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Further Evidence Against the Theory of a Recent Decrease in *c*

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ABSTRACT

Further investigations have been made of the claim by Norman and Setterfield,[1] and Montgomery[2,3] that c, *the velocity of light, has decreased in the last 300 years. No evidence has been found to support the claim. In view of the huge amount of evidence against this theory and virtually none for it, the author contends that the whole notion should be dropped from the creationist line of argument.*

INTRODUCTION

Before presenting the details of this further investigation into the Setterfield hypothesis, it is informative to pause and examine closely the way in which Setterfield, Norman and Montgomery have handled the data itself in the development and presentation of the theory. Nowhere in any publication have these authors presented in one table all of the 163 historic measurements of *c*, nor have they displayed in one graph all these *c* values against time. These omissions have been justly criticised by both Aardsma[4] and Humphreys,[5] and there is no doubt that this is an important matter. Only by treating this data set as a whole can the real behaviour of the measurements of *c* be established, and only by the examination of a graph like that presented by Aardsma,[6] Humphreys,[7] or Brown[8] can the real trend and scatter of the data be appreciated.

The reader must realise that whenever this entire data set has been subjected to regression analysis no significant equation has been derived that makes scientific sense. It was claimed in 1983 that when a log sine curve was fitted to 'all values of *c*' that a highly significant result was obtained.[9] It is also claimed that the statistically significant second degree equation of Hasofer supports the Setterfield theory.[10] Results will be presented later in this paper which clearly indicate that these claims are false. The most important result of three independent regression analyses of the entire data set is the establishment of a highly significant third degree equation which when combined with a regression confidence limits analysis indicates that there is no case for this theory and that the trend of *c* measurements has never differed significantly from a constancy at today's value.[11,12,13]

In 1981 Setterfield claimed that a log sine equation fitted the *c* versus time data almost perfectly, the coefficient of determination r^2 being 0.999 or better.[14] This analysis used 52 carefully selected values of *c*. With such a data set it would be **impossible not to find** a steady decrease. When challenged about the obvious bias of this set of data Setterfield claimed that when **all** measurements of *c* were used the log sine curve was still the one of best fit with an r^2 value that was better *'by a factor of two or more on the other curves'*. This claim must be regarded with scepticism! If a log sine curve is fitted to the entire data set used in 1987 the result is close to a horizontal straight line through the data, with statistical significance not reached when the equation is tested using an analysis of variance of the residual and regression sums of squares. The resulting F statistic does not reach the $p = 0.05$ level of rejection. The log sine equation itself will be examined in more detail later.

In their 1987 derivations Norman and Setterfield used the same approach they had used in previous work.[16] Here the 'best 57' values of *c* were used. Again there is that obvious bias that ensures that the equations derived **must** indicate a decrease of *c* with time.

A REPLY TO MONTGOMERY AND NORMAN

I thank Alan Montgomery and Trevor Norman for their critiques of my two publications.[17,18] Consider first the opinions of Montgomery.[19] The outstanding feature of his critique is that he has ignored about 90% of what I wrote. I can only assume therefore that he agrees with most of my results and conclusions or can find no fault in them. Chiefly he has ignored:

(1) The very significant degree three equation and its regression confidence limits analysis.
(2) The parameters of the distribution of *c* measurements **all** of which act **against** the Setterfield hypothesis.

(3) The convincing evidence of the 'unchanged' half-life of iodine-125 (^{125}I).
(4) The weighted means of the Setterfield 'varying constants' which do not vary one iota from the most recent value.
(5) The collapse of the support allegedly given by Van Flandern's results.

It is obvious that Montgomery ignores the remarkable strength of the case against the Setterfield theory. Consider the following quotes from his critique:

(1) '. . . *the regression lines on his* (Evered's analysis) *are not followed by an analysis of the residuals . . . Evered thus misses the statistical clues that would inform him that a linear function was inappropriate.'*

The use of the F statistic involves an analysis of variance testing sums of squares due to regression and sums of squares due to residuals, a fact of which Montgomery is surely aware. This is a direct test of the residuals and I report that the linear equation does not reach significance indicating that **it is inappropriate to the data.** Montgomery has missed the entire point here.

(2) '. . . *the equations . . . use data that includes the values in the 1966–1984 era which are measured using atomic clocks. . . . he* (Evered) *is testing a hypothesis other than the one Setterfield and Norman have proposed . . .'*

The 57 values of *c* from which **all** the current Setterfield equations are derived includes **eight** laser values where atomic clocks have been used. Surely Montgomery knows this.

