequisite of the argument is that 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.⁹¹

Yet a closer look shows that Airy’s enormous efforts at coping with ill-defined German variables were not due to the phenomenon itself, whose influence on the observational results, as illustrated above, could be easily avoided. All the measures taken were, in fact, caused by the particulars of the Greenwich observational regime. When it is asserted, with respect to the factory-like organization, that ‘division of labour demanded precise control over an increasing range of menials’,⁹² this unwittingly points to the source of all the troubles. The way in which ‘division of labour’ was actually practised in the Royal Observatory had ambiguous consequences. It could be a valuable means for rationalizing observatory business, but first and foremost it was a genuine threat to reliable data.

As early as December 1823 Bessel emphasized in a letter to Gauss that his findings could cause severe troubles for observatories, ‘such as, for example, the one at Greenwich, where, as Mr. Baily told me, *five* [observers] are employed’.⁹³ Bessel’s confidant, Francis Baily, a stockbroker and one of the founders of the Royal Astronomical Society, had indeed drawn an entirely unfavourable portrayal of the Royal Observatory:

The business of the observatory at Greenwich is conducted by 4 persons, of very inferior talent. Two watch by day, & two by night. Mr Pond himself seldom handles the instruments,

89 Schaffer, *op. cit.* (7), 119.

90 Schaffer, *op. cit.* (7), 119.

91 Schaffer, *op. cit.* (7), 118.

92 Schaffer, *op. cit.* (7), 119.

93 F. W. Bessel to C. F. Gauss, 11 December 1823, in *Briefwechsel zwischen Gauss und Bessel*, *op. cit.* (31), 426; original emphasis.

but merely superintends ... The King's warrant requires that each person should put the initials of his name to his observations: but this is never done. They are all presumed to be made by Mr Pond, although he has very little hand in the business. – This communication however is *private*.⁹⁴

It is difficult to disentangle in Baily's description just what reflects a businessman's view of astronomical affairs and what truly characterizes the Greenwich situation as it then was.⁹⁵ What seems certain is that the basic lesson of Bessel's 1823 report, namely not to mix observers, fell on deaf ears in the observatories' regimes. On the contrary, it was quite common for a number of observers to alternate in one and the same series of observations. For example, in the decade after 1825, by which time marking of observations had been introduced, one sees on average no less than six observers engaged in transit observations, of whom two would do the bulk of the work. But this habit was not peculiar to Pond. Despite the fact that Greenwich historians usually make a stark contrast between the situation under his leadership and that under his successor Airy,⁹⁶ this problematic kind of labour division survived all reforms. The dramatically novel element in the 'new system', as Airy called it in his autobiography,⁹⁷ was not his way of organizing the observational tasks, but rather Airy's extensive communications on matters of organization and his meticulous style of management.

It is true that Airy replaced several members of the staff for disciplinary reasons after he took up the position at the end of 1835, but the daily astronomical work remained largely untouched. In keeping with earlier times, two men were employed for observing transits, at times corroborated by at least three additional observers.⁹⁸ Accordingly, it is no surprise that the probable influence of differences between observers was not initially on the agenda of the new Astronomer Royal. In the *Astronomical Observations* for 1836, the first year for which Airy was solely responsible, he remarks with respect to the tables of the transit observations that the

eighth and ninth columns contain the adopted losing rate and the adopted error of the clock at 0^h sidereal [time] ... The observations are divided into groups, marked by the bars across these columns, whose limits are (in almost every case) the same as the limits of each individual's observations. The direct effect of *personal equation* is thus almost entirely avoided.⁹⁹

94 F. Baily to F. W. Bessel, 30 October 1823, BP, No. 162, f.4.

95 For the intersection between astronomy and financial business in Baily's life see W. J. Ashworth, 'The calculating eye: Baily, Herschel, Babbage and the business of astronomy', *BJHS* (1994), 27, 409–41.

