New Measurement of the Earth's Absolute Velocity with the Help of the "Coupled Shutters" Experiment

Stefan Marinov*

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An account is given of a new execution of my "coupled shutters" experiment. This time the following definite figures for the Earth's absolute velocity have been obtained: magnitude 360 ± 40 km/sec with equatorial coordinates of the apex $\delta = -24° \pm 7°$, $\alpha = 12.5^{\mathrm{h}} \pm 1^{\mathrm{h}}$ (for February 1984).

1 Introduction

I carried out the "coupled shutters" experiment for the first time in 1979 in Brussels [1, 2]. The precision achieved with that first experiment was not sufficient for accurately determining the Earth's absolute velocity. Thus with that experiment I could only establish that this velocity was not greater than 3,000 km/sec. The "coupled shutters" experiment is relatively very simple and cheap [1, 2], however no scientist in the world has repeated it. The general opinion expressed in numerous letters to me, in referees' comments on my papers, and in speeches at various space-time conferences which I attended or organized [3] is that my experiments are very sophisticated and difficult to execute. The *unique* discussion *in the press* on the *technical aspects* of my experiments is made by Chambers [4]. Here I should like to cite the comments of my anonymous *Foundations of Physics* referee sent to me by the editor, Prof. van der Merwe, on the 23 June 1983:

> I was informed by (name deleted) of the Department of the Air Force, Air Force Office of Scientific Research, Bolling Air Force Base, that Dr. Marinov's experiments were to be repeated by the Joint Institute for Laboratory Astrophysics. On inquiry, I learnt that JILA is not carrying out the experiments, because preliminary engineering studies had indicated that it lay beyond the expertise of the laboratory to achieve the mechanical tolerances needed to ensure a valid result.

After presenting my objections that the fact that JILA in the USA is unable to repeat my experiments cannot be considered as a ground for the rejection of my papers on the measurement of absolute velocity, Prof. van der Merwe sent me on the 24 January 1984 the following "second report" of the same referee:

> It is with regret that I cannot change my recommendation regarding Dr. Marinov's papers. In trying to justify the validity of his experimental work, Dr. Marinov highlights the points which cause the rest of the community so much concern. He states, "If I in a second-hand workshop in a fortnight for USD 500 achieve the necessary accuracy, then, I suppose, JILA can achieve it too." I know of no one in the precision measurement community who believes that measurements of the quality claimed by Dr. Marinov could be realized under such conditions and in so short a time. It will take very much more than this to change the direction of physics. I suspect that even scientists working in the most reputable laboratories in the U.S. or the world, would encounter great opposition in attempting to publish results as revolutionary as those claimed by Dr. Marinov.

In this paper I present an account of the measurement of the laboratory's absolute velocity, executed by me in Graz with the help of a new configuration of my "coupled shutters" experiment. Now the apparatus was built not in seven days but in four. As the work was "black" (a mechanician in a university workshop did it after working hours and I paid him "in the hand"), the apparatus was built predominantly over the weekend and cost 12,000 Shillings (USD 1000.–). The driving motor was taken from an old washing-machine and cost nothing.

As no scientific laboratory was inclined to offer me hospitality and the possibility to use a laser source and labora-

*Stefan Marinov (1931–1997), a Bulgarian born experimental and theoretical physicist who invented a new and highly original method to measure the anisotropy of the observable velocity of light (the "coupled shutters" experiment). He reported on the results of his experiment in a few short papers published in the peer-reviewed journals (*Physics Letters*, *General Relativity and Gravitation*, *Foundations of Physics*, etc.). After his formal education, Stefan Marinov worked from 1960 to 1974 with the research staff and also as an Assistant Professor, at the Faculty of Physics, Sofia University. Whilst there he devised and set up his first "coupled shutters" experiment and with it detected an anisotropy in the observed velocity of light. His life in Bulgaria was difficult: he was jailed in 1966/1967, 1974, and 1977, by the Bulgarian communist regime, for inappropriate "political thinking". In 1977 Marinov was deported from Bulgaria as a "political dissident". After a few years in Belgium, the USA, and Italy, he continued his research in Graz, Austria, which became his home until his tragic death in 1997. Despite the significant attention drawn to his experiment in the 1980's (many papers discussing his experiment were published in *Nature* and other journals), no other scientists attempted to repeat it. On the other hand, the experiment is simple, cheap, and can be easily repeated in any well-equipped physics laboratory. We therefore publish this detailed description of the experiment, as given by Marinov himself in *Deutsche Physik*, in 1992. The editors hope that this posthumous publication encourages and assists scientists who would like to repeat and enhance the "coupled shutters" experiment. (This paper was submitted by courtesy of Erwin Schneeberger, who was a close friend of Dr. Marinov, at Graz.)