(3) *'Evered's histogram of* c *values is almost useless.'*

Montgomery has ignored completely the values of skewness and kurtosis that shape this histogram. These values refute absolutely the Setterfield claim that the distribution of *c* measurements is 'markedly skewed'.

(4) *'It* (the regression technique) *tests a potential linear model against constancy, not a trend against constancy'*

What about the degree three equation which describes the behaviour of *c* **measurements (obviously not of** *c* **itself)** over 300 years and in that time **never differs significantly from constancy?**

(5) *'Evered claims that Hasofer's data supplied by Norman and Setterfield was markedly different.'*

And so it was, particularly in the vital 1727–1771 time range. It is regrettable that Montgomery did not check the references I gave. He then would have seen for himself. These very important alterations will be dealt with in detail later in this paper.

(6) Referring to my second paper — *'Evered's Figure 5 is hopelessly out of scale'*[20]

Figure 5 is to show **all** the *c* values and the accompanying scatter, something that has always appeared to frighten Setterfield and supporters. Like Setterfield before him, Montgomery breaks the data into smaller time groups and removes the embarrassing values before analysis. **Question** — why are the ESU/EMU results removed here but retained in Montgomery's original paper?[21] The answer is simple and predictable. In his critique of my paper these values work against him. In his original paper he claims they work for him.

(7) In criticising Figures 3 and 4 of my second paper[22] Montgomery is in fact criticising Setterfield. It was he (Setterfield) who claimed that

$$hc$$

and

$$\frac{q}{mc^2}$$

maintained a constant relationship with time.[23] Obviously they do not!

Montgomery's Figure 4 shows values for the electronic charge versus date of determination as a virtually horizontal line from 1913–1973.[24] How strange that he does not show all the *c* measurements for the same period. These also yield a virtually horizontal line not reaching statistical significance in the F or t test (the $p = 0.05$ level of rejection not being reached). This of course acts against his case.

Consider now the comments of Trevor Norman:[25]

(1) He (Norman) has confirmed Hasofer's degree two equation results, and they disagree with mine. They disagree for two very good reasons. Hasofer's equation is weighted; mine is not. Very importantly, Hasofer's equation was derived from a different data set (more on that later).
(2) Norman claims that I was wrong to apply 'Brown's method' in a regression confidence limits analysis. I fitted 95% confidence bands to the degree three equation as had Dr Brown to show clearly that the line of fit had not varied significantly from a line at today's value of *c* or 299792.5 km/s during the last 300 years.[26] The method used was that to be found in many reputable statistical textbooks.[27] It had nothing to do with the use of the $\sqrt{N}$ that Norman slates later in his paper. He may rest assured I do know how to apply Student's t when testing the mean of a data sample of assessing regression confidence limits.
(3) *'In Evered's discussion of his results he appeals to one's sense of the absurd.'* No, I do not appeal to one's sense of the absurd, but just appeal to one's sense of real science when I point out the absurd and nonsensical predictions made by the equations that are claimed to support the Setterfield case. In fact I issue an interesting challenge! Can any Setterfield supporter provide me with a statistically significant equation that supports the concept of decreasing *c*, that supports predictions of the behaviour of *c* which are not nonsense, and that is capable of supporting a value at creation of 10^{11} x *c* now? After all, these are Setterfield's criteria.

Sorry, I cannot accept the cosec^2 equation for reasons to be elaborated fully later in this paper. The test, which I have called 'scientific predictability', is of the most fundamental importance in any case where an equation is claimed to describe the behaviour of any entity of science. It was applied in 1941 to show that the predicted *c* values of de Bray and Edmonson were unreasonable.[28] It was used by Setterfield himself to reject 'other curves' which were suggested as substitutes for the cosec^2 equation in the pre-1987 days.[29] It makes very good sense to continue application of this test.

(4) The distribution to which I refer is that of the values of *c* measurements not that of residuals, a point made abundantly clear in the text. After all, it was Setterfield who claimed that this distribution supports his theory when very clearly it does not.

