The Empirical Evidence for the Relativistic Theories of Lorentz, Poincaré and Einstein: the 1881 and 1887 Michelson Experiments, Revisited

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The present paper begins with a quick revision of the undoubted connection between the inception of the theory of relativity and the "null" interpretation given to the experiments carried out by Michelson in 1881 and 1887. Lorentz always hoped that there would be a loophole in Michelson's analysis that would make his length-contraction hypothesis unnecessary. In this context, section II presents for the first time my detailed analysis of the 1881 experiment, and with this new information, revisits the 1887 Michelson-Morley experiment in section III. The overall result is that the pioneering experiments were designed to test only a particular

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value of solar motion, a hypotheses not supported by the empirical observations. However, the conclusions from both experiments overstepped the range of validity imposed by the experimental design itself. Thus, the crucial 1881 and 1887 experiments *never* were “null” and consequently there is no contradiction with the “positive” outcome of our own 2002-2005 MMMM experiment at CIF, in Bogota (Colombia): a successful first-time determination by optical means of velocity of Earth relative to a Newtonian frame of reference.

Dedication:

My long-standing connection to professor Valeri Dvoeglazov

In the editorial introduction to the timely book entitled *Einstein and Poincaré: the Physical Vacuum,* edited by my friend professor Valeri Dvoeglazov in 2006 [1], he noted that “unfortunately, there are almost no insights into the obvious relations between the ether concept and the recently discovered ‘dark matter’ and ‘dark energy’”. The present writer has been working in that direction for the past twenty years, and has postulated an energy-like dark matter (DM) fluid (or aether, if you wish) formed by discrete DM entities called sagions, which are the carriers of all energy and linear momentum in the universe; the DM fluid obeys the Lorentz invariant homogeneous Klein-Gordon equation (HKGE) proposed by Louis de Broglie as a basis for a quantum theory compatible with Einstein’s general theory of relativity. Our novel solutions in spherical coordinates (r,θ,ϕ) for the HKGE are formed by a background time-independent scalar field $B(r, \theta,\phi)$ and an entangled scalar field $E(Ct/r,\theta,\phi)$ [2], whose value is the same for all time-distance transformations between coordinate systems in relative motion (Doppler, Lorentz, Poincaré, or Einstein), thus side-stepping many theoretical controversies about the various approaches to relativity theory. Nonetheless, the present writer feels it is very important to clarify some issues that are still open at the empirical foundations of relativity, especially after his own empirical

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research [11-13] produced results opposite to the claimed "null" results of the 1887 Michelson-Morley (MM) experiment [3].

In 1997 at York University in Toronto, Canada, in one of the earlier symposia to honor Jean-Pierre Vigier —formerly the scientific assistant to Louis de Broglie, and at that time the last living connection to the great physicists of the early 20th century—, I had the pleasure of meeting and exchanging ideas with Valeri Dvoeglazov for the first time. One of my presentations there, a critical review of all MM-type experiments up to 1930 [4], is very relevant for present paper. Two years later, in 1999, Valeri and his colleagues at the then incipient School of Physics of the Autonomous University of Zacatecas (Mexico) organized a successful international workshop on CPT, neutrinos, and Lorentz invariance. They were kind enough to invite me to participate with a couple of papers, one of them my first approach towards a unified fluid and field theory [5], that I continued developing at recent Vigier Symposia [6-8]. My present view is that the discrete energy-like DM aether described at the Vigier Symposia may be identified with the fluid underlying Louis de Broglie's double-solution theory.

Last time that I met Valeri in person was at the Vigier Symposium held in 2000 at the University of California in Berkeley—my doctoral alma mater. On that occasion we had lunch on or near Telegraph Avenue and talked about my incipient photon model [9], that arose from ideas sketched at Zacatecas [5].

During the past twenty years Valeri has been kind enough to include contributions of mine in several of his books and journals on which he has been an invited editor. For instance, for the *Annals of the Louis de Broglie Foundation* in Paris I contributed a paper [10] calculating the effect of solar motion on the fringe-shifts in a MM interferometer, which provided

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the theoretical basis for the design of my repetition [11-15] of the MM experiment. Many thanks Valeri for your friendship and for that long-standing and helpful connection.

I. Introduction: the "null" interpretation of the MM experiment

An unprecedented change in the way scientists view Nature occurred over the short lapse of forty years from mid-1880s to mid-1920s; here we concentrate on the empirical foundations of relativity [3, 16]. In the general overview of the MM-type experiments [4] written in 1998, some issues were not identified, and some were not stressed enough. Their importance became obvious to this author during the design and implementation of our own MM experiment from 2002 to 2005. The present paper describes for the first time my detailed evaluation of the 1881 Michelson experiment [16]. A remarkable aspect is that several weak features regarding data collection and data reduction from the 1881 experiment were re-used in the 1887 MM experiment.

I.1. Lorentz and Michelson's 1881 and 1887 experiments

The Dutch professor of physics Hendrik Antoon Lorentz followed Michelson's research closely from the beginning, and in 1886 sent him a correction pertaining to the analysis of the optical path along the arm that was *presumably* transversal to the direction of motion in the 1881 experiment at Postdam. In the process of designing the 1887 experiment, Michelson acknowledged the validity of the criticism by Lorentz and modified his analysis accordingly [3, p.335].

To avoid, or at least to minimize misunderstandings, the reader will kindly forgive me for being explicit and direct in my

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comments, and for literally emphasizing the most significant issues, and even words as above.

Lorentz postulated the now famous length-contraction as the basis of his 1895 theory of the electron [17]; but he had doubts, as attested by a letter to Lord Rayleigh dated 18 August 1892 in which Lorentz asked: *"Can there be some point in the theory of Mr. Michelson's experiment which has as yet been overlooked?"* [18, p.32]. As argued here, our answer is positive: Yes professor Lorentz, indeed. MM only recorded the *residual fraction* of a fringe-shift, instead of the *total* fringe-shift as it should have!

I.2. Poincaré and Michelson's experiments

The French mathematical physicist Henri Poincaré does not mention Michelson by name in his 1905 book; he writes simply: "many *experiments* have been made *on the influence* of the *motion of the Earth*. The results have always been *negative*. But if these experiments have been undertaken, it is because we have not been certain beforehand… we might expect to find accurate methods giving positive results. I think that such a hope is illusory… I do not believe, in *spite of Lorentz*, that more exact observations will ever make evident anything else but the *relative displacements of material bodies*. *Experiments* have been made that should have disclosed the terms of the first order; the results were *nugatory* … Then *more exact experiments* were made, which were *also negative*" [19, p.171-172], emphases added. Poincaré often mentions Lorentz, and at the end of the book he states that "Lorentz's theory is very attractive" and that according to Lorentz it is not possible to measure "absolute velocity, but their relative velocity *with respect to the ether*, so that the principle of relativity is safe" [19, p.243-244], emphasis in the original.

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Comparing the two quotations above, it follows that, regarding motion, for Poincaré the aether had the status of a "*material body*". This is confirmed when, in his Socratic style, Poincaré asks: "Does our ether actually exist? We know the origin of our belief in the ether. If light takes several years to reach us from a distant star, it is no longer on the star, nor is it on the Earth. It must be somewhere, and *supported*, so to speak, by *some material agency*" [19, p. 169], emphases added.

A short digression, there is an immediate counter-question back to Poincaré: where does ether exist? Or, where is it contained? For Newton the answer was clear: in absolute space. In the context of propagation of gravity, aether had for Newton *both active* and *passive* roles as evidenced by the phrases "by and through" and "agent acting constantly" in the third letter to Bentley dated 25 February 1692/3: "that one *body* may *act* upon another *at a distance through a vacuum, without the mediation of anything else, by and through* which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. *Gravity* must be *caused by* an *agent acting constantly* according to certain laws, but *whether this agent is material or immaterial* I have left to the consideration of my readers" [20, p.54], emphases added. Note also that Newton did not decide between a material or immaterial ether.

