Chapter 8

Dayton Miller and the 'cosmic' solution

8.1 Miller's (1933) paper

In 1933, Dayton Miller's paper, 'The Ether-Drift Experiments and the Determination of the Absolute Motion of the Earth' appeared in the *Reviews of Modern Physics*, one of the most prestigious journals in physics. The authors of the papers published there are, and have been, regarded as experts in their field[1]. So the occasion was of no small significance.

Miller's paper was quite extensive and included a review of the original Michelson–Morley experiments, as well as a review of the replications performed by Edward Morley and Miller; a critical analysis of interferometer experiments by others; all capped by a review of his own later work from 1925 and 1926, the results of which, or so he claimed, could be used to determine the 'absolute' motion of the Earth.

As part of his review of the 1887 Michelson–Morley experiment, Miller did a reanalysis of the data which yielded an increased ethereal velocity of between 8 and 8.8 km s^{-1}, and his review of the Morley-Miller replications noted that the results there indicated that the velocity was somewhere between 7 and 9 km s^{-1}. While these values were considerably less than the Earth's orbital velocity of 30 km s^{-1}, they (and the fringe shifts on which they were based) were not *per se* zero. Similarly, the Mt. Wilson replications of 1921 and 1924 yielded a value of 10 km s^{-1}—still small but not nothing. On the basis of these values, Miller concluded that:

> Throughout all these observations extending over a period of years, while the answers to the various questions have been 'no,' there has persisted a constant and consistent small effect which has not been explained. (Miller 1933, p 222)

[1] For example, a review article by Bethe and Bacher, known informally as the 'Bethe Bible', covered all of nuclear physics and was a standard reference and used as a student text into the 1950s (Bethe and Bacher 1936).

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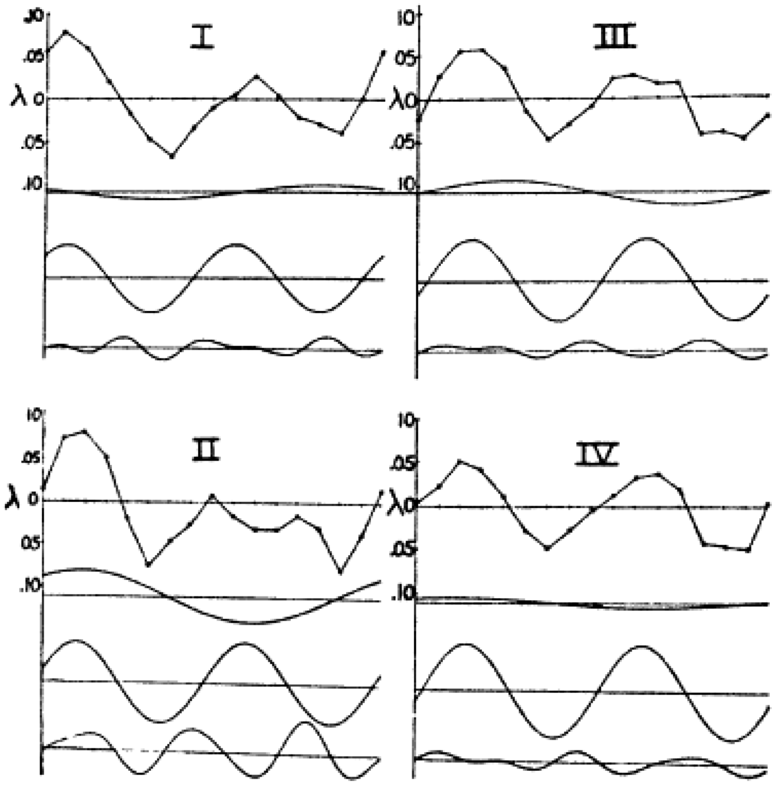

Figure 8.1. Harmonic analysis of ether drift observations. Source: Miller (1933).

Thus, while the reported fringe shifts may have translated into velocities that were unexpectedly small given the Earth's orbital velocity, they nevertheless did not constitute a null result. Or so it seemed to Miller's eyes. The question therefore was how were these non-null results to be 'explained'. As a true measure of ethereal velocity or just as a measure of systematic uncertainty?

Answering this question was greatly facilitated by an innovation introduced by Miller, namely, the use of a type of analogue computer that could be used to perform a Fourier analysis and thereby isolate the constitutive harmonics of the fringe data[2]. How this worked was that the data for each set of runs was first corrected by removing that part of the data that affected the steady drift of the fringes—which was assumed to be essentially linear for the purpose at hand, and due to causes not relevant for the determination of fringe shift. The remaining data was then averaged, the mean values connected, and the resulting 'curve' fed into the harmonic analyzer which then generated the first five harmonics (see figure 8.1).

As can be seen, the second harmonic dominates and registers that part of the fringe shift that varies according to the half-period of interferometer rotation. Which

[2] For a description and information regarding Miller's Henrici Harmonic Analyzer, see Fickinger (2006, pp 39–41).

means that it is in this harmonic that one would expect to see the effects of ethereal velocity or temperature gradients, or some combination of the two. Thus, when Miller described his earlier experimental results as being '*such as would be produced by a real ether-drift*' and '*as of an ether drift*' (Miller 1933, pp 217, 218, and 221, emphasis added), it was this second sinusoidal harmonic that he had in mind[3].

As indicated in figure 8.1, there was also a first harmonic, or full rotational period effect, but this was considerably smaller in amplitude. While there was some discussion about its importance, what it represented was not entirely clear[4]. So the main focus of attention was to identify the causes responsible for the amplitude and phase of the second harmonic.

Miller insisted that even when amplified the usual causal suspects—temperature gradients and mechanical deformation—had no significant influence. Thus, while temperature amplification did cause a 'steady drifting of the fringe system', there was no corresponding 'periodic displacement'. Fringe shift could be induced only if the interferometer arms were insulated 'in a very nonsymmetrical manner'. Therefore, as concluded by Miller:

> These experiments proved that under the conditions of actual observation, the periodic displacements could not possibly be produced by temperature effects. (Miller 1933, p 220)

Thus, the positive fringe shift data could not be explained away as being due to temperature gradients. Similarly, for mechanical distortion, or so Miller claimed. Assuming he was correct about this, the persistent non-null results meant that the Earth's relative velocity with respect to the ether was, on the basis of his more refined Mt. Wilson replications, not a result of systematic uncertainty but rather a real effect due to the ether, and hence *no less* than 10 km s^{-1}.