THE MONTGOMERY STATISTICAL ANALYSES

The hypothesis testing proposed in 1990 and 1991 by Montgomery may be subjected to the same criticism as the Setterfield work. Montgomery applied the t Test, the Run Test and the Mean Square Successive Difference Test (MSSD) to the same groups of *c* measurements used by Setterfield, but after he had eliminated values removed from the sample mean by more than three standard deviations.[30,31] What Montgomery did not do was report the application of these tests to the entire data set used by Aardsma,[32] Brown[33] and Evered.[34] If these tests are applied to the entire data set:

(1) The t Test fails to reach significance; but in any case this is irrelevant as the mean of the *c* data is **lower not higher** than 'c now'. As Montgomery himself rightly points out, the t Test *'is a very frequently used and robust test and its results are an important quality control on the interpretation of other tests'*. Its complete failure here really negates all his claims regarding alleged changes in *c*.

(2) The Mean Square Successive Difference (MSSD) Test reaches significance. Montgomery has applied this test to most of the tables of *c* values and claims the results support *'a monotonic decreasing trend* (of *c* measurements)'. When this test is applied to the 1987 data set as a whole it reaches a confidence level greater than 95%, but what does this mean? Such a result is associated with positive serial correlation, a condition characterised by runs, that is, a series of observations above or below the regression line or surface.[35] Runs in general will be considered shortly, but there is no evidence from this result of a long decreasing trend in the *c* values, a condition necessary if the Setterfield theory is to be supported by Montgomery's imaginary 'monotonic decreasing trend'. The presence of runs and an absolute absence of an overall decreasing trend is confirmed when the Durban Watson test, or a test of moving averages, is applied to this data set.

(3) The Run Test on the median still reaches statistical significance, but do be aware of what this means. It is appropriate to examine this test more closely.

Any sample consisting of numerical measurements or observations can be treated by using the letters a and b to denote respectively values falling above and below the median of the sample.[36] The resulting series of a's and b's are then tested for randomness on the basis of the total number of runs of a's and b's occurring above and below the median. The method is especially useful in detecting trends, clustering and cyclic patterns in data. Clustering of data or a definite trend usually results in a lower number of runs than expected. If there is a trend there will also be first mostly a's and later b's (or vice versa). If there is a repeated cyclic pattern there will be a systematic alternation of a's and b's and an unexpectedly high number of runs.

Now to support the Setterfield theory that *c* has decreased with time, since it was first measured you would expect mostly a's then later b's with the passing of time, which you just do not find in this data set. There is a series of about 60 runs alternating above and below the median culminating in a long run above the median with the accurate measurements post 1950. I repeat, there is no trend. There is a clustering of measurements about the median value of 299790 km/s, a value which is less than the true value of *c*, namely 299792.5 km/s. These results negate the claim by Montgomery that the measurements of *c* are time dependent over the whole period, that is, they show a trend. They simply show a clustering which upon closer examination of the data is usually seen to correspond with a change in the method of measurement.

It is also claimed by Montgomery that the 'changes' in *c* are supported by similar 'changes' in other physical constants. This claim will be examined as a separate issue later in this paper.

Montgomery's 1991 paper presents an additional analysis.[37] This is an analysis by error bar applied to data from 1876 to 1960. *'For error bars less than 100 km/s down to 0.75 km/s the t test results in confidence levels of 87% to 99.5% in single tail tests, with 5 of 6 results greater than 95% confidence levels'*. This is all very impressive stuff until you realise than once again the data set has been manipulated. The author states that the Kerr cell results and the 1932 result of Pease/Pearson have been removed. Not stated is the fact that some aberration technique (Bradley method) results have also been removed. Why?? Because once again we have data that acts against the theory, a fact that in the minds of Setterfield supporters always justifies its removal. If these values are added to this analysis, then in the top line of Montgomery's table 4 n = 45 , average velocity = 299786 km/s and the t Test fails to reach significance. This result is a much truer picture of the real situation and indicates no significant change from '*c* now'. These later aberration technique

measurements are retained in the results presented in Montgomery's Table 1, so why is there justification for removing them from the later analysis?

THE HASOFER ANALYSIS

The weighted regression analysis by Professor A. M. Hasofer is an excellent piece of statistical treatment.[38] This analysis further compounds the difficulties facing the advocates of the Setterfield theory. Hasofer reported:

(1) The degree one equation was not significant when the F statistic was calculated.
(2) The degree two equation was highly significant when so tested.
(3) This result depended heavily on the credibility of five *c* measurements from 1727–1771.
(4) A degree three equation was an even better fit to the data.

