This summary article, written by George E. Hudson of the New York University Physics Department and William Atkinson of the Radio Standards Laboratory of the National Bureau of Standards, resulted from an informal discussion period during the International Conference on Precision Electromagnetic Measurements, which was held August 14–17, 1962, in Boulder, Colo. Norman Ramsey of Harvard presided. Sponsored by the NBS Radio Standards Laboratory in Boulder, the IRE Professional Group on Instrumentation, and the AIEE Instrumentation Division, the conference was partially supported by the National Science Foundation.

The Redefinition of the Second and the Velocity of Light

By **George E. Hudson** and **William Atkinson**

A VERY popular session at the International Precision Electromagnetic Measurements Conference was the Thursday night meeting organized to discuss the proposed atomic definition of the second and methods for measuring the velocity of light. This discussion was moderated and stimulated by Professor Ramsey, who entertained the participants on numerous occasions throughout the two-hour meeting with remarks such as his invitation to proponents of the pendulum clock to speak up, and with his method of smoothing over a somewhat semantic debate concerning either the proper qualifications to be furnished with a definition of the second or the question as to whether such qualifications are or are not part of the definition. The dialogue and response to Ramsey's balm went more or less as follows:

(*Ramsey*, addressing the debators) "I believe I agree with both of you. Let us suppose that in 1966, or before, that some transition in some material, let us say x or ——"

(*Unknown participant*) "Hydrogen."

(*Ramsey*) "Cesium."

(*Participants*) Laughter.

(*Ramsey*) "Well, yes, cesium might well be recommended by the subcommittee. Then, following the recommended definition, there might appear a qualifying remark such as: 'It is interesting to note that the frequencies of hydrogen and thallium transitions vary up and down together with respect to cesium by parts in 10^{12}'."

This remark delighted the listeners, who were hoping the discussion would be heated and who did not exactly expect the participants in the discussion to be completely impartial in assaying the merits of systems they had worked on.

After Professor Ramsey's opening remarks, the first part of the meeting was devoted to the redefinition of the second. John Richardson, for the benefit of those present who had not heard Dr. Markowitz's résumé in the afternoon session, summarized the historical developments that had taken place to set the stage for the discussion. The International Committee of Weights and Measures desires to make (in 1966) a specific recommendation to adopt a definition of the second based on an atomic system. To help achieve this goal, the committee created a subcommittee called the Consultative Committee for the Definition of the Second (CCDS), which is responsible for making recommendations specifying which particular atomic or molecular transition should be chosen, what the pertinent experimental conditions should be to permit this transition to be utilized, and what value should be assigned to the frequency of that transition in order to be as consistent as possible with the present definition of the second.

The evening discussion session was fortunate in having present Drs. Essen, Markowitz, De Prins, Bononomi, Henderson, Mockler, and Richardson of the CCDS subcommittee. Proponents of cesium, thallium, hydrogen, ammonia, rotational molecular transitions, and optical-pumped devices were found who made statements about the suitability of these substances or techniques for defining the unit of time. With the exception of the proponents of cesium, which undoubtedly would be chosen if the choice had to be made in a matter of months, the proponents thought it was too early to concede the inadequacy of the techniques they were most familiar with or were developing. It should be added, however, that not all those working with ammonia endorsed it, that only one person spoke on the suitability of molecular rotations, and that there was definite disagreement with a well-presented argument Dr. Packard made in favor of the optically pumped devices. Professor Ramsey declared that the likelihood of the atomic hydrogen maser being accepted as the international standard would probably depend on the efficacy of a solution to the wall-shift problem.

