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THE MEASUREMENT OF VELOCITY WITH ATOMIC CLOCKS[1]

By Dr. HERBERT E. IVES

BELL TELEPHONE LABORATORIES

About twenty-five years ago the Physical Society held a discussion of the theory of relativity, at which widely divergent views were expressed. I recollect that Professor E. W. Morley, speaking of the Michelson-Morley experiment, declared with great emphasis that this was a *physical* experiment and must have a *physical* explanation. On the same occasion a distinguished chemist, still living, declared, as his only contribution to the discussion, that the meeting would be known in history as the last time a scientific gathering treated the ether as a subject for serious discussion.

In spite of this discouragement I shall venture to discuss my subject as Professor Morley would, in terms of an ether or fixed framework. I do this partly because I know of no way to discuss the behavior of variable measuring instruments, such as atomic clocks, except by comparison with real or postulated invariant instruments. Partly also I do this because of the belief, for which I shall attempt some justification, that the ether has not yet been "abolished." I hope that even if I do not convert you to this point of view, I can enlist your sympathy for my preference for it.

It is my purpose, in the next few minutes, to discuss what happens to the measurement of *velocity* when the clocks we use for the determination of time are atomic clocks, which vary in their rate, when moving, according to a relation for which experimental evidence has recently been obtained from a study of the Doppler effect in hydrogen canal rays.[2]

As a preliminary to this discussion we must look at the concept of velocity and velocity measurement, as it

[1] Address of the retiring vice-president and chairman of the Section on Physics, American Association for the Advancement of Science, Columbus, December 29, 1939.

[2] *Jour. Optical Soc. America*, p. 215, July, 1938.

existed before the idea of clocks variable with motion was entertained. When Newton enunciated his laws of motion he made use of a concept of velocity which was based on absolute lengths and times. These were supposed to be determinable by measuring rods and clocks which were unaffected by the processes involved in their use. It is of course a commonplace of the science of measurement that all material instruments are subject to variation with temperature, humidity, aging and other conditions. It was, however, the faith of the Newtonians that by sufficient attention to the choice of materials and the control of conditions, measuring rods and clocks could be so constructed and used that the assumption of their invariance could be practically justified. The path of progress, through the development of accurate chronometers, and rods of carefully chosen alloys, would be to approximate more and more closely to instruments whose readings would confirm ever more accurately the Newtonian laws based on his concept of velocity.

A characteristic of Newtonian velocities is relativity, or Newtonian relativity, as it is sometimes called, by which was meant that velocities could be measured with the instruments and observers uniformly moving in any conceivable combination. To visualize this characteristic we may picture two freight trains passing each other. Using the freight car as our unit of length, it is indifferent whether we determine the relative velocities of the trains by counting the number of freight cars passing a clock fixed on one train; or by the time it takes a mark on a passing freight car to pass two clocks on the other train, these clocks having been set together and moved to opposite ends of a car on it. No one called in question the time measurement in the first case because the clock was moving, or in the second because one of the clocks had been moved to the other end of the car. Nor were the distances challenged because the measuring units, the freight cars, were in motion.

What now happens to such measurements if our clocks can not be made invariant with motion? This question might be investigated by the assumption of some arbitrary relation between clock performance and velocity. Or a relation might be deduced from the postulation of certain general relations (such as Newtonian relativity) which it is desired to preserve in spite of variation of clock performance. I do neither here, but start from the relation recently shown to hold experimentally for the frequency of the radiation from a hydrogen ion in motion in a canal ray tube. That is, considering such an ion as a clock emitting pulses of radiation, the frequency of the resultant radiation varies according to the relation

$$\nu = \nu_0 \sqrt{1 - \frac{v^2}{c^2}}$$

where v is the velocity of the particle, and c the velocity of light.

In thus basing my discussion on the results of an experiment rather than on certain bald assumptions, I must remind you that there is still an assumption involved. It is that the frequency change observed in the canal rays is a direct consequence of their velocity alone, and not, as some spectroscopists have suggested, a secondary effect associated with collisions or other interaction between the moving particles. If the latter should ultimately prove to be the case, certain of the perplexities which we shall shortly meet would disappear, and this particular discussion would become largely academic—to be superseded by others with their own quota of perplexities.

