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HAS THE SPEED OF LIGHT DECAYED RECENTLY? — PAPER 2

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Received 30 November 1987 Revised 16 December 1987

Abstract

Because its historical research and statistical analyses have no depth, this book (Norman and Setterfield, 1987) fails to prove that the speed of light has decreased over the past three centuries. Its theoretical interpretations are flawed, and in some parts do not make sense.

Introduction

This monograph (Norman and Setterfield, 1987) is an invited research report prepared for SRI International. The principal author appears to be Mr. Setterfield, a non-degreed researcher who has been studying the velocity of light since 1980. I have been corresponding with him since 1985 when I reviewed the early versions of his manuscript. I know little about Mr. Norman who was not a co-author at that time. For clarity I divide the book into two parts and discuss them separately. I call the first half (chapters 1-3) Part One, a statistical analysis of 163 values for the speed of light measured experimentally since 1668 A.D. The second half (chapters 4-7), Part Two, discusses the theoretical implications.

Part One aims to prove a very controversial thesis: that the speed of light has declined by a few tenths of a percent over the last 300 years. Such a decline would be very significant to physicists because the speed of light, c, is involved in almost all phenomena. A decline would also be very important to creationists in particular because, extrapolated into the past, it could provide evidence that light traveled much faster in the early days of the cosmos. That would explain how Adam could see light from the stars within a few days of their creation [Genesis 1:14-18], and how light from distant galaxies could reach us in only thousands of years.

The alleged decline may actually have occurred but at this time it does not seem likely. In spite of the fact that I would welcome such a simple explanation for the speed-of-light problem, I have serious doubts about the monograph. I have questions about the depth of the historical research and the quality of the scholarship involved. These questions are extremely important in this kind of historical survey of experimental data because interpretation can be difficult. One needs to understand the details of a particular experiment fairly well in order to assess its reliability and accuracy. In this case, when a survey of hundreds of experiments over centuries of time is involved, it is vital for the reader to have confidence (A) that Norman and Setterfield have researched their subject in sufficient detail and (B) that there is no bias, conscious or unconscious, in their analysis or presentation. When I did my early reviews, I had naively assumed that these points did not need to be questioned. But when I received the finished book, my confidence was undermined by disturbing details that kept emerging as I examined the work.

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A Disturbing Quote

The first of these unsettling details was the way Setterfield quoted some of my words. It was a minor incident but it turns out to be typical of a major problem with the whole monograph. Shortly after I began corresponding with Setterfield, I sent him a brief note on 24 May 1985 thanking him for the articles he had sent me and saying of the general goals of his research, "It is a good work that needs to be done." Then Setterfield sent me a preliminary draft of what later developed into his book asking me to review it for technical problems. On 8 September 1985 I wrote him:

> . . . the most effective theme for a first article would be a rigorous statistical and analytical study of the historical measurements. It is very important that the essential point—the observed change in c—be established on rock-solid ground. I would leave out all theoretical arguments . . .

On 24 November 1985, I sent an even more specific letter, recommending "major surgery" for the article. In terms of the present book, what I had in mind was deleting Part Two (the theory) and expanding Part One (the empirical study), making a much deeper and more rigorous study, clearly explaining with diagrams the relevant history and experimental techniques. Setterfield added a little to Part One but nothing like what I had advised. As for Part Two, he modified his theory considerably but he ignored the essence of my advice, namely to drop the theory altogether.

With this background in mind, imagine my surprise when I found that, without asking permission Setterfield had spliced together parts of three of my personal letters and quoted them on the last page [p. 90]. And though he had not performed the "major surgery" I recommended, he included the sentence "It is a good work that needs to be done," from my May letter. Occurring among reviewer praises as they do, those words make it seem as though I approve of the book—even though I wrote the words before I ever saw the first draft! The quote seems to have deceived everyone who has seen it. To make myself perfectly clear: I did not think the book was a "good work" then, and I do not think so now.

This misquote opened my eyes and shook my confidence in the good faith of the authors. If Setterfield could use my own words in a misleading way on a matter of relative unimportance, then could I trust him not to slant much more important points in his favor? I began scrutinizing the monograph much more carefully, checking references, quotes and calculations for myself. I found many discrepancies, the most important of which I have summarized below. The reader can judge for himself whether or not the monograph has the necessary quality of scholarship and scientific objectivity.

