Michelson-Morley Experiments Revisited: Systematic Errors, Consistency Among Different Experiments, and Compatibility with Absolute Space

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Despite the null interpretation of their experiment by Michelson and Morley, it is quantitatively shown that the outcomes of the original experiment, and all subsequent repetitions, never were null. Additionally, due to an incorrect inter-session averaging, the non-null results are even larger than reported. Contrary to the received view, Illingworth's and other repetitions of the experiment were consistent with Miller's positive results. On the theoretical side, a new systematic error is uncovered: the angle between the projection of earth's velocity on the plane of the interferometer and the reference arm of the apparatus has been practically ignored. This phase angle produces a noticeable change in the position of the peaks from one turn to the next of the interferometer. Hence, the data analysis cannot be based on the average of fringe shifts during a session, but rather on the calculation of individual speed for each turn. This procedure was applied to the only two sessions reported in detail in the literature: Miller's September 23, 1925 at 03:02 in Mount Wilson and Illingworth's July 9, 1927 at 11:00 in Pasadena. Surpisingly, it was found that in both cases the measured speeds exactly correspond to the projection of earth's orbital velocity only. As a result, the evidence against a preferred frame completely dissappears.

1. Introduction

To the best of our knowledge, the only empirical evidence against the existence of absolute space (= ether in this paper) is the ***null interpretation*** given to the interferometry experiment carried out by Michelson in 1881[1], repeated with experimental and theoretical improvements by Michelson and Morley (M-M) in 1887 [2]. A hundred and ten years later, there is still controversy: some people argue that results were non-null and try to derive implications thereof [3], while others strongly maintain that results were null, and dismiss evidence to the contrary as experimental artifact [4].

To avoid second-hand interpretations, we revisited the original literature on M-M. It was found that a systematic application of standard statistical tests to the values originally reported does not support the null interpretation. Furthermore, two systematic errors were identified, one of them new to the best of our knowledge. Systematic error 1 (SE1) pertains to data reduction, while systematic error 2 (SE2) belongs to the theory. After removing SE1, speeds become larger than reported, hence closer to Miller's results. The SE2 implies that fringe-shifts during an experimental sessions present strong variations due to changes in magnitude and direction of the projection of velocity on the plane of the interferometer. The implications are two-fold: (a) data reduction cannot be done averaging fringe-shifts during a given session, and (b) the phase angle must be included in all equations.

Section 2 begins with a brief summary of the theory behind the experiment, leading to identification of SE1 and SE2. It continues with a critical review of the class of M-M experiments to show that (1) all experiments were qualitatively compatible with absolute space, and (2) the results never were null, neither in the original version [2] nor in the subsequent repetitions [5-15]. Section 3 contains our contribution to the controversy. Firstly, we remove SE1 from Illingworth's inter-session data [13]. And, secondly, we apply Illingworth's method to the M-M experiment [2], to Miller's measurements on Sept. 23, 1925 [7], and to his own observations on July 9, 1927 [13]. It is found that at a 90% confidence level, all experiments were non-null. The intra-session averages based on velocity exactly correspond to the range of variation of the projection of orbital speed at the moment and location of the observations. Section 4 closes the paper. Except for consistency with absolute space, we do not mention any other implication for our findings.

2. M-M Experiments Critically Revisited

Attention is restricted to experiments using local light sources, as the original M-M experiment [2], Morley and Miller's repetitions [5], Miller's work alone [6-8], the experiments of Piccard and Stahel [9-11], the refinement of Kennedy [8,12] and Illingworth [13], the repetitions of Michelson *et al.* [14], up to Joos [15]. As explained long ago by Robertson [16], Kennedy and Thorndike [17] started a new class of experiments: a null-result in the M-M experiment was assumed, thus implying a length-contraction in the context of special theory of relativity (STR), the objective was then to test the ensuing time-dilation and/or the isotropy of the space. Direct inspection of the literature confirms that all modern "M-M experiments" [18-22] actually belong to the Kennedy-Thorndike class (some of them explicitly acknowledge such a fact [19]). Since many proponents of the null interpretation discredit Miller's positive results by quoting a paper by Shankland *et al.* [28] we will comment on the latter as well.

Initial criticisms to M-M results addressed the design and operation of the intereferometer [23-27], leading to improvements in subsequent repetitions of the experiment [5-15]. However, there is still a systematic error (SE1), discovered a century ago by Hicks [23]: the inter-session averaging includes curves from two different calibration families. In his final data analysis, Miller [7] took this matter into account, but Illingworth [13] did not. This aspect partially accounts for the difference in speeds reported by Miller and Illingworth.

2.1. Summary theory of experiments

Let M-M experiment be carried out in right-handed horizon coordinates S(j), located at latitude j on the earth, with X-axis oriented towards local east, Y-axis towards local geographical north, and Z-axis along the earth’s radius; then, the interferometer is on the X-Y plane. For an observer at rest in a preferred frame Σ attached to absolute space, the time for light to travel length L (in cm) of the reference arm (RA) of an interferometer depends on w, the angle between the direction of RA and V_I. Velocity V_I is the projection onto the plane of the interferometer of $\mathbf{V}$, the velocity of earth in Σ, given by the vector addition $\mathbf{V} = \mathbf{V}_s + \mathbf{V}_o + \mathbf{V}_r$ (subscript s: solar motion in Σ; subscripts r and o: earth’s rotational and orbital motions). Now, let α,δ be the right-ascension and declination of the apex of $\mathbf{V}$ at some particular time; then, $V_I = |\mathbf{V_I}| = |\mathbf{V}|F(a, dj)$. Let ΔT be the difference in the time-of-travel over the closed paths along the two perpendicular arms of the apparatus at some specific time t, given by

$$\Delta T = \frac{Lb^2}{c}\cos 2w = A\cos 2(\Delta w - w_N) \tag{1}$$

where $b = V_I/c$, Δw is the angular position of RA relative to the Y-axis of S(j), and $w_N(a, d, j)$ is the counterclockwise angle from the Y-axis to $\mathbf{V_I}$. In any theory where time is universal across frames in relative motion, ΔT is the same for observers in Σ and S(j).