Skeptics, however, saw the fringe shift evidence as showing only that the relative velocity was *no more than* the reported values, and *possibly nothing* since the fringe shifts were likely just a measure of systematic uncertainty. How is such a difference of attitude with respect to systematic uncertainty to be resolved? On the skeptical side, Kennedy, Illingworth and Joos, as we have seen, constructed heavily insulted interferometers and generated a sequence of increasingly small fringe shifts and consequently increasingly lower limits for the ethereal velocity. The evident conclusion to be drawn from this sequence is that these diminishing limits provided a measure of the systematic uncertainty associated with the particular, and increasingly sophisticated, apparatus used. In any case, while there may have been

[3] Miller also used his analyzer to isolate the second harmonic of the original Michelson–Morley data which led to his reported increase in the detected velocity values to somewhere between 8 and 8.8 km s^{-1} (see Miller 1933, pp 205–6).

[4] The first harmonic was thought to be related to how the light beams were reflected off the mirrors of the interferometer and the nonorthogonal orientation required for visible fringes. But there was much disagreement and uncertainty about this. See, for example, the papers and discussion on the problem by Lorentz, Hedrick, and Miller from the 1927 Mt. Wilson conference (Michelson *et al* 1928, Miller 1933, pp 238–9).

a 'small effect', *it was neither constant nor consistent* as Miller claimed regarding his own replications.

While the results of this line of experimentation were bad news for Miller, he did have a plausible response, namely, that the experiments of Kennedy, Illingworth and Joos, as well as those of Piccard and Stahel, and Michelson, Pease and Pearson, showed only that the ether did not flow freely through objects but had a substantial aspect and because of that was blocked by the elaborate insulation employed in those experiments:

> In the [Kennedy, Illingworth and Joos] experiments, the interferometers have been enclosed in heavy, sealed metal housings and also have been located in basement rooms in the interior of heavy buildings and below the level of the ground; in the experiment of Piccard and Stahel, a metal vacuum chamber alone was used and in the experiment of Michelson, Pease and Pearson, the interferometer was in the constant temperature vault but did not have a vacuum case. If the question of an entrained ether is involved in the investigation, it would seem that such massive and opaque shielding is not justifiable. The experiment is designed to detect a very minute effect on the velocity of light, to be impressed upon the light through the ether itself, and it would seem to be essential that there should be the least possible obstruction between the free ether and the light path in the interferometer. (Miller 1933, p 240)

Hovering over this back and forth regarding systematic uncertainty was the question of what the underlying theory was that governed the operation of the interferometer. Here, as we have seen, the contenders were the Special Theory of Relativity and some form or other of an ether theory. But by 1933 the Special Theory of Relativity was in the ascendency and moreover had the advantage of simplifying the entire issue. Thus, the arguably null results of Kennedy, Illingworth and Joos, for example, could be seen as straightforward *instantiations* of the fundamental principles encapsulated by the Special Theory. Miller, by contrast, had no more than the possibility of a substantial ether with exactly the properties needed to accommodate his unexpectedly low velocity values. Ad hoc, to be sure, but not entirely hopeless.

In order to shore up his side of the debate, Miller developed an ingenious approach to justify the claim that his fringe shift data were not to be dismissed as no more than a reflection of systematic uncertainty. The cornerstone of Miller's counter argument was that:

> Previous to 1925, the Michelson–Morley experiment had always been applied to test a *specific hypothesis*. The only theory of the ether which had been put to the test is that of the absolutely stationary ether through which the Earth moves without in any way disturbing it. To this hypothesis the experiment gave a negative answer. The experiment was applied to test the question *only in connection with specific assumed motions of the Earth*, namely, the axial and

> orbital motions combined with a constant motion of the solar system towards the constellation Hercules with the velocity of about nineteen kilometers per second. The results of the experiments did not agree with these *presumed* motions. (Miller 1933, p 222, emphasis added)

So the idea then was to start by taking the fringe values at face value and then, and here's the ingenious part, to show that these values could be used to *reliably determine* the 'absolute motion' of the Earth[5]. What Miller had in mind here was co-opting a long tradition of determining the 'apex' of solar motion. While the idea may initially sound fantastic, Miller's proposal was just a variation of a traditional problem in spherical astronomy that dated back at least to Heschel[6]. This approach to defending the ether was suggested to Miller by Gustaf Strömberg, one of the astronomers at Mt. Wilson while Miller was conducting preliminary experiments there in 1925[7]. But Miller, to his credit, was able to execute the suggestion.

The experimental data required was a determination of the phase shifts as the interferometer was made to rotate during four epochs, in Miller's case April, August and September, 1925, and February, 1926. To then use the observed maximal values of these shifts to define vectors to that could be used to coherently locate the apex of the Earth's motion on the celestial sphere. Once that was located, one could then triangulate on the basis of the apex location and the Earth's relative motion with respect to the ether to obtain the 'absolute' motion of the solar system toward the apex. In short, the idea was to use the interferometer as a *more general instrument of discovery*, more like a *combination* of astrolabe and speedometer, rather than just as an ethereal speedometer.

Miller's discussion of a how all this was accomplished is quite extensive and takes nearly 17 pages of the 1933 paper. As demonstrated by Miller, the mathematics and methods of spherical astronomy showed that:

> The determination of the direction of the Earth's absolute motion is dependent only upon the direction in which the telescope [used to observe the fringe shift] points when the observed displacement of the fringes is a maximum; it is in no way dependent upon the amount of this displacement nor upon the adjustment of the fringes to any particular width or zero position. The actual velocity of the Earth's motion is determined by the amplitude of the periodic displacement, which is proportional to the square of the relative velocity of the Earth and the ether and to the length of the light path in the interferometer. (Miller 1933, p 226)

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[5] Miller first presented this proposal at a joint meeting of the American Physical Society and the American Association for the Advancement of Science in December 1925 (Lalli 2012, pp 178–80), (Miller 1926), and later at the 1927 Michelson–Morley conference (Michelson *et al* 1928, pp 352–67, Miller 1933).

