More Evidence that the Velocity of Light is not a True Constant

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ABSTRACT

An analysis by Dr Evered of the Setterfield hypothesis that c, *the velocity of light, has decreased in the last 300 years is examined. The data, methodology and conclusions of Evered are shown to be unreasonable. More appropriate methods and data show a more than reasonable probability of a change in the measurements of* c*. Furthermore, it is also probable that the change in* c *measurements is the result of a change in the values of* c.

THOSE 163 DATA

Before beginning discussion of Evered's papers[1–3] and responding to his discussion of my papers,[4–6] I would like to express my appreciation of Evered's hard work. He shows far more courage and conviction than his American counterparts who are too content with their own work.

Dr Evered's first complaint is that we spend so little time and space on all 163 data.

> *'Only by treating the data set as a whole can the real behaviour of the measurements of* c *be established and only by examination of a graph like that presented by Aardsma, Humphreys and Brown can the real trend and scatter of the data be appreciated.'*[7]

This extolling of the 163 data and Humphreys' graph[8] is self-serving and inappropriate. The scale of this graph compresses the data so badly that although there are supposedly 163 points, only 58 or 59 are visible to the eye. Is this to be lauded as exemplary work? In order to give the readers' eyes opportunity to see the distribution of *c* values Humphreys' scale would have to be expanded a thousand times. This is impractical. Most reasonable analysts would use several graphs with several scales to allow the readers some glimpse into the information. Others might omit the Cassini value (a value so spurious that even Evered cannot bring himself to use it in his analysis) in order to expand the graph scale by at least a factor of 3. I leave it to the readers to compare Evered's graphs[9] to mine[10] to see which ones allow the reader himself to see the relationships among the most important data points.

But just what are the characteristics of these 163 data points which make them so indivisible for analysis? Do they all come from the same methodology and equipment or are there 17 methods and many more sets of equipment? Is it not more reasonable to do analyses by method than rely on a single analysis which may be marred by a combination of systematic errors in the data? Suppose the shoe were on the other foot and a wonderfully well-fitted and undisputed regression curve weighted by undisputed error bars and undisputed data with statistically significant coefficients were to be presented from the 163 data and yet individually these 17 methods showed no trend. Surely, the trend would be deemed due to systematic differences in the various methods and probably spurious. This conclusion would be quite natural. But just as systematic errors between methods may lead to spurious trends in the data, it can lead equally to masking real trends in the data. What assurance has Evered given that some of his results are not the product of different systematic errors? None!

Evered has, in contradiction to the conditions he would impose on Norman and myself, omitted the Cassini datum to improve his case. I do not object to this; if the data is to be edited I insist on it. But I also insist that if Evered is going to remove outliers he must make a genuine effort to remove them all and not just the one that hurts his case the most. He can hardly cry foul at Norman's or my 'biased' editing when his own editing is even more so.

REGRESSION ANALYSES AND RESIDUALS

Since so much of Brown, Aardsma and Evered's work centres around regression line analysis I will spend a little time explaining some of its assumptions and uses. Firstly, a model or function is assumed to be appropriate to data. This may come from theoretical considerations, intuition or trial and error. A linear model is a frequent choice:

$Y = a + bx$ for some coefficients a,b.

Now individual data points do not always lie on the line, but for each (x_i , y_i)

$Y = a + bX_i + e_i$ where e_i is the error or residual.

Now under certain assumptions concerning the residuals, it can be demonstrated that the regression technique gives the most likely value for a and b, $\hat{a}$, and $\hat{b}$. These assumptions are:

(1) The expected value of the residuals is zero
(2) The variance of the residuals is constant
(3) The residuals are not autocorrelated, that is, they are independent of the random variable X.

Now, after the coefficients $\hat{a}$ and $\hat{b}$ have been calculated, it is necessary to validate these assumptions. This is not always easy to do. In my response[11] to Evered's first article[12] I suggested these be tested. Now Evered had tested his tertiary model with the F-test, which determines if a significant decrease occurs in the sum of the residuals by assuming the model, that is, a significant portion of the residuals is 'explained' by the regression model. Evered has misunderstood[13] my request to test the residuals to validate the regression assumptions. Because Evered has not used any weighting it is obvious his tertiary model does not meet assumption (2). To test assumption (3) a graph of the residuals is done. However, in the case of the 163 c data the graphs of Humphreys and Hasofer are so compressed that it is impossible to see any systematic pattern in the residuals. An analytical technique exists, which Evered mentions, called the Durban-Watson test for autocorrelation. When a regression line fails such a test it shows that the residuals are not independent of the independent variable, in this case time. This test when applied to the Evered, Hasofer and Aardsma regression lines rejects residuals' independence at the 99% confidence level. This is hardly surprising, since the trend in the value of error bars with time is so obvious as to be discerned by casual inspection.