Returning to Poincaré and Michelson, in *Science and Method* published in French in 1908 and translated into English in 1914 [21] Poincaré mentions Michelson several times, including a repetition of Fresnel's experiment (page 216), a subject not directly relevant here. The section entitled The Principle of Relativity begins thus: "But if the *ether* is *not displaced by the Earth's motion*, is it possible by means of optical phenomena to

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demonstrate the absolute velocity of the Earth, or rather its velocity in relation to the *motionless* ether? *Experience* has given a *negative reply*, and …" [21, p.217], emphases added. What is a "*motionless*" ether? This strange object is mentioned again on page 218. What kind of aether had Poincaré in mind? It cannot be a fluid that *certainly* would be "*displaced*" by motion of Earth, and whose elementary particles would be in permanent motion, similarly to the molecules of oxygen and nitrogen in air. Was Poincaré a supporter of the far-fetched notions of solid and crystal aethers [22] that proliferated in the second half of the 19th century? Be it as it may, let us turn to the "*negative reply*" from experience.

Poincaré continues thus: "Whatever be the method employed, we shall never succeed in disclosing any but relative velocities; I mean the velocities of certain material bodies in relation to other material bodies." [21, p.217] Yes, agreed. But nothing forbids us from choosing the far away Newtonian "fixed stars" as an anchor to define a reference or preferred frame. Accordingly, at the beginning of this century the present writer succeeded in measuring, by optical means, the velocity of Earth relative to the said preferred frame [11-15].

Then, Poincaré finally mentions an experiment: "Indeed, when the source of the light and the apparatus for observation are both on the Earth and participate in its motion, the experimental results have always been the same, whatever be the direction of the apparatus in relation to the *direction of the Earths's orbital motion*" [21, p.217], emphasis added. This description may refer either to the 1881 Michelson experiment or to the 1887 Michelson-Morley experiment. The phrase in italics pinpoints the weakest part in those experiments, and will be discussed at length in the present paper.

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Next, Poincaré discusses aberration and length-contraction, which may be "easy to explain so long as we neglect the square of aberration… But one day Michelson thought out a much more delicate process. He introduced rays …reflected by mirrors. Each of the distances being about a yard…" [21, p.218-219]. No doubt, this apparatus is the interferometer used by Michelson in the 1881 experiment, whose arms were one meter long (see section II below for previously unnoticed shortcomings in that experiment). The name of Michelson is mentioned approvingly three more times in that section [21, p.220-221].

I.3. Einstein and the Michelson experiments

Einstein's abundant correspondence became available in English translations by the end of past century [23-25] and clearly shows that he had been aware of the "null" interpretation of the MM experiment since his time at ETH-Zurich [26]. Einstein mentioned Lorentz for the first time in a letter to his girlfriend Mileva Marić dated 28 December 1901: "I now want to buckle down and study what Lorentz and Drude have written on the electrodynamics of moving bodies" [23, p.189-190], [24, p.72 and 100]. Einstein was talking about Paul Drude's recent 1900 book entitled *Lehrbuch der Optick*, translated into English as The Theory of Optics, chapter VIII of which in the Dover edition begins thus: "The assumption which will be adopted here is that the *ether always remains completely at rest*. Upon this basis H.A. Lorentz has developed a complete and elegant theory" [27, p.457-482], emphasis in the original. Drude discusses in detail Michelson's 1881 experiment (pp. 478-481), and Michelson and Morley 1887 experiment in somewhat less detail (pp. 481-482).

Strangely enough, in his public papers Einstein always downplayed the influence of Michelson experiments, even in

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August 19/1952 at Michelson's birthday centennial: "The influence of the crucial *Michelson-Morley experiment* upon my own efforts has been *rather indirect*. I learned of it through H.A. *Lorentz's decisive investigation* of the electrodynamics of moving bodies (1895) [17] *with which I was acquainted before developing the special theory of relativity*. Lorentz's basic assumption of an *ether at rest* seemed to me *not convincing* in itself and also for the reason that it was leading to an *interpretation* of the result of the *Michelson-Morley* experiment which seemed to me *artificial*" [18, p.35], emphases added.

I.4. Miller and the "null" interpretation of the MM experiment

After Michelson moved to Chicago, Dayton C. Miller continued the interferometer experiments with Morley from 1902 onwards, and had thus first hand knowledge on the empirical details of the MM experiment; he explicitly stated that "the effect did not have the anticipated magnitude. However, and this fact must be emphasized, *the indicated effect was not zero*; … This is quite different from a null effect now so frequently imputed to this [MM] experiment" [28, p.206], emphasis in the original. This is consistent with my 1998 review of all MM-type experiments up to 1930 [4], in the sense that all classical experiments observed a small, but non-zero, motion of Earth relative to an aether presumably at rest in Newton's absolute space. Surprisingly, in the conclusions many papers stated that they obtained a "null" result.

II. The 1881 Michelson experiment critically revisited

Albert Abraham Michelson, an officer of the U.S. Army, very early demonstrated a great talent for experimental research in physics, and for further training he went to the laboratory of

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Hermann von Helmholtz in Berlin (Germany), where Michelson designed and carried out an experiment to measure the relative velocity between Earth and aether, assumed at rest relative to Newton's absolute space (see figure 1A). Measurements were initially in Berlin, but due to tram vibrations, the apparatus was moved to Postdam, a quiet location in the outskirts of the city [16].

II.1. Design of the 1881 experiment

Michelson treated the velocity of Earth as the vector addition of two components: (a) orbital motion with an approximate speed of 30 km/s along the plane of the ecliptic, plus (b) solar motion towards Hercules constellation, that for early April 1881 was at an angle of 26º relative to the terrestrial equatorial plane (see figure 1B). Being a naval officer, Michelson was quite aware that azimuth associated with daily rotation of Earth should be taken into account, and he explicitly noted that "if the apparatus be so placed that the arms point north and east *at noon*, the arm pointing east would coincide with the resultant motion, and the other would be at right angles" [16, p.125], emphasis added. Please note that this result holds if, and only if, the Sun *only moves* towards Hercules with a speed of 30 km/s. If Sun moves with a larger speed, or if it moves in a different direction, then *Michelson's expectations do not hold*. As it turned out, the fringe-shift in the NE-SW direction was +0.034, which according to Michelson "should have been zero. The numbers are simply outstanding errors of the experiment" [16, p.127]. This large unexpected shift, which was one-third the size of the predicted 0.1 fringe-shift (see next paragraph), was eliminated as unwanted linear drift (page 128).

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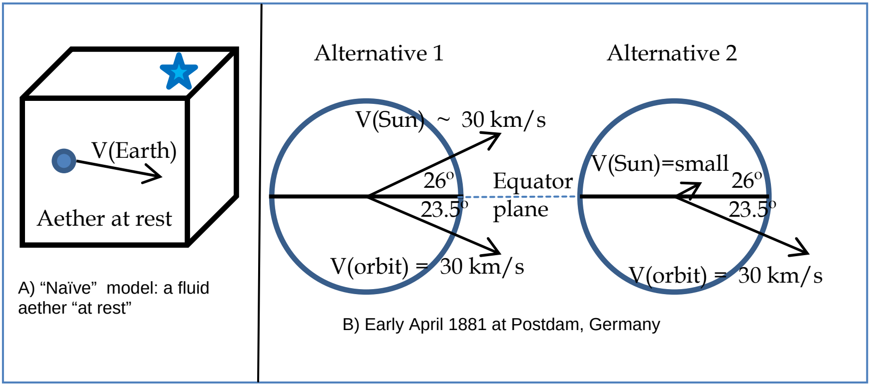

Figure 1. Michelson's analysis to design his experiment at Postdam in early April 1881 to test his hypothesis H-1881 (see text for details).

Since the speed of solar motion was unknown at that time, Michelson considered two alternatives: (1) magnitude similar to orbital motion, and (2) small magnitude relative to orbital motion. Michelson calculated the expected fringe-shift for his short one-meter arms interferometer at noon in early April at Postdam for each of the two alternative speeds and decided to take "the *mean of these two numbers* as the most probable … the displacement to be looked for is not far from one-tenth the distance between the fringes" [16, p.125], emphasis added.