96 E. W. Maunder, *The Royal Observatory Greenwich: A Glance at Its History and Work*, London, 1900, 100f; E. G. Forbes, *Greenwich Observatory. Volume 1: Orgins and Early History (1675–1835)*, London, 1975, 171; A. J. Meadows, *Greenwich Observatory. Volume 2: Recent History (1836–1975)*, London, 1975, 1–4.

97 G. B. Airy, *Autobiography* (ed. W. Airy), Cambridge, 1896, 123.

98 See the entries in the observer's column for the transit observations from 1836 to 1840 (from *Astronomical Observations Made at the Royal Observatory at Greenwich in the Year 1836 under the Direction of George Biddell Airy*, London, 1837, to *Astronomical Observations Made at the Royal Observatory at Greenwich in the Year 1840 under the Direction of George Biddell Airy*, London, 1842).

99 *Astronomical Observations in the Year 1836*, op. cit. (98), p. xx.

However, as we shall soon see, an effect of the ‘personal equation’¹⁰⁰ was occasionally noted. The same remark appeared in 1837, but a radical change in dealing with this issue did not take place until the following year’s observations were published. Immediately following the last quoted passage, a new paragraph was added:

In the years 1836 and 1837 the occasional effects of personal equation were neglected: but in 1838, the difference between the observations of Mr. Henry and Mr. Ellis attracted greater attention, and sometimes caused trouble in the determination of the clock-rates: and it was determined therefore to bring them into regular calculation.¹⁰¹

Thus only three years after he became its head, the effects of Greenwich labour division had lasting consequences on the way Airy ran the observatory.

A strikingly similar pattern was already evident under Airy’s former position as head of the University Observatory in Cambridge from 1828 to 1835. Despite the fact that a ‘constant difference’ between Airy and his assistant appeared very soon, this finding was obviously not taken into account in determining the rate of the clocks.¹⁰² The finding itself proves that Airy was well aware of the possibility of differences between observers. Hence the sharpest contrast between Pond and Airy was that once he realized its full importance the latter handled the issue at stake in a far more radical fashion. Perhaps as a silent answer to Bessel’s objections, Pond had made a first step towards reforming the observatories’ business by insisting that the observations be initialled. Yet a more profound and explicit reaction to the problems that might be caused by mixing observations can be seen no earlier than in the supplement to the observations for the year 1830, printed in 1833. Here, Pond inserted the following information:

During the first five months of 1830, the observations of the Transit were chiefly made by Mr. Taylor and Mr. T. G. Taylor, Jun., and for the remainder of the year by Messrs. Taylor and Simms. In reducing the observations, a constant difference of about $0^s.3$ was found to exist between these two observers, and the observations signed F. S., both in 1830 and 1831, have been diminished by that quantity, to render them accordant with those signed T.¹⁰³

This notice, along with that almost simultaneously published on the observations for the year 1832 in which Pond introduced the notion ‘personal equation’, prove that the menace potentially connected to the Greenwich system of labour division had been recognized years before regular control began. However, Pond did not return to this issue until the end of his term. Then, when matters in Greenwich eventually took a new direction under Airy’s command, Airy only made up for what his predecessor had failed

100 *Astronomical Observations in the Year 1836*, op. cit. (98), p. xxi.

101 *Astronomical Observations Made at the Royal Observatory at Greenwich in the Year 1838*, London, 1840, p. xiii.

102 G. B. Airy, ‘Preface’, in *Astronomical Observations Made at the Observatory of Cambridge*, Vol. 2, 1829, Cambridge, 1830, p. ii.