Two final points should be made about regression lines with respect to the c data controversy. First, the regression technique is model dependent. While Evered claims the data conforms to a tertiary model, Hasofer claims a significantly sloped quadratic model. The most important factor in such a difference of opinion may not be the weighting or data edits, but the initial choice of model. Also, there may be a yet undiscovered model which could make them both obsolete. Second, because the regression technique is model dependent it does not test the hypotheses directly. By this I mean that Evered's tests are all dependent on his assumption that a tertiary model is appropriate. If superior models exist or are discovered later then his initial assumption is invalidated and perhaps his conclusions also. The reason I have not proffered any regression line data in my paper is that there are other direct tests which are independent of models, error bars and weightings, and therefore are more conclusive.

Concerning Evered's regression line in particular, I would again point out that since he has used a mixture of dynamic time and atomic time c-data he has not tested the Setterfield hypothesis. It is no use to point out that Setterfield uses this data in his regression lines.[14] When one is supporting a claim, one can use assumptions and data that may be biased against you which demonstrates your claim can survive even biased data. However, this does not give the skeptics justification to use the same data or assumptions. This would be less than rigorous. Again, the use of data inconsistent with the hypotheses given invalidates any conclusions which may be drawn from the results until the same results can be demonstrated with the appropriate data. Evered has one other major problem. For his tertiary model to support constancy of c over trend his model must have coefficients not significantly different than zero. This simply is not true. In fact, he himself points out that his model decreases to zero in AD 987 and that it is scientifically nonsense. I would agree that his tertiary model makes ridiculous retrodictions. But how does this prove that every regression model will make ridiculous scientific predictions and retrodictions? Evered is using proof by example. This is not logical. If Evered could produce a tight fitting curve with insignificant coefficients this would enhance his case greatly, but he does not have one so far. Notwithstanding the selection of the wrong data Evered's claims are still dubious on the bases that:

(a) he uses a biased edit of the data,
(b) his model does not have residuals which conform to assumptions (2) and (3), and
(c) he has not demonstrated that his results are not the product of systematic errors from mixing data from different methodologies.

Concerning Brown's regression line,[15] I will say only this, that it does not offer any better substance than that of Evered. Aardsma's[16] contribution to the debate is somewhat different in that his is a weighted analysis which severely limits the effect of extreme data. The inclusion or exclusion of Roemer or Cassini affect the outcome only marginally. He performed one test with the atomic clock c values and one without. The Durban-Watson test of his line is significant at the 99% confidence level so that the straight line is not a proper model. As Evered points out it also fails to be significant in the F-test, showing that it does not explain a significant amount of residuals. But Aardsma's use of the line is not as a model but as a trend line to find an average slope of the decrease. In most data sets this would be the standard test. However, the strong autocorrelation demonstrates that there is a systematic rather than a random weighting pattern. The weighting