A factor that might influence the choice of the quantum system used to define the unit of time is the 1966 date. Presumably, the CCDS should make its recommendations before this date, but there is a question, because of the rapid advances being made, of just when the CCDS should submit its reports to the International Committee on Weights and Measures. Dr. Essen pointed out that frequencies of atomic transitions have already been compared by a number of nations, that they have been examined and tested more thoroughly than devices for realizing any other unit of measurement previously defined or redefined, and that there is ample evidence right now to support the adoption of an atomic standard for defining the unit of time. Professor Ramsey summarized the opinions expressed in a few remarks to which there was no voiced objection. The summary said in effect: We ought to stick to the 1966 date for defining a new unit of time. The definition ought to be the best that can be determined somewhat before that date by keeping our eyes open during the next two years for what will prove itself to be the best. Cesium is a good horse with a long head start and any other horse must show itself to be a really first-rate horse. There are some possibilities but no certainties that this will happen.

Should some standards prove to be better than cesium it would be difficult with currently operating techniques to effect the comparison between laboratories that would prove this to be so. Dr. De Prins noted that estimated accuracies within a single laboratory are often in the parts in 10^{12} to 10^{13} range, but comparisons between separate laboratories are reported in the 10^{10} to 10^{11} range. From the statistical viewpoint, trying to effect comparisons in the parts in 10^{12} range, atomic time comparisons have some advantage over frequency comparisons. During 1961 seven internationally located monitoring stations of the VLF stations GBR and NBA reported on the frequencies they received perhaps 90 percent of the days, so that only on relatively few days did all stations turn in a report. These missing data interfere with simple forms of such standard statistical tests as the analysis of variance, which can be made to resolve the observed variance into contributions from differences due to propagation and differences between the internationally located atomic standards themselves. A second factor interfering with the usual form of this test is the observed non-Gaussian nature of the frequencies reported by the monitors. With atomic time comparisons the failure of a report to be made on some day by a monitor is not so serious since, to obtain the average frequency difference between two monitors over a period, the monitors need only report reception times at the beginning and end of this period. The existing WWV pulses are not suitable for the prompt comparison of atomic time scales to the parts-in-10^{12} range. A slide presented at the meeting to give a comparison of atomic times assigned to WWV pulses by the Naval Observatory and by the National Bureau of Standards showed over a four-year period a discrepancy that averaged to 6 parts in 10^{11}, but that at times was in the parts-in-10^{10} range. Dr. Bender expressed the opinion that the need for a system capable of comparing time scales at various laboratories to 1 microsecond was really an urgent problem that needed a solution before 1966 in order to help in deciding what system and techniques should be used to define the second. Dr. Markowitz indicated that the Loran C modification that might be made by March 1963, should enable comparisons at the 1-microsecond level of accuracy to be made between the European and North American continents. During the week of the meeting there was a radio news broadcast indicating that the satellite Telstar was already being used for such purposes.

There was some question about the desirability of having two units of time intervals, one of an astronomical nature and the other of an atomic nature. The comment was made that these should differ appreciably so that there would be little possibility of forgetting to include a conversion factor in problems in which it was necessary to express a given interval sometimes in one unit and sometimes in the other unit. In response to this, it was pointed out that there are in use at present for scientific purposes seven types of time: Among these are Ephemeris Time, which is the time related to the present definition of the second, atomic time, and what has come to be called "universal time", of which there are three types. With regard to universal time, it is possible to speculate that had variations of the earth's rotational speed been more severe than they are, this quantity would have probably been called—and listed in tables by a name more nearly expressing what it is—angular position of the earth. Problems such as the possibility of the sun being overhead at noon are not made more serious by the proposal to define the second in terms of atomic transitions than they are at present with the second defined in terms of the motion of the earth about the sun, and not in terms of the completely independent and somewhat erratic revolution of the earth on its axis. It is possible to have several scales of time, a civil scale and a scientific scale, that have the same unit of interval but differ by a transformation which expresses the effects of discontinuities similar to leap year, and which might for convenience be incorporated into the civil scale. Dr. Richardson did express in his opening remarks the thought that it would be wise to give somewhere in the report of the CCDS to the parent committee the experimental limits, which admittedly might be poor, to support the idea that what we call time (or more appropriately, proper time) is really a single concept and not a complex concept containing two or more as yet unresolved aspects, say atomic time and gravitational time. In the case of mass, which does have both inertial and gravitational aspects, it is possible to give experimental limits that do (to a part in 10^{10}) support the equivalence of inertial mass and gravitational mass, which are both measured in terms of the same unit. These limits, however, are not given in the definition of unit mass so that, in this case, a precedent does not exist for supplying similar limits that would apply to the unit-of-time interval.

