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A BRIEF HISTORY OF ERROR

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Abstract: Observational errors are inevitable in astronomy, and statements of results are not complete without some estimate of the uncertainties involved. While we always strive to reduce those uncertainties, we know that some will remain. There have been times in the history of science when errors have masked second-order effects and actually assisted in the process of scientific discovery.

Key words: Astronomical research, observational error.

1 INTRODUCTION

We are all familiar with observational errors in astronomy, and experimental errors in the laboratory sciences. We know that we cannot eliminate them entirely: Heisenberg's uncertainty principle assures us of that, and in practice the errors are much larger than that principle would predict. Obviously, we strive to reduce errors as far as possible but, at the end of an investigation, we try to estimate the uncertainty that those errors will inevitably produce in the final result. Indeed, a result without some indication of its uncertainty is now considered incomplete, but this was not always so in the history of science.

Observational errors can be of two kinds: systematic and accidental. Both occur in astronomy. The story of the unfortunate assistant of the then Astronomer Royal, Nevil Maskelyne (1732–1811; Figure 1) is well known. The poor man was dismissed because his observations of the transits of stars were consistently about half a second 'late'. Had the customs of the times permitted, the assistant could justifiably have retorted that Maskelyne's determinations were half a second early! Thus, the concept of personal error came eventually to be recognized. It is still with us. In the large cooperative programmes that used to be undertaken at the Dominion Astrophysical Observatory in Victoria, B.C., and which involved several people in the measurement of spectrograms, care was always taken that some of the spectrograms were measured by everyone concerned so that personal errors could be checked and evaluated.

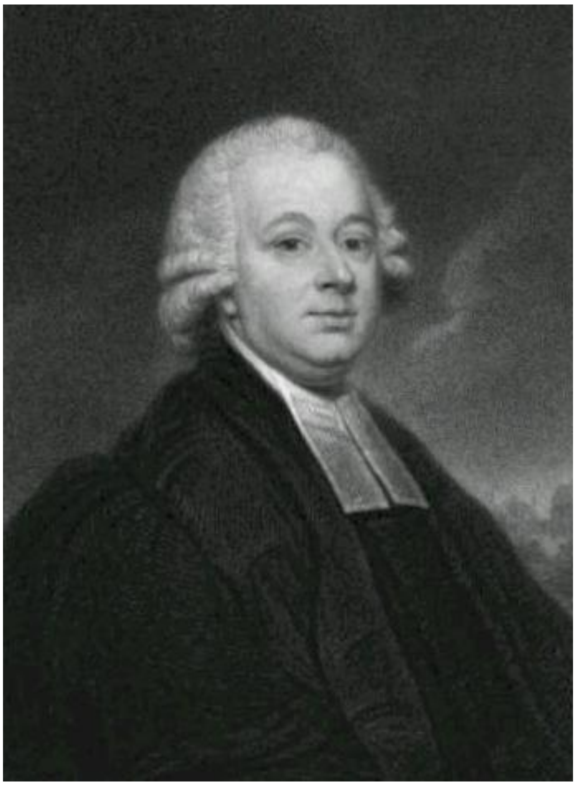

Figure 1: A portrait of the Reverend Nevil Maskelyne by Edward Scriven in 1836 (en.wikipedia.org).

While it is important to be aware of the existence of personal and other forms of systematic error, my chief concern in this paper will be with accidental errors. I will consider the work of Aristarchus of Samos, Tycho Brahe and Kepler, and Robert Boyle, and then go on to discuss some modern work in astronomy in which the evaluation of error is important.

2 ARISTARCHUS OF SAMOS

It is well known that Aristarchus (circa 310–230 BCE) devised a method for determining the relative sizes and distances of the Sun and the Moon and that by combining this with observations made during a total lunar eclipse he could, in principle, derive the absolute distances of the two bodies. The still extant text has been translated and annotated by Heath (1913).