Since M-M could not measure ΔT, they resorted to measuring the shift of interference-patterns. Following Hicks [23], let the displacement of the central interference band from a reference point P_0 be

$$\begin{aligned} z &= \frac{P_0 b^2 \cos 2w}{2\sin(\Delta q) - b^2\cos 2w} \\ &\cong \frac{P_0 b^2 \cos 2w}{2\Delta q - b^2\cos 2w} \end{aligned}. \tag{2}$$

The small angle $\Delta q = a - g \approx \pm 10^{-5}$ radians is a deviation from the ideal orientation of angles a and g (the angles of the two reflecting mirrors located at the ends of the two arms of the interferometer; typically, $a = g = 45°$ relative to the half-transmitting plate). The adjustment of Δq is carried out during the calibration procedure. For $b^2 < \Delta q$, the denominator is controlled by Δq:

$$\begin{aligned} z &\approx Kb^2\cos 2w \\ &= \pm|K|\,b^2\cos\left[2(\Delta w - w_N)\right] \end{aligned}. \tag{3}$$

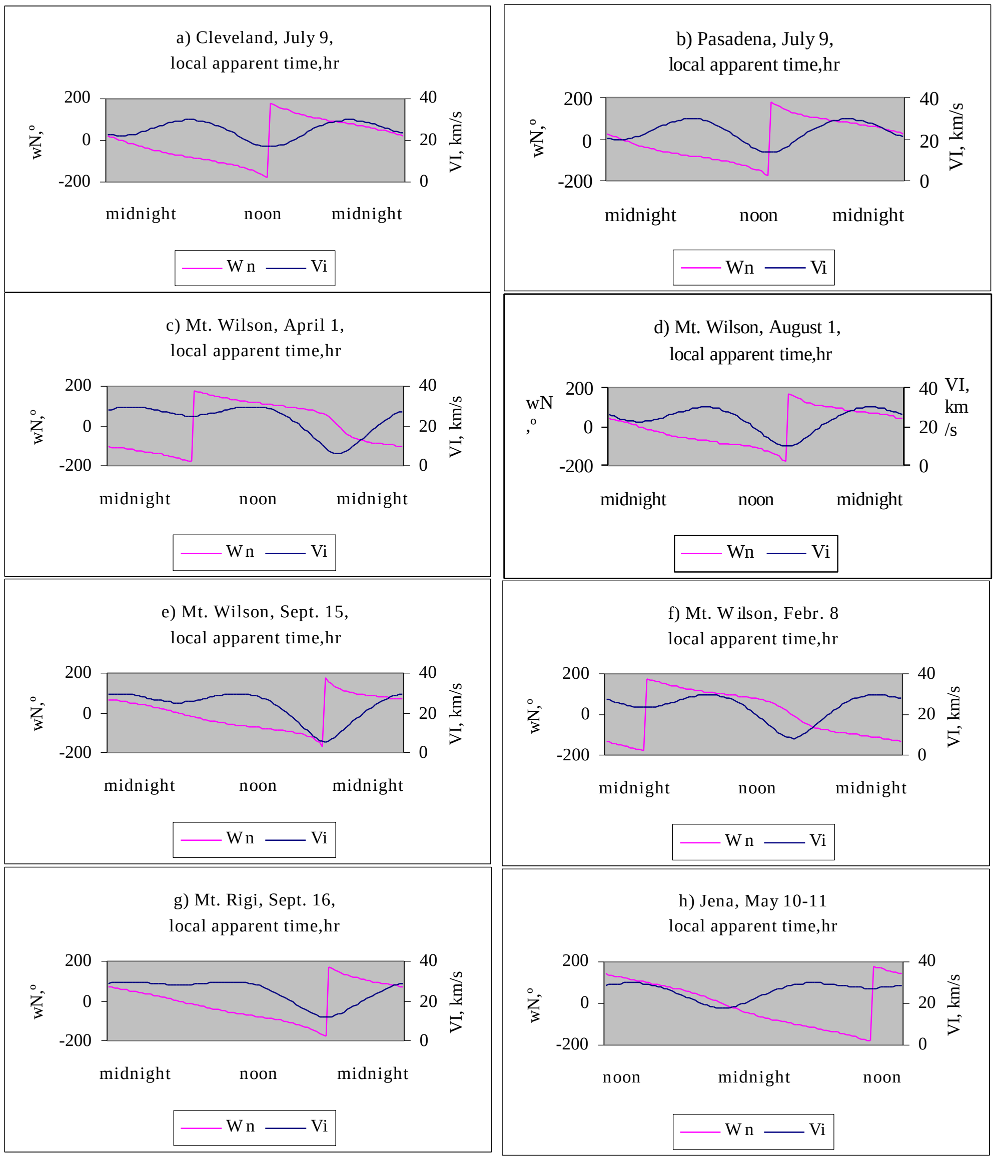


Figure 1. Projection of orbital speed onto the plane of the interferometer for typical experimental sessions at different places. The direction of V_I relative to the local north is ω_N, that exhibits strong variations in time-scales comparable to the duration of the sessions. Note that the minima for V_I coincide with changes of sign in the projection of V_I along N-S and E-W directions.