The second part of the evening discussion was devoted to experiments in progress, or proposed, for determining the velocity of light. The points were raised that such experiments should have as their aims an increase in the present accuracy of the value of c and a verification of the hypothesis that c is independent of frequency.

An experiment in progress at the NBS Boulder Laboratories and reported by Mr. Baird utilizes a microwave interferometer of the Michelson type to measure the speed of 50 Gc electromagnetic waves. The primary difference between the interferometer and earlier ones is the region of operation. The NBS model operates in the Fresnel or near field region. This permits the use of a diffraction correction that is calculated on the basis of a rigorous solution of Maxwell's equations. The accuracy of this determination will be about 3 or 4 parts in 10 million, which is equal to the best determination to date made by Froome. One of the most useful results of this work is the analysis of the diffraction correction; another feature of value is that the experiment represents an independent determination at a different frequency and employs a method different from those of previous experiments.

Dr. Boyne of the NBS in Washington analyzed a novel method utilizing an optical maser as a source. The method would necessitate the measurement of the difference frequency of two gas-laser lines of the helium-neon system. This frequency is about 1600 Gc, and Dr. Boyne explained the use of a modified cathode-ray–traveling-wave tube using a photo cathode for accurate measurement.

Dr. Essen of the National Physical Laboratory commented on experiments at the NPL aimed at determining lengths in terms of frequencies, employing the velocity of light. He is of the opinion that their own determination of c will be to a few parts in 10^8.

During this part of the discussion, Mr. McNish made the point that present light-speed measurements are all less accurate than length and time-interval measurements. Hence, attempts to define lengths in terms of a frequency standard and an assumed light speed would make length determinations more inaccurate than they ought to be in comparison with present techniques based on an independent length standard (wavelength of the krypton line). Dr. Ramsey stated also that, since we have an MLT system, the value of the speed of light, although a theoretically invariant constant, must still be determined experimentally.

There was some discussion of the practical necessity for knowing the speed of light to a high accuracy, and Dr. Richardson expressed an interest in finding out just what are the present theoretical limitations, in concept, on the constancy of c. Dr. Shimoda noted that estimates of the limit of the deviation of the Coulomb law from the inverse square seem to yield a figure of 1 part in 10^{16} as an interesting region for investigation. Such deviations would point to the possibility of a nonzero photon rest mass with a consequent dispersion in the speed of light. Besides atomic effects of this kind, Richardson also had in mind a general relativistic limitation on our definition of standards. Dr. Ramsey, too, felt that graviton-photon interaction, although estimated to have an extremely tiny cross section, might yield a variation in light speed which would be of interest, and of ultimate importance when sufficiently precise experiments became possible. With these sentiments the meeting came to a close.

It is apparent that the main functions and results of these discussions were to raise and pose more questions than could be considered in detail in such a meeting, and certainly we do not intend in this paper to attempt a complete consideration of these problems. However, one subject of especial interest to the writers was repeatedly skirted by several of the participants. Some of the thoughts advanced by Ramsey, McCoubrey, and Markowitz and Essen, and brought into sharp focus by Richardson's questioning, indicated a grave concern with the limitations which our present relativistic notions of space and time place on the definition of a standard clock, dissemination of time information over the earth, and the concept of the universal constancy of the phase speed of light. We wish to add a few comments, to be developed at length in a later analysis. In this task, let us take the point of view, held currently by most physicists, that time and space are relative concepts and that the presence of gravitational fields is a manifestation of the curvature of the space-time manifold. Since we ordinarily use continuous coordinate systems to order events, we can deduce that some properties of the coordinate system characteristic of an extended region reflect the curvature. Moreover, when the invariant space-time displacement between two neighboring events is analyzed into two components, one spatial, and one temporal, this resolution may be done in many ways.

