Better copies of all photos and graphics can be seen at the
on-line version of this paper:
http://www.orgonelab.org/miller.htm

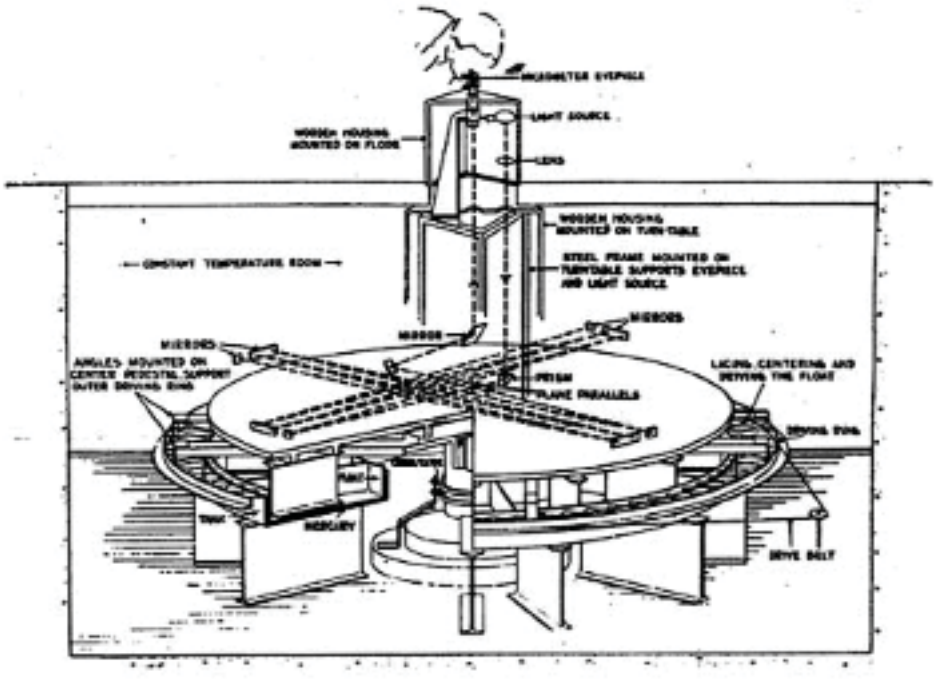

Figure 14. Apparatus used by Michelson–Pease–Pearson in their successful detection of an ether-drift of some unspecified quantity just under 20 km/sec. at Mt. Wilson, as reported in their 1929 paper. This positive result was inappropriately dismissed as a "negative" result because the experimenters had prematurely discarded the conceptual implications of an *Earth-entrained ether.* This experiment used the largest light-beam interferometer ever constructed by Michelson, with a 52-meter round-trip light path, coming close to the sensitivity found in Miller's 64-meter interferometer. It is shown here, situated in a basement location, in the ground, which, by itself, would also predictably reduce the measured result.

> 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 1933:240)

Miller had, by this time, acquired a lot of experience working on Mount Wilson, using his large interferometer in the specially constructed interferometer house. With a light path of 64 meters, Miller's apparatus was still significantly more sensitive than the best apparatus of Michelson–Pease–Pearson. Given that Michelson–Pease–Pearson did make some small

detection of an ether-drift from their efforts at Mount Wilson, in spite of the fact that it was located in a basement location, their report of detectable sidereal fringe displacements supports Miller's findings. It is also notable that this was the second time Michelson's work had significantly detected an ether, though in the first instance of Michelson–Gale (1925) the apparatus could only measure light-speed variations along the rotational axis of the Earth. These papers by Michelson and also by Kennedy–Thorndike have conveniently been forgotten by modern physics, or misinterpreted as being totally negative in result, even though all were undertaken with far more precision, with a more tangible positive result, than the celebrated Michelson–Morley experiment of 1887. Michelson went to his grave convinced that light speed was inconstant in different directions, and also convinced of the existence of the ether. The modern versions of science history have rarely discussed these facts.