It is regrettable that the anonymous writer in **Creation** magazine made so much of points number one and two, but **ignored completely** points three and four.[39] Some important facts about the degree two equation must be appreciated:

(1) This equation is only significant because of altered values supplied to Hasofer.
(2) Its predictions of *c* are scientific nonsense.
(3) If this equation is submitted to the usual Setterfield extrapolation it requires 2.2 billion years to reach his required value of *c* at creation. This is hardly evidence that supports an age of the earth of seven to ten thousand years.

Much more needs to be said about the first seven values of *c* used in the Hasofer analysis with standard errors *'as given by Norman and Setterfield'*. Values number one (Roemer, 1775, 292000 km/s) and number two (Cassini, 1696, 352000 km/s) both have reported standard errors of 18000 km/s and thus contribute very little to a weighted analysis. The next five values however are vitally important, and as has already been indicated the standard errors quoted are not the same as published earlier.[40] The reader must be aware that:

(1) Value number four (Delambre, 1738, 303320 km/s) is given a standard error of 65 km/s, a very low value indeed for a 'Roemer' estimation. From which reference was this value obtained? Setterfield references Newcomb, 1886, for this value of *c*.[41] May I quote directly from this reference:—
'In 1809 it (the time required for light to pass a distance equal to a radius of the earth's orbit) *was fixed by Delambre at 493.2 seconds from an immense number of observations of eclipses of Jupiter's satellites during the previous 150 years. This number has been received as a definitive result with a degree of confidence not at all warranted'* (my emphasis).
How then can Setterfield refer to this value as a definitive result? Presumably he knows better than did Newcomb. Newcomb goes on:—
'As not a trace of Delambre's investigation remains in print **(that is, in 1886)** *and probably not in manuscript it is impossible to subject it to any discussion'* (my emphasis).
This obvious problem has not stopped Norman and Setterfield. The reader is directed to an excellent paper by McMillan and Kirszenberg for a thorough discussion of the 'Roemer' method of measuring *c*.[42] It becomes obvious then that Norman and Setterfield have grossly exaggerated the relative accuracy of this method.
(2) Values three (Bradley, 1727, 303430 km/s) and five (Bradley, 1740, 300650 km/s) require special mention. It is obvious that the data of the first value also forms part of the data of the second, that is, it has been quoted **twice** to improve the Norman and Setterfield case. If corrected value number three should read 303430 km/s, value number five should read 299167 km/s, each with a standard error of 6000 km/s.
If the correct standard errors are used for these five vital values of *c* between 1727 and 1771 the mean weighting changes by a factor of 47 and the statistical significance of the degree two equation is lost.

The degree three equation agrees with the results of Brown[43] and Evered,[44] and of course describes the behaviour of *c* measurements, **not the behaviour of** *c* **itself.** If *c* followed the predictions of the degree three equation, then in the year 987 a d the value of *c* was **zero** and before that *c* was **negative,** that is, it moved toward the source!! I doubt that even the most ardent Setterfield supporter would push this line of argument. Once again the test of scientific predictability has failed.

THE COMMENTARIES OF M. E. J. GHEURY DE BRAY

The publications of de Bray are definitely among the Setterfield favourites, having been quoted many times. Indeed, they appear to have been the inspiration for the whole Setterfield line of argument, but neither Setterfield himself nor any of his supporters have ever followed the de Bray hypothesis to its logical conclusion. Let us examine de Bray's claims and opinions.

1927.[45] 21 values of *c* are listed from 1849 to 1926. In this paper there is no proposal that *c* is decreasing. The main line of argument is a criticism of the lack of accuracy in the reporting of results by other investigators.

1931.[46] De Bray reviews the values of *c* used in his 1927 paper, plus the 1928 value of Karolus and Mittelstaedt. He declares *'There are twenty-two coincidences in favour of a decrease in the velocity of light, while there is not a single one against it.'*

How on earth from the values of *c* under consideration could de Bray make such a claim? This result would require 23 estimates of *c* with each one lower than the previous one. **This is just not so!** In this data there are **nine** instances where a value of *c* is **higher** than the previous one.

1934.[47] Edmondson now suggests that the trend in *c* measurements is not a linear decrease but a sinusoidal fluctuation following the formula —

$$v = 299885 + 115 \sin 2\frac{\pi}{40}(t - 1901)$$

1934.[48] De Bray is now not nearly so sure of his linear fit and agrees that Edmondson's sine law of variation gives '*remarkable agreement*' with the observed values of *c*.