For example, any scalar projection of the displacement vector between two events in a timelike direction could be chosen as the temporal separation of the events—and any spacelike projection as the spatial component. The local directions of the coordinate-sys-

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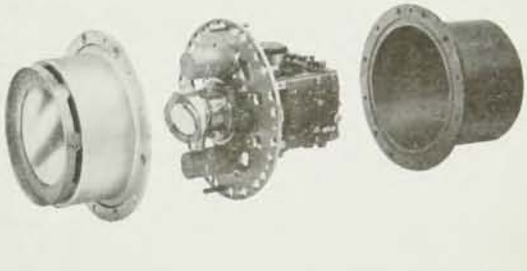

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In Europe: H. Dauber Associates, Inc., Ainmillerstrasse 1, 8 Munich 13, Germany tem axes could be utilized in this process. However, this method is much too arbitrary to be of general use. A better method, yet not a unique one, is to introduce a freely falling (i.e., inertial) frame of reference in the neighborhood of the events. This yields proper time and space components of the interval separating the events, for they are in principle measurable by an ideal standard clock and an ideal length standard attached to the inertial reference frame. But identical standards yield different space and time measures for different inertial systems. However, if the displacement between the events is a timelike one, there is *one* inertial frame (disregarding spatial rotation) which yields a temporal separation of the events, and a zero spatial separation. A somewhat analogous statement is valid, if the displacement is spacelike.

(1) **Standard clocks.** Such an instrument generates time intervals periodically. Given several clocks of different construction all of which operate without the necessity for a gravitational field (which excludes pendulum clocks!) it is ordinarily asserted that if these clocks are all resting in an inertial frame of reference, i.e., a "freely falling" reference frame, the clocks will record the same *proper* time. Moreover, if they are in different inertial reference frames, the proper time intervals which they generate are related by the Lorentz transformation between the frames, provided that the comparison is made with the clocks sufficiently near each other and for a short enough period of time. Hence, in the definition of the unit of time, one is inclined to specify that ideally all the parts of an atomic time standard should operate in an inertial frame of reference. However, this is not strictly realizable in an actual device. There is a spread in the velocity spectrum of the atoms in a cesium beam even though all are freely falling, hence, the atoms are in different inertial frames. So the practical realization of a time standard means that studies should be made of the effects of its departure from an ideal device. One should be able to correct the standard unit generated by an operating device to ideal field-free conditions. It is important that detailed studies of this be made in the near future. Present estimates indicate that the elastic distortions produced by operation in a reference frame on the earth's surface yield negligibly small effects. The second-order Doppler shift in a spectral line caused by temperature effects is *much* larger. We feel that, as far as gravitational effects on the earth's surface are concerned, the studies of the ideal standard time generator will lead to conceptual clarifications rather than to significant numerical corrections.

(2) **The constancy of the speed of light.** Any continuous space-time coordinate system, as a result of the curvature of space-time predicted by general relativity theory, must also partake of this distortion. Hence, the *coordinate* speed of light will differ from point to point because of gravitating masses. However, the speed measure over short enough distances in a freely falling—that is, an inertial—reference frame will always be the same invariant, c.

Often, an inertial reference frame can be introduced, locally and momentarily, so that its time axis coincides with that of the coordinate system and its spatial directions are in the spatial manifold of the coordinate system. Even in this case, coordinate length and time-scale values could disagree, even though generated by standard instruments (located at some other place), with the time and space units generated by standard instruments attached to the local inertial reference frame. They could disagree because of relativistic propagation effects through a gravitational field, or if portions of the coordinate system are accelerated, or in relative motion. This might result in local coordinate speeds for light which differ (near the earth) by as much as 1 or 2 parts in 10^9. This would be the case if the coordinate scale standards were located at a position where there is a different gravitational potential—or at some place in motion relative to the laboratory in which the coordinate light speed is being determined. However, a suitable choice of the space and time coordinates can of course be made so that in any small-enough region the coordinate speed and the proper light speed are equal. But this cannot be done everywhere simultaneously using only one coordinate system.