From the foregoing quotations, it is evident that the 1881 Michelson experiment was designed to test *only* the following hypothesis that I will label *H-1881: Is it possible to measure with Michelson's interferometer the velocity of Earth defined as orbital motion around Sun plus solar motion towards Hercules at 30 km/s maximum speed?*

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Figure 2. Raw data and its mean value for the four series. Note a significant dispersion of readings at each orientation of the apparatus (further comments in the text).

From a logical viewpoint there is a third alternative to Figure 1, namely: a more complex solar motion having, in addition to velocity towards Hercules, other velocity components yielding a *resultant solar speed larger than 30 km/s, in a direction other than toward Hercules*. As we know today, solar speed is indeed quite large [29], at least a factor of ten higher than orbital motion. Michelson cannot be blamed too much for overlooking this third alternative in 1881, because at the time solar speed was considered small. However, since Michelson's mind was anchored to the notion that fringe-shifts were smaller than one fringe he could not correctly interpret some "apparent" drifts of fringe-shift, which he summarily eliminated (see subsection II.5 for further comments).

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II.2. Raw data from the 1881 experiment

In a table [16, p.126], Michelson reported the raw data he obtained

Table 1. Mean values and standard deviations for each orientation in the four series (readings in twelfths of the distance between fringes)

		Series 1		Series 2		Series 3		Series 4	
Orientation	º	Mean	SD	Mean	SD	Mean	SD	Mean	SD
N	0	12.3	7.0	15.6	3.2	7.4	7.0	14.2	2.9
NE	45	12.3	7.0	15.8	2.8	7.1	7.1	23.4	2.9
E	90	12.3	7.0	15.9	2.2	7.8	7.2	15.2	2.3
SE	135	4.8	7.4	16.3	1.9	7.7	7.2	15.7	2.4
S	180	11.7	7.3	16.1	1.8	7.9	7.6	15.3	2.1
SW	225	11.7	7.2	2.6	1.7	8.1	7.9	15.7	2.2
W	270	11.3	7.5	16.4	1.5	8.2	8.1	15.9	2.6
NW	315	10.8	7.9	16.5	1.0	17.4	6.2	16.8	2.7
Averages		10.9	7.3	14.4	2.0	9.0	7.3	16.5	2.5

in four series of measurements, each one formed by five revolutions of his apparatus relative to the laboratory, *i.e.*, relative to the surface of Earth. Michelson explicitly noted that "the numbers in the columns are the positions of the center of the dark fringe in *twelfths* of the distance between the fringes" [16, p.127], emphasis in the original. In the first position the reference arm is oriented toward north (N), in the second same arm points northeast (NE), and so on in 45º steps clockwise until reaching last position with reference arm oriented toward northwest (NW). For each session, Figure 2 graphically shows Michelson's raw data, plus the mean values for each orientation over the five revolutions. In each session there is one outlier that Michelson (plausibly at first sight) attributed to a mechanical asymmetry or peculiarities of his interferometer (page 125).

For each orientation and series of data, Table 1 shows the mean of the raw data over the five revolutions *without removing outliers*. For each mean we have calculated its standard deviation (SD), also

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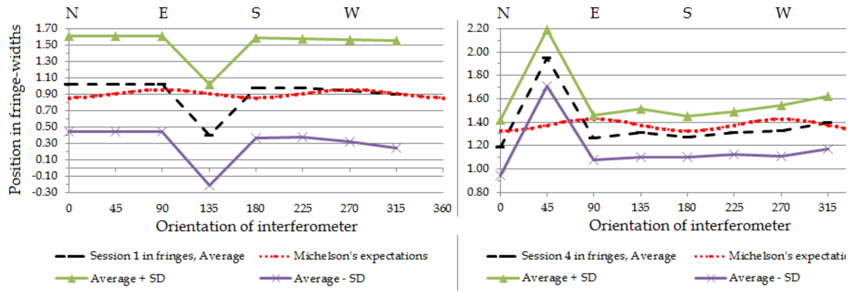

Figure 3. Average raw data curves (dashed lines) in sessions 1 and 4 with one-SD error band above and below each session mean. Average SD in sessions 1 and 4 respectively are 0.61 and 0.21. Dotted curves show Michelson's expectations.

included in Table 1. The last row shows overall averages for each series.

It is quite remarkable that Michelson did NOT calculate standard deviations for his raw data. As a consequence, in the whole paper there is a notorious absence of an analysis of the experimental error associated with each stage of his data reduction process. This is most surprising for an experiment carried out in Germany, homeland of Gauss —the creator of the theory of errors. To illustrate the importance of this matter, Figure 3 shows the means and the associated standard deviations in fringe-widths; note that values in Table 1 are divided by 12 to convert raw readings into fringe-widths (page 127). Michelson expected to observe in this 1881 experiment a harmonic curve with an amplitude of 0.05 of a fringe-width. This is the dotted curve shown in Figure 3, immersed within a very large experimental dispersion.

For sessions 1 and 3 last row in table 1 gives an average SD around 0.6 fringe-widths. The experimental dispersion shown on the left side of Figure 3 is *twelve* times larger than Michelson's expected amplitude of 0.05 fringe-widths for his harmonic

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variations. Likewise, for sessions 2 and 4, the last row in Table 1 gives an average SD of 0.2 fringe-widths, which is FOUR times larger than Michelson's expectations as seen on the right side of Figure 3. For this author the results shown in Figure 3 constitute a *strong indication* that *something is not right with the experiment*, and that the observations summarized in Table 1 above are not useful to test hypothesis H-1881, the objective of the experiment. However, as Michelson did not calculate his standard deviations, he continued his data reduction process, briefly described in the next subsection.

For completeness, let us note that SD values are very stable within a given series, and are much larger for series 1 and 3 corresponding to an East-West orientation of a footscrew at the base of the apparatus, and three times smaller for series 2 and 4 corresponding to a South-North orientation of the footscrew. Michelson noted that the outliers displace 90º together with the 90º rotation of the footscrew at the base of the apparatus between sessions. He thus attributed the outliers to some idiosyncracy of the interferometer and eliminated them by using in his calculations the average of the two adjacent points. Surprisingly, in Table 1 above, which did include the outliers, the SDs associated with all outliers seem to be immune to the peculiarities of the magic footscrew. I leave this fact as an open question.

II.3. Michelson's analysis and conclusions

As just stated, for each session Michelson calculated mean fringe-shifts without outliers (see table in [16, p.126]). *Mutatis mutandis*, Michelson's means are the same dashed curves shown above in Figures 2 and 3. Since he expected two harmonic cycles over a 360º rotation of the interferometer, Michelson averaged the four data for N to SE with the four data for S to NW. Michelson then

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proceeded to inter-session averaging of the means in the four sessions. As noted in his page 127, the average fringe-shift associated with N-S direction (= 0.022) was smaller than the average fringe-shift associated with NE-SW direction (= 0.034), which he expected to be zero under the assumption that Sun actually moves towards Hercules *only*.

Instead of considering the possibility that one of his design assumptions might be incomplete, and that his own data hinted at a real fringe-shift along the NE-SW direction (recall our comments in subsection II.1), Michelson applied a *linear correction* to eliminate the apparent drift along the NE-SW direction because "the numbers are simply outstanding errors of the experiment" (page 127), obtaining values which "represent the *displacements observed, freed from the error in question*" (page 128), emphasis added. The simulation in the next subsection II.4 clearly demonstrates that, *in some cases*, a linear correction not only distorts the empirical data, but may also be unnecessary and incorrect.

Michelson used the tiny residuals after the linear correction to plot his Figure 4, *without error bars*, showing "the actual curve together with the curve that should have been found if the theory had been correct" (page 128). Then, Michelson concluded that "the interpretation of these results is that there is no displacement of the interference bands. The result of the *hypothesis of a stationary ether is thus shown to be incorrect*, and the necessary conclusion follows that the hypothesis is erroneous" (next to last paragraph in [16, p.128], emphasis added).