1936.[49] De Bray now claims that if there is a variation in *c* it may be linear, cyclic or logarithmic. That really covers all options. He is obviously not nearly so sure of his original ground.

In the nine years 1927–1936 there is a marked change in the attitude of de Bray. Add to his opinion that of Birge[50] and that of Dorsey[51] and you have a much more realistic description of the predominant thinking at the time. This differs from the picture presented by Norman and Setterfield. Between 1930 and 1974 a committee of experts made periodic declarations of the 'best' value of *c*, depending on the results available at the time.[52] If these results are added to those presented by de Bray then:

(1) Application of the t Test indicates that the mean of the data from 1862 to 1974 does not vary significantly from 299792.5 km/s, the currently accepted value of *c*.

(2) Over this period these values of *c* show a slight but statistically insignificant **increase.** There is definitely no indication of the decrease that de Bray originally predicted. Therefore de Bray's claims do not provide support for the Setterfield theory.

THE LOG SINE (COSEC SQUARED) EQUATION

This was the original equation used to describe the alleged decrease in *c* from 1675 to 1961, after which *c* became 'constant'.[53] From 1981 until 1987 the log sine equation was the only one that Norman and Setterfield would accept, rejecting with vigour all others, including an exponential equation. It therefore came as a surprise when in 1987 an exponential equation (albeit a rather unusual one) was used to describe the claimed behaviour of *c*. This was of course the damped sinusoid, accompanied by its equivalent the degree eight polynomial.[54] (That these two equations are not at all equivalent has been well demonstrated.[55]) These two equations were immediately subjected to well-justified criticism because:—

(1) Predictions of *c* in the past are nonsensical, contradicting completely recent carbon-14 dating results.[56]

(2) Predictions of *c* in the future are also nonsensical.[57]

(3) It has been demonstrated theoretically that the damped sinusoid equation was incorrectly used and interpreted.[58]

The failure of these two equations has resulted in a readvocation of the log sine equation, particularly by Montgomery.[59] It is important therefore to examine this equation to see if its predictions make any more sense than those of the other two. It was claimed that this equation 'stops' in 1961 when *c* became constant, but this is a mathematical fantasy. The log sine equation in 1961 passes into another trigonometric quadrant. Expressed as the cosec^2 it goes through a turning point. In either case there is predicted a **rise** in *c* after 1961, just as there is a rise predicted by the other two equations already considered. By the cosec^2 formula *c* will equal 299803 km/s in 1990 and 299815 km/s in 2000. These values are rejected on grounds already described.[60]

There is another question regarding the use of a log sine equation. Surely many readers have asked, '*Why this equation?*' Indeed, this point has been raised by at least one creationist highly qualified in physics and who holds the responsible position of editor of the **Creation Research Society Quarterly.**[61] There are many examples of changing quantities in physics-radioactivity, Newton's law of cooling, the increase and decrease of voltage and current in capacitors and inductors, etc. These all change according to an exponential equation of the type

$$y = Ae(Kx)$$

a form of equation rejected by Setterfield and by the nature of the data from *c* measurements. A lengthy check of the appropriate scientific literature has revealed **no physical changes** described by a log sine equation. In 1984 Fackerell challenged Setterfield to produce references supporting the use of a log sine equation.[62] No reply was forthcoming. There is no doubt that the two cases claimed to be physical phenomena whose behaviour follows a log sine curve are both in error. The motion of tops or gyroscopes involves elliptic integrals.[63] The decay of novae and supernovae is essentially exponential.[64]

What is it about this equation that makes it so attractive? Answer — it can be manipulated with the greatest of ease. The log sine formula is

$$\log c(\text{time } t) = \log c(1961) + b \log \text{sine } T.$$

Now T is the value in degrees or radians in one trigonometric quadrant. **How do you decide to allot it to a time scale of years** so that it becomes log sine (*kt*)? Neither the 1981 nor the 1987 publication makes this at all clear. You could define this as anything you liked. Is it this plus the fact that the 1981 *c* values were so carefully selected that the r^2 values were so freakishly good, that is, 0.986? On