In addition to this variation of clock rate, I shall use the Fitzgerald contraction to represent the variation of length of the measuring rod used to determine the distance factor in velocity. This I may point out is in a somewhat different category from the frequency change. Strictly speaking, it is an inference from an experiment which is based on an assumption. The experiment is the Michelson-Morley experiment. The assumption is that the light signals used in the experiment are located in a medium through which the earth and the apparatus move. Put in another way, the assumption is that, in spite of their terrestrial origin, the light waves become part and parcel of the system of light waves from the fixed stars, with respect to which we learn, by the phenomena of aberration and the Doppler effect, that the earth moves. Or, put still differently, the assumption is that the medium conveying the light signals does not partake of or is not entrained by the movement of the earth. I do not wish to appear to question this assumption. I merely want to make clear an important difference in the evidence we have for the contraction of frequency, observed as a positive effect, and for the contraction of length, inferred from a nul effect. Until we can observe the Fitzgerald contraction by the rapid motion of objects in the laboratory this difference stands. I shall then assume the Fitzgerald contraction, merely noting that most of what I have to say is substantially unaffected if I were to confine myself to the variable clock rate, which is covered by my title.

Before experimenting with these instruments in the measurement of velocity let us take a moment to examine the physical model which we may imagine for rods and clocks which behave this way in motion. Lorentz and Poincaré have pointed out that a charged electrified sphere, held in shape by surface forces, would in motion preserve its equilibrium by taking the ellipsoidal shape represented by the Fitzgerald contraction. Similarly, a spherical reflecting shell at whose center is a source emitting pulses of radiation set off by the reflection of previous pulses, that is, a "singing" system, would, if subject in motion to the Fitzgerald contraction, space the pulses farther apart, with a resultant frequency reduced in the ratio $\sqrt{1-\frac{v^2}{c^2}} : 1$. This is the behavior of our atomic clock, which we note is different from the kind of clock which Newton would probably have picked to measure his "uniformity flowing" time. For that something of the nature of an hour-glass or clepsydra would be more inherently fitted, and for such a clock we have no evidence that a change of rate would occur on motion. Finally the electromagnetic mass of a charged sphere would be changed in motion, as shown by Lorentz, in the ratio $\frac{1}{\sqrt{1-\frac{v^2}{c^2}}} : 1$ by this same change in shape, corresponding to the results of the Kaufmann and Bucherer experiments. We can thus claim to have a concrete idea of a physical model which accounts for the properties of the instruments we are to investigate.

Let us proceed now to make velocity measurements with our rods and clocks. Let the measurement be that already discussed in connection with Newtonian measurements, namely, the relative velocity of two trains of "freight cars," the cars all having been matched in length by superposition and forming our units of length. We shall first measure the relative velocity by noting the number of "cars" on one train which pass a clock at a fixed point on the other. This corresponds to the familiar method of estimating train speed by counting passing telegraph poles, or to the method of reading the log on a ship. For this measurement we have the formula

$$V_A = \frac{v \pm w}{\sqrt{1-\frac{v^2}{c^2}}\sqrt{1-\frac{w^2}{c^2}}}$$

where V_A is the relative velocity as read, v and w are the Newtonian velocities, measured by invariant rods and clocks. The factor $\sqrt{1-\frac{v^2}{c^2}}$ shows the increase in reading due to the passing freight cars being contracted in length, the factor $\sqrt{1-\frac{w^2}{c^2}}$ the increase due to the clock running slow because of its velocity. Using this convention for measuring velocity, which we shall call convention A, we note that if the velocity of either body approximates the velocity of light, the apparent relative velocity approaches *infinity*.

This does not, however, mean that the velocity of light itself, so measured, would be infinite. If we consider light as a train of waves we regard these as of invariant length with respect to the Newtonian framework, or ether, so that if we could watch the passage of successive wave-crests, there would be no factor in the measurement for the contraction of length, as in the case of a material body. Our formula for the velocity of light is then

$$V_A = \frac{c \pm w}{\sqrt{1-\frac{w^2}{c^2}}}$$

Now we can not actually watch the crests of light waves pass a clock, but it is of interest to observe that this formula is the formula for the Doppler effect. Under certain conditions we can actually measure the change on the velocity of light by the Doppler effect, in conformity with this convention. Thus if we measure the Doppler shift of a line in the spectrum of a star in the east in the early evening, and again in the west in the early morning, the change in observed wavelength gives the change in the velocity of light relative to the earth's surface, due to the earth's axial velocity. This is the only interpretation, unless we are so hopelessly geocentric that we believe the whole stellar universe oscillates with a 24-hour period in order to maintain the velocity of light constant with respect to the earth!