103 *Astronomical Observations Made at the Royal Observatory at Greenwich for the Year 1830, Part 5: Supplement*, London, 1833, p. iv.

to do. A final argument for this claim might be that the measures ultimately taken, with only slight revision, directly followed the route laid out in Pond's notice regarding the determination of the personal equation.¹⁰⁴

Airy's place in this story is not that of a man who held Bessel's findings in high esteem from the very start. His contribution to the whole issue is quite different: he, or more precisely his instructions for the regular determination of the personal equation, completely changed the presence of Bessel's findings in astronomical discourse. Of course, constant differences between observers had already become an issue of direct control in the context of longitude determinations which in the majority of cases were calculated with absolute measurements of time. Making the necessary measurements could lead to bursts of activity, as for example in the report on the great 1833 Russian Chronometer Expedition into the Baltic Sea, which listed figures for a 'constant difference' between no less than thirty-seven pairs of observers. But such efforts were often fleeting.¹⁰⁵ When each enterprise had ended, attention to the phenomenon also faded away. Yet bound up with the routine of the 'personal equation', such differences between observers became the subject of ongoing, visible and thereby retrievable attention. In other words, they became the subject of a 'hot tradition' that explicitly addressed such differences in printed communications. Starting with the 1840 observations, each volume of the annual *Astronomical Observations* contained current figures for the personal equation of nearly all regular observers. Together with the expansion of the observatory's staff, this led to a continuous increase of determinations over the subsequent decades. Observations for 1850, for example, provide the results of twenty-three determinations between nine observers and take up ten printed pages.¹⁰⁶ When electromagnetic registering apparatuses were introduced as an integral part of Royal Observatory measurement in 1854,¹⁰⁷ they had no influence on the already established measures of control. As it soon turned out, the new device did not make the differences disappear.¹⁰⁸ And even more remarkable is the fact that the determinations continued even after the 'impersonal' or 'travelling wire micrometer' had become the standard technique for observing transits in Greenwich in 1915.¹⁰⁹ Although possible differences were now minimized to insignificant levels, figures for the personal equation were still published (but no longer applied

104 *Astronomical Observations Made at the Royal Observatory at Greenwich in the Year 1838*, London, 1840, pp. xiii ff.

105 See in detail Hoffmann, op. cit. (81), 249–56.

106 *Astronomical and Magnetical and Meteorological Observations Made at the Royal Observatory at Greenwich in the Year 1850*, London, 1852, pp. xxvi–xxxiii.

107 *Astronomical and Magnetical and Meteorological Observations Made at the Royal Observatory at Greenwich in the Year 1854*, London, 1856, pp. vi ff. See also 'Description of the galvanic chronographic apparatus of the Royal Observatory, Greenwich', *Astronomical and Magnetical and Meteorological Observations made at the Royal Observatory at Greenwich in the Year 1856*, London, 1858, Appendix.

108 E. Dunkin, 'Comparison of the probable error of a transit of a star observed with the transit-circle by the "eye and ear" and chronographic methods', *Monthly Notices of the Royal Astronomical Society* (1859/60), 20, 86–8; and E. Dunkin, 'On the probable error of a meridional transit-observation by the "eye and ear" and chronographic methods', *Monthly Notices of the Royal Astronomical Society* (1863/64), 24, 152–9.

109 W. M. Witchell, 'The story of the Greenwich transit circle', *Occasional Notes of the Royal Astronomical Society* (1952), 2, 20–33, 29.

to the data) until the beginning of the 1930s.¹¹⁰ Thus the routine of control outlasted the ‘active life’ of the phenomenon.