Constant	Rate of Change	
	Absolute	Relative
$\frac{e}{mc}$	3.8657×10^{-5}	200
$\frac{h}{e}$	1.0148×10^{-4}	524
$\frac{2e}{h}$	1.9355×10^{-7}	1
$\frac{h}{e^2}$	6.1597×10^{-7}	3
Y'	1.0990×10^{-6}	6
c	3.3356×10^{-6}	17

Table 1. *Rates of change of 'c related constants'.*

challenge the authors, in their 1983 publication, fitted a series of curves to quote *'all values of* c'.[65] It was still claimed that the log sine equation gave the best r^2 value. As reported earlier in this paper, a log sine curve fitted to the 1987 data set results in a statistically non significant, virtually horizontal straight line. To quote an expert in the subject, referring to the 1987 data set and to this equation:—

> *'The curve has to be so flat in the vicinity of the data points that large variations in* b *and* k *hardly affect the size of the residuals at all. You are right that* k *and hence the time of the creation could be chosen at will. The point is that a curve of best fit of this nature should never be extrapolated.'* (Dr Mark Evered, his emphasis)

THE SEVEN STATISTICAL TESTS

The 'seven statistical tests' that are supposed to support his theory are quoted as part of Setterfield's reply[66] to the papers of Aardsma,[67] Brown[68] and Humphreys.[69] Before examining this claim it is important to correct a serious statistical error that appears in **The Atomic Constants, Light and Time.[70]** On page 81 we read of the correlation coefficient r:—

> *'Values of r range between 0 and 1. If all points lie on the line then r = 1, no matter whether the line is sloping or horizontal.'*

This last statement is absolutely wrong!! A horizontal line of fit whether through all points exactly or through a scatter has a value of zero. This error is not corrected in the publication, so use by the authors of r or its square must be viewed by readers with caution. This is the most serious statistical error, but another also needs correcting. The term 'probable error' has been used by Setterfield to indicate the difference between an observed and a predicted value.[71] This is quite wrong. The correct term is 'residual'. The probable error was used in former times to denote 0.6745 times the standard deviation. This is the deviation as likely to be exceeded as not. This term, a poor one, is now very much obsolescent. It has been said of this term *'it is neither an error nor probable'*.[72]

Now to examine the seven statistical tests on which the theory rests.

(1) *'Data means were significantly above* c *now'*. Without the very erroneous Cassini value (352000km/s) the mean of all measurements of *c* presented in 1987 is 122.5 km/s below *c* now, thus completely negating this claim.

(2) *'The distribution of* c *values is markedly skewed'*. A calculation of the Pearsonian skewness of this distribution, -0.1105, negates this claim utterly.

(3) *'Median analysis of all points indicated that the hypothesis that the median level was equal to* c *now could be rejected.'* See my comments earlier in this paper relating to the use of this test by Montgomery. Realise too that the median value of this distribution is **below, not above,** *c* now.

(4) '*The Spearman Rank Test indicated that there was a strong correlation with the date of observation of all data.*' This test applied to the data set used in 1987 fails to reach significance.

(5) Setterfield claims that least squares fits to the data support his theory. Just how the proper application of regression analysis **rejects** the theory has been well established and requires no further comment.

(6) *'Analysis of the residuals indicated a non linear decay. Residuals reduced from 22000 to under 2000.'* This is from a reference *'Malcolm, 1982, personal communication'*. Note 1982! With the highly selective Setterfield data set of 1981–1982 this result is inevitable. With the larger data set used in 1987 the residuals of the degree one, degree two, and exponential equation fits are all high, but drop markedly when a degree three equation is used. The use of this reference by Setterfield in 1987 is thus very deceptive.

(7) Setterfield refers to the Mean Square Successive Difference (MSSD) Test results. Applied to the entire data set used in 1987 this test fails to support the Setterfield theory, as reported in the comment on Montgomery's analyses.

Thus Setterfield's claim that these tests support the theory of a recent decrease in *c* is without foundation.