tory mirrors, my first intention was to use as a light source, the Sun. As I earn my bread and money for continuing the scientific research, working as a groom and sleeping in a stall in a small village near Graz, I carried out the experiment in the apartment of my girl-friend. The sensitivity which I obtained with Sun's light (a perfect source of homogeneous parallel light) was good, but there were two inconveniences: (1) The motion of the Sun is considerable during the time when one makes the reversal of the axle and one cannot be sure whether the observed effect is due to the delay times of the light pulses or to the Sun's motion; (2) One can perform measurements only for a couple of hours about noon and thus there is no possibility to obtain a 24-hour "sinusoid" (explanation of the measuring procedure follows). On the other hand, at fast rotation of the axle the holed rotating disks became two sirens, so that when my apparatus began to whistle the neighbors knocked on the door, asking in dismay: "Fliegt schon der Russe über Wien?" (Is Ivan already flying over Vienna?). After a couple of altercations, my girl-friend threw out of her apartment not only my apparatus but also me.

Later, however, I found a possibility to execute the experiment in a laboratory (Fig. 1). The scheme of the experiment, its theoretical background and measuring procedure, are exactly the same as for the Brussels variation [1, 2]. Since the description is extremely simple and short, I shall also give it here, noting that the mounting of the laser and of the mirrors on the laboratory table lasted two hours.

But first, following the example of *Nature* which gives interesting quotations from its editions many years ago, I should like to also give one similarly:

> If it were possible to measure with sufficient accuracy the velocity of light without returning the ray to its starting point, the problem of measuring the first power of the relative velocity of the Earth with respect to the aether would be solved. This may not be as hopeless as might appear at first sight, since the difficulties are entirely mechanical and may possibly be surmounted in the course of time.

The names of the authors are Michelson and Morley, the year of publication is 1887. This is the paper in which Michelson and Morley give their account of the historical experiment for "measurement" of the two-way light velocity. The paper was published in two journals: *The Philosophical Magazine* and *American Journal of Science*. After giving this general opinion, Michelson and Morley proposed an experiment which is almost the *same* as my deviant "coupled mirrors" experiment [5, 6, 2]. They proposed to use a bridge method with two selenium cells where the null instrument is a telephone. I must emphasize that I could not succeed in finding a *single* paper or book treating the historic Michelson-Morley experiment, where information on their one-way proposal should be given. Let me note that in the Michelson-Morley experiment one compares the two-way light velocity in two mutually perpendicular directions, but one cannot measure its value.

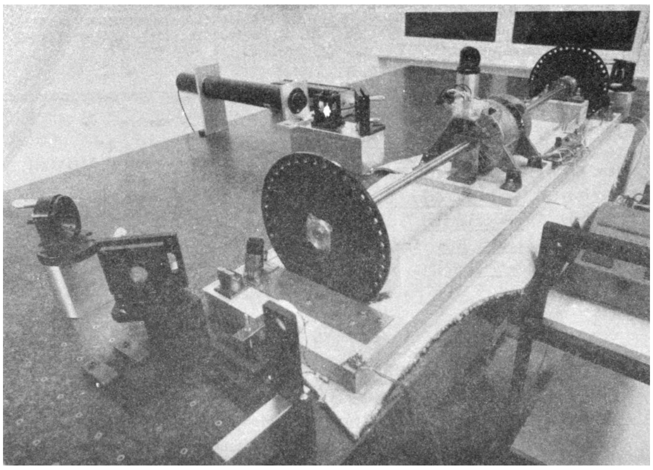

Fig. 1: The Graz "coupled shutters" experiment during preliminary measurements in the air of the laboratory; when performing measurements in vacuum the laser was mounted in parallel with the axle and the regulator for motor's velocity (to be seen between the motor and the far disk) was taken outside the evacuated space. At the left corner of the apparatus' plate one sees the socket for one of the reflecting mirrors for the case that Sun's light should be used (the socket of the other reflecting mirror is at the far right corner). The mechanician spent considerable time (and I lost money) for mastering the *adjustable* reflecting mirrors for Sun's light which have not been used in the laser arrangement, so that the price of the actually used apparatus had to be less than the half.