The sign of $K = P_0/2\Delta q$ depends on the sign of Δq, *i.e.*, displacement to the right or to the left of P_0. Hence, for a given velocity described by V_I and $w_N(a, d, j)$, z may be positive or negative, thus leading to two families of displacements z^+ and z^-. Of course, for a given calibration only one of them occurs. Also note that: (a) Eqs. (1) and (3) have the same structure, so that there is a one-to-two correspondence between ΔT and $z^{\pm}$. And, (b) Eq. (3) is a first order approximation, whereas eq. (1) is exact. So that for large $b > 3\times 10^{-3}$, eq. (3) is not applicable.

The existence of the two families $z^{\pm}$ is the origin of ***systematic error 1*** (SE1) in the reduction of data, recurrent in all experiments from M-M to Illingworth [13], with the notable exception of Miller's work [7, pages 210-211]. In Hick's words: *"the adjustment of the mirrors can easily change from one type to the other on consecutive days. It follows that averaging the results of different days in the usual manner [i.e., as M-M did] is not allowable unless the types are all the same. If this is not attended to, the average displacement may be expected to come out zero — at least if a large number are averaged"* [23, page 34].

In practice, the initial calibration focuses the interferometer to observe a displacement at $\Delta w = 0$

$$Z_0^{\pm} = \pm A\cos(2w_N)\ , \tag{4}$$

where $A = |K|\,b^2$. The RA is typically oriented towards the local north. For other positions of the apparatus, the experimenter observes relative fringe-shifts,

$$\begin{aligned} \Delta Z(w) &= z - Z_0^{\pm} \\ &= \pm A\left\{\cos\left[2(\Delta w - w_N) - \cos 2w_N\right]\right\} \end{aligned} \tag{5}$$

Then, eq. (5) shows that a rotation of RA through *p*/2 (from north to east or west) produces a shift

$$D \equiv \Delta Z\left(\frac{p}{2}\right) = \mp 2A\cos 2w_N = -2Z_0^{\pm}\ . \tag{6}$$

Note that, contrary to M-M expectations, $|D| < 2A$, except for $w_N = 0$. Again, there are two families for D (according to the sign of $Z_0^{\pm}$). Although M-M were aware of diurnal variations of $w_N(a, d, j)$, they chose to ignore them, and assumed $w_N = 0$ always. This is the origin of ***systematic error 2*** (SE2): lack of a consistent consideration of $w_N(a, d, j)$.

Although the observable is z, the measured variable is x which depends upon the details of each experiment. In the original experiment x is the reading in a micrometer head-scale. Then, $z = kx$, where conversion factor k is obtained during the calibration. In his final design, Miller used a telescope to read z directly in tenths of fringe. Kennedy [12] and Illingworth [13] used small stone weights to bring the central fringe into focus. In Europe, Piccard and Stahel [9-11] and Joos [15] automatically photographed the fringes.

A series of measurements at different positions Δw in a rotation of the interferometer leads to a plot ΔZ vs Δw. Qualitatively, the presence of two cycles over a 360º-rotation of the apparatus confirms eq. (5). Quantitatively, the position and amplitude of the peaks lead to A and $w_N(a, d, j)$ respectively. To obtain the latter, Miller [7] used a mechanical harmonic analyser, while Piccard and Stahel used the least-squares method [9].

On the other hand, Illingworth [13] did not measure the entire curve. Rather he proceeded to estimate D directly from 90º-rotations from either of two initial positions of the apparatus: (A) RA towards north, then

$$D_A = 2A\cos 2w_N\ . \tag{7a}$$

(B) RA towards north-east, then

$$D_B = 2A\sin 2w_N\,. \tag{7b}$$

Thus, contrary to conventional belief, the results from Illingworth experiment [13] cannot be directly compared to those based on $D_0 = 2A$. This otherwise obvious result has been masked by SE2.

From the empirical D, V is obtained as

$$V = V_o\sqrt{|D|/D_R} = C\sqrt{|D|} \tag{8}$$

where $C = V_o\big/D_R^{1/2}$. In the original experiment, M-M used orbital speed V_o = 29.8 km/s to calculate a reference fringe displacement $D_R = \pm 2Lb_o^2/l = \pm 0.4$ wavelength, where l is wavelength of light-source. Obvioulsy, the signs of D_R and D must be the same. Since D_R is conventionally positive, then the empirical displacement to enter eq. (8) must be $|D|$. However, the conventional inter-session data reduction process is based upon D, not $|D|$. This is another form of systematic errror 1 (SE1).

Summarizing the previous discussion,

$$V_0 = V_I \text{ for } D = D_0, \tag{9a}$$

$$V_A = V_I\sqrt{|\cos 2w_N|} \text{ for } D = D_A, \tag{9b}$$

$$V_B = V_I\sqrt{|\sin 2w_N|} \text{ for } D = D_B. \tag{9c}$$

2.2. Michelson-Morley expectations

M-M expected that V_I would be approximately parallel to RA at the beginning of an experimental session, and that it would stay approximately constant throughout. In particular, they expected $V_I \approx V_o \approx 30$ km/s. However, M-M's expectations were unwarranted. To be fair to M-M, let us not include solar motion, and restrict analysis in this paper to the projection of V_o onto the plane of the interferometer. As a first order approximation, let the center of earth move with constant angular speed w_o = 360°/365.2422 days on a circular trajectory on the plane of the ecliptic, and let time t be measured at the observer's meridian from midday March 21 (the vernal equinox on the northern hemisphere).