The Roemer Affair

On the question of depth of scholarship, an important example is how Mr. Setterfield has handled the data of a 17th-century astronomer, Ole Roemer, who was the first man to determine the speed of light. Roemer timed the eclipses of Io, a satellite of Jupiter, and noted the increasing delay as the earth moved in its orbit away from Jupiter, correctly ascribing the delay to the additional distance light had to travel. He accurately recorded (to the nearest second) the dates and times of over 50 eclipses from 1668 to 1678. Reckoning the speed of light from this data requires detailed calculations, and astronomers have obtained widely differing results from Roemer's data over the centuries. About 1973, an astronomer from the University of Virginia, Samuel Goldstein, began doing some careful analyses of the data, going back to photocopies of Roemer's original notebooks. At first he made a logical error in the analysis but when the mistake was discovered a few years ago, he corrected his work. The result was very important to Setterfield because a single accurate set of data from 300 years ago is more important to establishing the slope of the possible decay curve than dozens of experiments today (Figure 1). So one would expect that Setterfield would handle Roemer's data with particular care, perhaps going back to the notebook entries himself and repeating the analysis on his own. At the very least, one would expect him to correspond directly and frequently with Goldstein on the issue.

However, Setterfield apparently has not done this and he seems to have made a serious error as a result. On page 11 of the monograph, Setterfield quotes Goldstein as giving a speed of light 2.6% faster in Roemer's day than now, citing as his reference 21: "Goldstein, S. J., private communication, Feb. 25, 1986." I asked Setterfield for a copy of the Goldstein letter. Setterfield wrote in reply that the letter had not been sent to him and he did not have a copy of it. Instead, he had copied the quotation from a preprint of a new paper for *Ex Nihilo* by Vivian Bounds. When I wrote Goldstein for a copy of his letter to Bounds, he sent it. Also he added the information that he had stated his result ambiguously, apparently misleading both Bounds and Setterfield. What Goldstein had meant to say was the speed of light according to Roemer's data was 2.6% *slower* than it is now. Professor Goldstein has given me permission to quote the following from his 2 November 1987 letter to me: "The new result is that the velocity of light was slower in 1668 to 1678 by 2.6% than it is today. I do not think that the difference is significant, however." Here are the two values for the speed of light:

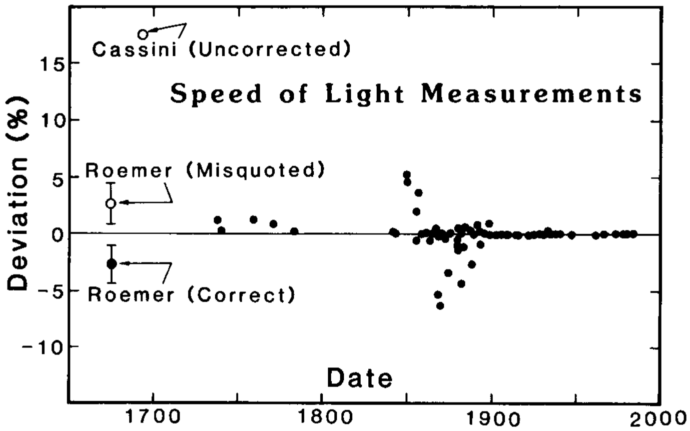

Figure 1. The 163 data points used by Norman and Setterfield, plus the correct Roemer point, showing the percentage deviation from today's value, 299,792 km/s. The two hollow dots represent incorrect data included in the book's 163-point fit (see text). If the two incorrect points are excluded and the correct Roemer point is included in a properly-weighted linear least-squares fit, no significant decay is seen. The book has no figure showing all 163 points. [Adapted from Gerald Aardsma, Institute for Creation Research].