2 Theory of the "coupled shutters" experiment

A rotating axle driven by an electromotor, located exactly at the middle of the axle, has two holed discs at its extremities. The distance from the centres of the holes to the centre of the axle is R and the distance between the discs is d. Light from a laser is divided by a semi-transparent prism and the two beams are directed by a couple of adjustable mirrors, to the opposite ends of the rotating axle, so that the beams can pass through the discs' holes in mutually opposite directions. Any of the beams, after being chopped by the near disc and "detected" by the far disc, illuminates a photocell. By means of a galvanometer one measures the difference in the currents generated by both photocells. If covering one of the cells, one measures the current produced by the other cell.

One arranges the position of the laser beam with respect to the discs' holes in such a manner that when the axle is at rest the light of the laser which passes through the near hole illuminates *half* of the far hole. One then sets the axle in rotation, gradually increasing its speed. Since the light pulses cut by the near holes have a transit time in order to reach the far holes, with the increase of the rate of rotation less and less light will pass through the far holes, when the distant holes "escape" from the light beam positions, and, conversely, more and more light will pass through the far holes, when the distant holes "enter" into the light beam positions. For brevity I shall call the first kind of far holes "escaping" and the second kind of far holes "entering".

If one assumes that the holes as well as the beams' cross-sections are rectangular and the illuminations homogeneous, then the current I_{hom} produced by either of the photocells will be proportional to the breadth b of the light spot measured on the surface of the photocell when the axle is rotating, i.e., $I_{hom} \sim b$. When the rotational rate of the axle increases by ΔN, the breadth of the light beam passing through the "escaping" holes will become $b - \Delta b$, while the breadth of the light beam passing through "entering" holes will become $b + \Delta b$, and the produced currents will become $I_{hom} - \Delta I \sim \sim b - \Delta b$, $I_{hom} + \Delta I \sim b + \Delta b$. Thus

$$\Delta b = b \, \frac{\Delta I}{I_{hom}} \,, \tag{1}$$

where ΔI is the *half* of the *change* in the *difference of the currents* produced by the photocells.

One rotates the axle first with $\frac{\Delta N}{2}$ counter-clockwise and then with $\frac{\Delta N}{2}$ clockwise, that corresponds to a change ΔN in the rate of rotation. Since

$$\Delta b = (d/c)\, \pi \, \Delta N R \,, \tag{2}$$

for the one-way velocity of light one obtains

$$c = \frac{2\pi \, \Delta N R d}{b} \, \frac{I_{hom}}{\Delta I} \tag{3}$$

In my experiment the holes, as well as the light beams, were circular, not rectangular. Consequently, instead of the measured light spot's breadth, one has to take a certain *slightly different* "effective" breadth. As the breadth b can never be measured accurately, the discussion of the difference between real breadth and "effective" breadth is senseless. Much more important, however, was the fact that the illumination in the beams' cross-sections was not homogeneous: at the centre it was maximum and at the periphery minimum. Thus the simplified relation (1) did not correspond to reality if under I_{hom} one would understand the measured current. I shall give here a certain amelioration of formula (1), which was omitted in Ref. [1], because of a fear that the presumed referee would consider my analysis as an "artificial speculation" in a search "to adapt the observed values to the theoretical formula". Now I am no more afraid of the referee. The illumination will be assumed to increase *linearly* from zero on the periphery of the light beam to a maximum at its center where the beam is "cut" by the holes' rims. The *real* current I which one measures is proportional to a certain *middle* illumination across the whole light beam, while the *real* current ΔI is proportional to the *maximum* illumination at the centre of the light beam. On the other hand, one must take into account that when the holes let the light beam fall on the photocell, first light comes from the peripheral parts and at the end from the central parts. When half of the beam has illuminated the photocell, the "left" part of the beam begins to disappear and its "right" part begins to appear, the breadth remaining always *half* of the beam. Then the holes' rims begin to extinguish first the central parts of the beam and at the end the peripheral parts. Here, for simplicity, I suppose that the cross-sections of the beams and of the holes are the same (in reality the former were smaller than the latter). Thus during the first one-third of the time of illumination the "left" half of the light beam appears, during the second one-third of the time of illumination the "left" half goes over to the "right" half, and during the last one-third of the time of illumination the "right" half disappears. Consequently, the *real* current, I, produced by the photocell will be related to the *idealized* current, I_{hom}, corresponding to a *homogeneous illumination with the central intensity* and *generated by a light spot having the half-breadth of the measured one*, by the following connection