The observer as apparatus: remembering Bessel’s findings

From a contemporary point of view, the particulars of the Greenwich Observatory turn out to be the condition for a quite effective memory mechanism, which with every determination of the personal equation ensured the continued presence of Bessel’s findings. As an enduring problem the ‘constant difference’ won a place of its own in the knowledge of practical astronomy and gave birth to the personal equation as a particular means of control. In time, this means gained an individual space for itself in printed astronomical observations, served as a motivation for technological innovation and, above all, became part of a long-lasting routine. However, along with the transformation into an issue of control and debate, the profound alterations in the concept of the observer that accompanied Bessel’s initial findings were partly distorted and obscured. In characterizing the main result of his inquiry as the identification of a kind of ‘involuntary’ phenomenon, Bessel summarized what was really surprising for him (and not only for him) in the outcome of the comparisons. The discovery that the activity of observation partly escaped wilful control created a completely new situation. From now on, one had to consider, as a fundamental condition of astronomical observation, the senses in general, not some mere shortcomings that could be overcome with the help of instruments. In Kantian terms the required interaction of the eye and the ear in observing transits turned out in some sense to be a ‘form of intuition’. Just this status warranted placing the observing observer alongside the instruments of observation. In contrast to the eighteenth-century concept of the instrument, for Bessel and his contemporaries an instrument’s error was no longer a sign of its imperfection but rather the unavoidable characteristic of its construction and functioning. His oft-quoted dictum, that every instrument ‘is made two times’, the first time in ‘the workshop of the instrument-maker’ and the second time by the astronomer ‘through the registers of the necessary corrections’,¹¹¹ conceded what, according to John Herschel, was completely normal:

With regard to errors of adjustment and workmanship, not only the possibility, but the certainty, of their existence, in every imaginable form, in all instruments, must be contemplated. Human hands or machines never formed a circle, drew a straight line, or erected a perpendicular, nor ever placed an instrument in perfect adjustment, unless accidentally; and then only during an instant of time.¹¹²

¹¹⁰ *Observations Made at the Royal Observatory Greenwich in the Year 1930 in Astronomy, Magnetism and Meteorology*, London, 1932, 13.

¹¹¹ F. W. Bessel, ‘Ueber die Verbindung der astronomischen Beobachtungen mit der Astronomie’ (1840), in *idem*, *Populäre Vorlesungen über wissenschaftliche Gegenstände* (ed. H.-C. Schumacher), Hamburg, 1848, 408–57, 432.

¹¹² J. F. W. Herschel, *A Treatise on Astronomy*, London, 1833, 69.

‘Involuntary’ errors of the observer perfectly match this picture of errors as indicators of the instrument’s materiality (its construction and its function), which could be analysed but never suppressed by human effort. In a letter to Struve, Bessel assumed that the whole trouble might be connected to the ‘propagation of the seen and heard in the brain’.¹¹³ Of course this was a highly speculative idea,¹¹⁴ but one that clearly denotes how error was understood as an intrinsic feature of the observer’s physical circumstances. As a consequence, astronomers handled differences between observers, once they had recovered from their surprise, as just one more constant error resulting from the material conditions of observing. They either arranged their observational work appropriately or they included figures for the differences in ‘the registers of the necessary corrections’. In the long run, the similar handling of observers’ and instruments’ errors resulted in the classification of the observer as one astronomical instrument among others. In 1855 the new director of the Paris Observatory, Urbain-Jean-Joseph Leverrier, took it simply as a matter of course that ‘the human organism, seen as an observational apparatus, has its own more or less regular and constant errors just like a divided circle, a pendulum clock, or a transit instrument’.¹¹⁵

As one more entry in the long list of instrumental errors, astronomers saw constant differences between observers as a feature of the observer’s body in the very same manner that, for example, the rate of a clock intrinsically characterized the clock’s function. When Leverrier claimed that the determined disagreements ‘are due to certain physiological particularities, to certain effects [*affections*] of the nervous apparatus which coordinate our movements or our impressions’,¹¹⁶ his comment matched Bessel’s attribution of the phenomenon to brain processes. Lorraine Daston and Peter Galison’s suggestion in their 1992 paper ‘The image of objectivity’ that the determination of the personal equation was understood as a means for compensating for fallacies of the observer *in personae* – that is to say, of their character, personal habits and temperament – therefore seems rather problematic.¹¹⁷ Their long quotation from Walter Maunder’s history of Greenwich Observatory, which might substantiate Daston and Galison’s claim, offers a rather isolated perspective on this issue and is taken from the end of the nineteenth century. Of course, the physiological framing of differences between observers had been challenged in the 1860s and 1870s, but it was Maunder’s acknowledged hero Airy who early on linked the phenomenon

113 F. W. Bessel to F. G. W. Struve, 11 October 1823, BP, Letter-Vol. 14, 202.

114 In his 1823 report Bessel less clearly put forward the idea that the phenomenon resulted from the different times needed for the coordination of impressions on the eye and the ear (see *Astronomische Beobachtungen*, op. cit. (3), 8. Abtheilung, VII).