[6] See Miller (1933, pp 224–5), Nassau and Morse (1927), Stromberg (1932a), Stromberg (1932b), Hoskins (1980) and Abad *et al* (2003).

[7] See Swenson (1972, pp 208–9) and Lalli (2012, p 178).

That's the mathematical theory. The question then was whether the actual fringe observations would yield a coherent location of the apex and determination of 'absolute velocity'. Miller's answer was yes:

> The close agreement between the calculated and observed apparent apexes would seem to be conclusive evidence of the validity of the solution of the ether-drift observations for the absolute motion of the Earth and also for the effect of the orbital motion of the Earth, which hitherto has not been demonstrated. (Miller 1933, p 237)

For this 'close agreement', see figure 8.2 which gives the locations on the celestial sphere of the theoretically calculated apex, as well as the apex as determined by the fringe data from each of the four epochs.

The 'absolute' velocity of the solar system toward the apex, can then be determined on the basis of the Earth's relative (orbital) motion of 10 km s^{-1} with respect to the ether and the location of the apex and the line and direction of the solar system's motion toward the apex. This is basically a simple matter of trigonometry, what Miller refers to as the 'triangle law', and yields as '*a first approximation*' an absolute velocity of 200 km s^{-1}. Further refinement led to a final value of 208 km s^{-1} (Miller 1933, p 233). Putting this all together yields the following composite of relative and 'absolute' motion:

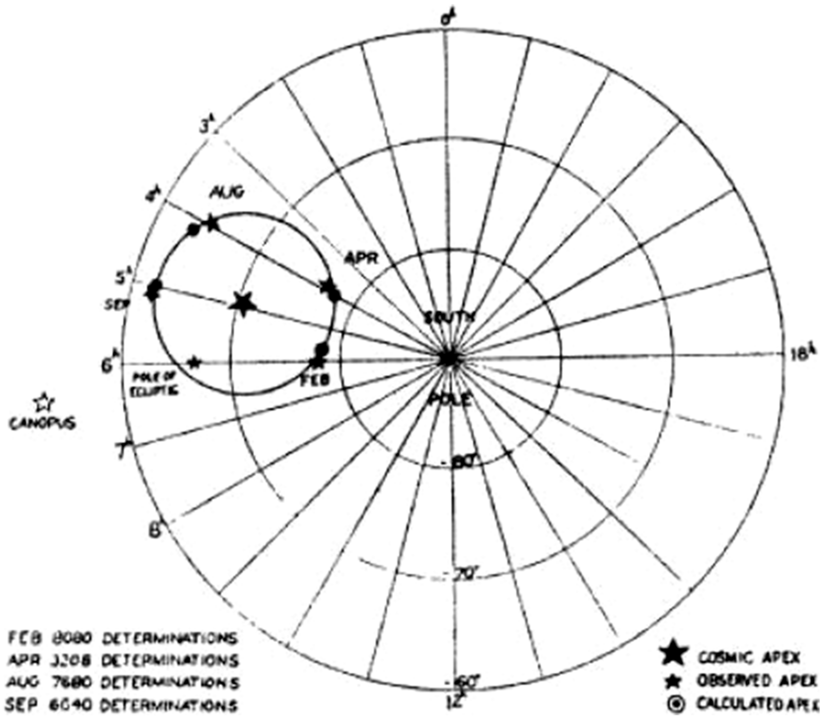

Figure 8.2. Observed and calculated apexes of the absolute motion of the solar system. Source: Miller (1933).

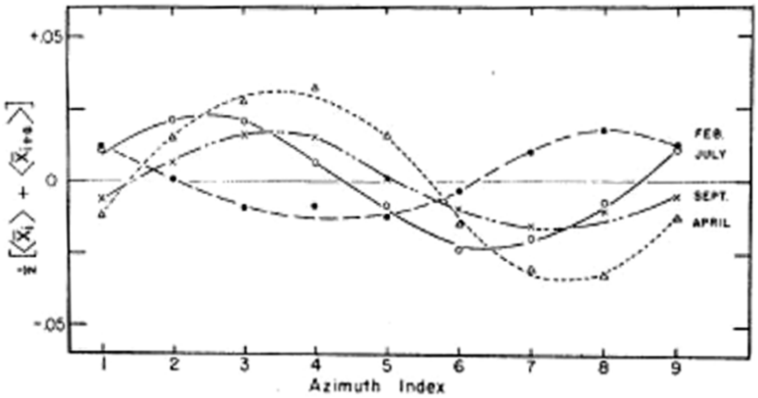

Figure 8.3. Second harmonics in the $\{<x_i^{\mathrm{bar}}> + <x_{i+8}^{\mathrm{bar}}>]$ for the four epochs of the Mt. Wilson data. Source: Shankland (1955).

> The fact that the Sun is moving towards the southern apex with a velocity of 208 kilometers per second and at the same time is apparently moving, with respect to the near-by stars, in the opposite direction towards the constellation Hercules with a velocity of 19 kilometers per second, indicates that the group of stars as a whole is moving towards the southern apex with a velocity of 227 kilometers per second. (Miller 1933, p 234)

As a final corollary, Miller notes that:

> … the results here obtained are not opposed to the results originally announced by Michelson and Morley in 1887; in reality *they are consistent with and confirm the earlier results*. With additional observations, the interpretation has been revised and extended. (Miller 1933, p 237, emphasis added)

In sum, Miller's argument is that his fringe shift data must be interpreted as yielding a *true ethereal effect* because otherwise that data could not have formed the basis for his *theoretically sound and coherent determination* of both the direction and velocity of the absolute motion of the solar system. But things were not quite that simple. There was some sleight of hand involved since the azimuth locations for the maximum amplitudes were displaced from what they should have been and Miller had to employ a sort of mean value to compensate[8]. In order to appreciate what's involved here, see figure 8.3 which shows the dramatic phase difference for the February 1926 results, as well as the displaced azimuths for all of the epochs. As Miller reported, the axes were displaced from the meridian 'for February 10° to the west of north; for April the displacement is 40° east; for August 10° east; and for September 55° east' (Miller 1933, p 235). None of this should have occurred given Miller's analysis.