Regarding Michelson's Figure 4 and his conclusions, several comments are in order. In the first place, for a modern reader a curve without error bands is rather strange. Recalling Figure 3 above, it may be expected that the statistical errors associated with

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Michelson's "actual curve" are much larger than the 0.05 amplitude of his expected dotted curve "drawn on the supposition that the displacement to be expected is one-tenth of the distance between the fringes" (page 128). Hence, the "actual curve" with small amplitude close to zero and *without error bars,* shown as Figure 4 in Michelson's paper [16], is simply meaningless.

Secondly, let us grant for the sake of the discussion that observed fringe-shifts had a very small experimental error. In that case Michelson was right in asserting that there was "no displacement of the interference bands". Since such an experimental outcome was contrary to the expected displacement of "one-tenth of the distance between the fringes", the correct conclusion is that hypothesis H-1881 was not proven. In other words the conclusion should be: *Michelson's interferometer could not measure the velocity of Earth defined as orbital motion around Sun plus solar motion toward Hercules at 30 km/s maximum speed.* This is quite different from Michelson's conclusion that "*the hypothesis of a stationary ether is thus shown to be incorrect*", and significantly oversteps the design objectives of the 1881 experiment at Postdam.

For additional remarks see the next subsection where we try to answer a more general question: was the 1881 experiment capable of measuring the whole speed of Earth's motion?

To end this subsection fairly, it must be acknowledged that the present writer has the benefit of hindsight in two senses: (1) the current evidence that net solar motion has a high speed in a direction other than the Hercules constellation [29], and (2) our experiment [11-13] demonstrated that a Michelson interferometer *operated at small angular steps* does yield large fringe-shifts—really, and beyond any doubt.

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II.4. Simplified simulation of the 1881 experiment

Let us consider a simplified example that contains just the basic ingredients of Michelson's experiment and his data reduction process. Firstly, let the objective be to measure $y(\varphi)$, which is a sinusoidal curve of unknown amplitude A. Secondly, let the experimental procedure be to collect $y_{\text{obs}}(\varphi)$, which are the residuals of $y(\varphi)$ after subtracting the integer component of $y(\varphi)$; see eqs. (1). And, thirdly, in the data reduction process let a linear correction $y_{\text{corr}}(\varphi)$ be applied to cancel the value of observed data at $\varphi = 45^{\circ}$ and 315°, that is along the NE-NW direction, see eq. (2). The final output of Michelson's experiment is $y_{\text{M}}(\varphi)$ defined by eq. (3). To avoid misunderstandings, please note that Michelson used equation (3) below with a negative sign, and that the origin for the horizontal coordinate x in his equation in page 128 is on the right side.

$$y(\phi) = A\sin\phi, y_{obs}(\phi) = y(\phi) - \text{int}\{y(\phi)\}, \quad \phi = 0^{\circ}, 45^{\circ}, 90^{\circ}, \ldots, 360^{\circ} \quad (1)$$

$$y_{corr}(\phi) = b + m\phi, b = y_{obs}(45) - 45m, \quad m = \frac{y_{obs}(315) - y_{obs}(45)}{315 - 45} \quad (2)$$

$$y_M(\phi) = y_{obs}(\phi) - y_{corr}(\phi), \quad \phi = 0^{\circ}, 45^{\circ}, 90^{\circ}, \ldots, 360^{\circ} \quad (3)$$

The linear correction was implemented by Michelson, who argued that elimination of "this gradual change, which should *not in the least* affect the periodic variation for which we are searching, would of itself necessitate an outstanding error … If therefore, we can eliminate this gradual change, we may expect a much smaller error" [16, p. 127], emphasis added. Of course, the periodic variation is still there. But, the linear correction projects the observed data upon a new horizontal thus distorting the shape and amplitude of observations—this is a sort of rotation as seen in Figures 4 and 5.

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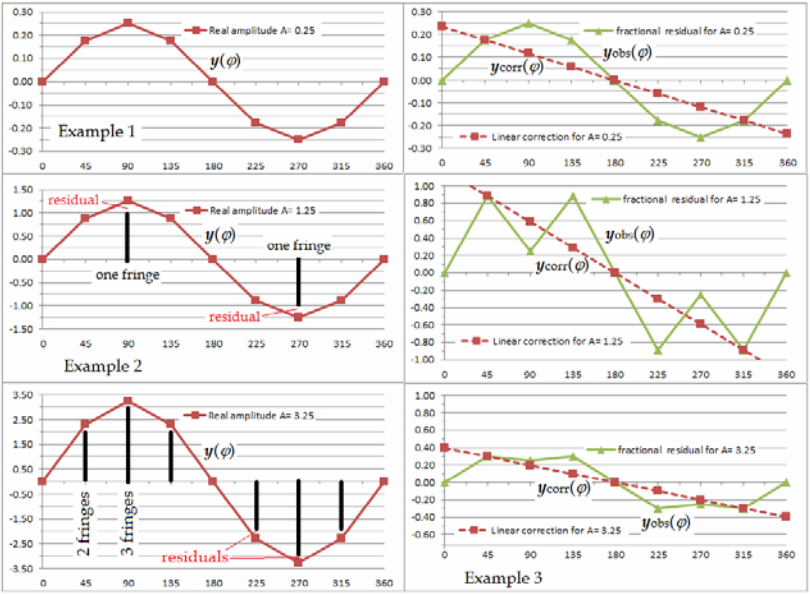

Figure 4. Determination of the sinusoidal curve by measuring residuals y_{obs}, which are graphically defined on the left side of the figure. The resulting observed curves are shown at right, together with a linear correction to cancel *apparent* linear drifts.

Figure 4 shows three examples for different values of the unknown amplitude $A = 0.25$, 1.25, 3.25. The left side shows $y(\varphi)$ the curve to be found by recording succesive values of $y_{obs}(\varphi)$ for $\varphi = 0°, 45°, 90°, \ldots, 360°$, as shown on the right side of Figure 4, together with the linear correction defined in eqs. (2).

Example 1 has $y(\varphi) < 1$ for all φ ; it is evident that $y_{obs}(\varphi) = y(\varphi)$ for all φ. This means that, in such cases, Michelson experiment is correctly defined. On the contrary, in examples 2 and 3 there are one or more $y(\varphi) > 1$. In such cases the observed data $y_{obs}(\varphi) < 1$ is very different from the original sinusoidal curve, and there is no obvious relationship between the shape of the curve and the value

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of the original amplitude A. In some cases, such as example 3, one may be tempted to interpret variations in $y_{obs}(\varphi)$ as experimental error and fit a smooth curve to $y_{obs}(\varphi)$. The resulting amplitude $A_{obs} < 1$ will be with certainty smaller than the original amplitude, which by necessity is $A > 1$.

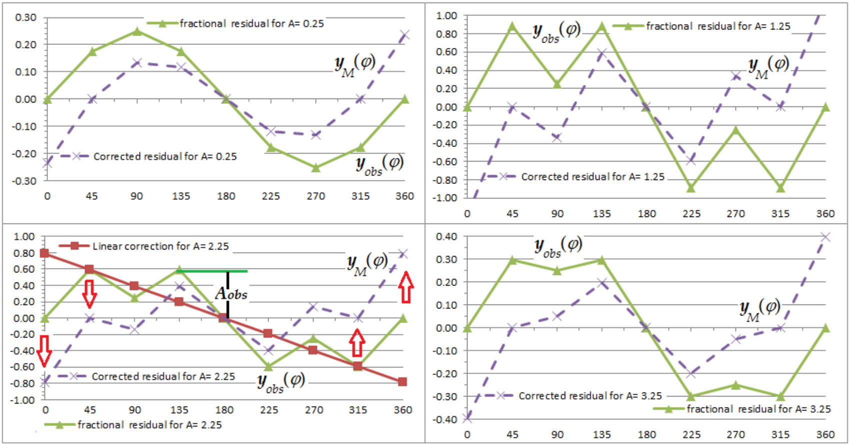

Figure 5. Final curve $y_M(\varphi)$ after applying linear corrections to the three examples in Figure 4. Details of the process are self-explanatory in the observed and final curves for A = 2.25 shown at the bottom on left side.