This convention of velocity measurement requires that the clock be on one body, the measuring unit on the other. It becomes inoperative, if one of the bodies is a point without extension, or, in the case of light, if the velocity of a single light pulse is to be measured. For these conditions another convention, entirely equivalent in the Newtonian system, is available. It consists in noting the time of passage of a point on one "train" past the two ends of a freight car on the other. This corresponds to timing a race by watches at the two ends of the course. Here at once we meet with a fundamental difficulty. The clock at the far end of the freight car, or the race course, must be set to agree, not only in rate, which we can assume can be accomplished with sufficient precision, but for its zero, with that at the initial point. Let us set the two clocks together at the initial point. We must then transport one clock to the distant point. We must do this at *some* velocity, and while so doing we change its rate. In consequence we accumulate a difference of setting, which remains after the clock has reached the distant point. Our readings of time interval are therefore functions of the speed with which we have moved the clock.

The formula for relative velocity applying to this case, which will be called the B convention, is

$$V_B = \frac{v \pm w}{\left(1 \pm \frac{vw}{c^2}\right) - \left(\frac{v \pm w}{c}\right)\left[\frac{\sqrt{1+\frac{w_c^2}{c^2}} - 1}{\frac{w_c}{c}}\right]}$$

where w_c is the observed velocity of transport of the clock as measured by the A convention above.

For the case where the velocity v is the velocity of light we have

$$V_B = \frac{c \pm w}{\left(1 \pm \frac{w}{c}\right) - \left(1 \pm \frac{w}{c}\right)\left[\frac{\sqrt{1+\frac{w_c^2}{c^2}}-1}{\frac{w_c}{c}}\right]}$$

which becomes

$$= \frac{c}{\left[1 \pm \frac{w_c^2}{c^2}\right]}.$$

for small values of $\frac{w_c}{c}$. That is, the measured velocity of a particle moving with the velocity of light or of a light pulse is always *greater than c*.

Inspecting these formulae, we see that, as the observed velocity of transport of the clock becomes infinitely small, the limiting value for observed relative velocity is

$$V_B' = \frac{v \pm w}{1 \pm \frac{vw}{c^2}}$$

which for $v = c$ gives

$$V_B' = c$$

We can tabulate these results as given in Table 1.

TABLE 1

	Formulae for relative velocities	Particle moving with velocity of light	Velocity of light
Convention A	$V_A = \frac{v \pm w}{\sqrt{1-\frac{v^2}{c^2}}\sqrt{1-\frac{w^2}{c^2}}}$	$V_A = \infty$	$V_A = \frac{c \pm w}{\sqrt{1-\frac{w^2}{c^2}}}$
Convention B	$V_B = \frac{v \pm w}{\left(1 \pm \frac{vw}{c^2}\right) - \left(\frac{v \pm w}{c^2}\right)\left[\frac{\sqrt{1+\frac{w_c^2}{c^2}}-1}{\frac{w_c}{c}}\right]}$	$V_B = \frac{c}{\left[1 - \frac{1}{2}\frac{w_c^2}{c^2}\right]}$	$V_B = \frac{c}{\left[1 - \frac{1}{2}\frac{w_c^2}{c^2}\right]}$
Convention B' $\left(\frac{w_c}{c} \simeq 0\right)$	$V_B' = \frac{v \pm w}{1 \pm \frac{vw}{c^2}}$	$V_B' = c$	$V_B' = c$

With this table before us several questions present themselves. It is pertinent to ask which convention of measurement is "correct." (In the Newtonian scheme this question does not arise, since they are all equivalent.) I think the only possible answer is that one is as correct or incorrect as another. The thing which is undermined is the idea, inherited from the Newtonian framework, that velocity is, from the standpoint of measurement with available instruments, a uniquely determinable quantity. We are in this matter the slave of our instruments. My own conclusion is that the Newtonian framework, with its invariant rods and clocks, is the only firm foundation, of sufficiently wide base, that we can use as a starting point for speculation.