[8] See Shankland *et al* (1955, p 172).

As Miller admitted, he had no explanation for the displaced azimuths and attributed it to some sort of rotation around an assumed underlying meridian that pointed to the celestial apex of the absolute motion of the solar system (Miller 1933, pp 234–5). But even putting that problem aside as a technical annoyance to be dealt with at a later time, Miller's determination of the direction and velocity of the absolute motion of the solar system required a substantial ether with properties suitably calibrated to yield Miller's values.

At the 1927 Mt. Wilson conference, Miller announced that his 1925 and 1926 data showed that there was 'a constant relative motion of the Earth and the ether at [the Mt. Wilson] observatory of 10 km s^{-1}' and 'a constant motion of the solar system in space, with a velocity of 200 km s^{-1}, or more, toward the apex in the constellation Draco' (Michelson *et al* 1928, p 361). Michelson, who was at the conference posed the following telling question about these velocity determinations:

> … why should the ether be dragged along by the Earth to the extent of 19/20 *and not some other fraction*? If this really occurs, then we must suppose that there will be a great difference between the drag on the surface of the Earth and a thousand miles above it. (p 394)

Reading between the lines, the point here is that one would expect such a number to be based on some fundamental property of the supposed ether and not just an ad hoc response to inconvenient experimental values. As a follow up to Michelson's question, Lorentz extensively commented, compressed here, that:

> If … we should be obliged by the facts to introduce a substantial ether again, it would, of course, be a very difficult problem to find out what its properties are, What would happen, for instance, in case matter should turn out to be only partly permeable for the ether, nobody can tell. For this reason the question about the ratio 19/20 could not well be raised before the properties of the ether were better known. We can even leave open the possibility that the motion of the ether may be irrotational …. A further possibility would be a compressible ether …. I tell you all this only to show how numerous the different possibilities for the theory are, if we are compelled by new experiments to go back to the notion of a substantial ether. (p 395)

Exploring such possibilities and developing a theory of an ether consistent with Miller's results was not, we expect, an appetizing prospect given the ascendant status of the Special Theory of Relativity. As a result, and unfortunately for Miller, by 1933 he had become for most scientists an earnest though essentially quixotic figure[9,10].

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[9] See Lalli (2012, 196–201).

[10] Although contemporary usage of 'quixotic' seems appropriate to describe Dayton Miller's quest for an ether, we believe that this is an injustice to Don Quixote. Anyone who has read the novel knows that the Don changed the world around him for the better. For a sympathetic view of Miller, see the appraisal by Thomas Roberts below in chapter 8.3 noting that 'he was a prisoner of his time'.

His ingenious determination of the ‘absolute motion’ of the solar system was not taken seriously. It had come too late. Thus, in at least one respect the fate of his 1925–1926 Mt. Wilson experiments was not unlike what happened to Thiegberger’s purportedly positive results for the presence of the Fifth Force. Excommunicated but without direct evidence as to what had gone wrong.

8.2 Shankland’s 1955 reanalysis of Dayton Miller’s data

> Einstein … ought to give me credit for knowing that temperature differences would affect the results … I am not so simple as to make no allowance for temperature[11].

In 1955, Robert S. Shankland and his collaborators decided to give Miller his day in court, albeit posthumously, and undertook an extensive analysis of Miller’s Mt. Wilson data from 1925 and 1926 as well as his argument for the cosmic implications of that data[12]. After giving a brief history of Miller’s interferometer experiments Shankland[13] proceeded to provide a statistical analysis of Miller’s data, after which he then moved on to a consideration of the ‘systematic local disturbances such as may be caused by mechanical effects or by nonuniform temperature distributions in the observation hut’ (Shankland 1955, p 172).

Up to now, both here and in the preceding chapter, we have focused our attention on systematic uncertainty and at best only mentioned statistical uncertainty in passing. Shankland’s analysis presents a good opportunity to rectify this omission since it reveals and illustrates at least some aspects of the connection between the two types of uncertainty. We shall have more to say about the connection in later chapters. But for now, Shankland’s analysis of Miller’s data is a good place to start.

The basic idea underlying Shankland’s application of the ‘standard analysis of variance’ is to view Miller’s readings of the fringe shifts as a subject to ‘personal factor’ where the presumed variations between Miller’s visual determinations and the actual value are subject to random error that will vary according to the normal (Gausian) probability density function. Miller’s data sheets for a run of 20 continuous rotations of the interferometer contained entries in 16 columns for the azimuth points of the interferometer (as measured from the viewing telescope), and 20 rows for each complete turn of the interferometer. Mirror adjustments were made at the beginning of each run in order to keep the fringe shifts centered for maximum visibility. After averaging all values, a linear correction was applied to compensate for the overall fringe shift which was ‘assumed to be steady or linear, throughout the time of one turn’ which was about 25 s (Miller 1933, p 169). Alternatively stated, Miller ‘applied a *linear correction* for fringe pattern drift so that the averages closed

[11] Miller quoted in ‘Goes to Disprove Einstein Wrong’, Cleveland Plain Dealer, 27 January 1926. For what caused Miller’s pique, see Lalli (2012, p 184).

[12] For the many revisions and extensive efforts involved in producing this analysis, and for Shankland’s motivating personal connections with Miller, see Lalli (2012, pp 201–11).

[13] We will use Shankland as shorthand for R S Shankland, S W McCuskey, F C Leone, and G Kuerte.

as a periodic function in the 360° rotation' (Shankland 1955, p 169, emphasis added). The final data (with mean values connected by straight lines) was then fed into the harmonic analyzer for Fourier decomposition.