115 ‘... *l’organisme humain, considéré comme un appareil d’observation, a lui-même ses erreurs plus ou moins régulières et constantes, tout comme un cercle divisé, une pendule ou une lunette méridienne.*’ U.-J.-J. Leverrier, ‘Rapport sur l’Observatoire impérial de Paris et projet d’organisation’, *Annales de l’Observatoire impérial de Paris* (1855), 1, 1–68, 16.

116 ‘... *elles sont dues à certaines particularités physiologiques, à certaines affections de l’appareil nerveux qui sert à coordonner nos mouvements ou nos impressions.*’ Leverrier, op. cit. (115), 16.

117 L. Daston and P. Galison, ‘The image of objectivity’, *Representations* (1992), 40, 81–128, 104.

to nervous processes. Discussing the advantages of the electromagnetic recording of transits originally developed in the United States,¹¹⁸ he summarized in December 1849 that the

practicability of this method of recording observations being fully established, it then becomes an important question for the observing astronomer, whether this method is or is not more accurate than the usual method of observing by the combination of eye and ear. The question is, really, whether the connexion between the nerves of the eye and of the finger is or is not closer than that between the nerves of the eye and of the ear: it is purely a physiological question, which can be settled only by experience.¹¹⁹

The way in which Airy here settled the issue between the old and new methods also resolved the issue of the differences between observers as ‘purely a physiological question’.¹²⁰ This did not prevent continuing determinations of the personal equation, together with the recording devices, from having additional effects on the attitudes and efficiency of the observatories’ staff. Airy’s discussion of registration techniques implicitly challenges a second and more general claim of Daston and Galison’s paper. According to their argument, the scientist’s strong tendency towards methods of inscription in the course of the nineteenth century was mainly propelled by a prior change in the concept of scientific objectivity: ‘Interpretation, selectivity, artistry, and judgment itself all came to appear as subjective temptations requiring mechanical or procedural safeguards.’¹²¹ If this were so one might expect the introduction of registration apparatus in transit observations to be directly motivated by fundamental distrust of the observer’s personal behaviour. Yet apart from the fact that the observer still had the crucial function of marking the exact moment of the star’s transit by hitting a key, the promise of the new method was primarily connected to physiological considerations.

From the astronomers’ point of view, the mechanization of the workplace was not justified by the observer’s intellectual fallacies and potential misjudgements, but rather by bodily conditions. This above all explains how Bessel’s findings ultimately acquired the full status of a scientific challenge. Supporting or attacking the new method now immediately led back to the question of why differences between observers occurred, a claim which was nicely reinforced by the flood of publications on this issue that commenced soon after electromagnetic registration began to replace the

118 E. Loomis, *The Recent Progress of Astronomy; Especially in the United States*, New York, 1851, 212–36.

119 G. B. Airy, ‘On the method of observing and recording transits, lately introduced in America; and on some other connected subjects’, *Monthly Notices of the Royal Astronomical Society* (1849/50), 10, 26–34, 29.