Shankland Team's 1955 Critique of Miller

As previously pointed out by Swenson, Shankland's 1955 critique of Miller's work was undertaken with "extensive consultations" with Einstein, who like Newton and others before him had assumed only a static or stagnant ether, through which the Earth passed without material affect and, hence, without entrainment close to the Earth's surface. Shankland in fact was Miller's student for many years, and only emerged to become a professional advocate of Einstein's relativity after the death of Miller in 1941. Shankland became Chairman of the Physics Department at Case following Miller's retirement and death, building his professional career upon publications misrepresenting the Michelson–Morley experiments as the most solid evidence on the question, and publishing widely read interviews with Einstein (Shankland 1963, 1964, 1973a, 1973b). Shankland later took up administrative positions within government agencies developing nuclear energy—he rarely discussed Miller's positive ether-drift measurements in any of these papers except in the 1955 paper under discussion here. In this sense, it is legitimate to view Shankland and other members of his team (all Einstein advocates from Case) as biased reviewers of Miller's work.

The very first sentence in the Shankland team's 1955 paper began with the falsehood, now widely parroted in nearly every physics textbook, that the Michelson–Morley experiments had a "null" result. The third sentence in the Shankland paper was similarly false, claiming that *"All trials of this experiment except those carried out at Mount Wilson by Dayton C. Miller yielded a null result within the accuracy of the observations."* This kind of chronic misrepresentation of the slight positive results of many interfero-

meter experimenters, including Michelson–Morley, Morley–Miller, Sagnac, Michelson–Gale, and Michelson–Pease–Pearson, suggests an extreme bias and deliberate misrepresentation. The fact that this is a popular bias does not excuse it. By redefining all the positive results observed by what may in fact have been the majority of ether-drift researchers, as mere expressions of "observational inaccuracy," Shankland narrowed his task considerably.

These and other sentences in the Shankland paper revealed its bias from the get-go, and gave it the spirit of an autopsy, where Miller was dissected without careful concern, and certainly where no advocate of ether theory appeared to be involved in the process. It is possible that by the 1950s there was nobody left who could fill Miller's shoes to make an adequate defense. Ether-theory was then being compared to "the search for perpetual-motion machines" (Swenson 1972:239), and such ridicule surely must have had a silencing effect upon the entire fields of physics and astronomy. Swenson also suggests that, during his later years, Miller was largely ignored and isolated. This appears to be correct, as according to an interview with Shankland made in 1981, shortly before Miller died, he gave all of his interferometer datasheets—hundreds of pages of measurements—to his one-time student Shankland, with the somewhat bitter statement that he should "either analyze the data, or burn it" (Kimball 1981:2). In that same interview, Shankland also blamed Miller for having blocked the awarding of a Nobel Prize to Einstein for his relativity theory—clearly, Miller's work was a major obstacle to the Einstein theory of relativity, and for that reason may have given Einstein and his followers sleepless nights.

The title of the Shankland paper, and its overall representation suggests the authors had made a serious review of "*the* interferometer observations" of Miller, to include some kind of comprehensive and inclusive evaluation—but this was not the case. There were two basic approaches to the Shankland team's analysis: 1) a search for random errors or statistical fluctuations in Miller's data, and 2) a review of selected datasets which they claimed demonstrated significant thermal artifacts in the data. We can review these claims.

Shankland Team's Evaluation for Random-Statistical Variations

The Shankland paper did present a statistical analysis of a portion of Miller's published 1925–1926 Mount Wilson data, concluding that his observations *". . . cannot be attributed entirely to random effects, but that systematic effects are present to an appreciable degree*" and that "*the periodic effects observed by Miller cannot be accounted for entirely by random statistical fluctuations in the basic data"* (Shankland et al. 1955:170). Also, the Shankland team admitted they *". . . did not embark on a statistically*

sound recomputation of the cosmic solution, but rather [looked for] . . . local disturbances such as may be caused by mechanical effects or by nonuniform temperature distributions in the observational hut" (Shankland et al. 1955:172). In short, they admitted the harmonic patterns in Miller's data could not be due to any systematic measurement error, nor result from any mechanical flaws in the interferometer apparatus itself—while simultaneously admitting a disinterest in computation of any potentially validating ether-drift axis ("cosmic solution") from his data. These were important admissions, as the suggestion is that unless they could find some other fatal flaw in his data, Miller had really got it right, and measured a real Earth-entrained ether-drift.