While Miller's linear compensation for overall drift the system seems innocent enough, the consequences were otherwise, as will be shown later when we review a more recent 2006 analysis. But for now we'll proceed, as Shankland did, giving Miller's drift correction the presumption of innocence. Shankland's statistical analysis begins by letting $\bar{x}_i$ be the average of the ith column of azimuth points, and $\bar{x}$ the average of all 320 entries on the data sheet where the x_{ij} 'have already been freed of the linear drift in the same manner as Miller did it'. Assuming then that these 320 values may 'be considered random samples drawn from a normal population,' and in addition that the arrangement (of each data sheet) into columns and rows is a random one, the application of statistical theory yields a statistic F (as a function of $\bar{x}$ and the $\bar{x}_i$ values) of the variance of $\bar{x}$ (for each data sheet) where:

> … the probability of obtaining by pure sampling fluctuation an $F > 1.71$ is only 0.05; the probability of obtaining an $F > 2.21$ is only 0.01. These probabilities are accepted limits for rejecting the hypothesis that the array could have arisen by sampling fluctuations in a normally distributed population. *When a large number of data sheets are analyzed, one would expect only one out of twenty to exhibit an* $F > 1.71$ *if the population of which the sheets are samples consists only of randomly fluctuating data*. (Shankland 1955, p 169) (emphasis added)

But in fact, the ratio of F values that were greater than 1.71 was considerably more than one out of 20 (figure 8.4). This meant that:

> … the fluctuations in the column means cannot be attributed entirely to random effects, but that systematic effects are present to an appreciable degree. (Shankland 1955, p 170)

And there you have, succinctly stated, one way of establishing a *connection between statistical and systematic uncertainty*. Shankland also offered a 'second method' involving the use of 'an autocorrelation analysis' to show that 'the periodic effects observed by Miller cannot be accounted for entirely by random statistic fluctuations in the basic data' (Shankland 1955, p 170). But we shall postpone our review of this second method until we deal with a later 2006 analysis of Miller's data by Thomas Roberts.

Shankland employed yet another form of statistical analysis this time to determine:

> What part of the observed average amplitude for the second harmonic, deduced by harmonic analysis, may reasonably be attributed to random statistical fluctuation in the data? (Shankland 1955, p 171)

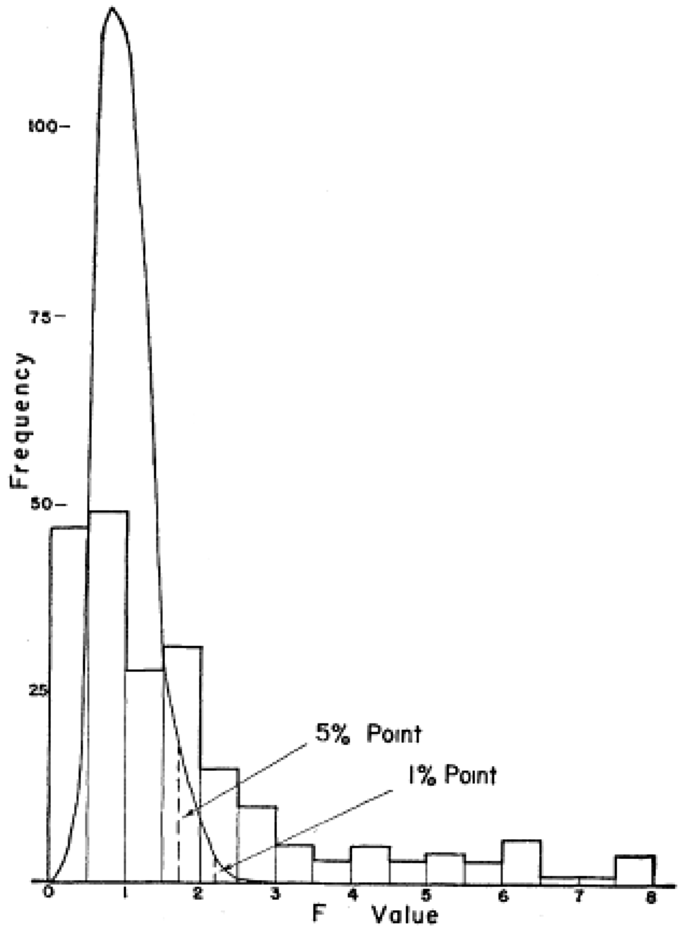

Figure 8.4. The distribution of *F*-values for 216 sets of Mt. Wilson data. The smooth theoretical curve is normalized so that the area under this curve is equal to the area of the histogram. Source: Shankland (1955).

Figure 8.5 displays (for a single data sheet) the observed fringe shift data (after removal of the assumed linear drift) for each of the 16 azimuth positions as well as the average value of the shift at each of the azimuth locations. The dominant second harmonic clearly makes its appearance when these average values are connected. Moreover, the distribution of the data points (above and below the average values) 'indicates an approximately normal population'. Assuming this to be so, one can view the statistical problem as a species of sampling error where the data points are construed as constituting a randomly drawn sample from the underlying real values (assumed to be normally distributed). Cutting to the chase, the conclusion to be drawn is that 'not more than 15% of the second-harmonic amplitude can be due to statistical causes' (Shankland 1955, p 171).

Shankland's statistical analyses suggest that the distinction between statistical and systematic uncertainty is not going to be entirely clear cut since systematic