Further, let e = 23.45° be the obliquity of the ecliptic, and let the earth be a perfect sphere rotating with constant angular speed w_r = 360°/24 hr around her axis. Then, the projection of V_o along the X- Y- and Z-axes of the horizon coordinates S(j) are V_E, V_N and V_P respectively, given by

$$\frac{V_e}{V_o} = f_E(t) = -\sin w_o t \sin w_r t - \cos e \cos w_o t \cos w_r t \tag{10a}$$

$$\frac{V_N}{V_o} = f_N(t, j) = -\cos j \,\sin e \cos w_o t + \sin j\,(\cos e \cos w_o t \sin w_r t - \sin w_o t \cos w_r t) \tag{10b}$$

$$\frac{V_P}{V_o} = f_P(t, j) = -\sin j \,\sin e \cos w_o t - \cos j\,(\cos e \cos w_o t \sin w_r t - \sin w_o t \cos w_r t) \tag{10c}$$

Therefrom, V_I and w_N immediately follow. Figure 1 shows the 24-hour variation of V_I and w_N on typical dates of the most relevant experiments (the horizontal axis is local apparent time, LAT = $t + 12$ hr). Contrary to M-M's expectations, there may exist strong variations during a single session. For instance, during a one-hour period in Cleveland, from 12:00 to 13:00 on July 9, V_I changes from 18.1 to 16.8 km/s. The southeasterly direction w_N changes from –151.5° to –176.4°. Likewise, for the afternoon session, in the period from 18:00 to 19:00, V_I changes from 28.4 to 29.6 km/s, the westerly direction w_N changes from +96.0° to +86.0°. Several remarks arise from Figure 1:

Table 1. Miller measurements at Mt. Wilson

		Bounds 95 % C.L.	
Date	**VI, km/s [7]**	**Lower, km/s**	**Upper, km/s**
April 01/25	10.1	9.1	11.1
Aug.01/25	11.2	10.2	12.2
Sept. 15/25	9.6	8.6	10.6
Feb. 08/26	9.3	8.3	10.3

Remark A. The magnitude of V_I and the position of the peak is not the same from one turn of the interferometer to the next within a session, much less from session to session.

Remark B. Hence, results from different sessions, even in the same day cannot be averaged. Furthermore, within a given session, readings at a given Δw, but at different turns, ***are not*** trials at the same observable. The average of such readings necessarily is smaller than the maximum reading.

Remark C. Magnitude of V_I drifts during a given session, and within each turn, leading to a hitherto unrecognized component of total drift. For instance, in the thermally controlled experiments of Illingworth, the readings drifted within each turn (see Table II [13]).

Remark D. Experiments carried out at different latitudes and times do not necessarily produce the same results.

Remark E. In general, the daily variation of V_I (considering orbital motion V_o only) does not have a sinusoidal shape. Of course, results are not necessarily the same when solar motion is added.

2.3 Summary review of all M-M experiments

Original Michelson-Morley experiment. M-M obtained their noon and afternoon curves *via* an incorrect inter-session average noted by Hicks [23] (see 2.1 above). M-M concluded that *"the relative velocity of the earth and the ether is probably less than one sixth the earth's orbital velocity, and certainly less than one fourth"* [2, page 341]. They do not state the method followed to obtain *D*. Assuming that they measured the amplitude, then 5 km/s ≈ V_I < 7.5 km/s. Despite the incorrect averaging procedure (SE1), and despite the absence of error bounds, this is a non-zero result.

Hicks corrected M-M results for drift and SE1. From the plot ΔZ—Δw, he concluded that curves were qualitatively consistent with eq. (5) above [23, page 37]. These are the same curves reported by Miller as fig. 3 [7, page 206], and recently reproduced by Vigier [3, fig.1]. Miller measured the amplitude of M-M curves with his mechanical harmonic analyser to obtain *"a velocity of 8.8 km/s for the noon observations, and 8.0 km/s for the evening observations"* [7, page 207]. As expected, elimination of the cancelling error (SE1) leads to speeds higher than those obtained by M-M. Then the observations of M-M are about 50% of the expected V_I for the noon session, and about 28% for the afternoon session (see Figure 1.a). It is a pity that neither M-M, nor Miller reported statistical errors for these measurements. In section 3 we apply Illingworth's method to M-M data, and obtain results of the same order of magnitude, plus the corresponding errors.

Morley-Miller experiments. In the repetition by Morley and Miller (July 1904) [5], they incorrectly averaged morning and afternoon curves, thus ignoring diurnal variation of V_I and w_N (Remark B above).

The previous mistake was later corrected by Miller himself to find *that "the morning and evening observations each indicate a velocity of ether drift of about 7.5 km/s"* [7, pages 216-217]. Note the same order of magnitude as in the original M-M experiment.

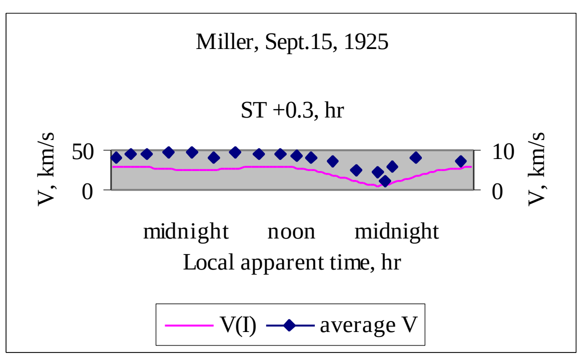
Miller experiments. He carried out a life-long series of experiments at Cleveland and Mt. Wilson [6-8]. To check seasonal effects, Miller measured V_I at diferent epochs with the results in Table 1. The probable error for these measurements is ±0.33 km/s [7, page 238]. The standard interpretation of such statistical error (and, the only correct one as far as we know) is that the true speed, say for the April 1 experiment, falls in a band from 10.1 –0.33 to 10.1 + 0.33 km/s at a 50% confidence level (C.L.) [29]. Of course, a matter as delicate as the nature of space and time must be solved with higher confidence. Hence, Table 1 shows error bounds at 95% C.L. for normally distributed observational errors.