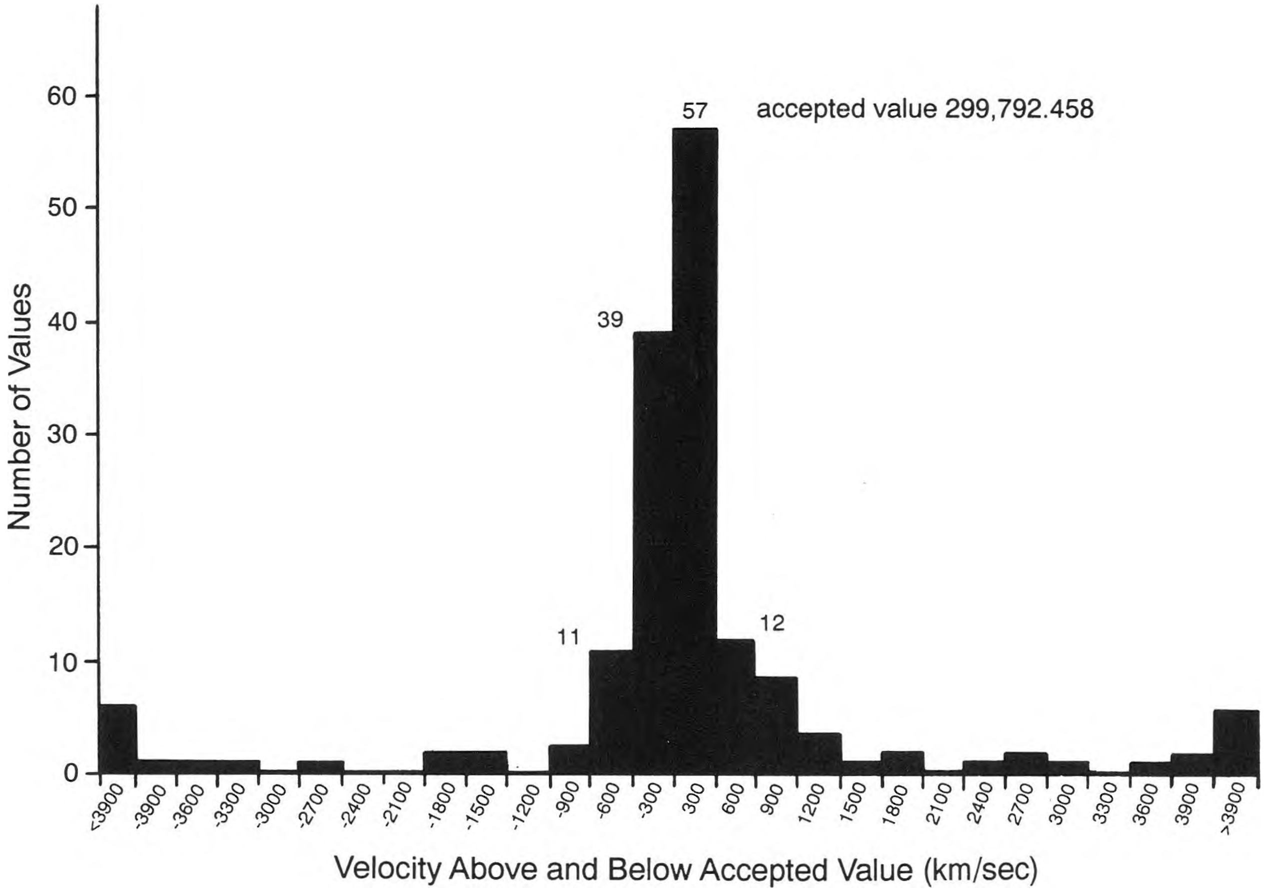

Figure 1. *Histogram of Hasofer's data (without Roemer) at 300km/sec intervals.*

ratio between atomic clock values and non-atomic values is over 10,000 to 1 and pre-1945 to post-1945 non-atomic values is over 1100:2. Such ratios should indicate caution in accepting the results.

Consider the following: the lowest 18th century value of *c* is 300,460 (Lindenan, 1783) and the highest 20th century value is 300,420 (Sollenberger, 1933). They are separated by 40 km/sec and 150 years for a rate of decrease of 0.27 km/sec/yr. Surely any rate of decrease less than this defies common sense, yet Aardsma claims 0.01 km/sec/yr for non-atomic data, a full 27 times lower. For data including atomic clock values he claims 0.000014 km/sec/yr. Surely, he should have been alarmed that the addition of 8 points to 155 should result in a 700-fold change in the rate! There are sound reasons for doubting the use of a weighted trend line over data with such large variations in error bars. Other weightings, piecewise analysis, careful editing or using a method not so dependent on error bars are alternative options. Aardsma chose none of these options. Norman's trend lines by method also suggest Aardsma's results are dubious.

The inclusion of 292,000 km/sec as the Roemer value datum is also dubious. Since Aardsma[17] pointed out Setterfield's supposed misinterpretation of Goldstein's letter, Bowden[18] has quoted the work of Mammel who pointed out that Goldstein erred and that Setterfield's published value was actually correct. Mammel, using different observations by Roemer, arrived at 318,000 km/sec, a value which has been confirmed independently by Chaffin.[19] In all my analyses I omit the Roemer datum as its value is not likely 292,000 km/sec and there is too much contention over it. Its value is not very important to the testing of the variable *c* hypothesis.