$$\begin{aligned} I &= \frac{1}{2} \int_0^1 I_{hom} \, x \left(\frac{2}{3} - \frac{x}{3} \right) dx \; = \\ &= \frac{I_{hom}}{6} \left(x^2 - \frac{x^3}{3} \right) \Bigg|_0^1 = \frac{I_{hom}}{9} \,. \end{aligned} \tag{4}$$

In this formula $I_{hom} dx$ is the current produced by a strip with breadth dx of the light beam; at the periphery of the beam (where $x = 0$) the produced current is zero and at the centre (where $x = 1$) it is $I_{hom} dx$. The current $I_{hom} dx$ is produced (i.e. the corresponding photons strike the photocell) during time $\frac{2}{3} - \frac{x}{3}$; for the periphery of the beam this time is $\frac{2}{3} - \frac{0}{3} = \frac{2}{3}$ and for the centre of the beam this time is $\frac{2}{3} - \frac{1}{3} = \frac{1}{3}$. The factor $\frac{1}{2}$ before the integral is present because the *measured* breadth of the light spot over the photocell is *twice* its *working* breadth. Putting (4) into (3), one obtains

$$c = \frac{2\pi \Delta N R d}{b} \, \frac{9I}{\Delta I} \,. \tag{5}$$

According to my absolute space-time theory [2, 6, 7] (and according to anybody who is acquainted *even superficially* with the experimental evidence accumulated by humanity), if the *absolute velocity's component* of the laboratory along the direction of light propagation is v, then the velocity of light is $c - v$ along the propagation direction and $c + v$ against. For these two cases formula (5) is to be replaced by the following two

$$\begin{aligned} c - v &= \frac{2\pi \Delta N R d}{b} \, \frac{9I}{\Delta I + \delta I} \,, \\ c + v &= \frac{2\pi \Delta N R d}{b} \, \frac{9I}{\Delta I - \delta I} \,, \end{aligned} \tag{6}$$

where $\Delta I + \delta I$ and $\Delta I - \delta I$ are the changes of the currents generated by the photocells when the rate of rotation changes by ΔN. Dividing the second formula (6) by the first one, one obtains

$$v = \left(\frac{\delta I}{\Delta I}\right) c\,. \tag{7}$$

Thus the measuring method consists of the following: One changes the rotational rate by ΔN and measures the change in the current of either of the photocells, which is $\Delta I \simeq \Delta I \pm \delta I$; then one measures the difference of these two changes which is $2\delta I$. I made both these measurements by a differential method with the same galvanometer, applying to it the difference of the outputs of both photocells. To measure $2\Delta I$ I made the far holes for one of the beam "escaping" and for the other "entering". To measure $2\delta I$ I made all far holes "escaping" (or all "entering").