Aristarchus' method was perfectly sound in principle, and his treatment of the geometry of lunar eclipses was superb. In practice, however, the method was flawed because it depended on assessing when the Moon was exacty half full. This is difficult to do; Neugebauer, as quoted by Mickelson (2007), claimed that it is difficult to determine the time of quadrature to within a day or two. My own attempts suggest one can do considerably better than that, but the ratio of the distances of Sun and Moon from the Earth is so sensitive that even an error of an hour or two can make a great difference to the derived result. When the Moon is exactly at quadrature, then the angle Sun-Moon-Earth

(SME) is 90° and the angle Moon-Earth-Sun (MES) can be measured, at least in principle. The ratio of the distance of the Sun to that of the Moon is the secant of MES. Unfortunately, since MES is also nearly 90°, the secant is large and varies rapidly as the angle changes, and the angle itself is difficult to measure. This misled Aristarchus into grossly underestimating the ratio of the two distances, an example of how an apparently small observational error can have very large effects. He found that the Sun was between 18 and 20 times as far away as the Moon. At first sight, this might seem like an attempt, however inadequate, to estimate the errors of observation. Aristarchus, however, assumed that the angle MES was exactly 87°, presumably on the basis of an attempted measurement, and the range of values he gives for the ratio of the distances of the Sun and Moon arises solely from approximations he was forced to make to the ratio of two lengths in his geometrical construction. He was clearly a superb geometer, and must have been well aware how sensitive his result was to any errors of observation, yet he assumed that his determination of the angle MES was correct. For all that, Aristarchus' result was a great advance on Anaxagoras' (circa 500–428 BCE) conclusion that the Sun was a fiery stone that was bigger than the Pelopennesos, still more on a culture that could believe that the Sun was close enough to scorch the Earth when the apprentice charioteer, Phaeton, took over the reins from the Sun-god Helios!

3 TYCHO BRAHE AND KEPLER

Before Tycho Brahe (1546–1601; Figure 2), the positions of planets were not measured systematically and the possibility that there might be errors in the measurements that were made was not taken into account. As is well known, Tycho took great care not only over the observations themselves but in the preparation of the graduated circles with which the observations were to be made. He achieved a precision of about two arcminutes in planetary measures (he could do better on stars—see Thoren, 1990), as did Ulugh Beg (1394–1449) before him, and Jai Singh (1688–1743) shortly after him. This is just about the limit of what can be achieved observationally without the aid of the telescope. The fund of data that Tycho obtained enabled his assistant and successor, Johannes Kepler (1571–1630; Figure 3) to demonstrate, after many false starts, that the orbits of the planets were elliptical. We know that Kepler rejected one false start because the orbit he obtained for Mars deviated systematically in one part by 8 arcminutes from Tycho's observations. Kepler knew that Tycho would not have made so great an error.

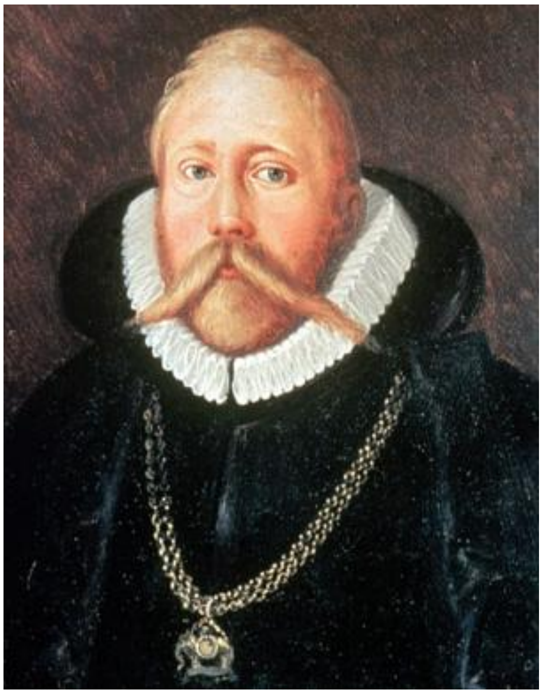

Figure 2: A portrait of Tycho Brahe by Eduard Ender (en.wikipedia.org).

What might have happened if Tycho's observations had been more precise? Suppose that he had been able to detect the deviations from simple ellipses caused by the mutual perturbations of the planets. Kepler, of course, was working before Newton and did not know that the inverse-square law of gravity would explain the three laws of planetary motion that he had discovered. Still less did he understand the possibility of the planets perturbing each other. Would he have been content to accept elliptical orbits as a first approximation with some un-