Are the regression lines of Hasofer[20] and Norman[21] of any use or are they flawed? Certainly, none of them can satisfy the three assumptions about the independent distribution of residuals. However, had these regression lines not been done there would be serious questions about any consistent discernible pattern or curvature to the data. There is a certain usefulness to being able to describe the shape of the curve, particularly when some of the coefficients of determination are an impressive 0.96. But these

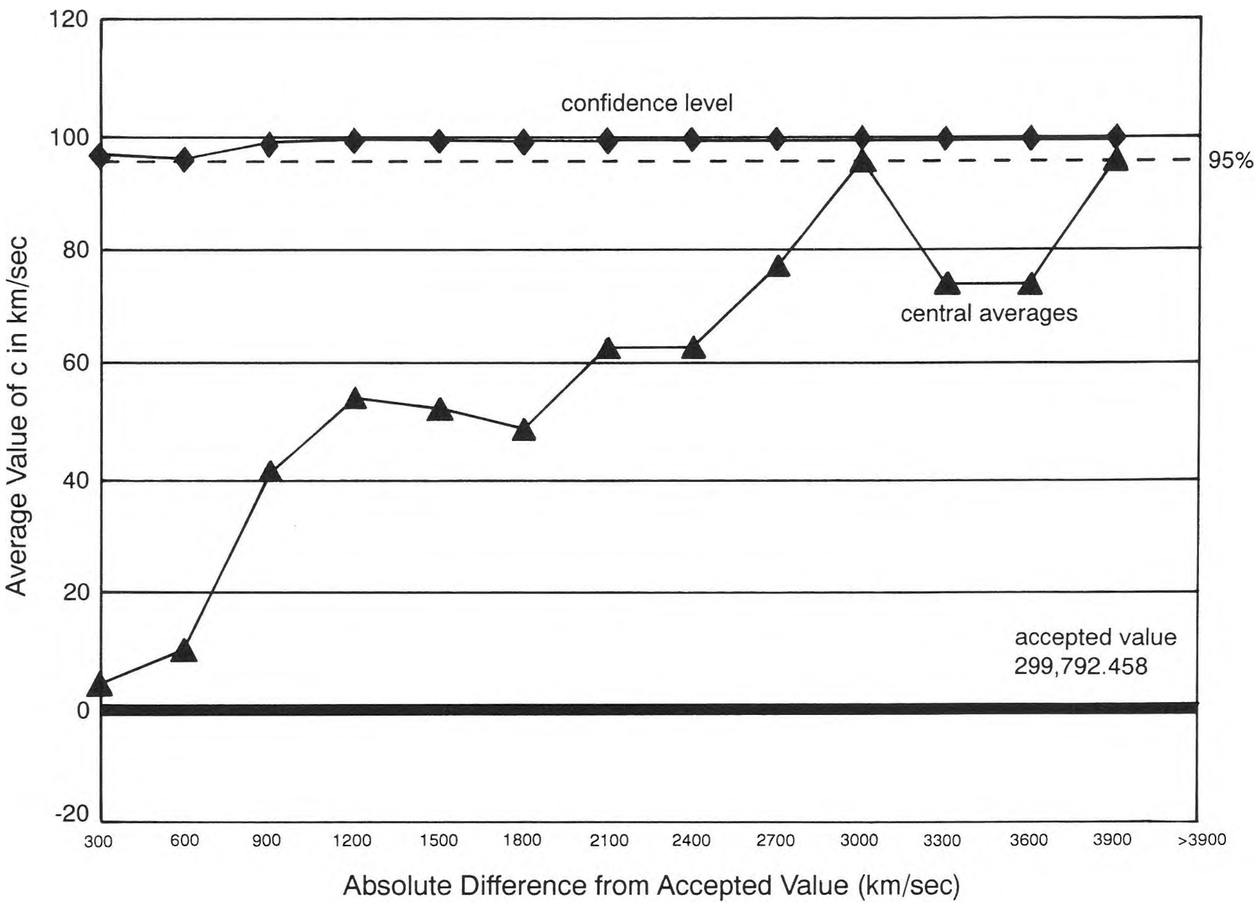

Figure 2. *Average values and confidence levels of distribution by 300 km/sec grouping around accepted value.*

curves may or may not prove useful in finding the elusive mechanism for the variable *c* hypothesis.