These remarks concerning the speed of light are valid for vanishingly small electromagnetic field strengths, for the energy density of a radiation field (even ignoring quantum effects) must itself act as a source of gravitational curvature to space-time. That is, when one speaks of the locally invariant light speed, c, he is contemplating a property of the *characteristics* of the field equations, which are used to ascribe a local Minkowski topological structure to space-time.

(3) **Dissemination of time.** Our present views on the nature of space-time had their origin in the analysis of methods of synchronization of clocks which are in relative motion. The full implications of these views are not easily kept in mind because of our almost daily use of the approximate Newtonian concept of an absolute universal time flowing smoothly everywhere. The more correct notion contained in the relativity theory and gravitational theory of Einstein is that we can generate in any restricted region of space-time, by a suitably constructed and operated clock, a *proper* time scale. In terms of the readings of such clocks, and in the same neighborhood of space-time, inertial observers will always agree (up to a linear Lorentz transformation) in their descriptions of similar physical phenomena occurring in the neighborhood.

In practice, to describe, convert, and compare happenings at large distances and different times, people must construct "coordinate systems" which assign sets of numbers to events. By relating the coordinate scales at every point over extended space-time regions to scales of inertial reference frames of limited extent and which move with the coordinate system in their neighborhoods, one obtains a measure of the metric

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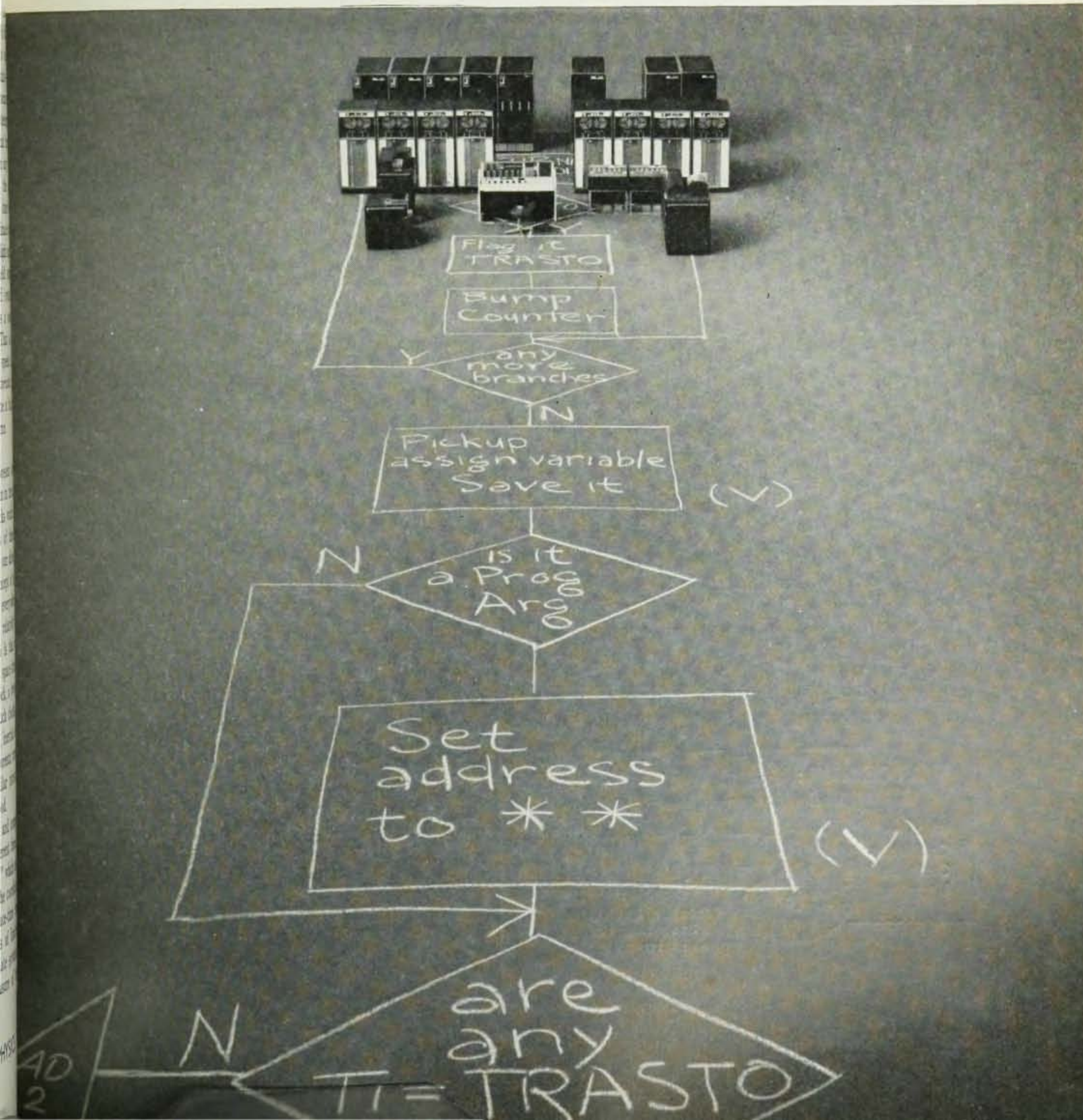