3 Measurement of c

In the Graz variation of my "coupled-shutters" experiment I had: $d = 120$ cm, $R = 12$ cm. The light source was an Ar laser, the photocells were silicon photocollectors, and the measuring instrument was an Austrian "Norma" galvanometer. I measured $I = 21$ mA (i.e., $I_{hom} = 189$ mA) at a rotational rate of 200 rev/sec. Changing the rotation from clockwise to counter-clockwise, i.e., with $\Delta N = 400$ rev/sec, I measured $\Delta I = 52.5\,\mu$A (i.e., the measured change in the difference current at "escaping" and "entering" far holes was $2\Delta I = 105\,\mu$A). I evaluated a breadth of the light spot $b = 4.3 \pm 0.9$ mm and thus I obtained $c = (3.0 \pm 0.6)\times 10^8$ m/sec, where error is taken as only the error in the estimation of b, because the "weights" of the errors introduced by the measurement of d, R, ΔN, I, ΔI were much smaller. I repeat, the breadth b cannot be measured exactly as the peripheries of the light spot are not sharp. As a matter of fact, I chose such a breadth in the possible uncertainty range of ± 1 mm, so that the exact value of c to be obtained. I wish once more to emphasize that the theory for the measurement of c is built on the assumption of rectangular holes and light beams cross-sections and linear increase of the illumination from the periphery to the center. These simplified assumptions do not correspond to the more complicated real situation. Let me state clearly: The "coupled shutters" experiment is not to be used for an *exact* measurement of c. It is, however, to be used for sufficiently accurate measurement of the variations of c due to the absolute velocity of the laboratory when, during the different hours of the day, the axis of the apparatus takes different orientations in absolute space due to the daily rotation of the Earth (or if one would be able to place the set-up on a rotating platform). The reader will see this now.

4 Measurement of v

The measurement of c is an *absolute*, while the measurement of v is a *relative*, taking the velocity of light c as known. According to formula (7) one has to measure only two difference currents: $2\Delta I$ (at "escaping" *and* "entering" far holes) and $2\delta I$ (at "escaping" or "entering" far holes). The measurement in the air of the laboratory had two important inconveniences: (1) Dust in the air led to very big fluctuations in the measured current differences and I had to use a big condenser in parallel with the galvanometer's entrance, making the apparatus very sluggish; (2) The shrill of the holed disks at high rotational rate could lead to the same gloomy result as when executing the experiment in the apartment of my girlfriend. Thus I covered the whole set-up with a metal cover and evacuated the air by using an oil pump (this amelioration cost an additional 9,000 Shilling, i.e. USD 700,–). The performance of the experiment in vacuum has also the advantage that those people who wish to save at any price the false dogma of the constancy of the velocity of light, cannot raise the objection that the observed effect is due to temperature disturbances.

The measurement of ΔI is a simple problem as the effect is *huge*. Moreover all existing physical schools cannot raise objections against the theory presented above. However, the measurement of δI which is with three orders lower than ΔI has certain peculiarities which must be well understood. When changing the rotation from clockwise to counter-clockwise, the current produced by the one photocell changes, say, from I_1 to $I_1 + \Delta I_1 + \delta I_1$ and of the other photocell from, say, I_2 to $I_2 + \Delta I_2 - \delta I_2$. One makes I_1 to be equal to I_2, changing the light beam positions by manipulating the reflecting mirrors micrometrically. One can with difficulty obtain an exact compensation, so that the galvanometer shows a certain residual current I'. The current change ΔI_1 will be equal to the current change ΔI_2 only if the experiment is *entirely symmetric*. But it is difficult to achieve a complete symmetry (and, of course, I could not achieve it in my experiment). There are the following disturbances: On the one hand, the distribution of the light intensities in the cross-sections of both beams and the forms of the beams are not exactly the same; thus the covering of the same geometrical parts of both beams when changing the rotation of the axle does not lead to equal changes in the light intensities of both beams and, consequently, to $\Delta I_1 = \Delta I_2$. On the other hand, although the photocells were taken from a unique Sun collector cut in two pieces, even if the changes in the illuminations should be equal, the produced currents may become different (the current gain at the different points of the photocells is not the same, the internal resistances of the cells are not equal, etc. etc.). Thus after changing the rotational rate from clockwise to counter-clockwise, I measured certain current I'', but $I'' - I'$ was *not* equal to $2\delta I$, as it *must be* for an entirely symmetric setup. However, measuring the difference $I'' - I'$ during different hours of the day, I established that it was "sinusoidally modulated". This "sinusoidal modulation" was due to the absolute velocity v. All critics of my "rotating axle" experiments vociferate