Of interest from the perspective of the politics of science, is the fact that *this statistical analysis was not undertaken by any of the four members of the Shankland team listed as authors of the paper!* The analysis was in fact undertaken by Case physics student Robert L. Stearns, for his Master's Thesis (Stearns 1952)—Stearns was given only a footnote credit in the Shankland paper.

Stearns, who performed the analysis, informs us about the large amount of data gathered by Miller. He mentions (Stearns 1952:15–17) the existence of "316 sets of data . . . by Miller in 1925–1926" for the centrally important Mount Wilson experiments. Each dataset was composed of 20 turns of the interferometer, with sixteen data points per turn (a total of 320 data points per dataset). Miller noted his work at Mount Wilson was undertaken at four different seasonal "epochs," each of which encompassed a period of around ten days, centered on the following dates: April 1st, August 1st, and September 15th, 1925, and February 8th, 1926 (Miller 1926, 1933). It must be kept in mind, that these Mount Wilson data from 1925 and 1926 provided the most conclusive and foundational observations for Miller's ether-drift calculations and conclusions, as presented most clearly in his 1933 paper. As detailed below, the Shankland team mentions these Mount Wilson data, but in a manner which confuses them with his earlier and less significant efforts, including various control experiments conducted at the Case School. The significance of this confusion of dates will be highlighted momentarily.

Shankland Team's Assertion of Temperature Artifacts

Regarding possible temperature artifacts in Miller's data, this objection was raised early on in the history of ether-drift interferometry, and specifically rebutted by Miller when he was still alive. A letter exchange between Miller and Georg Joos from a 1934 issue of *Physical Review* records part of this debate, and appears to be one of the few *published* criticisms on

the temperature issue Miller ever received while still alive. Miller had this to say about the problem: *"When Morley and Miller designed their interferometer in 1904 they were fully cognizant of this . . . and it has never since been neglected. Elaborate tests have been made under natural conditions and especially with artificial heating, for the development of methods which would be free from this [thermal] effect"* (Joos & Miller 1934). The Shankland critique never made any systematic evaluation of possible thermal artifacts using a larger set of Miller's data, as was done with the statistical evaluation. Instead, they appear to have "gone fishing" in Miller's data for something by which they could simply dismiss him. For example, Miller's own 1923 temperature-control experiments were brought into discussion, where radiant parabolic heaters were used to artificially create a general doubling of the size of interference fringes. Miller describes these experiments:

> Several electric heaters were used, of the type having a heated coil near the focus of a concave reflector. Inequalities in the temperature of the room caused a slow but steady drifting of the fringe system to one side, but caused no periodic displacements. Even when two of the heaters, placed at a distance of three feet from the interferometer as it rotated, were adjusted to throw the heat directly on the uncovered steel frame, there was no periodic effect that was measurable. When the heaters were directed to the air in the light-path which had a covering of glass, a periodic effect could be obtained only when the glass was partly covered with opaque material in a very nonsymmetrical manner, as when one arm of the interferometer was completely protected by a covering of corrugated paperboard while the other arms were unprotected. These experiments proved that under the conditions of actual observation, the periodic displacements could not possibly be produced by temperature effects. (Miller 1933:220)

Perhaps without intending to do so, after examining Miller's laboratory notes for the Cleveland temperature control experiments, the Shankland team confirmed Miller on this point:

> In the experiments where the air in the optical paths was directly exposed to heat, large second harmonics (0.35 fringe for one heater, and about twice this value for two heaters) were always observed in the fringe displacements, and with the expected phase. Shifting the heaters to a different azimuth produced a corresponding change in the phase of the second harmonics. *When the optical paths and mirror supports were thermally insulated, the second harmonics were greatly reduced to about 0.07 fringe*. (Shankland 1955:174; emphasis added)

This statement confirmed the wisdom of Miller's approach. The added insulation reduced the thermal effects from a nearby radiant heater to only

20% of the un-insulated readings. I have an ordinary commercially available electric radiant parabolic heater at my home, and it gets so hot you cannot stand closer than 12″ without burning yourself, or possibly catching your clothing on fire. If Miller had used a parabolic heater even half as strong as this, it would certainly have been a source of heat much stronger than anything present in his Mount Wilson experiments, particularly at night, during foggy or overcast conditions, and when the entire interferometer house was covered over with a tent, with the apparatus and light-beam path covered with cork, glass, and paper insulation. Consider a radiant heater at several hundred degrees C, creating a steep thermal gradient but only a 0.07 fringe shift in the insulated interferometer. How much *less* of an effect would be produced by a human body, or even from the inside of a solar-heated wall? Assuming an environmental thermal effect only one-tenth that seen with the parabolic heater (a wood composite wall radiating inside the structure at perhaps 50 °C?), fringe shifts of *only 0.007* would have been produced, *well below observational detection*. Miller's datasheets, for example, recorded observations *"in units of a tenth of a fringe width,"* though readings down to hundredths of a fringe were possible with care. Overall accuracy of the ether-drift measurements approached a hundredth of a fringe after mathematical averages of many readings were extracted. The Shankland paper nevertheless used these control experiments as a weapon against Miller, claiming without evidence that heater-type effects *might* have occurred in his Mount Wilson experiments, even where no such heater or remotely similar heat source was present. But why would the Shankland team shy away from undertaking a more systematic evaluation for temperature artifacts? They could have, for example, evaluated only Miller's daytime interferometer experiments, and looked for a thermal effect from the southerly wall of the structure during the various epochs—if they could have shown an effect present in daytime data which was not present at night, it would have devastated Miller's claim, and proved their case. However, this obvious analytic procedure was not performed, or, if it was done, not reported.

The Shankland paper also resurrected the temperature criticisms by Joos and Miller (1934), but without reference to Miller's rebuttal in the same published exchange. If the periodic effects observed by Miller were the product of temperature variations, as was claimed by Shankland and Joos, then why would that variation systematically point to the same set of azimuth coordinates along the celestial *sidereal clock*, but *not* to any single terrestrial coordinate linked to civil time (see Figure 11)? Miller repeatedly asked this question of his critics, who had no answer for it. The Shankland team likewise evaded the question.

It is clear Miller had been deeply engaged in the problem of temperature effects, and worked hard to know exactly how they might be produced, and how to eliminate them. The Shankland paper, however, seized upon Miller's open acknowledgment of fringe-shifts from air heating by powerful radiant heaters during control experiments, and a few other sentences written in his lab book, and tried to claim thermal anomalies were probably the source of whatever periodic effects were subsequently measured by Miller at Mount Wilson, when no radiant heaters were used, and when the empirically developed control procedures were put in place. Without some kind of independent experimental evidence to support such a claim of a thermal influence, their dismissal was illogical.

The Shankland paper also went through a series of arguments about the interferometer house, how the wall materials, roof angles, interferometer glass housing, etc., might result in a definable effect upon the air temperature in the light-beam path, concluding only that they could not rule out such an influence—that it *". . . is not in quantitative contradiction with the physical conditions of the experiment"* (Shankland et al. 1955:175). Given their ignoring the sidereal nature of the periodicities, this statement could hardly be taken seriously, and certainly did not constitute a rebuttal of Miller's data.