tensor field components, g_{ij}. In fact, the measure of the local *coordinate* speed of a light ray constitutes a partial determination of these local-component metric values. But, in order to make rational use of this information, one must be careful to use the *same* coordinate system over the whole macroscopic space-time region of interest.

Now, in principle, each proper clock *could* be used, in conjunction with suitably broadcast radio signals, to construct its own space-time coordinate system. There would be one such coordinate system for each clock, and, according to the theory of relativity, such coordinate systems would by no means directly yield the same description for observed phenomena. It is important to recognize and emphasize a limitation on the coordinate time and distance scales generated by one clock (even a proper, i.e., inertial, clock) at each point of an extended region containing a gravitational field. These scales can, in principle, be made to agree by a continuous transformation everywhere over the same region with those generated by another such clock. But the transformation would not in general be linear in the space-time variables. More important, for neither clock is it possible to find a continuous transformation of coordinates which would yield the *proper* time and space scales at every point of the region. Instead one must use the metric field components to effect such a comparison (after all, this is how the g_{ij}'s were determined originally!).

On the other hand, let a space-time coordinate system for the earth be chosen so that the time coordinate is generated by a single clock in the inertial system of the fixed stars (or a satellite clock, provided the time unit is properly corrected for the various frequency shifts). Then one finds that the *proper* time for an observer on the surface of the earth depends on his latitude, and that the coordinate light speed at the equator is greater or less than c by the equatorial speed of the earth. These coordinate effects are a reflection of the fact that no rigidly rotating frame of reference exists whose coordinate axes are all orthogonal in the relativistic sense. A set of inertial reference frames to cover the earth continuously cannot be found for which the spatial axes are always and everywhere perpendicular, for which the time coordinate measures the proper time, and for which the speed of light has the one coordinate value c.

Space does not permit us a more detailed examination of these questions. We merely hope that our description of these effects has made clearer the limitations which must be considered in adopting a conceptually correct redefinition of the unit of time. It is also our opinion that these ideas and problems furnish us with additional reasons why an atomic standard for proper time, as well as one for length, should be adopted. Such standard units are definable over restricted regions of space and time in such a manner as to satisfy the conceptual requirements of relativity theory.