Source	Date	Value (km/s)
Roemer / Goldstein	1668-1678	292,000
Recent measurements	1983	299,792

In other words, the Roemer data, when correctly analyzed, would flatten out the decay curve instead of making it tilt, thus working against the thesis of this book. Setterfield's mistaken value for the Roemer data was 307,600 km / s. He did not use the value in his analysis of the measurements using the Roemer-type method [p. 11], but he did include it in his analysis of all 163 values [p. 25]. Goldstein's revision was an important bit of knowledge. Setterfield misunderstood it because he seems to have violated several commonly-accepted practices among scholars: (1) Trace important data either back to its original sources or at least as far back as practically possible, (2) Never rely on an indirect quotation and (3) Ask for permission to publish private communications. The error bounds about the Roemer point, according to Goldstein, are a few percent. That is enough to include the possibility of no change since the 17th century but it casts doubt on the possibility of significant decay since then. More importantly, Setterfield's whole procedure in the Roemer case makes me doubt that he has really researched his subject sufficiently.

The Cassini Data

The next historical measurements the book cites were done by the astronomer Cassini in 1693, using Roemer's methods. Setterfield lists [p. 12] Cassini's value as 352,000 km / s, 20% higher than the corrected Roemer point, and 17% greater than points a few decades later, which are only a fraction of a percent greater than today's value. Since early estimates of c based on the Roemer data differed from the present value by similarly large amounts causing computational problems, the high Cassini value is probably an error of calculation. If an analysis like Goldstein's were done on Cassini's original data, a much more accurate value might be recovered. Setterfield cites [reference 14] some articles in French by Cassini, but he acknowledged in a letter than he was actually depending on a secondary source's interpretation rather than on a direct analysis of his own. As in the case of the Roemer point, he did not include it in the analysis of the Roemer-type data, but did include it in the fit of all 163 data points. The Cassini point is so high and so long ago that it exerts a strong leverage on the fit (see Figure 1). Setterfield either should have tried a Goldstein-type analysis of Cassini's data or excluded the point from the fit since he acknowledges the value needs correction [p. 12, Table 1, note 2].

The Missing Coefficient

Consider the question of conscious bias: have the authors knowingly withheld information detrimental to their hypothesis? One possible example of such bias is on page 13. There the authors withheld the *correlation coefficient* of a statistical analysis of 63 observations of stellar aberration angles (change of angle of starlight due to the earth's motion). The correlation coefficient is a statistical index which, in this case, indicates the degree of relationship between the speed of light and the date of measurement. A coefficient close to zero suggests little correlation; a coefficient near -1.0 implies a decrease in c as time goes on. For all but one of the data sets analyzed, the authors report correlation coefficients. Most are in the neighborhood of -0.95 to -0.98 which are the kind of numbers the authors want. In the case of the aberration data, they did a statistical analysis of all 63 points and got a decay rate (or slope) of -4.83 km/s per year. They reported the correlation coefficient on a subset of the data, 13 points, getting a coefficient of -0.947. However, they did not report the correlation coefficient for the whole set of 63 points. Why not? The correlation coefficient is -0.409, a rather low value. Is it just coincidence that the single unreported coefficient is the one least supportive of the authors' hypothesis?

A Misleading Zero

Setterfield's Figure I on page 15 shows another example of possibly conscious bias, in this case a slanted presentation of data. The figure shows the "difference in c values" for a selected set of aberration observations from 1740 to 1940. However, it does not say what speed "zero" represents, i.e., it does not show what reference value of c the authors subtracted from the measured values to get the differences they plotted. Nowhere in the related text can I find a statement as to what the zero is for Figure I. In contrast to this absence, the very similar Figure II [p. 23] clearly states its reference point: "zero = 299,760 km/s," a value close to the one accepted today. Figures III and IV (the last figures in the book) show absolute values of c rather than differences but they each do have a dotted line showing today's value for reference. So all the book's other figures tend to lead the reader into believing that the zero of Figure I must be today's value of c. That was what I thought also, until I began to check the values plotted in Figure I against the values listed in Table 3 [p. 14]. Then I found that the zero in Figure I represents 299,500 km/s, an arbitrary number whose only significance seems to be that it coincides with the authors' fitted line on the right-hand side of the graph. The value is 292 km/s less than today's value, 299,792 km/s. So why did the authors displace the zero and neglect to tell us its value, in contrast to what they did in Figure II? The values plotted in Figure II are all above today's value of c, in support of the book's hypothesis. But if you plot a line corresponding to the c of today in Figure I, you will find that *over half the points in Figure I are well below today's value.* In other words, the undeclared shift of Figure I's zero conceals some visual evidence against the c-decay hypothesis. Is it mere coincidence that this shift took place in the only figure where more candor would have worked against the book's main idea?