The Shankland paper finally attempted to correlate several selected daytime interferometer runs with temperature measurements made at the same time. They acknowledged difficulty in correlating low fringe-shift values with low temperature differentials, but found one set of high fringe-shift values correlated with slightly higher temperatures, even while noting another set where high values correlated with lower temperatures. Finally, they complain that *". . . no temperature data are available to reveal thermal conditions at the roof, which may be responsible for the large fringe displacements at the times of highest altitudes of the Sun"* (Shankland et al. 1955:176). If this sounds confusing, a reading of the full original text provided little clarification.

Failing to show anything damning from daytime datasets, when temperature gradients inside the interferometer house might be expected to be at a maximum, they turned their focus to nighttime datasets. Once again, only a few of Miller's datasheets were selected out to prove their case. Data from two nights (30 August 1927 and 23 September 1925) with stable air temperatures were reviewed—these nights showed very clear and systematic fringe variations (Figure 4 in Shankland et al. 1955:176), but because the azimuth of the fringes changed minimally over the approximate 5 hours of observation, the critics complained "it *would be extremely unlikely if the fringe shifts were due to any cosmic effect"* (Shankland et

al. 1955:177). Apparently, the Shankland team was so locked into the older "static ether" assumptions of the original Michelson–Morley experiment, they were unclear about what they should have seen in Miller's data. In 1927, at a *Conference on the Michelson–Morley Experiment* held at Mount Wilson Observatory, where Michelson, Lorentz, Miller, and others made presentations and engaged in open debate, Miller addressed this question: *"Observations were made for verifying these [static ether] predictions . . . but it did not point successively to all points of the compass, that is, it did not point in directions 90° apart at intervals of six hours. Instead of this, the direction merely oscillated back and forth through an angle of about 60° . . ."* (Miller 1928:356–357). The reason for this is that Miller's detected axis of ether-drift is oriented reasonably close (within 60°) to both the Earth's axis of rotation and the axis of the plane of the ecliptic.

Another important fact which nearly escapes detection in the Shankland paper is that the 30 August data were made in Cleveland, while the 23 September data were from Mount Wilson, and *neither were a part of the published Mount Wilson data Miller used for calculations of the ether-drift*—both dates are well outside of the 10-day epochal periods identified by Miller. Furthermore, not all of the interferometer datasheets for a given date—which presumably would have had similar weather and temperature conditions—were included by the Shankland team for critical review. They selected only those datasets that appeared to support their argument of a claimed thermal anomaly. For example, they selected *"ten sets of observations, Nos. 31 to 40 inclusive, made in the hut on the Case campus between midnight and 5:00 a.m. on August 30, 1927"* and *". . . runs 75 to 83 inclusive taken from 12:18 a.m. to 6:00 a.m. on September 23"* (pp. 176–177). Other than making the *claim* that these selected data gave them the *impression* of being the result of temperature errors, they had no other stated criterion for bringing them into discussion. This biased data selection, or rather *data exclusion,* procedure forces one to ask*: What about datasets No. 1 to 30, and runs 1 to 74?* Similar unexplained data selections or data exclusions occur throughout the Shankland paper, leaving one to wonder if the *unselected and excluded* data, which constituted the overwhelming majority of it, simply could not provide support for their criticisms. One can imagine the howl of protest which would have occurred if Miller had taken this approach, arbitrarily excluding data from his calculations which superficially suggested something other than a real ether-drift. A third dataset from 30 July 1925 was highlighted by the Shankland team as it contained one extremely large peak where Miller noted "Sun shines on interferometer." This data does appear to have been a part of Miller's published Mount Wilson analysis. However, the Shankland team extracted

only *"observations Nos. 21 to 28 inclusive, made between 1:43 a.m. and 6:04 a.m. on July 30, 1925."* Obviously, at around 6:00 *a.m.* the sun rose and caught Miller and his assistant off-guard. What about observations Nos. 1 to 27, or other early-morning data, where the sun *didn't* shine on the interferometer? These other data were not brought into discussion, except they did note that the runs prior to the sunshine incident demonstrated *". . . an extremely erratic behavior . . . we have no ready explanation for this apparent departure . . . "* Here, the Shankland team basically confesses their grab-bag of "ready" explanations was empty, and the idea that those data were expressing a real ether-drift was simply too "impossible" for them to consider. The fact that Miller included the note about the Sunlight on this datasheet speaks to his honesty.