In example 3 the amplitude of the smoothed curve will be $A_{obs} \sim 0.3$, which is ten times smaller than the real amplitude $A = 3.25$. In this case it would be ludicrous to claim that the *real* amplitude is 0.3, and even more absurd to claim that due to experimental errors the amplitude may be considered to be zero (*i.e.*, a “null” result). No! The only logically reasonable conclusion is that the design of the experiment (*i.e.*, the choice of $y_{obs}(\varphi)$) is not appropriate to measure sinusoidal curves having one or more $y(\varphi) > 1$. This is precisely the case in Michelson’s experiment when total solar motion has a speed much larger than 30 km/s.

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In examples 2 and 3 the scatter of points around Michelson's "linear correction" *apparently* shows a linear drift of data of unknown origin. Michelson interpreted the apparent drift as caused by an unidentified real physical agent and subtracted it. However, in the present simulation there are no unidentified physical agents, and Michelson's interpretation is not appropriate.

Just consider example 1 in Figure 4. The curve for $y_{\mathrm{obs}}(\varphi)$ on the right side of the figure is the correct representation of the sought after $y(\varphi)$. Any further correction is not only unnecessary, but also wrong, as can be seen in Figure 5 for the case A = 0.25. Indeed, the final curve $y_{\mathrm{M}}(\varphi)$ has a different shape and a smaller amplitude.

For the other three cases in Figure 5 having A > 1, the resulting $y_{\mathrm{M}}(\varphi)$ curves depict unexpected variations between successive measurements, which in an experiment would be interpreted as observational errors. However, such variability arises from an incorrect choice of the method of observation $y_{\mathrm{obs}}(\varphi)$, which in Michelson's case was recording only the fractional component of a total fringe-shift containing both an integer plus a residual.

II.5. Stationary aether and Newton's preferred frame

The assumption of an aether introduces an unnecessary complication in the experimental design. Consider the cube shown in Figure 1A which may represent an isolated room filled with air, or an isolated swimming pool filled with water, either of them rigidly attached to a larger building; in this sense, the walls of the cube are at rest relative to the larger building. The velocity of water in the pool is defined in equation (4) as the average velocity $\langle \mathbf{V} \rangle$ of the N molecules of water contained in the pool, where each molecule j moves with *velocity* $\mathbf{V}_{\mathrm{j}}$ *relative to the walls of the pool*. The water in the pool is at rest if the average velocity is zero, and the water is in motion otherwise:

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$$< \mathbf{V} > \equiv \sum_{j=1}^{N} \mathbf{V}_j ,\ < \mathbf{V} >= 0 \text{ fluid at rest},\ < \mathbf{V} > \neq 0 \text{ fluid in motion} \quad (4)$$

Please note that the fluid may be at rest, while concomitantly the average speed of water molecules relative to the walls of the pool may be $C > 0$. Then, it follows that Poincaré's "motionless" aether cannot be a fluid. For convenience, one may choose a preferred frame of coordinates at rest relative to the walls of the pool, with one axis pointing toward a fixed mark on one of the walls, say the star shown in Figure 1A, which represents Newton's fixed stars. A similar definition holds for air, or for a more tenuous classical aether.

Consider now a material particle in motion, say a swimmer in the pool, or a tennis ball traveling in air. There is no difficulty in defining the velocity of the particle relative to the walls of the cubic room or of the swimming pool, which always are at rest by definition. Likewise, it is easy to define the dual problem where the material particle is at rest and the fluid is in motion relative to the walls of the cube, as in a wind tunnel.

On the contrary, one introduces unnecessary complications if motion of a material particle is defined in relation to a moving fluid, with average velocity given by equation (4).

Some proponents of aether treated in [30] talk about aether entrainment. This notion is reminiscent of a well known but rather complex classical phenomenon in the theory of *viscous* fluids: the formation of boundary layers (see any textbook on fluid theory, say [31, pp.140-146]). Simplifying many caveats and issues, if relative speed between a material particle and a viscous fluid is small, a *thin* layer of fluid —called the boundary layer— always remains attached to the material particle. In that case the relative speed between the particle and the boundary layer is zero. In a Michelson experiment on the surface of Earth one may assume as a

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reasonable first approximation that the laboratory is within the boundary layer of a fluid aether, so that the apparatus is at rest relative to the boundary layer. But beyond that thin layer (how many meters?) the fluid has a speed profile that smoothly increases up to some limiting speed associated with the motion of Earth relative to some local bulk of aether. So a new question appears, at what distance from Earth is attained the limiting speed? In such models the only known thing is that, beyond the boundary layer, the relative speed between particle and fluid is always non-zero.

However, what happens if the Reynolds number associated with aether is large? This may occur if the relative speed between aether and Earth is large in some undefined sense. By analogy with classical fluids, in those cases the aether flow would be turbulent, and wakes may appear, as in airplanes, and so on. In entrainment models there is also the implicit assumption that the aether is viscous. Is there any evidence regarding aether's viscosity? In my humble opinion, aether entrainment introduces too many additional and unnecessary hypotheses.

For previous reasons, in the analysis of the MMX the present writer avoids aether as an unnecessary intermediary, and strictly adheres to Newton's approach, namely: refer all velocities to a preferred frame. For instance, in the calculations for the design of our experiment [10] and in our concluding analysis [13] the words "aether" and "aether wind" *never* appear.

III. The 1887 MM experiment critically revisited, once again

For the 1887 experiment at Cleveland (USA), Michelson, then teaching at Case School of Applied Science, joined efforts with the chemist Edward W Morley, from nearby Western Reserve

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University. The Michelson-Morley experiment (MMX) [3] improved the hardware relative to the 1881 experiment, in particular the rigidity of the apparatus and the stability of rotation. However, for the collection of data MM used the same 1881 protocol, consistent–let me repeat once again–in recording *only* the residual fraction of a fringe-shift rather than the total number of fringe-widths produced in each successive change of orientation of the apparatus through 22.5º relative to the laboratory.

In present paper I want to *explicitly* argue that the crucial 1887 MM experiment was designed to test *only one component* of the motion of Earth relative to Newton's preferred frame. Since in Miller's words [28, p.206] the observed "*effect did not have the anticipated magnitude"* of 30 km/s, the only possible conclusion was that the *motion tested for was not measurable* with MM experimental setup. Any interpretation or extension by MM, or by anybody else, beyond that clear and summary answer must be justified, and most likely would require additional assumptions that must be clearly stated.

III.1. Design of the 1887 experiment

As already mentioned in section I, the design for the 1887 experiment incorporated a correction suggested by the Parisian M.A. Potier in 1881, and by Lorentz in his 1886 paper [32]. It also explicitly supposed "*the ether being at rest*" [3, pp.334-335], see Figure 1A.

Surprisingly, MM's analysis for the expected fringe-shifts did *not* include solar motion, which was taken into account in the earlier 1881 experiment at Postdam. For this more sensitive apparatus with eleven-meter arms, MM predicted a 0.4 fringe-shift—as in the 1881 experiment, the *expected* fringe-shift was *smaller than one fringe*. In MM's words "in what precedes, *only*

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the orbital motion of the Earth is considered. If this is combined with the *motion of the solar system*, concerning which but little is known with certainty, the *result* would *have to be modified*; and it is *just possible that the resultant velocity at the time of the observations was small* though the chances are much against it. The *experiment* will therefore be *repeated at intervals of three months*, and thus all uncertainty will be avoided" [3, p.341], emphases added. This quotation merits several comments, made in the same sequence as the phrases in italics.