The second question refers specifically to the B′ convention, that is, the use of clocks transported infinitely slowly. This will be recognized from the resulting formulae as that embodied in the Lorentz transformations and in Einstein's Special Theory of Relativity. The latter was actually evolved in terms of the velocity of light taken as a constant "*c*." The fact that this postulate is equivalent to the prescription of clocks moved infinitely slowly, of lengths measured by rods laid alongside, has long been recognized, but not, I think, with the critical attitude that it deserves. What Einstein's second postulate amounts to (as is clear from his own original statement, rarely reproduced in full) is *a statement of a measuring procedure which is to be adhered to.* There is no philosophical justification offered for this procedure, which, as we have seen, is not the only one. It is justified only on the ground that the velocity of light is "*c*" "as derived from experiment." The fact is that the velocity of light has been measured practically by methods equivalent to the B′ convention. Had the clocks used been moved with other than infinitesimal velocity, experiment would have yielded different values for the velocity of light. The second postulate is then a prohibition on the use of any but one measuring procedure. This prohibition has no explanation except when studied, as we have studied it, with reference to the Newtonian framework. Many of the popular paradoxes and metaphysical speculations connected with the subject are largely peculiarities of this procedure. I have in mind particularly those speculations which transfer the properties of our rods and clocks to the nature of space and time, and seek to overlay the Newtonian conception of these with warped and imaginary attributes. This prescription of a special convention of velocity measurement, I may remind you, applies not only to the Special Theory of Relativity, but to the General Theory, which is a speculation on the properties of the so-called "chronotopic interval," which is a function of the same convention of measurement.

I am well aware that in thus objecting to a prescribed but not unique measuring procedure as the method of studying a branch of physics I am out of step with many present writers on physical theory. The statement has been made, and repeated with approval, that the sole object of physical theory is the ability to predict the results of experiment. Formulae whose derivation is shrouded in obscurity, but work, are extremely popular. Notwithstanding this, such an attitude, while justified perhaps in engineering, or in a new and largely unsystematized branch of science, deserves, I feel, no permanent place in a system of natural philosophy.

A topic intimately related to this study of velocity measurement with atomic clocks is the question of the existence of a fundamental reference framework or medium such as I have assumed in this discussion. I have expressed the opinion above that the Newtonian framework with its implied invariant rods and clocks is the only satisfactory basis for discussion of this subject. The luminiferous ether of Maxwell and his followers has the characteristics of such a framework. In it the velocity of light has the value c when measured by invariant rods and clocks. It has been claimed, and the claim has received wide popularization, that this framework or medium is non-existent because the theory of relativity shows that it is unnecessary.

I want for a moment to examine this claim in the light of the previous discussion. What it actually amounts to is this: that in place of a physical concept, built on a wide range of experiments, which include all the wave phenomena exhibited by light, and such well-established facts as the aberration of light, we are asked to substitute a procedure of measurement which narrowly restricts our freedom of instrumental manipulation. That is, if in measuring velocities we agree to use only that method of time measurement which employs two clocks, and agree never to move our rods or clocks at more than negligible velocities, and if we are content not to worry about the reason for using this restricted measuring procedure, then we do not need, we can indeed, in a sense, "abolish" the ether. It is an eminently practical procedure, since we can not ordinarily move our rods and clocks at any but negligible velocities; but that it "abolishes" the ether or removes its need for the full consideration of the subject is, I think, an unwarranted claim. It is equivalent to telling the driver of a car that he can dismiss the engine from his mind, because if he presses the ignition button and moves clutch and throttle in a certain way the car will go; or to telling a locomotive engineer that geography is no longer necessary, because he can reach his destination if he will stick to the tracks and obey the signals. These working rules do not, I contend, "abolish" the engine or geography.

There is another argument which is often made against the luminiferous ether, namely, that it can not be physically detected, and hence, being unobservable, is equivalent to being non-existent. This argument, whether or no it is even tenable philosophically, is, I think, untenable in the light of one of the classic experiments in the field of optics, namely, the Sagnac experiment. In this experiment a beam of light is divided and sent in two directions around the periphery of a disc, which can be set in slow rotation. When so rotated an interference pattern between the two beams is shifted, by exactly the amount predicted on the assumption that the light is located in a fixed medium, with respect to which the apparatus revolves. (On a much larger scale, using the earth as the disc, the Michelson-Gale experiment gives the same result.) The Sagnac experiment has been repeated recently by Dufour and Prunier with various modifications, such as a stationery light source outside the apparatus, always with the same result. It shows that the velocity of light with respect to the disc is $c \pm r\omega$.