Miller obtained the speeds in Table 1 by fitting a sinusoidal curve to his observations (see his figures 22 and 26 [7]; Figure 22 was reproduced as Figure 3 by Vigier [3]). However, as noted in Remark E, the curve produced by orbital motion is not sinusoidal (although for the July measurements in Cleveland it looks so). Miller's observations are qualitatively closer in shape to our curves 1.c) to 1.f) than to the sinusoidal curves fitted to them. This is exemplified with the September 15 curve shown in Figure 2.

The scale for Miller's curve is about 30% of the scale for V_I. The corresponding speed is thus consistent with the values in Table 1. This difference in amplitude is still an open question (Miller suggested solar motion $\boldsymbol{V}_s$), that may be associated with the intra-session averaging (see Remark B in 2.1 and 3.2 below). A final answer must be provided by new experiments.

Shankland *et al.* applied two statistical tests to Miller's data to conclude that *"there can*

Figure 2. Variation of the projection of orbital speed at the latitude of Mt. Wilson, during September 15, given by eqs. 10 (see main text). The points are the average observations reported by Miller [7] (for a more recent source, see fig 3, Vigier [3]). Scales for predicted and observed velocites are on the left and on the right, while time scales are below and above respectively. Observed values are about 1/3 V_I .



be little doubt that statistical fluctuations alone cannot account for the periodic fringe-shifts observed by Miller"[28, page 171]. Apparently, Shankland *et al.* were not aware that phase angle w_N was epoch-dependent, see figures 1.c) to 1.f). They stated: *"the four curves should have a common maximum (or minimum) at i = 1 [i.e., Dw = 0]; only the amplitude may be different at different epochs"*[28, page 172] (original emphasis). Shankland's mistake is another manifestation of SE2. Thence, they concluded that Miller results were experimental artifacts. On the contrary, Miller's results are clear confirmation of eqs. (1), (2) and (10) in this paper.

Piccard and Stahel's experiments. From 96 turns of an interferometer in a balloon over Belgium they obtained a speed of 6.9 km/s with a probable error of 7 km/s. According to conventional statistical practice [29], the result simply means that at 50% confidence level the true speed is in the interval from 0 to 13.9 km/s. Moreover, there is no reason to believe that one particular value (say, 0 km/s, or 13 km/s) is more likely than another. Then, Piccard and Stahel result is completely consistent with those of Miller (see Table 1). Surprisingly, they concluded quite differently: *"Nous n'avons donc pas pu déceler un vent d'éther"*[9, page 421] (original emphasis). However, they added as an afterthought: *"Toutefois, notre limite de précision ne suffit pas pur confirmer ou réfuter les mesures de Miller."*

They repeated the experiment in Brussels. Their results are (translating from French*): "60 turns of the apparatus produced an average displacement of 0.0002 ± 0.0007 fringes, which are incompatible with Miller's results"*[10]. Not so. Using eqs. (8) and (9a) for their equipment, we get 1.7 ± 3.1 km/s. Assuming that 3.1 km/s was a probable error (as in the balloon experiment), a one-tailed test says that true speed was lower than 9.3 km/s at 95% C.L. Again, compatible with Miller's results. Brylinski [30] long ago criticized the interpretation of Piccard and Stahel on similar grounds. They unconvincingly replied thus (our translation): *"all our measurements have given ether winds lower than the probable error of our measures, so that we cannot conclude in favor of Miller, as Brylinski does."*[31] This misinterpretation of statistical data consitutes **a *third systematic error (SE3)*** that unfortunately continued up to Shankland in the fifities: *"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****"*[28, page 167] (emphasis added).

Piccard and Stahel repeated the experiment at Mt. Rigi in Switzerland. From 120 turns of the interferometer they found (translating from French): *"a sinusoidal curve whose amplitude is 40 times smaller than the curve that Miller would have predicted, all these within the limits of our probable errors.... this curve corresponds to an ether wind of 1.45 km/s."*[11] Again, note SE3. Also, this is not a zero speed. Unfortunately, they did not report the probable error.

In this experiment Piccard and Stahel reported that the phases for the session between 05:00 and 06:00 were distributed as if random, between 0 and $\pi/2$. From Figure 1.g), during such observational period the w_N associated with orbital motion changed sign and magnitude from + 10.5° to –4.0°

Kennedy's experiment. In contrast with Miller's long optical path, Kennedy developed an accurate small apparatus, where temperature could be controlled during an experimental session [12]. He carried out the measurements at Pasadena, and succinctly reports that "*there was no sign of a shift depending of the orientation.*" To our knowledge, ***this is the only experiment that ever reported a null result***.

However, since Kennedy was looking for shifts produced by 90° rotations from a reference position, eq. (7a) tells that, if RA points north, the expected shift tends to zero when $\cos 2w_N \approx 0$, *i.e.* when w_N is close to being a multiple of 45°. For September 16 at Pasadena this occurs four times during the day, around 02:30, 08:50, 17:05 and 18:30 local apparent time (see eqs. 10). Kennedy says that "*the experiment was performed....at various times of day, but oftenest at the time when Miller's conclusions require the greatest effect*" which for "*the middle two weeks of September, when the present work was done corresponds to local solar times varying from 6.30 A.M. to 5.30 A.M.*" [12, page 628]. This time period seems to be midway between 02:30 and 08:50, but Kennedy does not explicitly state the *initial* orientation of his interferometer, so that we cannot draw any definite conclusions.

At any rate, Kennedy's qualitative report is quite surprising in view of the detailed experiments carried out by Illingworth with Kennedy's own interferometer and at the same location, as discussed next.