1) Since MM acknowledged that "only the orbital motion of the Earth is considered" it follows that the hypothesis tested in the MM experiment was H-1887: *Is it possible to measure orbital speed of Earth around the Sun with Michelson's interferometer?* Indeed, the expected 0.4 fringe-shift was calculated for an orbital motion of 30 km/s.
2) Regarding "motion of the solar system… little is known with certainty". This attitude is not the same as in 1881. MM could have assumed some values of solar speed as in 1881; why didn't they? Were MM, perhaps, concerned that a non-zero solar speed might increase expected fringe-shift beyond one fringe? In that case the expected sinusoidal variations would have amplitude $A > 1$, and as previously argued in Section II.4, a correct experimental design would require recording the full fringe-shift. This prompts a different question, did MM have, in their day, the technical capability to measure the full fringe-shift? This latter issue is tackled in next section III.2.
3) At any rate, MM were quite aware that "the result would have to be modified", *i.e.*, that the expected fringe-shift would *certainly* be different. Indeed, the motion of Earth relative to a Newtonian preferred frame is a unique

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indivisible physical concept: a time-dependent net speed along some direction. A completely different matter is that for calculations, or for expository reasons, the motion of Earth may be considered the vector addition of several components: orbital velocity around the Sun, plus solar velocity relative to the center of our galaxy, plus the velocity of Milky Way relative to the local group of galaxies, and so forth. One may naively posit that Nature does not identify a particular experiment on Earth, and that Nature does not adjust herself to react only to the component of velocity that such experiment is trying to measure! It follows that experiments seeking to measure the motion of Earth necessarily refer to total net motion. In that case the amplitude of the expected sinusoidal variations is approximately $A > 10$. Taking into account the simulation discussed in II.4, one may conclude that the 1887 MM experiment was not designed to measure $A > 10$, and thus was doomed *ab initio*.

4) MM considered a *small* "resultant velocity at the time of the observations" quite unlikely. Why did MM omit the obvious alternative: a *large* resultant velocity of Earth at the time of the observations? This omission prompted us to repeat the experiment; for a brief description see Section III.4.
5) MM suggested repeating the experiment "at intervals of three months". MM never did it. It was Dayton C. Miller who, almost forty years later, finally performed the experiments between April 1925 and February 1926 [28, pp.228-231].

Be that as it may, the scope of the MM experiment was restricted by the *very* design of the experiment to test for H-1887 only.

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III.2. The execution of the 1887 MM experiment, and the continuation by Miller

When I prepared the overview of MM-type experiments in 1998 [4], I had never carried out the experiment, so that some aspects were missed or did not receive the attention they deserve. Let us stress two issues.

III.2.a The number of fringes between two consecutive orientations

Or, from the observer's viewpoint, how may an experimenter count the total number of fringes shifting from one orientation of the apparatus to the next?

The 1881 Michelson experiment. Let us start with the pioneering 1881 experiment. In the first position (P1), one of the arms called the reference arm is parallel to a South-North line, the interferometer is calibrated to have a *reference* fringe (RF) close to a fiducial point, say an arrow in the field of view of the telescope. Michelson measured the position of the center of that RF relative to the arrow, and wrote it down, this is the first register or R1. Next the apparatus was turned 45º to position 2 (P2) so that the reference arm was parallel to a line from SW to NE, Michelson checked the horizontality of the apparatus and waited until vibrations faded away; he looked at the interference pattern, identified the reference fringe that he expected to be near the fiducial arrow, measured the position of the center of that RF relative to the arrow, and wrote it down; this is the second register or R2. And so on every 45º until five revolutions of the apparatus were completed.

First question: what is a reference fringe? My own experience is with laser light where all fringes are alike, but I have been told by my colleagues from optics that in experiments with non-

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coherent light there is always a brighter fringe that is easy to identify. So, let that bright line be the reference line for Michelson in 1881. Incidentally, that bright line does not help much for my own experiment.

Second (much harder) question: how many fringes shifted from the S-N position P1 to the SW-NE position P2? For Michelson the answer was simple: the difference between the two readings, *i.e.*, R2-R1. But, was that right? How could he possibly know? Was that mere wishful thinking? Just remember that Michelson was conditioned by the scientific knowledge at the end of the 19th century, whereby the Sun moved at most at 30 km/s—hence the expected small shift (see section II).

In my humble opinion, in the 1881 experiment with separate or independent measurements at each orientation it was *impossible* to know how many fringes shifted between the two separate and independent orientations of the apparatus.

The 1887 MM experiment. For the 1887 experiment MM introduced a conceptual change in design. Rather than a succession of separate measurements, each one at a different orientation, the apparatus was placed on a stable base so that there was only one calibration per session. The stone base with the apparatus on top was floated in mercury, and slowly rotated at a rate of one revolution every six minutes, *i.e.*, every 360 s. Relative to the 1881 experiment, the spatial resolution in 1887 improved by a factor of two: the interference pattern was checked every 22.5º (instead of 45º) through a telescope also placed on the rotating platform. In a revolution there were 16 readings (= 360º/22.5º), so that observer had to walk around the mercury bath, and quickly look through the telescope eyepiece every 22.5 seconds (= 360 s/16).

In their paper MM stated that “the cross wire of the micrometer was set on the *clearest* of the interference fringes at the instant of

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passing one of the marks … the reading of the screw-head on the micrometer was noted … on passing the second mark, the same process was repeated, and this was continued till the apparatus had completed six revolutions" [3, p.339], emphasis added. So, MM measured the position of *the* "clearest" fringe every 22 seconds at each mark, but they did not know what happened during the 22-second interval between successive marks. For instance, did the clearest fringe drift several fringes before arriving at its current position? Or, is *the* "clearest" fringe at the second mark the *very same* fringe observed at the first position? In summary, in the MM experiment it is not clear whether it was possible to measure, at each orientation, the accumulated fringe-shift over all previous orientations.

Miller experiments. For his own experiments Miller [28, pp.208-214] built in 1904 a more massive apparatus with higher resolution: effective length of arms was three times the arm length in the MM experiment. He continued Michelson's practice of taking readings every 22.5º. The rotation rate was increased to one revolution every 50 seconds, *i.e.*, the observer shouted a reading every *three seconds*! The values in units of a tenth of a fringe-width were recorded by an assistant on a prepared form (page 213). In Miller's words: "the observer has to walk around a circle about twenty feet in diameter, *keeping his eye at the moving eyepiece* of the telescope attached to the interferometer which is turning on its axis steadily, at the rate of about one turn in fifty seconds; the observer must not touch the interferometer in any way and yet *he must never lose sight of the interference fringes*, … The string attached to the float … used as a sensitive guide to assist the observer in maintaining the proper circular path" [28, p.211], emphases added. So, the arc distance between consecutive marks was about four feet ($20\pi/16$) that the observer had to walk with his

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head attached to an eyepiece! The meticulous and continuous attention maintained for hours by the observer and his assistant are awe inspiring. Since Miller required keeping the eye "at the moving eyepiece" and never "lose sight of the interference fringes", it is evident that he was aware of the issues under consideration in this subsection, namely, the need to *record the real* fringe-shift. To attain such an impressive goal Miller implemented *continuous monitoring* of interference fringes by the naked eye. It is very sad that an improper operational protocol implemented by Miller himself destroyed the continuity of his monitoring (see the final part of next subsection).

Present writer completely agrees with Miller that the only way to count the total number of fringe-shifts is by *continuous* monitoring of interference fringes. In our case, during 2002 in the preliminary stage of our experiment, we carried out three-day long sessions over several weekends, *day and night*, with an observer *permanently* looking at the pattern of interference-fringes appearing over a frost-glass screen. The objective was to write down the succession of positions and times (hour, minutes and seconds) when the observer could discern by the naked eye a motion towards left or right of a *selected* fringe, *the same* fringe was followed throughout the whole weekend session. Let me once again thank the several students of my Newtonian Mechanics course, and my daughter Natalia and her friends, for being the observers during night-shifts in the basement of CIF.

It was found that the interference-pattern in our interferometer, at rest on top of a pneumatic thirteen metric ton concrete table, was stable, and that the eye could only distinguish changes at the scale of a few minutes, typically five to ten. With that empirical observation we benefited from modern technology, and installed a video camera for the 2003-2005 experiment. To optimize data

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handling, only a photograph of the pattern was recorded and stored every minute (see section III.4).