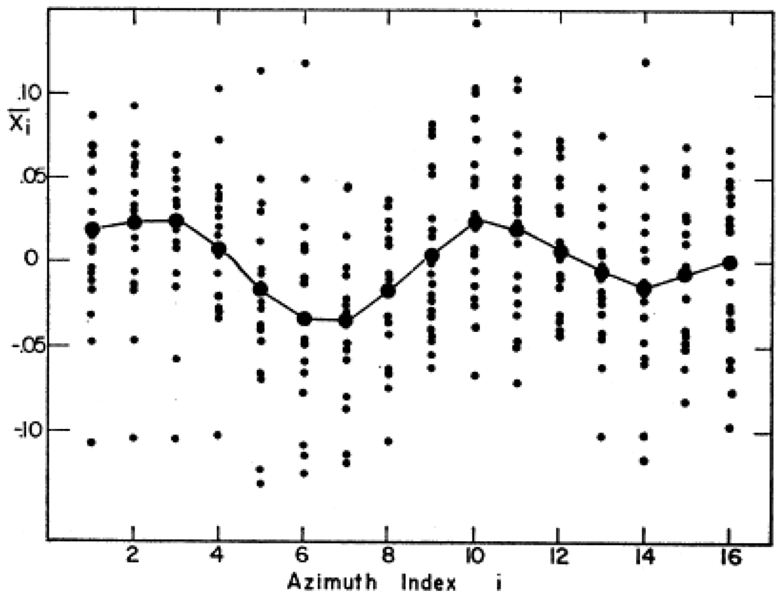

Figure 8.5. The individual column means are plotted as a function of azimuth position for the July, 1925 observational data. Large circles and the connecting curve show the second harmonic effect exhibited by the averages, $<x_i^{\mathrm{bar}}>$, due to ordering in azimuth. Units for the ordinate are fringes. Source: Shankland (1955).

uncertainty that can be dealt with by statistical means gets lumped into the category of statistical uncertainty while the residual uncertainty get categorized as systemic. We shall return to this distinction in later chapters.

After completing the statistical analyses, Shankland moved on to a glaring problem that we earlier drew attention to. Miller's data, taken at face value, indicate a phase incoherence among the epochs. If there were an ether, Miller's own calculations show that the fringe shifts from the different epochs should differ *only in amplitude and not in phase*. But as discussed above, the phase of shift data from February 1926 is badly out of synch with the phase similarity of the other epochs (Shankland 1955, p 172).

There was, in addition, the related problem of the displacement of the azimuth values for the maximum shift (for all the epochs), which Miller, as we have seen, expressly admitted that he had no explanation for. He attributed it to some sort of rotation around an assumed underlying vector that pointed to the celestial apex of the ('absolute') motion of the interferometer. These complications had serious consequences for the determination of the *apex* of the Earth's motion since Miller had to introduce ad hoc adjustments in order to achieve anything like a consistent determination of the apex. For the details, see Shankland (1955, p 172). As concluded by Shankland:

> It seems to us on the basis of this discussion that the internal consistency of the cosmic solution is not so great a surprise as it appears at first glance. It

certainly is not cogent enough to serve as a logical support of the claim that the half-period effect observed is a true aether-drift effect. (Shankland 1955, p 172)

Historically, that has been the final judgment on Miller's otherwise ingenious attempt to salvage some form of the ether theory. At this point, Shankland shifted his attention to the systematic uncertainties involved. There were two principal suspects: mechanical effects and nonuniform temperature distributions. The question was whether these could be held 'responsible for Miller's results'. In other words, what was it that had led him astray?

Shankland focused his attention on two sorts of mechanical effects: (1) sagging and bending of the beams, and (2) oscillations because of roll about the center of mass. The only relevant amplification of mechanical disturbance mentioned by Miller was that a mass of 282g placed on the end of one of the beams produced a shift of one fringe. Other than that, Shankand's analysis of the possibly disturbing mechanical effects was as a result restricted to the purely analytic. So, for example, the motion of the apparatus may be construed as analogous to that of a hanging top. But the resulting 'rapid oscillations cannot account for the true second-harmonic period' revealed in several of the data sheets (Shankland 1955, pp 173–4).

More interesting, and consistently at issue throughout the many replications of the Michelson–Morley experiment, were the temperature effects. One simplifying factor in trying to determine the effect of temperature variations is that the fringe shift and temperature gradients were both expected to show their influence in terms of the magnitude of the second harmonic of the Fourier transform. Both were directly competing, as it were, for control and occupancy of the second harmonic.

In order to get a handle on possible temperature effects, Miller conducted a number of tests in 1923 with variations of insulation and temperature gradients caused by heaters. Definite effects were noted, but were greatly reduced with thermal insulation. Among the most revealing of these tests are four sets of observations that given in table 8.1. In both the 'Controls' and the 'Heat' sets, thermal insulation was employed, but heaters were employed only in the 'Heat' set.

Table 8.1. Laboratory heating trials (units: fringes). Source: Shankland (1955).

Periodic	Controls		Heat	
Amplitudes	Set 17	Set 28	Set 18	Set 29
A_1	0.006	0.006	0.010	0.021
A_2	0.015	0.010	0.049	0.052
A_3	0.006	0.005	0.009	0.005
A_4	0.004	0.003	0.006	0.011
A_5	0.001	0.001	0.003	0.002

> Not surprisingly … the effect on the second harmonic A_2 is much the largest, as is to be expected on physical grounds. Furthermore, the phases of the second harmonic … have values consistent with the position of the heater. (Shankland 1955, pp 173–4)

Shankland went on to emphasize that,

> these tests reveals small but certain temperature effects, in contrast to Miller's statement that he had shown the absence of periodic effects caused by artificial heating when the light path was thermally insulated …. (Shankland 1955, p 174)

Unfortunately, it was not possible to proceed any further along these lines because the only temperature data available from Miller's 1925 and 1926 experiments were four wall thermometers. This was not enough to go on:

> In reality, however, the effects of temperature on the apparatus must have been very complex, being mixed contributions of changes in density of the air in the optical paths, angular deflection of the mirror supports, and thermal expansion of the steel frame, the latter effect introducing a long time lag. It is practically impossible to carry through calculations which would predict the over-all behavior of the interferometer due to temperature anomalies, since hardly any of the necessary data for such calculations exist. (Shankland 1955, p 175)

But that was not the end of Shankland's attempt to relate the effect of temperature variations on Miller's results. He went on to examine the 'local factors' that differed among Miller's many repetitions of the experiment. Particularly noteworthy were ten sets of observations made on the Case Cleveland campus between midnight and 5:00 A.M. on August 30, 1927. During this time, the wall temperatures were 'remarkably constant' to within 0.4 C, and the second harmonics were correspondingly almost identical in phase and amplitude:

> This behavior *persists throughout almost five hours* of sidereal time as the Earth makes nearly 1/4 of a revolution and would be *extremely unlikely* if the fringe shifts were due to any cosmic effect. On the contrary, it strongly supports our hypothesis that local temperature conditions are the dominant factor producing the observed second harmonics. (Shankland 1955, p 175, emphasis added)

This is a paradigmatic instance of separating out the *best available data* and focusing on what those data show as opposed to what's shown by lesser quality data[14]. Data from the April 2 and September 1925 runs at Mt. Wilson displayed

[14] Of course, there is some danger inherent in focusing attention on what one thinks is the best or most reliable data because of the risk of introducing personal bias or just plain mistake. A more serious example of the danger will be discussed below.

similar behavior and especially damning was the fact that despite the stable conditions 'the maxima of the second harmonic curves were definitely removed from the north point throughout the night':

> This behavior throughout nearly six hours of sidereal time *conclusively rules out cosmic effects*. (Shankland 1955, p 175, emphasis added)

And with that knockout blow, we end our review of Shankland's reanalysis and move on to a 2006 examination based on updated methods of signal extraction from a sea of noise.

8.3 Roberts' 2006 analysis of Dayton Miller's data

Before moving on to a recent analysis of Miller's data by Thomas Roberts, we want to briefly step back again in time and by so doing set the stage for Roberts' analysis. In 1934, Joos and Miller locked horns over the question of 'disturbances caused by local and temporal variations of temperature.' As noted by Joos:

> … if, assuming a length of the light path of 30 m, one calculates what difference in temperature of the two branches of the interferometer produces a displacement of 1/10 of a fringe (this is the order of magnitude observed). One gets the astonishing result that a difference of 1/500° is sufficient. The mere warmth of the body of the observer who, in Mr. Miller's experiments, stands near the interferometer can produce such an effect. (Joos 1934, p 114)

Miller made the reasonable response that:

> It should be borne in mind that the ether-drift observation … depends upon a regularly periodic variation in the position of the entire fringe system, and the period is twenty-five seconds throughout. The temperature would have to increase and decrease, with periodic regularity in each twenty-five seconds! to produce the results. Any irregular fluctuation will be eliminated in the long series of turns. (Miller 1934, p 114)

But Miller went further and also claimed that:

> The observer maintains a constant relation to the apparatus and if the warmth of the observer's body is effective, it would be a continual heating effect which produces a *continuous drift of the fringes, which is of no effect in the calculated results*. (Miller 1934, p 114, emphasis added)

It is this additional defense by Miller that, as we shall see, provides the key to understanding the role played by temperature gradients in the case of his interferometer experiments. More precisely, it was Miller's initial removal of what he took to be the data reflecting the 'continuous drift' that caused the systematic uncertainty to overwhelm the reliability of the presumably corrected data.

To further set the stage for Roberts' analysis, we return to Shankland's 'second method' for showing that 'the periodic effects observed by Miller cannot be accounted for entirely by random statistical fluctuations in the basic data' (Shankland 1955, p 170). What's significant about this method is that it was applied to the *raw data* from five data sheets, that is, to the data *before* Miller applied his 'compensation for shift' in order to remove what he took to be a 'steady or linear' shift. The five data sheets used were 'typical' insofar as the amplitudes of the second-order harmonic of the data *after* the compensation had been made were of 0.021, 0.045, 0.059, 0.082, and 0.123, and thus spanned 'nearly the entire range' of the second-order harmonics. The second method then was to subject the *raw data* to an 'autocorrelation' analysis. Unfortunately, no details were given as to the exact nature of this analysis other than a reference to a student's Master's thesis. In general, autocorrelation analyses constitute a class of statistically based methods of extracting a signal from a background of noise.

Now as noted above, Miller in his response to Joos asserted that the '*continuous drift of the fringes … is of no effect in the calculated results.*' But as shown by the autocorrelation analysis, this is not so. The results of this analysis are shown in table 8.2. As can be seen, the amplitudes for the 'uncorrected' second harmonic of sheets 15 and 23 (which had the smallest second-order amplitudes on Miller's analysis) showed a considerable reduction after being 'corrected for random effects'. With respect to data sheet 15, Miller's corrected value for the amplitude of the second harmonic *loses* 0.008 of a fringe to random effects as compared with the second harmonic based on Miller's data with the assumed linear drift removed. Similarly, though with smaller effect, for data sheet 23. Thus, as concluded by Shankland:

> It is again apparent that random statistical processes contribute considerably to the periodic effect when it is small but that the larger amplitudes are relatively unaffected and cannot be explained in this manner. (Shankland 1955, p 170)

Table 8.2. Summary of autocorrelation analysis. Source: Shankland (1955).

Sheet	Miller's Observed A_2	Correlogram: Uncorrected A_2	Correlogram: Corrected for random effects
15	0.021	0.032	0.013
23	0.045	0.054	0.043
79	0.059	0.062	0.060
75	0.082	0.079	0.078
42	0.123	0.129	0.126

Now while the loss of 0.008 of a fringe to random effects may seem a small technicality, it gets *elevated to a level of explanatory prominence* in Roberts' mathematically more up to date analysis. Roberts' aim was to give an explanation of what it was about Miller's data led him to conclude that there was *at least* a small but measurable relative motion with respect to the ether. Shankland and his collaborators 'did not fully resolve the issue, because they merely showed a loose correlation between signal and temperature drift, but did not give any argument or discussion of how that could generate such a remarkable result' (Roberts 2006, p 2).

The key to understanding what had gone wrong was Miller's apparently benign elimination of the interference shift data on the assumption that it was essentially linear in nature. Thus, Roberts' overall approach was to *reincorporate* the data eliminated by Miller's assumption that the steady drift was linear, and then to determine (using modern methods) what remained of Miller's second harmonic along with a determination of the relevant statistical uncertainty. Oddly, Roberts appears to have missed the fact that Shankland's autocorrelation analysis was based on a similar approach.