Illingworth experiment. Results are reported in a detailed paper [13], that allows a similarly detailed analysis. As demonstrated by reviews above, most papers exhibit an inconsistency between observation (a non-zero velocity) and interpretation (a null result). This paper is no exception. Illingworth's abstract reads: "*The ether drift experiment as performed by Kennedy with a reduced optical system in helium has been repeated with the same apparatus somewhat modified and the same results obtained. The interferometer has been improved by resilvering the mirrors so that 1/1500 of a fringe shift could be detected by an observer with good eyes, and 1/500 by an observer with poorer eyes. Additional readings, which eliminate steady thermal shifts of the fringes, have been made and these show no ether drift to an accuracy of about one kilometer per second.*"

As usual in other papers, a high experimental resolution is suggested by quoting small fringe-shifts. However, Illingworth's Table I immediately tells us that the quoted sensitivity (1/1500 to1/500 fringe-shift) is not that good: 3 to 5 km/s. This velocity resolution is from 10% to 17% of the velocity to be measured! (Not an excellent resolution as suggested by the experimenters).

Illingworth did not measure the whole ΔZ *vs.* Δw curve, but estimated the average displacement $\Delta Z(p/2)$ of eq. (6) by $D = ky$, where y is the statistic

$$y = \frac{x_E + x_W}{2} - \frac{x_N + x_S + x_N}{3} . \tag{11}$$

Illingworth correctly notes (page 695) that the second term "*eliminates the effect of steady thermal drifts*" during a rotation of the interferometer. As noted in Remark C of section 2.2, this "thermal" drift may contain a new intrinsic component due to earth's rotation. Note also that the average in the first term is required to eliminate first-harmonics effects.

TABLE 2 **Summary of Illingworth's results**

Time		05:00		11:00		17:00		23:00	
		y_n for each session in terms of weights*							
Session	n	1A	1B	2A	2B	3A	3B	4A	4B
July 1	1	0.12	-0.33	0.35	-0.11	0.12	0.22	-0.05	0.12
July 2	2	0.57	0.12	-0.21	-0.18	-0.28	-0.23	0.09	0.09
July 3	3	0.00	0.00	-0.03	-0.26	-0.72	-0.40	-0.63	-0.03
July 4	4	0.10	-0.22	-0.15	0.06	-0.08	0.02	-0.22	-0.13
July 5	5	0.32	0.00	-0.11	0.19	0.09	0.08	0.00	0.12
July 6	6	-0.01	-0.05	0.24	0.10	0.15	0.15	-0.20	-0.02
July 7	7			-0.07	-0.03	0.15	0.07		
July 8	8			-0.03	0.02	0.18	-0.18		
July 9	9			-0.03	0.08	0.03	-0.03		
July10	10			0.00	0.12	-0.05	0.05		
ILLINGWORTH'S CALCULATIONS BASED ON SIGNED VALUES OF y_n*									
Inter-session average $<y_n>$		+0.18	-0.080	-0.0040	-0.0010	-0.041	-0.025	-0.17	+0.025
V, km/s		+2.12	-1.41	-0.32	-0.16	-1.01	-0.79	-2.07	+0.79
Probable error,km/s		1.23	1.06	0.96	0.87	1.23	1.01	1.33	0.84
Upper bound 50% C.L.		+3.35	-0.35	+0.64	+0.71	+0.22	+0.22	-0.74	+1.63
Lower bound 50% C.L.		+0.89	-2.47	-1.28	-1.03	-2.24	-1.80	-3.40	-0.05
*Illingworth definition of y has the reverse sign of eq. (11) in main text.									

For session n, y_n is the *intra-session* "*average displacement, due to orientation, in terms of weight*" taken over the ten turns of the session (see Table II). The *inter-session* average is $<y_n>$, reported by Illingworth in his Table III (page 695), taken over N different sessions. Therefrom, Illingworth calculated velocity from "*$V = 112D^{1/2}$ where D is the fringe displacement caused by a rotation through a right angle*" [13, page 695], where $D = k<y_n>$, with k = 1weight/500 fringe. According to Illingworth, D (and y) may be positive or negative, which immediately leads to difficulties. For unknown reasons, when V is imaginary (*i.e.*, negative D), Illingworth reported a negative velocity. Illingworth's Table III is repeated as Table 2 below.

Turning now to statistical errors. Illingworth reported a probable error, in terms of fringe-shift, based on a normal distribution. In Table 2 we have converted probable errors into km/s, using the equation for V in the previous paragraph. Assuming with Illingworth that negative velocities are meaningful, we also show in Table 2 the ensuing upper and lower bounds at 50% C.L.

As noted above for the Piccard and Stahel case, the standard interpretation of statistical errors is that the true ether velocity is within the error bounds at some specified C.L. For instance for session 1A at 11 a.m., the average velocity is 2.12 km/s, the true velocity being between 0.89 and 3.35 km/s at 50% C.L. Of course, for higher confidences the uncertainty band is wider. Similarly for the other seven sessions. Clearly, Illingworth results were not null.

However, Illingworth was not very certain as to what the interpretation should be, as exemplified by the following rather obscure paragraph from his conclusions: *"Since in over one half the cases the observed shift is less than the probable error the present work cannot be interpreted as indicating an ether drift to an accuracy of one kilometer per second"* (page 696).

Michelson, Pease and Pearson experiment. They reported their findings in a sketchy paper [14], with no error bounds, concluding that: *"The results gave no displacement as great as one-fifteenth of that to be expected on the supposition of an effect due to a motion of the solar system of three hundred km/s"* (paper in *Nature*). Since they report a relative displacement, the corresponding solar velocity is then $300(1/15)^{1/2}$ = 77.5 km/s, which is not null by any means.

In the *JOSA* paper, they say that the relative displacement was one-fiftieth (= 1/50, a misprint??), leading to a solar velocity of 42.4 km/s. Again, a clearly non-null speed.