Bias in the Analysis

In statistical studies a more subtle bias can occur, either deliberately or not, in the choice of analysis methods. A sentence on page 10 epitomizes an attitude which seriously unbalances the whole analysis: "Indeed, if c was constant, error theory indicates that there should have been a random scatter about a fixed value." Here the authors made a mistake about error theory. Many types of measurement or interpretation errors are indeed random. Such errors would cause the estimated value of c to fall randomly above and below the true value. However, other types of error are *systematic*, non-random errors introduced by the equipment or method. Systematic errors are usually unknown (otherwise the experimenter would work to eliminate them) and they can favor one side of a measurement. If there is some mechanism by which the errors to tend to fall systematically on the high side, then as experimental techniques improve with time, the one-sided errors would decrease and there would be a false downward trend in measured values, even if the true value of c were not to change. I did not think of this possibility myself until Gerald Aardsma of the Institute of Creation Research pointed it out to me.

This possibility of systematic high-side error explains one feature of the data that has bothered me for a long time— the steady, smooth flattening of the curve as it gets closer to our own time. Page 53 shows a curve which has c decreasing at over 10 km/s per year in the year 1740. By 1935 the rate is down to about 1 km/s per year and by 1979 there is essentially zero change. In other words, c has stopped changing for the first time in 300 years in our own decade, just when we can really measure it precisely. *Why should the rate of change of c diminish in direct proportion to our ability to measure c more accurately?* Setterfield nowhere answers this very natural question directly. Instead he keeps deferring it later and later into more and more intricate theory until both question and answer have receded from view in the mists and fog of Part Two. The simplest explanation would be that as measurement accuracy has increased, bias from systematic errors has decreased.

There are several ways to reduce the impact of such systematic errors. One is to do a *weighted* curve fit to the data, which gives measurements of high precision more emphasis. The idea is that if the estimatable random errors are low, then the unknown systematic errors are also likely to be low. However, Norman and Setterfield do not mention doing weighted fits and unweighted fits agree with their results wherever I did spot checks. So it appears that they did not make the extra effort of protecting their analysis in this way. A second method of protection is to lump all the data from different methods together into one big analysis. The reasoning is that different experimental methods will have different systematic errors, so the systematic errors are less likely to affect the slope of the overall curve. Norman and Setterfield did do this but they included the erroneous Cassini point and the misquoted Roemer value, both of which were quite high. In all their other data fits, they isolated the data by type of measurement and treated each type separately, again making their analysis more vulnerable to systematic error. As other contributors to this mini-symposium have shown, *weighted* curve fits of *all* the data show no statistically significant decrease in the value of c.

Thus the whole book depends on the validity of the one implicit assumption Norman and Setterfield have made on page 10. However, it is very significant to me that when steps are taken in the analysis to reduce the effect of possible systematic error, the alleged decay of c disappears.

A Suppressed Explanation

A few decades ago, a National Bureau of Standards scientist carefully examined the possibility of decay in c and suggested that systematic error could explain the trend [Dorsey, 1945]. Norman and Setterfield dismiss both the 110-page treatise and the suggestion with a few sentences:

> However, *Dorsey did not address the main problem.* He failed to demonstrate why the measured values of c should show a systematic trend with the mutual unreliability of the equipment. . . Furthermore, in the seven instances where the same equipment was used in a later series of experiments, a lower c value has always resulted at the later date. *Dorsey had no satisfactory explanation for this phenomenon* [p. 10, emphasis mine].

Words such as these would certainly discourage the reader from examining Dorsey's article for an explanation of the trend. However, the reader would be deceived. In his introduction, Dorsey offers a very plausible explanation, one held by other experimental physicists experienced in c measurements:

> As for the drift [the decrease] itself, they see it, prior to any critical study of several reports, as probably, in large part, a *psychological* phenomenon facilitated by the low precision of the earlier work, which not only obscured the effect of systematic errors, but made impossible the experimental discovery of such errors unless their effects were great. They see it as probably arising in large part from two all but universally acting causes: (1) the observer's exaggerated opinion of the accuracy of his own work, and *(2) his inability to avoid being influenced in some measure by his preconceived opinion as to what he should find.* . . An observer who thinks he knows approximately what he should find labors under a severe handicap. *His result is almost certain to err in such a direction as to approach the expected value.* The size of this unconsciously introduced error is, obviously, severely limited by the experimenter's data, by the spread in his values. *The smaller the spread, the smaller, in general, will be this error* [Dorsey, p. 2, emphasis mine].