III.2.b The standard deviations in Michelson's and Miller's raw data

Regarding the 1881 Michelson's raw data, it was noted in Section II that he did not calculate the standard deviation (SD) associated with each orientation, but SDs could be easily calculated from the data for the five individual turns of the apparatus (see Table 1 above). In general, the paper reporting the 1887 experiment [3] was less detailed than the pioneering paper reporting the 1881 experiment [16]; for instance, raw data is not reported in 1887. In the latter, MM only included a table showing, for each session, the means over the six turns of the apparatus [3, p.340], *without* individual standard deviations associated with each mean value. Thus, we do not have the slightest clue whether the dispersion of raw data in 1887 was smaller or larger than in 1881. As might be expected from this omission, MM do not include error bars in the concluding Figure 6 [3, p.340]. Consequently, the same comments made regarding the 1881 experiment are valid here: the absence of error bars seriously calls into question the validity of the conclusions offered by MM.

Returning to Miller's thousands of experimental sessions, a run in his experiment was the average of twenty consecutive turns during some 1,000 seconds (20 turns x 50s/turn), *i.e.* about 16 minutes. A facsimile of the data sheet for a run at Mount Wilson on September 23/925 is shown as Figure 8 in [28, p.213]. On the right side of that sheet the ominous word "adjust" appears three times. What is it?

Like Michelson and MM, Miller also was psychologically anchored to the belief that fringe-shifts should be small. As a result

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during the execution of his experiments Miller implemented a procedure that, in retrospect, was *not* appropriate. In Miller's words: "the adjustments are maintained so that the *central fringe* of the field of view … is *never more than two fringe widths* from the fiducial point. Often the *temperature drift* is such that the fringes drift more than this before a set of twenty turns is completed. When this occurs, the *fringe system* is *restored* to its central position simply by *placing a small weight* of two or three hundred grams on the end of the arm or by *removing* a weight from the arm" [28, p.212], emphases added.

In the first part of the above quotation, Miller interpreted observed shifts larger than two fringes as caused by an unwanted "temperature drift" that should be eliminated. This *might* be *partially* correct, but if a correction is actually required, it is done during the process of data reduction—*never* by modifying the apparatus during a run. Furthermore, recalling our simulation in II.4 above, at least part of the observed apparent drift may be a real effect caused by the *real high speed* of solar motion. So, "adjustments" were *not* needed at all.

Present writer is a neutral outsider without emotional ties (one way or another) to Miller, and besides, he is knowledgeable in the field of experimentation. For him it was almost unbelievable to read the final sentence in the above quotation. Indeed, the "*placing*" or "*removing*" of weights shortens or enlarges via (un)bending the lengths of the arms, thus violating a fundamental rule in experimentation: use the *same* apparatus in a run. Thus, in the case shown in Figure 8, Miller used one apparatus for revolutions 1 to 5, a second different apparatus for revolutions 6 to 9, a third different apparatus for revolutions 10 to 19, and a fourth apparatus for the last, twentieth, revolution. Hence, the twenty turns cannot be averaged, as Miller did. Also, it is surprising that

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Miller did not calculate standard deviations associated with each mean over twenty turns. At any rate, neither the mean nor the SD would have any value in cases where adjustments are made during a run.

Another unwanted by-product of Miller's adjustments is that the *continuous* observation of cumulative fringe-shift was spoiled. Let us peruse Miller's data sheet in Figure 8 [28, p.213]. At position 1 in the first turn the reading in tenths of a fringe-width was +10, and at position 17 at end of fifth turn the reading was –15; this is a net change of 25 = 2.5 fringe-widths toward the left (the latter from the minus sign). The arm of the interferometer was modified, *i.e.* "adjusted", and in the sixth turn the reading at position 1 is now 0. The fringes continued moving to the left, and at position 17 at the end of the ninth turn the reading was –10; this corresponds to one additional fringe-width to the left. The arm of the interferometer was modified for a second time, *i.e.* "adjust", and in the tenth turn the reading at position 1 was now +8. The fringes continued moving to the left, and at position 17 at end of 19th turn the reading was –21; this is a net change of 29 = 2.9 fringe-widths toward the left. Altogether a total of 2.5 + 1.0 + 2.9 = 6.4 fringe-widths to the left. The arm of the interferometer was modified for a third time, *i.e.* "adjust", and in the 20th turn the reading at position 1 was now +1. After this modification of the apparatus the fringes started to move to the right, and at position 17 at end of 20th turn the reading was +4; this is a net change of 3 = 0.3 fringe-widths towards the right. Altogether for the twenty turns of the interferometer 6.4-0.3 = 6.1 fringe-widths to the left in 16 minutes. This is a significantly large fringe-shift!

Contrary to some opinions, the previous exercise clearly shows that large fringe-shifts were present in Miller's data. Unfortunately, Miller threw away that information by introducing the adjustments.

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At the beginning of this 21st century James De Meo went to Cleveland to unearth Miller's data sheets. He succeeded, and he was extremely generous in letting this author have a copy of that valuable information. The present writer considered the possibility of recovering the hidden total fringe-shifts from Miller's data sheets, using the procedure described above. However, it was realized that such an exercise would only be of *qualitative* value, because due to *the* "adjustments" Miller's interferometer kept varying over one run, as well as from run to run. So, this writer finally decided to go ahead with his own experiment.

III.3. Comments on the conclusions of the 1887 MM experiment

In the context of the foregoing discussion, the conclusions offered by MM in their crucial 1887 paper were quite misleading: "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… It appears, from all that precedes, reasonably certain that if there be any *relative motion* between the *Earth and* the luminiferous *ether*, it must be *small*; … If now it were legitimate to conclude from the present work that *the ether is at rest with regard to the Earth's surface*, according to Lorentz there could not be a velocity potential, and his own theory also fails" [3, p.341], italics added. Additionally, as already stated above, the concluding Figure 6 [3, p.340] *without* error bars is not meaningful. In 1887 the situation is worse than in 1881, where at least it is possible to recover the SD associated with the raw data.

Strictly speaking, MM's correct conclusion should have been something like this: our experiment only measured "less than one-sixth the Earth's orbital velocity, and certainly less than one-fourth" of the said expected orbital velocity. The experimental

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error was such and such. However, solar motion was not included in the design of our experiment, and it is unknown at the present time whether an experiment designed to test, by optical means, for a non-zero solar velocity may be successful.

Many people focus on the conclusions, and *may understand* that MM *actually* measured "*the relative velocity of the Earth and the (luminiferous) ether"*, under the most general conditions. No, not quite! The MM claim significantly overstepped the range of validity of their experiment, which ignored solar motion and only registered residual fractions of a fringe-shift, instead of total fringe-shift.

Since in the concluding sentences MM did not emphasize the limitations of their experiment, Lorentz, Poincaré, Einstein, and *almost* everybody else took their concluding remarks at face value (just recall the quotations in the Introduction).

III.4. Our 2003-2005 MMMM experiment at CIF, Bogota (Colombia)

Of course, this author is well aware that MM actually wanted to test a more ambitious hypothesis that may be called *H-aim: Is it possible to measure the motion of Earth relative to a Newtonian preferred frame with Michelson's interferometer?* Our answer is: yes, it is possible, but it requires a slight modification in the collection of data. To identify an appropriate experimental procedure this writer calculated the response of the MM apparatus when solar velocity is also included; the ensuing theoretical predictions [10, 33] were kindly published by Professor Dvoeglazov.

Let us focus on our *realistic* calculation of the expected fringe-shifts in the MM experiment at Cleveland. The word "realistic" is

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stressed to indicate that solar motion was included, contrary to MM's toy calculation *without* solar motion.

Our calculations are for the same apparatus used by MM in 1886, placed at the same geographical coordinates in Cleveland, for the same dates and hours of day, and use a modern value of solar velocity taken from [29]. The results are in Figure 4A [33, p.97]. Amplitude of the oscillation exceeds fifteen fringe-shifts! This is in stark contrast to MM's expected 0.4 fringe-shift *without* solar motion. Thus a correct design of any modern repetition of the MM experiment must be capable of measuring large fringe-shifts (this is a large amplitude *A* in the simulation in II.4).