THE VARIABLE CONSTANTS

The Atomic Constants, Light and Time does not only deal with '*changes in* c'. There is also a presentation of other physical constants that must have changed (at least by Setterfield theory) if *c* has diminished in the last 300 years. Setterfield, with support from Montgomery, has asserted that constants related to *c* have changed '*in lockstep with* c'. If any constant has so changed then its **rate** of change must be statistically related to the **rate** at which *c* has changed, irrespective of the **direction** of that change. Table 1 shows the absolute and relative rates of change of

$$\frac{e}{mc},\ \frac{h}{e},\ \frac{2e}{h},\ \frac{h}{e^2},\ Y' \text{ and } c.$$

It can be seen that the relative rates of change, instead of being similar, vary by factors up to 524. The claim that rates of change of slope are similar may be proved or disproved by testing for significance of difference. Such testing indicates that no two rates of change in Table 1 are statistically alike. With such large differences statistical testing is not really necessary. Plain common sense will suffice. And how has *c* behaved during this time, from 1890 to 1981? It has already been demonstrated that consideration of the de Bray values and those of the expert committee indicates **no significant change** from 1869 to 1974. **There is obviously no quantitative change in these constants relative to** *c* **or relative to each other.** The Setterfield claim is entirely negated.

The 'change' in the Quantised Hall Resistance

$$\frac{h}{e^2}$$

of Table 15c in **The Atomic Constants, Light and Time** requires special attention.[73]

(a) Though only a five year period is covered, the Rh values of Table 15c of **The Atomic Constants, Light and Time** do show a statistically significant increase with r = 0.9206. By Setterfield theory this must equate to a decrease in *c*.

(b) The text and Table 24 of the same publication indicate that *c* has become constant, or varied by only one part in 10^{12} or so, during this period of time.[74]

(c) The 1986 estimate of Planck's constant[75] (lower than the previous estimate) would, by Setterfield theory, indicate that *c* is increasing again in this period, and indeed this is exactly what Setterfield does claim. It must be said that the Setterfield theory is flexible when it can indicate that *c* is constant, decreasing and increasing **all at the same time.**

The Rydberg Constant is claimed by Setterfield to be one of the *'constant constants'* independent of any changes in *c*.[76] The authors state that a linear fit gives an increase of 4.95 x 10^{-4}cm^{-1} per year. No value is given for r the correlation coefficient, but this is 0.388 with t not significant at the $p = 0.05$ level of rejection. Setterfield is thus correct in claiming that this increase is not statistically significant. **But there is a problem!**

Examine Table 16 of **The Atomic Constants, Light and Time** and it is at once obvious that this increase is very largely due to the 1913 value of 109737. The value differs from the mean of the data by **twelve** standard deviations and thus must be considered an absolute outlier. Without this one very aberrant value there is a **decrease** of 7.46 x 10^{-4}cm^{-1} per year, r = -0.5853 and t is significant at the $p = 0.01$ level of rejection. This so-called invariable constant thus becomes a variable, **but in direct opposition to Setterfield theory that it should be** *c* **independent!** In actual fact of course, this 'change' reflects differences in the estimated value of the Rydberg Constant as time passed and measuring techniques improved in accuracy. Surely no one can seriously claim from the evidence presented that *c*, the Rydberg Constant or any other constant under consideration has actually changed in value.

Method of measurement	Coefficient of Variation	
	Absolute	Relative
Roemer	**1.3024**	**3151**
Aberration	**0.1287**	**311**
Toothed Wheel	**1.7841**	**4317**
Rotating Mirror	**0.2109**	**510**
Kerr Cell	**0.002278**	**5.5**
ESU ***EMU***	**2.1498**	**5201**
Waves on Wires	**0.5425**	1313
***Electronic, post* 1945**	**0.0004133**	1

Table 2. *Coefficients of variation of* c *as determined by various measuring methods.*

THE ABERRATION (BRADLEY'S METHOD) RESULTS

Table 3 of **The Atomic Constants, Light and Time** displays values of *c* measured by the aberration method pioneered by Bradley. This table contains 63 values of *c* made from 1740 to 1935, a period of 195 years. Included in this table is a subgroup of 13 values made at the Pulkova Observatory in Russia. Norman and Setterfield,[77] Montgomery[78] and an anonymous writer in **Creation** magazine[79] have given great weight to the aberration results. After all, they argue, if there has been a decrease in *c* over this long period it should surely be revealed in this large number of values. Just what do the aberration values show in this regard?

(1) The mean of all the aberration values is 299869 km/s. This is 77 km/s above 299792 km/s, but in the standard t Test does not reach the 0.05 level of rejection of the null hypothesis. Thus this mean does not differ significantly from 'c now'.