120 Two of the most eminent physiologists at that time, Johannes Müller and his student Hermann Helmholtz, both rejected the idea that such differences pointed to physiological causes, admitting that they should be understood in terms of a (psychological) problem of attention. See J. Müller, *Handbuch der Physiologie des Menschen für Vorlesungen*, 2 vols, Coblenz, 1833–40, i, Part 1, 654 ff; H. Helmholtz, ‘Messungen über den zeitlichen Verlauf der Zuckung animalischer Muskeln und die Fortpflanzungsgeschwindigkeit der Reizung in den Nerven’, *Archiv für Anatomie, Physiologie und wissenschaftliche Medizin* (1850), 276–364, 331.

121 Daston and Galison, *op. cit.* (117), 98.

traditional method.¹²² The fierce debate between the Swiss astronomer Adolph Hirsch and the French astronomer Charles Wolf in the 1860s offers an interesting case.¹²³ Hirsch and Wolf's quarrel over the physiological or psychological interpretation of the constant differences between observers was not merely a quarrel between a strong supporter of electromagnetic transit recording and a strong supporter of the old eye-and-ear method. Furthermore, the debate also demonstrates that it was precisely people like Hirsch, who rejected any literally personal or wilful influence of the observer on the phenomenon at stake, who promoted mechanization. In Bessel's terms, mechanization was the concern of astronomers who believed in the 'involuntary' character of the resulting differences.

Relying on Simon Schaffer's 'Astronomers mark time', as did Daston and Galison, it can indeed appear plausible that all efforts to manage differences between observers nourished a kind of moral police. Schaffer characterizes the situation in Greenwich very accurately and on most points is right. But his fundamental claim, that 'the problem of personality was an aspect of human character, but it was *therefore* manageable by astronomical discipline',¹²⁴ gives a somewhat distorted summary of the meaning of the phenomenon to which Bessel had once alluded. 'Attention to discipline' was not the indispensable answer to Bessel's findings. It was rather a by-product of the Greenwich system of labour division, which in itself turned out to be the source of all the troubles. Thus 'disciplining' was not inevitable in practical astronomy, but rather a feature emerging from a particular regime of observing. To understand the measures taken in Greenwich, which rapidly proliferated to other observatories like those in Brussels (1840) and Paris (1843),¹²⁵ and in time to other observational tasks,¹²⁶ one must attend to how they were intrinsically forced by the style of observational work. One might debate the extent to which alternating observers in one and the same series of observations, and the resulting introduction and further career of the personal equation itself, were an unavoidable offshoot of the emergence of astronomical observatories as the first authentic sites of big science. Bessel's findings can clearly be considered a necessary but by no means sufficient precondition.

Tracking the history of the personal equation casts light upon fundamental changes in the conditions of scientific work over the course of the nineteenth century. It does not testify to any distrust in the human observer or allow the conclusion that the observer

122 For a detailed analysis see Canales, *op. cit.* (8).

123 J. Canales, 'Exit the frog, enter the human: physiology and experimental psychology in nineteenth-century astronomy', *BJHS* (2001), 34, 173–97, 189–93.

124 Schaffer, *op. cit.* (7), 125; original emphasis.

125 *Annales de l'Observatoire de Bruxelles*, Vol. 12, Brüssel, 1857: 'Observations faites à la lunette méridienne 1840–47', IV. For the start of the regular determination of the personal equation in Paris see Canales, *op. cit.* (8), 121–5.

126 See the discussion of such a case in solar astronomy of the 1910s in K. Hentschel, 'A breakdown of intersubjective measurement: the case of solar-rotation measurements in the early 20th century', *Studies in History and Philosophy of Modern Physics* (1998), 29, 473–507; and the control measures taken in scintillation counting in the 1920s discussed in J. Abele, 'Wachhund des Atomzeitalters'. *Geigerzähler in der Geschichte des Strahlenschutzes*, München, 2002, 53–82.

was framed in wholly different terms to his instruments. On the contrary, a return to the emergence of a phenomenon called ‘constant difference’ reminds us that the observer ended up aligned with his instruments. What in Maskelyne’s time had been understood as an important source of trust and distrust emerged in Bessel’s inquiry as but one source of constant error among many others.