The Shankland team also identified datasets Nos. 56–58 from 8 July 1924, which was part of Miller's control experiments made in a basement location at Case physics laboratory—the temperatures were very stable, and the fringe oscillations were quite small, and they argued these data were a proof for thermal effects on the apparatus. However, it was this very problem of basement and dense surrounding materials that led Miller on the path to use the apparatus in locations not subject to significant ether shielding or Earth entrainment. After 1921, Miller only used the Case School laboratory to undertake control experiments, and that is why those particular data were never published.

The Shankland paper concluded its temperature criticisms by discussing a few additional datasets: Nos. 113–118 from April 2nd, Nos. 88–93 from August 8th, 1925, and Nos. 84–91 from February 11th, 1926, (Shankland et al. 1955:177). Here, the amplitudes and phases were claimed to have been "nearly alike," but insufficient detail was given to allow a review of the critic's claims, and it did appear they were once again incorrectly misinterpreting Miller's data along the lines of static ether assumptions.

As in almost all the cases given above, *none of these data were analyzed systematically*, nor were they presented in such a manner that the author's criticisms could be factually reviewed. I got the impression that they simply scanned through a pile of Miller's datasheets, and with a wave of the hand, picking and pointing to only selected parts, dismissed it all as the product of thermal artifacts. Miller's detailed control experiments were basically ignored, as was the fact that, for all these experiments, the interferometer was enclosed in a small house covered over with a tent, while the apparatus was shielded with cork insulation, and the light-beam path covered with glass and paper panels—with a full rotation occurring in less than a minute, one is left to wonder how any observable thermal variations could develop within Miller's data, especially variations with a sidereal-cosmic component.

For the casual reader, who had not undertaken a careful review of Miller's original experiments, the Shankland paper might appear to make a reasoned argument. However, the Shankland paper basically obfuscated and concealed from the reader most of the central facts about what Miller actually did, and in any case was so unsystematic and biased in its approach, excluding from discussion perhaps 90% or more of Miller's extensive Mount Wilson data, as to render its conclusions meaningless.

As a final note, after undertaking my research into the archives of both Miller and Shankland at Case University, and urging them and the faculty of the CWRU Physics Department on the importance of the original Miller datasheets, they were finally located and placed into the CWRU Archive.

Conclusions

My review of this important but sad chapter in the history of science left me both astonished and frustrated. Miller's work on ether-drift was clearly undertaken with more precision, care, and diligence than any other researcher who took up the question, including Michelson, and yet his work has basically been written out of the history of science. When alive, Miller responded concisely to his critics, and demonstrated the ether-drift phenomenon with increasing precision over the years. He constantly pointed out to his critics the specific reasons why he was getting larger positive results, while others got only small results, or no results. Michelson and a few others of the period took Miller's work seriously, but Einstein and his followers appeared to view Miller only as a threat, something to be "explained away" as expeditiously as possible. Einstein in fact was catapulted into the public eye following the end of World War II. Nuclear physics was then viewed as heroic, and Einstein fast became a cultural icon whose work could not be criticized. Into this situation came the Shankland team, with the apparent mission to nail down the lid on Miller's coffin. In this effort, they nearly succeeded.

The Shankland conclusions against Miller were clearly negative, but the one systematic statistical analysis of his Mount Wilson data merely confirmed what Miller said all along, that there was a clear and systematic periodic effect in the interferometer data. The Shankland paper also confirmed Miller's contention that this periodic effect was *not the product of random errors or mechanical effects.* The Shankland team subsequently searched for temperature artifacts in Miller's data, but they failed to undertake any systematic analysis of his centrally important Mount Wilson data in this regard. Instead, they made biased selections of a few published datasets and unpublished datasets obtained from different periods in Miller's research, from different experimental locations, and

including from his control experiments at the Case School.