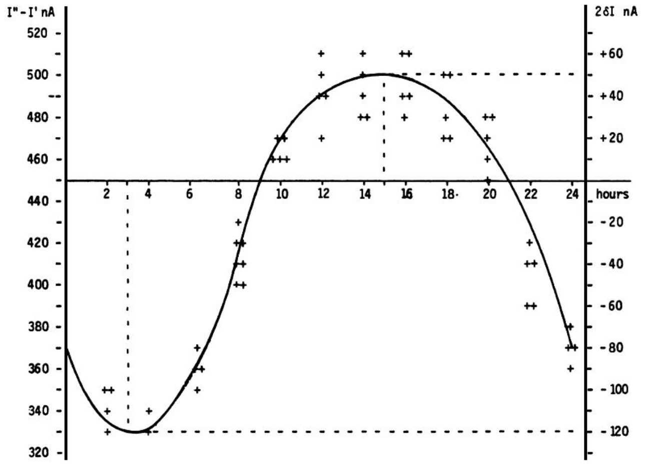

Fig. 2: Measurement of $2\delta I$. The points give the measurements at the even hours for the days from the 9th to the 13th February 1984.

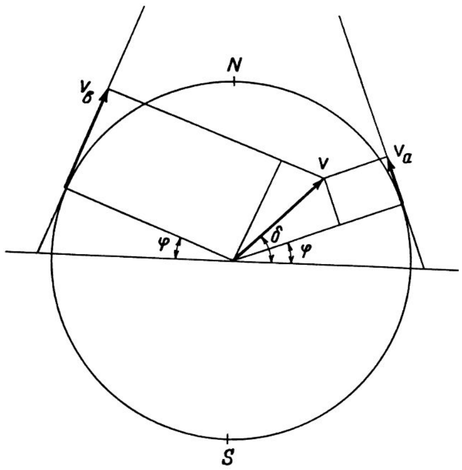

Fig. 3: The Earth and its absolute velocity at the two moments when the laboratory meridian lies in the velocity's plane.

mostly against the vibrations of the axle, asserting that these vibrations will mar the whole measurement. Meanwhile the axle caused me *absolutely no troubles*. When measuring in vacuum the axis of the apparatus pointed north/south.

I measured the "sinusoidal modulation" over 5 days, from the 9th to the 13th February 1984. As I did the experiment alone, I could not cover all 24 hours of every day. The results of the measurements are presented in Fig. 2. The most sensible scale unit of the galvanometer was 10 nA and the fluctuations were never bigger than 20 nA. The daytime hours are on the abscissa and the current differences on the left ordinate. After plotting the registered values of $I'' - I'$ and drawing the best fit curve, the "null line" (i.e., the abscissa) is drawn at such a "height" that the curve has to cut *equal* parts of the abscissa (of any 12 hours). Then on the right ordinate the current $2\delta I$ is taken positive upwards from the null line and negative downwards. Since 105 μA corresponds to a velocity 300,000 km/sec, 10 μA will correspond approximately to 30 km/sec. Considering the fluctuations of the galvanometer as a unique source of errors, I took ± 30 km/sec as the uncertainty error in the measurement of v.

When $2\delta I$ has maximum or minimum the Earth's absolute velocity lies in the plane of the laboratory's meridian (Fig. 3). The velocity components pointing to the north are taken positive and those pointing to the south negative. I always denote by v_a the component whose algebraic value is smaller. When both light beams pass through "escaping" holes, then, in the case that the absolute velocity component points to the north, the "north" photocell produces less current than the "south" photocell (with respect to the case when the absolute velocity component is perpendicular to the axis of the apparatus), while in the case that the absolute velocity component points to the south, the "north" photocell produces more current. If the light beams pass through "entering" holes, all is vice versa. Let me note that for the case shown in Fig. 3 (which does not correspond to the real situation, as in reality v_a is negative) both velocity components point to the north and both v_a and v_b are positive. In this case the "variation curve" no longer has the character of a "sinusoid"; it has 4 extrema (for 24 hours) and the "null line" must be drawn tangentially to the lowest minimum.