(2) It is stated that all 63 listed values show a decrease of 4.8 km/s/year. No value of r, the correlation coefficient, is given by the authors, but it has been calculated by Humphreys to be 0.409. This results in a coefficient of determination of 0.167, a low value indeed. The coefficient of determination may be defined as the ratio of the sum of squares due to regression to the total sum of squares, and is equivalent to that fraction of change in *c* that may be explained by a corresponding change in time. Here it equals 0.167 or 16.7%. What about the change in *c* **not** explained by a change in time, here 83.3%? The authors do not bother to tell the reader. In 1981 and 1983 there was **quibbling about values of r^2 as high**

as 0.91. How times have changed when a value of **0.167** will now be accepted by Norman and Setterfield.

(3) The **13** Pulkova values in the cited Table **3** are given a separate treatment by the authors. Surely, they argue, these measurements all made with the same apparatus are convincing evidence for a trend of decrease in *c*. Do not be deceived by this claim. The Setterfield assertion is simply incorrect. He claims the mean of the Pulkova data is 88 km/s above *c* now. In actual fact the mean of the Pulkova data is **299776** km/s, that is, **16.5** km/s **below** *c* now. Any claims that this subgroup of the aberration data support the theory are thus negated.

The real nature of the aberration data, and indeed of the data from all methods for measuring *c*, is revealed by an examination of Table 2 in this text. Here are listed the absolute and relative coefficients of variation of each data set. The coefficient of variation, which expresses the sample standard deviation as a percentage of the sample mean, provides for comparison of the dispersion of two or more different data sets. It can be seen that in comparison with the post World War II electronic measurements the scatter in the different groups varies from large to enormous. Norman and Setterfield have no qualms basing a radical theory on such grossly scattered data. The reader may draw his or her own conclusions as to the validity of such a procedure.

THE RELATIVE SCATTER OF THE VALUES OF *c*

The claim has been made that

'Indeed if c *was constant error theory indicates that there should be a random scatter about a fixed value. This has not been observed.'*[80]

Before testing this claim the reader should re-examine Aardsma[81] or Humphreys' Figure 1[82] or my Figure 5.[83] These are the **only** figures in all the relevant publications that tell the true story of the history of *c* measurements, because they alone include **all** values. It can be seen that there is, after the first old and inaccurate values, a more or less equal distribution of points on either side of the zero line until about 1950 when the much more accurate electronic measurements were made. After that all points virtually lie on a horizontal line at today's value. Theory would predict an equal number of points on either side of the zero line if *c* was constant (in the real world a distribution not statistically different from an equal number). This is testable by the application of a chi square test. There is here one degree of freedom and the calculated chi square value to be exceeded at the 0.05 level of rejection is 3.84. Any value less than this indicates that the Setterfield claim is false. The chi square value of the whole 1987 data set equals 2.14, so the number of points on either of the line is not significantly different from an equal number on either side.

This test may also be applied to the aberration results which are also more or less equally scattered either side of the *c* now value. In this case the chi square value equals 0.06. a value virtually equal to zero. This again is the sort of result expected from a set of data points equally scattered either side of a central value.

What happens when this test is applied to the data from which Norman and Setterfield have derived their defunct equations? First in 1983; 52 selected values are used and the chi square value equals 7.20. Finally, in their Table 11, 1987;[85] 57 selected values are used and chi square equals 8.17. These two values of chi square are well in excess of the value for rejection at the 0.05 level. In other words, the Setterfield claim of the non random scatter of points about a line at today's *c* value **applies if and only if there has first been a careful selection of the points concerned.**

It has now become so painfully obvious that **when the whole 1987 data set is used the Setterfield theory collapses like a house of cards. Virtually every claim that has been made in support of the theory is wrong!**

CONCLUSION

The Norman and Setterfield publications have been dominated from the outset by one theme — How can the available data be used, be manipulated, to support the idea that *c* has decreased with time? It seems that neither the authors nor their supporters have ever started with the question — **Does** the data indicate or support a decrease in *c* with time? The bias attached to the former approach stands out like a beacon from the first writings of 1981 and thereafter. It is useless calling on the ideas of de Bray for support. If de Bray had enjoyed the advantage of knowing the post-1950 electronic measurements it is very unlikely he would ever have proposed his theory. No such excuse exists for Norman, Setterfield and their supporters.

I have been accused of being insulting when I wrote that a decrease in *c* exists only in the minds of those advocating the theory. I would go one step further and say that if **all the available data** had been properly considered the theory should not have arisen in the first place.

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