Joos experiment. This is the last experiment in the class of M-M experiments. The equipment was carefully designed and fringe shifts were photographically registered. The negatives were directly read using a transmission photometer [15]. Joos personally analysed the results of May 10, 1930. His Figure 10 (page 403) exhibits an impressive regularity on $2w$, as qualitatively expected from eqs. (1) and (3) above.

On that date Joos made measurements every hour. As can be seen from Figure 1.g), the values of V_I and w_N vary from one hour to the next, so that Joos 'curves for individual measurements do not need to have the same amplitude and shape. Indeed, Joos observed such differences (see his Figure 11, page 404). Unfortunately, Joos did not expect such variations (again, another instance of SE2), so that he rejected all large amplitudes as due to experimental errors (he particularly mentions session 11 at 23:58). From the smaller amplitudes, Joos obviously obtained a small velocity that he reported (translating from

TABLE 3. Inter-session averages based on absolute values $|y_n|$

REINTERPRETATION OF ILLINGWORTH EXPERIMENT								
Session	1A	1B	2A	2B	3A	3B	4A	4B
$<|y_n|>$	0.187	0.120	0.122	0.115	0.185	0.143	0.198	0.085
V_A, V_B, km/s	2.16	1.74	1.75	1.70	2.15	1.89	2.23	1.46
Upper bound 95% C.L., km/s	3.24	2.55	2.26	2.06	2.87	2.39	3.31	1.85
Lower bound 95% C.L., km/s	0	0	1.00	1.24	1.02	1.21	0	0.92
V_I, km/s	2.36		2.05		2.42		2.33	
? ?$_N$|,°	16.4		21.6		18.8		11.6	
Projection of V_o, July 9	time 05:30	28.7 km/s	time 11:30	17.1 km/s	time 17:30	27.1 km/s	time 23:30	22.1 km/s
Ratio Projection V_o/V_I	12.2		8.3		11.2		9.5	

German) as *"an ether wind smaller than 1.5 km/s"* (page 407). Even then, this is not a zero velocity.

Reinterpretation of some experiments

3.1. Inter-session averages

Illingworth's paper. As noted in section 2, the correct method is to use $|y|$, and always report a positive V (which is the magnitude of velocity). We have applied this correction to table 2 above (Illingworth's Table III). Results are in Table 3, where we have noted that the calculated velocity is either V_A or V_B, not V_I as mistakenly pressumed thus far (recall eqs. 7 and 9).

It may be seen that velocities are now clustered in the range [1.4, 2.2] km/s, and not around zero, thus vindicating Hicks [23] prediction that inter-session averages over the two calibration families would lead to an incorrect value of zero (recall 2.1 above).

Since N is small, we have calculated the error bounds using a two-tailed Student's t-test at the 95% C.L., rather than a normal distribution. As shown in Table 3, the lower bound is higher than zero in over half the cases.

From eqs. (9b) and (9c) we can obtain the phase angle from $\tan(2w_N) = (V_B/V_A)^2$. Thence V_I follows (see table 3). The values for V_I are now in the narrow interval [2, 2.4] km/s, for measurements taken at different times of day. Table 3 also shows the projection of orbital speed (from eq. 10), which is not the same for all sessions.

The ratio of the projection of V_o to V_I is also in Table 3, suggesting that inter-session average velocities are about a factor of ten lower than expected by M-M. This value should be contrasted with the much higher ratio between fringe-shifts given in Table I of Shankland *et al.* [28] for Illingworth's experiment: 175. Evidently, the relevant ratio is the one obtained here in terms of velocities.

Original Michelson-Morley paper. We have used Illingworth statistic y (eq. 11) to obtain V_A from original M-M data. The average for the three noon sessions is 6.22 km/s with a standard deviation on the mean of 0.93 km/s. For the 18:00 observations the avearge is 6.80 km/s with a much larger standard deviation on the mean of 2.49 km/s. These values are compatible with the original findings of M-M and with Miller's recalculation. Again, clearly non-null results.

3.2 Intra-session averages

Illingsworth's paper. Since there are changes in the sign of D from session to session, a final question arises. Were there changes in the calibration of the interferometer during a given session? The answer is most likely positive. Illingworth states that *"starting with the field of view exactly balanced it was noted how many weights were removed or added to balance again after a rotation of 90°"* (page 694), which suggests that the apparatus was balanced at the beginning of each turn. Indeed, his Table II shows the readings for session 2A on July 9, 1927, where each turn starts with a reading of zero with the RA looking north, and ends with a non-zero value with the apparatus at exactly the same position after one turn (which is the starting position for the next turn). However, for the next turn the starting reading is zero again.

Table 4 shows the values of y_j for each individual turn j in session 2A of July 9, 1927,