Roberts analysis proceeds in two stages. In the first, after reviewing Miller's data reduction algorithm, he goes on to plot the raw data from the exemplary data sheet given by Miller in his 1933 review. He then, in effect, superimposes the linear correction and the raw data which reveals 'what the eye can clearly see—the systematic drift … is clearly not at all linear during many if not most of the turns' (Roberts 2006, p 4). And what the eye can clearly see in this case is confirmed by Roberts' more formal analysis, which includes an examination of the Fourier transform of Miller's data (for a single set of turns) and his reduction procedures. Since this is highly technical, we'll move directly to Roberts' conclusion, which is that the frequency 'spectrum clearly shows that the low-frequency bins (corresponding to a slow systematic drift) *completely dominate* any real signal' (Roberts 2006, p 5, emphasis added). Accordingly, using a χ^2 test for fit, it follows that,

> … it is clear that [Miller's data] will have a good χ^2 for *any* sine wave with amplitude corresponding to speed X less than about 30 km s^{-1} and phase corresponding to any direction Y whatsoever. (Roberts 2006, p 4, emphasis added)

This result reveals the great danger in attempts to clean up one's data in order to eliminate irrelevant and distracting annoyances in the hope of facilitating a more manageable analysis of data[15]. On the other hand, it can be shown, as Roberts does, that Miller's data can be made to yield something better than 30 km s^{-1}.

Applying more advanced techniques for extracting a signal from its background, Roberts modeled Miller's data from an interferometer run 'as a sum of a periodic signal plus a systematic drift' (Roberts 2006, p 5). After examining 67 of Miller's data runs drawn from a variety of years, Roberts' conclusion is that:

[15] For an extended analysis of the dangers of using selection criteria, usually referred to as 'cuts', to extricate 'good' data, see Franklin (1998).

> … *there is no signal in Miller's data*, [though] *it can set an upper limit on any signal with period* 1/2 *turn* and on the 'absolute motion of the Earth [where] a reasonable estimate of the overall errorbar is 0.015 fringe for all sidereal times and all epochs of data. The errorbar is dominated by the systematic drift, and the availability of many runs does not decrease it. That implies an upper limit of 0.025 fringe at the 90% confidence level (1.65σ). (Roberts 2006, p 7, emphasis added)

Thus, while the existence of a signal with a half turn period is *compatible* with the data, such a signal (because of the large error bars) cannot be extracted from Miller's data. Nevertheless, Miller's data does serve to impose an upper limit on the amplitude of any such signal. In addition, as also shown in Roberts' initial analysis of Miller's data, here again the data have nothing to say about the phase of such a signal and thus do not impose any limits on phase.

Roberts' upper limit of 0.025 fringe could apply to a determination of absolute motion *or* conventional systematic disturbances such as temperature irregularities or mechanical effects. Assuming—as Miller was inclined to do—that his data were indicative of 'absolute motion', this upper limit constrains the velocity of such motion as follows:

> This [upper limit] must then be increased by 1/cos(latitude) to account for the worst-case projection onto the plane of the interferometer. Figure 20 of Miller (1933) relates fringe shift to his model of absolute speed, yielding an upper limit on the Earth's absolute motion of 6 km s^{-1} (90% confidence level). (Roberts 2006, p 7)

As the reader may recall, Miller's claim regarding the various interferometer experiments was that:

> Throughout all these observations extending over a period of years, while the answers to the various questions have been 'no', there has persisted a constant and consistent small effect which has not been explained. (Miller 1933, p 222)

Roberts' analysis shows that, *based on Miller's own data*, the persistence of this 'constant and consistent small effect' can mean nothing more than the fact that the ethereal velocity *is no more* than 6 km s^{-1} and *possibly nothing at all*. In other words, what would be Miller's preferred reading (in this case) of *at least* 6 km s^{-1} is not compatible with contemporary error analysis. This application of Roberts' analysis thus answers the question we posed at the beginning of our discussion of Miller's work. That is, whether the interferometer data was best interpreted as showing that the ethereal velocity was *no less* than around 6 km s^{-1}, what Miller would have liked. Or whether the data showed, as the skeptics urged, that the ethereal velocity was *no more* than around 6 km s^{-1} and possibly zero. The skeptics, alas, were right—a triumph over Miller's quixotic optimism.

Roberts' analysis also provides an explanation of the problem of the displaced azimuths and the inconsistency of phase among the epochs. While Miller admitted that the displacement of the azimuths was 'unexplained', the explanation based on Roberts' analysis is that the reported displacement was not statistically significant precisely because Miller's data had nothing to say about phase and could only place a restriction on the maximum velocity of the Earth's motion through the ether[16].

As we have seen, the raw data that is taken to be a null result are by and large not exactly equal to zero, and may appear to be a positive result. The experimenter argues that the result is equal to zero within reasonable estimates of systematic and statistical uncertainties[17]. Miller, himself, attempted to show that this was *not* the case for his results by considering possible temperature and mechanical effects that might mimic a positive result and demonstrating that they were not large enough to explain away his results as other than genuinely positive. Later work by Shankland and his collaborators and by Roberts demonstrated that Miller was incorrect. They argued that Miller's observations could set only an upper limit for the velocity of the Earth and more importantly that they were consistent with a zero velocity.

Still, speaking on Miller's behalf, Roberts offers this bit of mitigation:

> Dayton Miller was a prisoner of his time. In the 1920s and 1930s digital signal processing was unknown, and the serious flaws of the data reduction algorithm used by all such experiments went unnoticed. Also, the use of errorbars and quantitative error analyses were in their infancy. These aspects of the state of scientific knowledge combined to permit him to be fooled into thinking his interferometer measurements did indeed determine the 'absolute motion of the Earth'. Even in 1955, Shankland *et al* did not have knowledge of these aspects of Miller's analysis. (Roberts 2006, p 7)

And that is both a very fair assessment and a cautionary tale since as Roberts notes: 'We are all prisoners of our time.'

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[16] Roberts also provided analyses of the 1887 Michelson–Morley and 1927 Illingworth interferometer experiments (Roberts 2006, pp 8–9). For similar results regarding the 1887 experiment, though using different methods, see Handschy (1982).

[17] We believe that there are no precise numerical criteria for what is a reasonable estimate. This issue will be discussed later.

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