As can be seen from Fig. 3, the two components of the Earth's absolute velocity in the horizontal plane of the laboratory, v_a and v_b, are connected with the magnitude v of the absolute velocity by the following relations

$$v_a = v \sin(\delta - \phi), \qquad v_b = v \sin(\delta + \phi), \tag{8}$$

where ϕ is the latitude of the laboratory and δ is the declination of the velocity's apex. From these one obtains

$$\begin{aligned} v &= \frac{\left\{v_a^2 + v_b^2 - 2\,v_a v_b \left(\cos^2\phi - \sin^2\phi\right)\right\}^{\frac{1}{2}}}{2 \sin\phi \cos\phi}\,, \\ \tan\delta &= \frac{v_b + v_a}{v_b - v_a}\,\tan\phi\,. \end{aligned} \tag{9}$$

Obviously the apex of v points to the meridian of v_a. Thus the right ascension α of the apex equaled the local sidereal time of registration of v_a. From Fig. 2 it is to be seen that this moment can be determined with an accuracy of $\pm 1^{\mathrm{h}}$. Thus it was enough to calculate (with an inaccuracy not larger than ± 5 min) the sidereal time t_{si} for the meridian where the local time is the same as the standard time t_{st} of registration, taking into account that the sidereal time at a middle midnight is as follows:

22 September	—	0^{h}	23 March	—	12^{h}
22 October	—	2^{h}	23 April	—	14^{h}
22 November	—	4^{h}	23 May	—	16^{h}
22 December	—	6^{h}	22 June	—	18^{h}
21 January	—	8^{h}	23 July	—	20^{h}
21 February	—	10^{h}	22 August	—	22^{h}.

The graph in Figure 2 shows that on the 11th February (the middle day of observation) I registered in Graz ($\phi = 47°$, $\delta = 15° 26'$) the following components of the absolute velocity at the following hours (for $2(\delta I)_a = -120$nA, and $2(\delta I)_b = 50$nA)

$$\begin{aligned} v_a &= -342 \pm 30\,\text{km/sec}, \quad (t_{st})_a = 3^{\rm h} \pm 1^{\rm h}, \\ v_b &= +143 \pm 30\,\text{km/sec}, \quad (t_{st})_b = 15^{\rm h} \pm 1^{\rm h}, \end{aligned} \tag{10}$$

and formulae (9) give

$$\begin{aligned} v &= 362 \pm 40\,\text{km/sec}, \\ \delta &= -24° \pm 7°, \quad \alpha = (t_{si})_a = 12.5^{\rm h} \pm 1^{\rm h}. \end{aligned} \tag{11}$$

where the errors are calculated supposing $\phi = 45°$.

The local sidereal time for the observation of v_a (i.e., the right ascension of the absolute velocity's apex) was calculated in the following manner: As for any day the sidereal time increases by $4^{\rm m}$ (with respect to the solar time), the sidereal time at midnight on the 11th February (which follows 21 days after midnight on the 21 January) was $8^{\rm h} + 1^{\rm h}\,24^{\rm m} = 9^{\rm h}\,24^{\rm m}$. At $3^{\rm h}$ middle European (i.e., Graz) time on the 11th February the local sidereal time on the 15th meridian was $9^{\rm h}\,24^{\rm m} + 3^{\rm h} = 12^{\rm h}\,24^{\rm m}$. On the Graz meridian the local sidereal time was $12^{\rm h}\,24^{\rm m} + 2^{\rm m} = 12^{\rm h}\,26^{\rm m} \simeq 12.5^{\rm h}$.

Important remark. I now establish that when calculating the local sidereal time of observation of v_a for my interferometric "coupled mirrors" experiment [2, 6, 8, 9], I made a very *unpleasant error*. As Sofia ($\lambda = 23° 21'$) lies westwards from the middle zonal meridian ($\lambda = 30°$), I had to *subtract* the difference of $6° 39'$, which corresponds to $27^{\rm m}$, from the local sidereal time of the zonal meridian. Instead of doing this, I *wrongly* added. Thus the numbers given by me are to be corrected as follows:

Observation:	Wrongly calculated:	To be corrected to:
12 July 1975	$(t_{si})_a = 14^{\rm h}\,23^{\rm m}$	$(t_{si})_a = 13^{\rm h}\,30^{\rm m}$
11 January 1976	$(t_{si})_a = 14^{\rm h}\,11^{\rm m}$	$(t_{si})_a = 13^{\rm h}\,17^{\rm m}$
Right ascension of the apex of the Sun's absolute velocity	$\alpha = 14^{\rm h}\,17^{\rm m}$	$\alpha = 13^{\rm h}\,23^{\rm m}$

I beg the persons who will refer to the measurement of the Sun's absolute velocity determined by me in 1975/76 to cite *always* the corrected figures given here and not the wrongly calculated figures presented in [2, 6, 8, 9, 10, 11] and in some others of my papers.