TABLE 4. Intra-session variability of velocity

	Miller's session at 03:02, Sept. 23, 1925				Illingworth's session 2A, July 9, 1927		
Turn, j	y_j	$\|y_j\|$	V_A, km/s		y_j*	$\|y_j\|$	V_A, km/s
1	-1.17	1.17	9.62		-0.50	0.50	3.54
2	-0.83	0.83	8.13		+0.33	0.33	2.89
3	-0.83	0.83	8.13		-0.33	0.33	2.89
4	-0.33	0.33	5.14		-0.33	0.33	2.89
5	-3.00	3.00	15.42	adjust	-0.83	0.83	4.57
6	+0.50	0.50	6.30		+1.17	1.17	5.41
7	-1.67	1.67	11.50		+0.50	0.50	3.54
8	-0.50	0.50	6.30		+0.17	0.17	2.04
9	-1.67	1.67	11.50	adjust	0.00	0.00	0.00
10	-0.17	0.17	3.64		-0.50	0.50	3.54
11	-1.50	1.50	10.91				
12	-0.33	0.33	5.14				
13	+0.83	0.83	8.13				
14	+0.67	0.67	7.27				
15	-2.00	2.00	12.59				
16	-1.00	1.00	8.90				
17	+0.33	0.33	5.14				
18	-0.83	0.83	8.13				
19	-0.50	0.50	6.30	adjust			
20	-0.50	0.50	6.30				
Average	|-0.72|	0.96	8.22		|-0.03|	0.47	3.13
U.B. ** 95%CL	1.17	1.29	9.61		0.45	0.71	4.17
L.B. ** 95% CL	0.28	0.63	6.83		0	0.23	2.09
Results in km/s							
Average	7.58	8.72	8.22		0.91	3.42	3.13
U.B.** 95% CL	9.63	10.10	9.61		3.36	4.21	4.17
L.B. ** 95% CL	4.70	7.06	6.83		0	2.42	2.09
* Illingworth's y has reversed sign relative to eq. (11)							
** U.B.= upper bound, L.B.= lower bound, CL= confidence level							

calculated with eq. (11) from Illingworth's table II. It is immediately seen that the sign changes several times, thus supporting our conjecture that the calibration family did not remain constant throughout the session. Illingworth's average $y_9 = \langle y_j \rangle = -0.03$ is quite different from the, in our view, correct average $\langle |y_j| \rangle = 0.47$. Indeed, the former leads to $V_9 = 0.87$ km/s, while the latter yields $V_9 = 3.43$ km/s. Velocity was also calculated for each turn, the average speed being 3.13 km/s, which coincides with the value obtained via $|y_j|$. The corresponding 95% error bounds are shown in Table 4.

Previous velocities must be compared with V_A calculated with eqs. (9b) and (10). Assuming that calibration and preparation took 20 minutes, and taking into account the difference between civil time and local apparent time (LAT) at Pasadena (about 10 minutes), let the actual observation period be LAT from 11:30 to 11:45. During this time period, on July 9 in Pasadena, V_A varied from 2.44 to 6.68 km/s. **Surprisingly, Illingworth's results in Table 4 coincide with the projection of orbital speed.**

At any rate, even is this equality is a mere coincidence, the velocity uncertainty band [2.4, 4.2] km/s is close to Miller's uncertainty band [7.1, 10.1] km/s discussed below. The latter also coincides with the general order of magnitude obtained by M-M, Morley-Miller, Miller, and Piccard and Stahel in Belgium.

Miller's paper. Although there is enough data to plot a complete curve, in order to show that Illingworth's method of data analysis based on y is consistent with Miller's method based on the whole ΔZ-Δw curve, we calculated Miller's velocity for his session of September 23, 1925 at 03:02 [7, fig. 8, page 213], using Illingworth's procedure. There were 20 turns in the sessions, for each individual rotation j we calculated y (eq. 11), $|y|$ and V (see Table 4). Assuming that Mt. Wilson Observatory is approximately located at longitude 117.5º, the LAT for the beginning of this session was about 03:12.

The average velocity based on $|y|$ and V_A are 8.72 and 8.22 km/s, which are the same at the 95% C.L. (bounds also shown in Table 4). These values must be compared to V_A given by eqs. (9b) and (10) which varied from 8.00 to 11.88 km/s during the observation period (LAT from 03:15 to 03:30). *Again, quite surprisingly, Miller's observations coincide with the projection of orbital speed at the same time and location.*

4. Concluding remarks

In this note we analysed each one of the individual papers in the class of M-M experiments to find that all observed a non-zero velocity, but—with the notable exception of Miller—also all interpreted their results as zero. The qualitative shape of the curves produced by rotation of the interferometer exhibits the theoretical $2w$-dependence (with amplitude smaller than expected) in the following cases: original M-M [2], Miller [7], Piccard and Stahel [11]. The shape of the curves observed by Miller over 24-hr periods is qualitatively similar to the shape of the curve depicting the variation of the projection of orbital speed on the plane of the interferometer (fig 1 and 2). However, the observed amplitude is only about 30% of the theoretical curve. This means that qualitatively there is consistency between observation and absolute space, up to some constant factor.

On the quantitative side, the overall conclusion is that the speed was always different from zero. The inter-session averages were consistently lower than expected from orbital motion alone. However, the difference is not tens to hundreds of times lower as suggested in the literature (see for instance, Table I of Shankland [28]), but somewhere from 2 to 10.

However, for the only two sessions wholly reported in the literature (Table 4) the calculation based on the individual rotations of the interferometer completely agree with our predictions (eq. 10) based on earth's orbital motion.

It would be an extraordinary coincidence indeed, if the only two complete sessions reported by different experimenters (Miller and Illingworth) constitute an experimental artifact. This implies that the standard procedure of averaging individual fringe-shifts to obtain a session average, and then to average them again to obtain an inter-session average, simply averages away the observation sought after. This result was foreseen by Hicks [23] a century ago.

Therefore, it may be conjectured that if the data reduction of all M-M experiments were carried out calculating the speed associated with each turn, and then averaging, the results would be consistent with earth's orbital motion (as in the only two sessions available). This is a matter to be confirmed by experiment, either by interferometic methods, or even better by a direct measurement of time-delay to confirm eq. (1). For an apparatus similar to M-M original interferometer A = 0.37 femtoseconds, while A = 1 fs for the Miller interferometer [7].

From a Popperian view-point [32], a single experiment suffices to demonstrate that absolute space does not exist, regardless of the existence of other evidence consistent with that notion, say, the Escanglon experiments [3,7]. Hence, the emphasis in this paper is on demonstrating that the results from M-M experiments are consistent with absolute space.

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