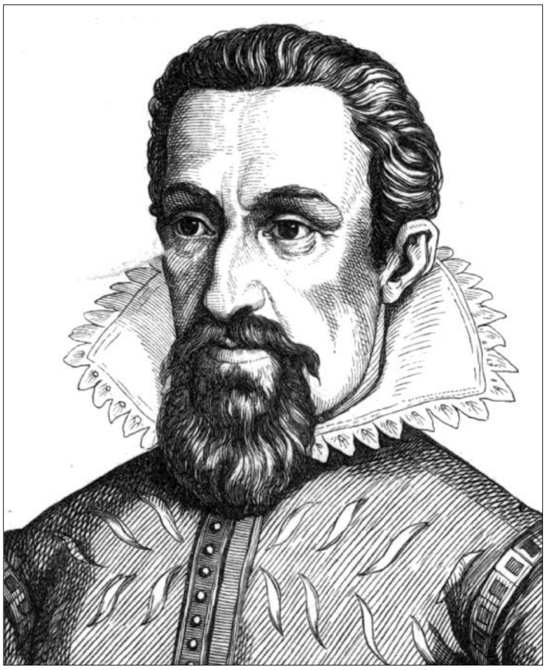

Figure 3: Johannes Kepler (thescienceclassroom.Wikispaces.com).

known force causing deviations from the ellipse, or would he have insisted on finding the 'true' orbit that would satisfy these supposed extremely precise observations obtained by Tycho? One suggestion (Kurth, 1959: 42–43) is that he might have arrived at a model for the Solar System rather like the early quantum theory of the atom. Kurth supposed that Kepler would have regarded the mean orbital motions of the planets as fundamental and, since his Third Law related these to the mean distances of the planets, which had only discrete values, he might have concluded that only discrete values of the mean motions were permitted in the Solar System. The satellite system of Jupiter and the later-discovered one of Saturn might seem to confirm this notion. Kurth argued that a completely consistent theory of the planetary motions could be constructed along these lines

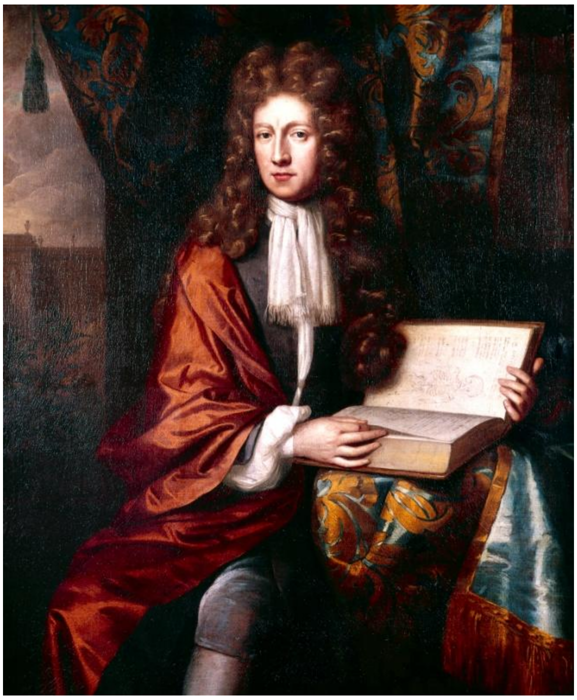

Figure 4: The Honorable Robert Boyle (http://wellcome images.org/).

and remarked that, had Kepler proceeded in this manner, "… physical science and philosophy would have developed in a completely different way." (ibid.). Whether or not one finds Kurth's argument convincing, it does draw attention to the fact that the limited precision of Tycho's observations is what enabled Kepler to derive elliptical orbits, which, in turn, enabled Newton to show that the one law of gravity would explain the falling of a stone to Earth and the motions of the Moon and planets.

4 ROBERT BOYLE

I learned in my schooldays that Robert Boyle (1627–1691; Figure 4) was the uncle of the Earl of Cork and the Father of Chemistry. He was not an astronomer, but his most famous discovery, that of the relation between the pressure and volume of a given mass of gas, combined with the later inclusion of temperature in the relationship, has proved to be of the utmost importance in the study of stellar structure and evolution and certainly deserves a place in the history of astronomy.

Boyle's law, of course, is that for a given mass of gas at a constant temperature, the pressure, P, and the volume, V, obey the relation

$$PV = \text{constant} \quad (1)$$

Boyle was well aware that his values of the product PV were not exactly constant and he wrote:

> Now although we deny not, but that in our table some particulars do not so exactly answer to what our formerly mentioned hypothesis might perchance invite the reader to expect; yet the variations are not so considerable, but that they may probably enough be ascribed to some such want of exactness as in such nice experiments is scarce avoidable. (Boyle, 1662: 159).