Our calculation also explains why MM observed what they did. Figure 4.B [33, p.97] shows the expected observations when only the residual fraction of the fringe-shift is recorded, as in the MM experiment [3] (recall the simulation in II.4).

To empirically check the foregoing theoretical analysis, the present writer decided to repeat the MM experiment, with important improvements, from early 2002 to February 2005; this is the Michelson-Morley-Miller-Munera (MMMM) experiment. The first year, 2002, was dedicated to design and preliminary measurements. The experiment itself ran over a period of 26 months from January 2003 to February 2005 at the International Centre for Physics (CIF, for the initials in Spanish) located on the campus of National University in Bogota. The observed data [11-15] support our theoretical predictions.

We used a stationary interferometer (*i.e.*, rotation period of 24 solar hours), laser green light, automatic video recording of the fringe-pattern every minute for a total of 1,440 frames per daily rotation, and during 2002, developed software to convert video images to digital fringe-shifts. It is worthwhile to stress a *very*

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significant difference between our MMMM experiment and all classical MM-type experiments: *our high* orientational *resolution*. Let us be explicit. The MM and Miller experiments used an *angular distance of 22.5º* between two consecutive readings. In our stationary interferometer this would be the same as taking a reading every 90 minutes, or every one and a half hours. Our preliminary observations during 2002, mentioned in III.2.a above, clearly indicated that such an interval *was too long* because a reference fringe could move over several fringe-widths during that 90 minute interval. As already explained in III.2.b, Miller himself did observe a fringe shift of 6.1 fringe-widths in sixteen minutes, but unfortunately his "adjustments" hid that information. For a comparison recall that Miller's apparatus, with its thirty-three meter long arms, could produce fringe shifts 16.5 times larger than our interferometer with two meter long arms, so Miller's observations translated to our apparatus mean a fringe-shift of 0.37 fringe-widths (= 6.1/16.5) in sixteen minutes, completely compatible with our 2002 preliminary observations.

Next, we decided to record one image of the interference-pattern every minute, that is, every 0.25º (= 360º per day/1440 minutes per day) [11-15]. Thus, our angular resolution was *ninety times better* than the 22.5º in MM and in Miller experiments. As a result, we *succeeded where Michelson-Morley and Miller failed*, and we were able to witness the slow drift of the reference fringe, and *count* the total number of *fringes drifting forward and backward* during one turn of our apparatus, which, as already stated, was one solar day.

There is an additional aspect that also deserves attention. In designing our process for data reduction we were very concerned about the unfit handling of the effect of ambient temperature upon fringe-shift in the earlier experiments (just recall comments above

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in III.2.b). Since in optics it is well-known that speed of light propagation is affected by ambient temperature, humidity and pressure, we acquired commercial data loggers to measure humidity and temperature, but at that time in the local market there were no loggers for ambient pressure. To have an idea of the magnitude of the possible effect, professor German Arenas mounted a miniature interferometer inside a vacuum chamber. We used both the naked eye and a video camera to witness fringes passing by as pressure increased from a moderate vacuum up to atmospheric pressure. Two lessons: (1) The need to include during data reduction a correction for fringe drifts associated with variations of atmospheric pressure, leading to our implementation of stochastic corrections in our data reduction process [34]. In contrast, the subject of *ambient pressure* is notoriously *absent* in the crucial experiments by Michelson, MM, and Miller. (2) A video camera was indeed suitable to register fringe shifts in our experiment, as effectively implemented in our MMMM experiment.

From our data reduction process we obtained significant periodical diurnal and annual fringe-shift variations correlated with terrestrial motion. In the classical MM-type experiments the approach was to suppose a solar velocity and predict the expected fringe-shift. We solved the inverse problem, *i.e.*, from the observed fringe-shifts we calculated solar motion relative to a Newtonian frame attached to the fixed stars [12, 13]. Our solar velocity is compatible with other values of solar velocity obtained with different methods by other investigators [14].

Our recent presentation in Moscow at PIRT-2017 [15] reports an *astonishingly high correlation* between our calculated terrestrial velocity and the observed periodical variations in the *raw* data of a hi-tech experiment at Stanford University carried out with

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microwave cavities near 0 K, and thermally controlled within ±5 x 10^{-6} K. However, in the process of data reduction the Stanford group interpreted the said periodical variations as unwanted "*mechanical perturbations*", subtracted them, and interpreted the white-noise residual as support for the conventional interpretation of the MM experiment [35]. On the contrary, in our view the Stanford experiment confirms our MMMM experiment: their unwanted "mechanical perturbations" are highly correlated to velocity of Earth projected onto a *vertical plane* containing the two microwave cavities at their laboratory in Palo Alto (California), where velocity of Earth is defined as vector addition of orbital motion *plus our solar velocity* reported in [13] relative to a Newtonian preferred frame.

IV. Closing remarks: the indubitable validity of our "positive" experiment

Since the foregoing claims cast shadows on the very empirical foundations of contemporary science, it is not a great surprise that our "positive" results are mostly ignored, or at best labeled as "anti-relativistic". The latter is very far from our intentions: in short, this author still *believes* in the scientific method, where experiment and/or observation is the final arbiter. Besides, the present writer is neither anti-Einsteinian, nor anti-Newtonian, nor anti-Cartesian … , nor anti-anybody. Quite the opposite, I greatly admire and respect the scientific work of all of them. Our business is to fulfill Einstein's dream for a unified theory of Nature, based on a fluid aether similar to the classical aethers of Newton and Descartes [22], but using Einstein's field approach where force is not a primitive notion. Granted, such an inclusive process of synthesis requires abandoning, or rejecting, or completing, or

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reformulating Einsteinian, Newtonian and Cartesian ideas, but always in a constructive manner as manifested throughout present paper.

Please also note that the conclusions from our MMMM experiment do not result from "single experimenters, working unseen, based on relatively few tests" as was the case with Michelson's twenty turns of his interferometer in 1881 to collect 160 readings [16, p.126] (see also Table 1 above), or with the scanty thirty six turns in the MM experiment in 1887 to collect 576 data points summarized in a laconic six line table of their paper [3, p.340]. No, our work consisted of 1,440 data per day over hundreds of days covering a two-year span; that is, in a single day we doubled the 736 data collected by both Michelson in 1881 and MM in 1887 together. Furthermore, our ideas and results have been presented at several small international symposia and three large international conferences on different continents—SPIE-2007 in San Diego, PIERS-2009 in Beijing, and PIRT-2017 in Moscow—and published in journals and outlets available to us, for instance [4-15]. Needless to say, the raw data from the MMMM experiment is available for any interested person to check: just come to Bogota, copy the more than 300 CDs, and take them with you.

The overall conclusion in this paper is that the 1881 and 1887 experiments were designed to test only a particular value of solar motion. But the empirical observations did not support those limited hypotheses. However, in both cases the conclusions offered by Michelson in 1881 and by Michelson and Morley in 1887 significantly overstepped the range of validity imposed by the design of each experiment. Thus, the crucial 1881 and 1887 experiments *never* were "null". Consequently, there is no contradiction with the, indubitable in our opinion, "positive"

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outcome of our MMMM experiment at CIF: a succesful *first time ever* determination by optical means of the velocity of Earth relative to a Newtonian frame of reference.

Our MMMM experiment is *à la Miller* in several senses: (a) Measurements were taken throughout the day, and not merely at noon and 6 pm, where Michelson had *assumed* some convenient conditions, as cancellation of orbital speed by solar motion towards Hercules. (b) The experiment was repeated at several epochs of the year, not a scanty observational period of two hours per day during three almost consecutive days, for a total of six hours. Actually, our experiment had many long sessions from several days up to a couple of weeks in a row during 26 months from January 2003 to March 2005. (c) The interference pattern was continuously monitored to count the *whole* fringe-shift (an integer plus a fraction). However, in order to avoid confusion [30, p.182], it is explicitly stated here that in our analysis corrections by hand are completely absent; in particular we did not apply Miller's multiplication of final speed by some constant, as in his Table V [28, p.235].

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