Miller's most conclusive 1925–1926 Mount Wilson experiments encompassed a total of 6,402 turns of the interferometer, recorded on more than 300 individual datasheets. That was the data the Shankland team should have focused on and evaluated systematically. Instead, only a few of Miller's datasheets from these most centrally important experiments were selected—certainly less than 10% of the data available to them was brought into discussion—and then only after being firstly dissected to extract only those data that could most easily be misconstrued as evidence for presumed temperature anomalies. For certain, some of the data held up for public critique came from Miller's control experiments at Case, or possibly from trial runs when technical "bugs" were being worked out in the apparatus and building. Miller is no longer alive to inform us about his data, but the Shankland team willy-nilly lumped together both published and unpublished data, without comment.

Even though they were content to pick and choose data as they wished, they could not come up with a coherent and solid critique by which Miller's work could be conclusively dismissed—some of the data they selected merely confirmed Miller, though the Shankland group seemed ignorant of the basic ether-drift astronomy by which such an interpretation could be made. When alive, Miller openly stated he had addressed and corrected for thermal effects upon the apparatus, and yet the periodic elements of his measurements persisted—the Shankland paper ignored Miller on this important point.

The Shankland group undertook no new experiments of their own, neither on the question of ether-drift, nor on the subject of thermal perturbations of light-beam interferometry—they made essentially an "armchair analysis" of Miller's data. Only *some* of Miller's original data was carefully selected to make a rather unbelievable claim that small natural ambient temperature gradients in Miller's Mount Wilson observation hut *might* produce fringe shifts in the insulated interferometer similar to what Miller himself previously observed in his control experiments using strong radiant heaters. The Shankland paper argued there *must have been* "thermal effects" in Miller's Mount Wilson measurements, but provides no direct evidence of this.

At no time did the Shankland group present evidence that temperature was a factor in creating the periodic cosmic-sidereal fringe shifts observed by Miller in his published data, even though this was their stated conclusion. In fact, they presented evidence from Miller's own lab notebooks which implied thermal gradients in the Mount Wilson interferometer house would have been *below the observational limits* of the insulated apparatus.

The larger issue of periodic or harmonic effects in the data, expressed in nearly identical cosmic sidereal coordinates at different seasons and at all hours of the day, was never addressed or evaluated by the Shankland group. Neither was any attempt made to show *exactly how* an external temperature phenomenon could affect the interferometer readings to yield such a systematic sidereal effect. This issue was almost totally avoided by the Shankland team.

A reading of Miller's 1933 paper shows the picayune and biased nature of the Shankland team procedure, as the systematic sidereal periodicities observed by Miller expressed themselves nearly uniformly across the board, though at differing magnitudes. From 1906 to 1926, Miller undertook more than 200,000 separate readings, more than 12,000 turns of the interferometer demonstrating harmonic periodicities constantly pointing to the same general axis of ether-drift in the cosmos—a factor which was completely independent of the time of day or season of year in which the experiments were undertaken. At best, the critics provided only an *ad hoc argument*, a claim or suggestion without substance, that some small part of Miller's data *might* contain an undefined temperature effect.

From all the above, it appears the Shankland group, with some degree of consultation with Einstein, decided that "Miller must be wrong" and then set about to see what they could cherry-pick in his archive to support their *a priori* conclusion—which is not a scientific method.

As I have discussed previously, Miller found the ether-drift effect to be stronger at higher altitudes and also to be small when the experiment was undertaken in heavy stone buildings or when the interferometer light-path was encased in wood or metal shielding. In my studies over the last 40+ years, I've found many examples from the fields of biology, meteorology, and physics that independently support the assertion of a subtle energetic force with similar altitude-dependent and metal-reflective properties—notably in the works of Wilhelm Reich, Giorgio Piccardi, and Frank Brown (DeMeo 1979, 1989a, 1989b, 2000, 2002, 2004). Likewise, there are many new findings in astrophysics, where anisotropy of cosmological factors have been discovered, which are congruent with Miller's identified axis of ether-drifting (Miller 1933:241, Allais 1997, 2002).