Here is a clear recognition that the results have been affected by experimental errors, a recognition that is repeated a few pages further on (Boyle, 1662: 162) together with a suggestion as to the cause of the error:

> In the meantime (to return to our last-mentioned experiments) besides that so little variation may be in great part imputed to the difficulty of making experiments of this nature exactly, and perhaps a good part of it to something of inequality in the cavity of the pipe, or even in the thickness of the glass …

This appears be one of the earliest discussions of experimental or observational errors in the literature of science. Once again, we are prompted to ask: what if Boyle had been able to make his apparatus more uniform and thus to reduce his experimental errors? If he had reduced those errors to a small enough value, then he would have become aware of real departures from the simple form of his law. It is doubtful, however, if, at that time, the van der Waals corrections could have been derived, or even formulated, and the insight given by the ideal form of the law might have been lost to science until well into the nineteenth century.

The lesson from both Kepler's work and Boyle's is that observational or experimental errors can be helpful in masking second-order effects and thus enabling scientists to concentrate their attention on the major factors at work in a given situation until they can develop the analytical tools needed to deal with the minor factors. Interestingly, a similar point was made by Airy (1850: 102) in an address to which I shall have occasion to refer later:

> In this, as in all other cases in natural philosophy, the more the accuracy of observations is increased, the greater becomes the complexity of the laws of nature which it is necessary to take into account; and the investigation, which at first was intended only for the purpose of correcting the numerical coefficients of a known theory, may lead to the discovery or verification of a subordinate theory of a totally different kind.

5 GAUSS

After Maskelyne's encounter with personal errors, the next step towards full recognition of the importance of observational errors came in the nineteenth century and, again, astronomers led the way. The discovery of the first minor planets early in that century created a need for a means of determining at least a preliminary orbit from a few observations in order that a newly-discovered planet might be recovered after its conjunction with the Sun. As is well known, Carl Friedrich Gauss (1777–1855; Figure 5) provided the answer (Gauss, 1809). Since six orbital elements have to be determined (the period, P, the major semi-axis, a, the orbital eccentricity, e, the inclination of the plane of the orbit to that of the sky, i, the longitude of periastron, ω, and the longitude of the ascending node, Ω), three observations, each giving a position and a time are just sufficient. Of course, if one proceeds with only three observations it is implicitly assumed that those observations are exact and the derived orbit may well be only an approximation to the true one—but a close enough approximation to serve the purpose of recovering the planet as it emerges from the glare of the Sun. Once sufficient observations have been obtained, then a more accurate orbit can be determined, but the orbital elements become over-determined and the question arises: what are their most accurate values given the inevitable errors of observation? Obviously, at best, only a few of the observations will lie exactly on the derived orbit.

Gauss provided the solution to this problem by showing that the best orbit was the one which made the sum of the squares of the residuals of the observations from the computed orbit a minimum. This applied to all problems in which it was required to find the best set of values of several variables to satisfy a given set of observations. In the special case of one variable, Gauss's solution reduced to the intuitively-obvious one of taking the arithmetic mean. Of course, he assumed a particular kind of distribution of the observational errors: the 'normal', or as we often say nowadays, the 'Gaussian' distribution—a point to which we shall return later. It became fashionable to supplement any numerical value with an estimate of its 'probable error' or 'mean error'. The quantity sought was equally likely to lie within the range of the probable error as outside it, while it was twice as likely to lie within the range of the mean error as outside it. I remember Erwin Finlay-Freundlich (1885–1964) once joking that British astronomers were more optimistic than their German colleagues, since the British quoted probable errors while the Germans quoted mean errors! In fact, the divide seems to be one of time rather than nationality. In the mid-nineteenth century, German astronomers often quoted probable errors, but perhaps they changed to mean errors more quickly than their British colleagues. Whichever value is preferred, it has precise meaning only if the observational errors do, in fact, follow a normal distribution.

Figure 5: A portrait of Carl Friedrich Gauss by C.A. Jensen in 1840 (en.wikipedia.org).

6 STELLAR PARALLAX

After Gauss had solved the problem of determining the orbits of the newly-discovered minor planets, the next major problem was the determination of stellar parallaxes. James Bradley (1693–1762) had given astronomers a pretty good idea of the size of the quantity they were looking for (about one arcsecond) and a number of claimed determinations were made in the subsequent years, although they failed to carry conviction.

As is well known, three astronomers succeeded in the period 1837–1840, namely F.W. Bessel (1784–1846; Figure 6), F.G.W. Struve (1793–1864; Figure 7) and T.J. Henderson (1798–1844). By that time, astronomers had had an opportunity to absorb Gauss's lessons on the theory of errors and all three of these men quoted probable errors for the parallaxes that they derived for 61 Cygni, Vega, and

Figure 6: A portrait of Friedrich Wilhelm Bessel by C.A. Jensen in 1839 (en.wikipedia.org).