Now as I have shown elsewhere,[3] if on this apparatus we measure our time by atomic clocks carried (infinitely slowly) around the disc, thus employing the Einstein measuring convention, we would decide that the velocity of light is c, instead of $c \pm r\omega$. But we would find that these clocks were out of setting with one left at the origin, showing quite conclusively that the measured value c was a consequence of the movement of the transported clocks. Now if we use a very large disc, a portion of whose periphery is indistinguishable from a chosen straight path, we find that for both straight path and disc path the velocity of light measures c by the Einstein convention, but by continuing around the disc we have clearly demonstrated that this value is a function of the method of using the clocks. We must of course admit that the light has the same velocity whether sent along the short straight path or the short portion of the curved path indistinguishable from it. The only reasonable conclusion is that it has the value $c \pm r\omega$ in both cases. The properties of a moved clock (and the Fitzgerald contraction) give a complete explanation of why we obtain the value c in one case; $c \pm r\omega$ in the other.

The significance of the Sagnac experiment has been questioned by adherents of that article of faith that the velocity of light is always "c," but in my opinion

[3] *Jour. Optical Soc. America*, p. 296, August, 1938.

without success. It has been contended that it is the same kind of experiment as the Foncault pendulum, owing its result, in some manner not made clear, to the influence of "all the matter in the universe." "All the matter in the universe" thus becomes a new fixed framework, which is going far afield, for questionable gain, from the luminiferous ether which so directly and accurately explains the experiment. At any rate it can not be gainsaid, I think, that all the experiments in this field, from the aberration of light, through the Michelson-Morley and Kennedy-Thorndyke experiments, the recently demonstrated variation of atomic clock rate and the Sagnac and Michelson-Gale experiments, are consistently and satisfactorily described in terms of a luminiferous ether.

In conclusion, let me summarize the point of view I have adopted in these remarks, and the chief points I have tried to make. I have endeavored to present the subject from the standpoint of the experimental physicist, to whom the properties of his apparatus must be constantly under scrutiny, so that he may not ascribe to the phenomena he is observing what are in fact peculiarities of his instruments. I have taken the variation of atomic clock rate with velocity as indicated by experiment, and investigated the influence such variation will have on the measurement of one of the fundamental factors in physical theory, namely, velocity. It develops that a variety of "velocities" can be measured with such clocks, no one of which has any *a priori* claim to be chosen as "correct." They are all deviations from the simple Newtonian concept of velocity, which is in terms of rods and clocks which are unaffected by motion. I urge the merit of the Newtonian framework as the only unambiguous basis for the idea of velocity. I have further pointed out that one of the conventions for measuring velocity when using variant clocks is equivalent to the Lorentz transformations and the Second Postulate of the Restricted Theory of Relativity. Conversely, this postulate is nothing more than a specification of a method of measurement to be used to the exclusion of others. Conclusions drawn from it are consequently of limited applicability and significance. They constitute no ground for revolutionizing the Newtonian ideas of space and time. I have considered the popular claim that the ether has been "abolished," and pointed out that the essence of this claim is that for the ether it is proposed to substitute a particular prescribed measuring procedure, which has no justification except as derived from considerations involving the ether.

Reverting to experimental findings I have reviewed the experiment of Sagnac, having in mind the claim that the ether can not be detected experimentally. I have asserted that, in the light of the experimentally found variation of clock rate with motion, this experiment does detect the ether, and that it and others in this field are all in complete agreement with the existence of a fixed framework. These views will be recognized as those of the earlier students of the subject—Fitzgerald, Larmor, Lorentz—but not of those who would shift the burden from variant measuring instruments to the nature of space and time. If you remember my opening paragraph, you see that I stand with Professor Morley.

CONSTITUTIONAL BARRIERS TO INVOLVEMENT OF THE NERVOUS SYSTEM BY CERTAIN VIRUSES[1]

By ALBERT B. SABIN, M.D.

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IT has been realized for some time that in nature not all who are infected with certain neurotropic viruses exhibit signs of central nervous system disease; indeed, the number of those who develop paralysis or encephalitis may constitute a very small proportion of the total number of the animal or human population which is infected. The hypothesis that exposure to small doses of an unmodified, virulent, neurotropic virus can immunize without producing infection is still without experimental basis, because in the laboratory one usually finds that the dose is either large enough to initiate multiplication and infection or is too small to give rise to an immune response. Why, then, is infection apparent in some and inapparent in others? Instead of assuming that the major portion of a population is resistant because it has somehow acquired a specific immunity, we may inquire whether it might not be the other way around, *i.e.*, that immunity develops without disease because of some preexisting constitutional resistance. In the investigations which I shall now summarize an attempt

[1] Address delivered on December 28, 1939, to the Section on Medical Sciences upon receipt of the Theobald Smith Award of the American Association for the Advancement of Science. The work which forms the basis of this communication was carried out at the Rockefeller Institute for Medical Research in association with Dr. Peter K. Olitsky.