Notable in this respect are the experiments of Cahill (2006a, 2007) of the Chemistry, Physics, and Earth Science Department at Flinders University in Australia; DeWitte (in Cahill 2006b) working with the Belgian telecommunications company Belgacom in Brussels; Galaev (2000a, 2000b, 2001, 2002) at the Institute for Radiophysics and Electronics, National Academy of Sciences of Ukraine; Múnera (2002, 2009) of the Physics

Department, Universidad Nacional de Colombia at Bogotá; and Múnera, Deckers, Arenas, and Alfonso (2006) and Múnera, Deckers, Arenas, Alfonso, and Lopez (2009) from diverse institutions including the Physics Department at Bogotá and the Max Planck Research Center in Hamburg, Germany.

All of these newer studies have basically confirmed the Miller results, including its general axis of ether-drift and sidereal-day velocity components, *"down to the details"* (as expressed by Galaev).

To close, I ask the reader to imagine that Michelson–Morley's 1887 experiment, which ran over only 6 hours on four days, had resulted in a claim that "the ether has been detected," and that Dayton Miller had undertaken his years of work with 200,000 observations showing "the ether cannot be detected." It does not take much consideration to conclude that—in such a fictional case—Miller would today be cited in every physics textbook as having "proved the ether did not exist," and nobody would refer to Michelson–Morley. The fact that the present-day situation is totally opposite of this fictional example is a testament to the intensely political nature of modern science, and how major theories often develop into *belief-systems*, which demand the automatic suppression of any new finding which might undermine the faith and "popular wisdom" of politically dominant groups of academics. And that "wisdom" today is: ***Space is empty and immobile, and the universe is dead.*** I submit that these are unproven, and even *disproven* assertions, challenged in large measure by Dayton Miller's exceptional work on the ether-drift.

I should also add how modern astrophysics today accepts without hesitation many theoretical concepts which basically fill the vacuum of cosmic space, and appear either superficially or dramatically similar to the Miller type of tangible and measurable cosmic ether, in spite of their detection difficulties. Examples are the "dark matter," the "neutrino sea," the "intergalactic or interstellar medium," and "cosmic plasmas" (DeMeo 2011). The fields of parapsychology also could find a potent explanatory mechanism in such a cosmic medium, for transmission of sensory or inertial impulses via as-yet-unclear excitations or mechanisms more similar to old wave-theory than modern particle-based or consciousness-based "intentional" postulates.

By Ocam's Razor, if not also by the similarities of their properties, I postulate these are all one and the same thing, as per the well-known example of ten blind scientists in a room with an elephant, each one grabbing ahold of, and describing what only *appear* to be uniquely different parts of a unitary cosmic anatomy.

Acknowledgments

My thanks to the Case Western Reserve University (CWRU) Archivists, and to Bill Fickinger of the CWRU Physics Department, for their help during the research phase of my investigations, and also for help in locating and securing in archives Miller's 1000+ original datasheets, which had been lost. Most of these sheets were recorded in pencil, which unfortunately does not photocopy very well. Consequently, a visit to the CWRU Archive may be necessary for accuracy, in case others may wish to review them. This problem also makes it nearly impossible to re-review Miller's findings based upon those original datasheets, so I must defer to his own concluding statements in his published papers. In my view, rather than another re-analysis of Miller, this subject begs for a serious replication atop Mount Wilson, performed in both a critical and open-minded spirit which takes into account all the necessary parameters for the possibility of a matter-inhibited and Earth-entrained ether-drift. All photos courtesy of the CWRU Archive. The author has also developed a comprehensive webpage list with download links, of historical articles on the ether-drift experiments, including for most of the papers cited in this article at http://www.orgonelab.org/energyinspace.htm

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