α Centauri, respectively (see my discussion in Batten, 1988; 120–124). Some years ago, in conversation, Albert van Helden suggested to me that it was precisely because these three quoted probable errors that their determinations were accepted as convincing by their contemporaries.

An interesting commentary on that thought is the history of Struve's determination of the parallax of Vega. He published an initial value of

Figure 7: A portrait of Friedrich Georg Wilhelm Struve by C.A. Jensen (comons.wikimedia.org).

0.125 ± 0.055″ (Struve, 1837). Had this been accepted, it would have been the first successful determination of the parallax of a fixed star, but Struve himself regarded the probable error as too large. Shortly afterwards, Bessel (1838) published his parallax for the two components of 61 Cygni, giving the value of 0.3136 ± 0.0141″, and in the opinion of most of us became entitled to claim the priority to which Struve had come so close. Struve (1840) soon followed with a revised parallax for Vega of 0.2613 ± 0.0254″. He had much reduced the uncertainty of his result but, ironically, his first value for the parallax of Vega was considerably closer to the modern value (0.133″) than was his revised value. Perhaps we should be more aware that our estimates of uncertainty are themselves uncertain!

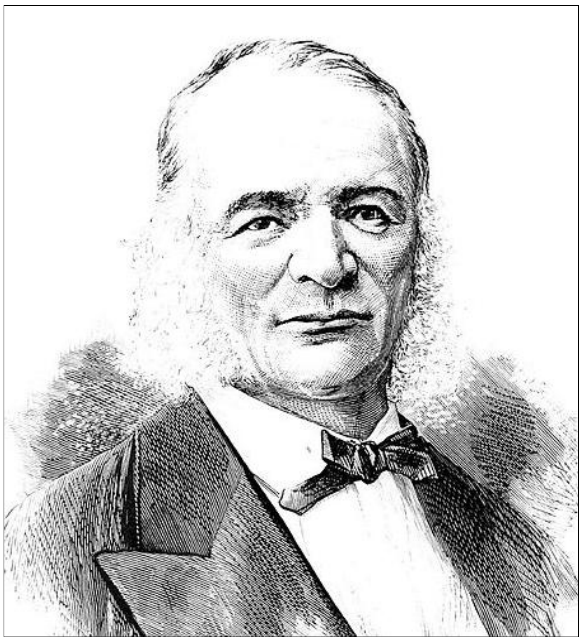

Figure 8: Otto Wilhelm Struve in 1879 (commons.wikimedia.org).

7 PRECESSION

One of the fundamental constants of celestial mechanics is the rate of precession of the equinoxes and Wilhelm Struve's astronomer son, Otto Wilhem Struve (1819–1905; Figure 8) won the Gold Medal of the Royal Astronomical Society in 1850 for his determination of the constant of precession and the solar motion with respect to nearby stars. The two quantities are linked observationally and are difficult to separate. G.B. Airy (1801–1892; Figure 9) was at that time the President of the Royal Astronomical Society, so it fell to him to deliver the customary address explaining the work that was so honoured and giving the reasons for awarding the medal. I have already quoted from this speech; in it Airy (1850: 108) singled out for special mention Otto Wilhem Struve's treatment of the uncertainties of his determinations:

> The two investigations which relate to the determination of the direction and magnitude of the solar movement are, in my opinion, very admirable; but the third, which exhibits the amount of uncertainty in the result depending on venial or probable errors of observation, is, in my judgment, even more valuable.

Modern astronomers undertaking a similar determination of these quantities would regard a discussion of the uncertainties as an essential part of the paper, but Airy's wording suggests that in 1850 such a discussion was still unusual. The derivation of the solar motion from the observations is so linked to that of precession that small observational errors, such as Airy thought could not be ruled out, can have a large effect on the final result for the apex of solar motion, as Otto Wilhem Struve himself pointed out. Just as with Aristarchus' method, the final result is very sensitive to the errors of observation.