Dorsey explains, giving the example of Cornu in 1874, that most researchers introduce this bias honestly. There is always a wealth of reasons to reject any given datum.

Consider a hypothetical example, illustrated in Figure 2. In 1875 famous experimenter Alpha publishes his measurement of c: 302 ± 1 Mm/s. Not being omniscient, Professor Alpha does not know (a) that the true speed of light is exactly 300 and (b) that his dog has chewed about 7 mm from the meter stick, thus shifting all his results upward by 2 Mm/s. Ten years later young Dr. Beta is pondering his own measurements. He is not omniscient either but fortunately he is using a correct meter stick. Also he has a more easily readable stopwatch, so his time measurements are more accurate. He has ten data points, three of which are below 300, a value which Beta considers ridiculously low. He is sure that the right value is somewhere at or above 301. He knows how meticulous Alpha was. Beta pours over his notes to find out what is wrong with those three low values. Aha! All three were taken right after he wound up the stopwatch; none of the others were. Let us see, a wound-up watch would run a bit faster, it would read longer times than it should and longer times would give lower speeds . . . that *must* be the reason! With joy he excludes the three offending points, writes his article (diligently explaining in a footnote why he excluded three bad measurements) and publishes his new value for c: 301.0 ± 0.5 Mm/s. Years later, Beta's student Gamma wonders why published values of c have been steadily declining . . .

Thus an experimenter's natural reluctance to publish a value too different from preceeding measurements introduces a systematic bias into his results.* If the preceeding values were too high, the bias will cause a tendency to be high also. But as methods improve with time, experimenters will be constrained by the precision of their results to numbers closer and closer to the true value. The curve would flatten out in exactly the way that Setterfield's curves do [Figures III and IV, pp. 43, 54]. This explanation by Dorsey answers my question as to why the speed of light should stop changing in our decade. If Norman and Setterfield were not satisfied with Dorsey's reasoning, they should have at least outlined it for the reader and explained the reasons for their dissatisfaction. Instead, they divert the reader from Dorsey's explanation and offer no clear alternative.

Part Two: Physical Implications of Decay in C

The second half of the book is an attempt to interpret the effects of the presumed decay of c on electromagnetism, mechanics, gravitation, quantum physics, radioactive decay, time, paleontology, stars, galactic redshift and cosmology. The grandiose scale of this thinking suggests that the authors have succumbed to a common occupational disease among physicists, which I call the "Newton syndrome." I recognize the symptoms well, since I have suffered from them more than

*Editor's note: This concept has been mentioned in a previous Quarterly. See DeYoung, D. B. 1976. The precision of nuclear decay rates, *CRSQ* 13:36-41.

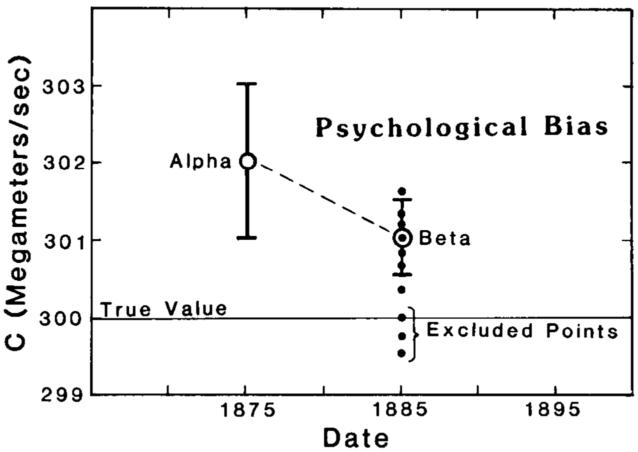

Figure 2. Hypothetical illustration of the effect of psychological bias on experimenters selecting which data to publish. Experimenter Beta influenced by experimenter Alpha's high published value of c, finds an excuse to reject three data points he considers too low. Beta's published average is below Alpha's, but still above the true value. here depicted as 300 Mm/s: As methods get more accurate published values of c decline and asymptotically approach the true value.

once myself. Isaac Newton was the archetypal theoretical physicist and we all imitate him by trying to find the One Great Key which will unlock the riddle of the universe and enable us to make the Grand Unified Theory of Everything.