5 Conclusions

Comparing the figures obtained now by the Graz variation of my "coupled shutters" experiment with the figures obtained some ten years ago in Sofia by the interferometric "coupled mirrors" experiment, one sees that within the limits of the supposed errors they overlap. Indeed, on the 11 January 1976 I registered in Sofia the following figures

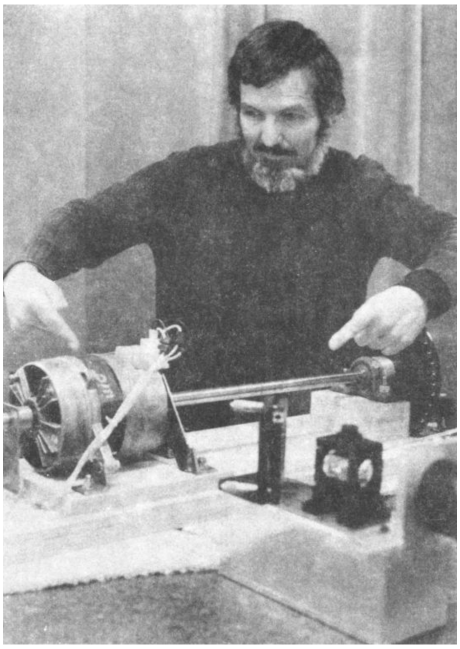

Fig. 4: February 1984. Explaining the essence of the "coupled shutters" experiment. My fingers show the ways in which both light beams go from the one perforated disk to the other. One can see on the photograph only a small part of the laser producing the initial light beam which is split by the semitransparent mirror seen in the photograph. The reflected beam goes to the left, while the refracted beam, after a reflection on the mirror seen in the photograph, goes to the right. Between the perforated disks, these two beams proceed in the opposite directions. The person who gave me a possibility to carry out my "coupled shutters" experiment in his laboratory took from me the solemn promise that I shall never say where have I carried it out. To my question, why is he so afraid, the answer was: "I do not wish one day to be poisoned by certain special services."

$$\begin{aligned} v &= 327 \pm 20\,\text{km/sec}, \\ \delta &= -21° \pm 4°, \quad \alpha = 13^{\rm h}\,17^{\rm m} \pm 20^{\rm m}. \end{aligned} \tag{12}$$

As for the time of one month the figures do not change significantly, one can compare directly the figures (11) with the figures (12). The declinations are the same. As the Graz measurements were done every two hours, the registration of the right ascension was not exact enough and the difference of about one hour is not substantial. I wish to point only to the difference between the magnitudes which is 35 km/sec. I have the intuitive feeling that the figures obtained in Sofia are more near to reality. The reason is that *I profoundly believe in the mystique of the numbers,* and my Sofia measurements led to the magic number 300 km/sec for the Sun's absolute velocity (which number is to be considered together with 300,000 km/sec for light velocity and 30 km/sec for the Earth's orbital velocity). The Graz measurement destroys this mystic harmony.

The presented account on the Graz "coupled shutters" experiment shows that the experiment is *childishly simple*, as I always asserted [1, 2]. If the scientific community refuses to accept my measurements for so many years and nobody tries to repeat them, the answer can be found in the following words of one of my *best physical and moral teachers*:

> ***Terrible is the power which an authority exerts over the world.***
>
> *Albert Einstein*

I wish to add in closing that with a letter of the 29 December 1983 I informed the Nobel committee that I am ready at any time to bring (for my account) the "coupled shutters" experiment to Stockholm and to demonstrate the registration of the Earth's absolute motion. With a letter of 28 January 1984 Dr. B. Nagel of the Physics Nobel committee informed me that my letter had been received.

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