8 DISCUSSION AND CONCLUDING REMARKS

Probable errors or mean errors are reliable guides to the actual uncertainties of derived quantities only if the residuals do indeed conform to a Gaussian distribution. I would like to discuss this in the context of the area of astronomy that I know best: the determination of orbital elements of spectroscopic binaries. Between 1970 and 1990 I obtained 52 spectrograms of the primary component of the wide visual binary 70 Ophiuchi, at a dispersion of 4 mm nm^{-1} (or 2.5 Å mm^{-1} in the older convention). I have excluded from consideration a number of spectrograms obtained at a lower dispersion, but included two values determined with a radial-velocity scanner. Figure 10 shows a histogram of the residuals from the velocity-curve eventually calculated from the orbital elements determined from these observations (Batten and Fletcher, 1991). The distribution approximates to a Gaussian one, but it is not clear that it is one. There is an asymmetry to the side of negative residuals and an apparent minimum just at the zero of the abscissae, where the maximum ought to be. These features are probably only statistical deviations inevitable when dealing with relatively small numbers of observations. I feel fairly confident that had I obtained two to four times the number of observations, the histogram would become much closer to a Gaussian distribution, but the point of this discussion is that observers often determine the orbital elements of a spectroscopic binary from many fewer observations than I have used for 70 Ophiuchi, and, even with over fifty observations, we still cannot be sure that the actual distribution of the residuals is Gaussian. The late D.M. Popper (1913–1999) many times commented that the true uncertainties of orbital elements were often appreciably greater than the published mean errors. This is probably true of many other areas of astronomy, and indeed of other sciences. Caution is necessary in dealing with all empirical results!

Figure 9: An undated print of Sir George Biddell Airy (commonswikipedia.org).

Another point of interest in the histogram is the outlying observation that shows a residual of +0.91 kms^{-1}. The next largest residual is no

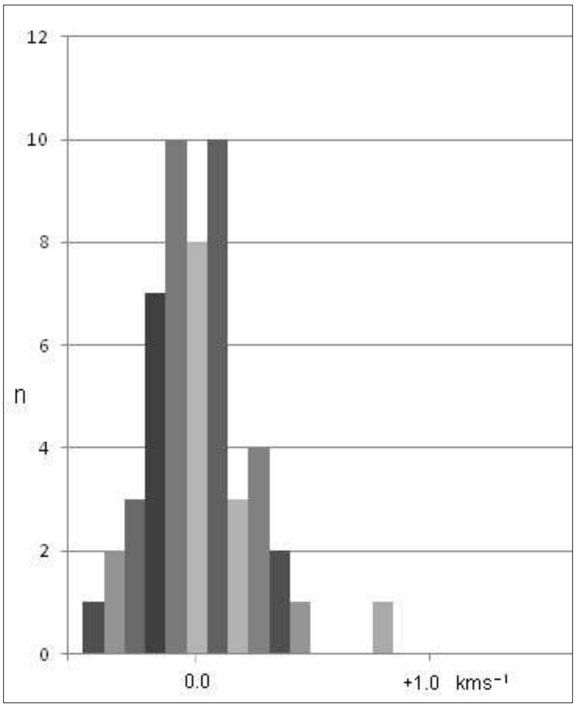

Figure 10: Numbers of residuals from the computed orbit of 70 Oph A. Each bar has a width of 0.1 kms^{-1} and the residuals run from –0.55 kms^{-1} to +0.46 kms^{-1}, plus an outlier (see text) at +0.91 kms^{-1}. The bar that contains 8 residuals is centred on 0.0 kms^{-1}.

more than –0.55 kms^{-1} and none of the others exceeds ±0.5 kms^{-1}. We all have it drilled into us early in our scientific education that a discordant observation should not be discarded *just* because it is discordant. I can assure readers that the observation was included in the solution! It is legitimate, however, to examine a discordant observation to see if there is some reason for the large residual that might permit one to reject the observation. In the present case, the spectrogram is one of the best in the whole series. The internal (line-to-line) scatter of the plate is one of the smallest and the spectrograph seems to have been in perfect focus. The observation has to be accepted, even though it has a residual at least twice nearly all the others in the series. In the all-too-few years that I knew R.M. Petrie (1906–1966) before his untimely death, he more than once told me that he had often found that one of the best observations of a spectroscopic binary stood off the computed velocity curve by more than any of the others. Here is an aspect of observational error that has been neglected and deserves further investigation. Who knows to what it might lead?

Of course, there are many situations in which we know that the distribution of residuals will depart from a Gaussian one, and methods of estimating the uncertainties of derived quantities in these situations have been devised, although it is beyond my competence to discuss them in detail. In the context of determining orbital elements, however, the least-squares criterion has become so entrenched that I doubt if it will be forsaken in the foreseeable future.

This discussion has led to the conclusion that not only is error inevitable in our search for the truth, but that it may even play a positive role in helping us to reach that goal. Perhaps this is one of the most important lessons that we scientists can teach to others.

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