Unlike Newton's work, however, Setterfield's theory has neither a strong foundation nor sound construction. Starting from the shaky hypothesis that c really is decaying, Setterfield reasons approximately as follows: (1) He assumes implicitly that energy *is conserved* [pp. 29,33]. But in a cosmos where the speed of light is not constant, every physical law is likely to change, so this is an arbitrary assumption. Of course he must start somewhere, but he should discuss the assumption for the reader's benefit. (2) He assumes that *distances remain constant,* that is, measuring rods and even space itself are unaffected by the change in c. He only discusses this assumption in two sentences of the introduction [p. 4]. It needs more justification than that because General Relativity suggests that there is a deep connection between the speed of light and space. (3) Using the mass-energy equation $E = mc^2$, step one, and the c-decay hypothesis, Setterfield then deduces that *mass is proportional to* $1/c^2$, meaning that as c decreases, mass increases rapidly [p. 31].

Setterfield was at this stage when I informally reviewed the early versions of this book in 1985. Later I thought of an inconsistency and sent him a letter on 10 January 1987 containing the following simple example (Figure 3): Imagine a mechanical clock made of two equal masses tied together with a lightweight rod of fixed length, spinning freely in space about the center of mass. The rotation period is the basis of a clock by which we can measure the speed of light. It is easy to show that if steps 1, 2, and 3 are correct, then the rotation rate of the masses is directly proportional to c. This means that as c decreases, this particular mechanical clock slows in exact proportion. Setterfield had already deduced that atomic and nuclear process rates would decrease proportional to c [pp. 33-43]. I explained that the light-and-mirror clock used in relativity textbooks would similarly slow down, and that probably the rates of all physical processes are proportional to c. I wrote:

> It looks like any clock ticks in lockstep with the speed of light, be the clock atomic, electromagnetic, mechanical, or gravitational. Here is the problem: if our measuring rods for distance remain constant in size, and if all our clocks have a rate proportional to c, then *how could we measure any change in the speed of light?*

There was silence from Australia for about the space of a month. Then I received a letter from Setterfield and within it was a new theory. I did not respond, and within six months the finished book was published, containing the new theory. Now Setterfield says that there are two types of time, "atomic" and "dynamic." Atomic time slows with c. Dynamic time, the periods of planets in their orbits, does not change. The reason it does not change, he says, is because he thinks gravitation is like magnetism, and he thinks that a change in the magnetic force constant (permeability) is responsible for the alleged change in c. This reasoning by analogy is tenuous and open to criticism, but without pausing he plunges onward. The Newtonian gravitational field strength, g, should be replaced by a magnetic-like field B* which is the product of g and a new parameter, the gravitational analog of magnetic permeability. However, he does not explain exactly what B* and the new parameter are. What are their units and values? How might we measure them? Without a definition of these new terms, his equations become meaningless.

This is not the end of Setterfield's revelations. There is also a new type of mass, M*, which somehow differs from the mass he discussed in the previous pages, m. One would assume that m is the familiar mass that physicists all know, but it is not, says Setterfield: "In a dynamical context, we appear to be measuring mass in terms of M* and acceleration in terms of B* . . ." [p. 44]. So if the new mass and acceleration are what we have been measuring in physics then what are the old mass m and the old acceleration g? Setterfield does not say.

Somehow the gravitational constant and masses change with c in such a way as to keep the period of

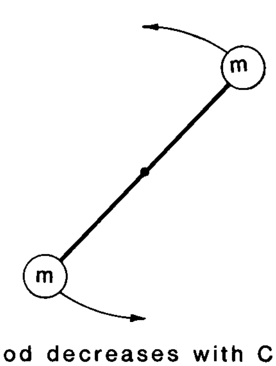

Figure 3. Humphreys' counterexample to Setterfield's theory: a clock made of two masses tied together and rotating freely in space about the center of mass. Setterfield's early theory implies that this typical mechanical clock should slow down in exact proportion to the amount of slowing of the speed of light, thus making it impossible to detect any decrease in c with such clocks.