THE DISTRIBUTION OF DATA TESTS

If the regression lines are difficult to use to interpret this data, what tests are there which could be applied to the entire data set as Evered demands? These tests must be independent of error bars since many are missing and evaluation is subjective. The tests must also be insensitive to outliers whose selection might also be subjective. I can think of four tests including the MSSD and Run tests which we will examine later. The first is a test of the distribution of the number above and below the accepted value of *c*. If the measurements of *c* are decreasing with time there should be a significantly higher number of values above the current value than below. Figure 1 shows a histogram of Hasofer's data (without Roemer) at 300 km/sec intervals. A binomial distribution is used to test the hypothesis that this data is evenly distributed about 299,792.458 km/sec vs the hypothesis that the data is skewed above the accepted value. The statistic

$$z = \frac{x - E[x]}{St.\ Dev.\ [x]} \tag{1}$$

is approximately normal with

$$E[x] = \frac{1}{2}(number\ of\ data) \tag{2}$$

and

$$St.\ Dev.\ [x] = \sqrt{\frac{1}{4}(number\ of\ data)} \tag{3}$$

The z values ranged from 1.74 for ±600 km/sec to 2.48 at 3000 km/sec. The ratio of the data above to below is 96 to 67 for a *z* value of 2.27. The confidence levels range from 95.9% to 99.3% with 8 of 14 significant at the 99% confidence levels. Now could anyone guess these results from Dr Evered's histogram?[22] His histogram lumped 118+ values into one bar 1000 km/sec wide straddling the accepted value. Thus only the bar 500 to 1500 km/sec from the accepted value of *c* showed any skew in the data. Figure 2 shows that the centralized averages for the data by ranges are all above the accepted value up to ±3900 km/sec.

There is a second test which can be applied. One can count the number of ups and downs for each pair of consecutive data. There are 99 downs, 4 ties and 60 ups in Hasofer's data for a *z* value of 3.01 which is significant at the 99.9% confidence level. The problem is that the sequencing of data is not unique. Because some data have the same dates they can be ordered from highest to lowest or vice versa. To avoid this problem, data with the same date should be averaged. When this is done the *z* value drops to 1.47 with a confidence level of 92%. These tests show there is a systematic trend for *c* values to be higher than the current value and have the advantage that they are positional tests without dependence on individual values or their error bars.

What has Evered to say to the evidence presented in my paper?[23] There is no discussion or explanation of the MSSD and Run tests on *c* data or *c* dependent data. These tests have the advantage of testing the hypothesis as given, testing it directly and testing it on data which is not subject to systematic errors between methods. The 18 of 19 MSSD and Run tests rejecting constancy is a pattern of tests which cannot be explained by a random normal distribution of the data. The number of tests is far in excess of any reasonable expectation on the assumption of *c* constancy. The only test which Evered manages to challenge is the Rydberg constant. Here he edits one value and produces a trend line which is significant. What he fails to mention is that I also edited this value before applying my tests. The t-test, MSSD and Run tests do not yield confidence levels which would reject constancy. Of the four tests applied to the same data three fail to reject constancy. Yet he remarks

> *'this "change" reflects differences in the estimated value of the Rydberg Constant as time passes ... 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.'*[24]

Is Evered suggesting here that since one statistic indicative of a trend is likely false that all of them are false? This would be a non-sequitur of the highest order, suggesting that statistical hypothesis testing is a worthless proposition altogether. The 95% confidence level is by definition one where the statistic is likely to be wrong one time in 20. With such a caveat, it is an extremely untenable position to interpret one result so strongly. For Evered to take the results of one test on a set of data to negate 38 of 45 t-tests, MSSD and Run tests on many sets of data is totally absurd.

So how does Evered respond to the MSSD and Run tests? By insisting that the results are not valid because they do not use all the data. (Back to the holy 162 data!)

THE MSSD AND RUN TESTS

I have responded to the challenge of using all 162 data with a binomial test of 'aboves' and 'belows', as well as 'ups' and 'downs'. I will also respond in a positive fashion to Evered's analysis of the MSSD and Run test results on all 162 data. Firstly, the MSSD and Run tests conducted by Evered both reached significance. Now Evered's constant claim is that my tests are valid *'only if there has first been a careful selection of the points concerned.'*[25] This obviously cannot be applied to his results on these tests. Evered also states *'when the whole 1987 data set is used the Setterfield theory* [sic hypothesis] *collapses like a house of cards.'*[26] This comment also does not apply to the above tests. How does Evered interpret these results? *'Such a result is associated with positive serial correlation, a condition characterized by runs.'*[27] This is true. What he is saying, whether he intends to or not, is that there is a trend. He follows with *'but there is no evidence from this result of a long decreasing trend in the* c *values.'*[28] These statements are contradictory. The statistic **may** be giving the wrong inference but Evered is clearly standing on his head when he makes the claim that there is no evidence for a trend. He may wish to claim that he can substantiate a false inference but he clearly cannot claim that there is no evidence of a trend to explain. The fact that he spends half a page explaining the results away is proof that there is evidence of a trend to explain away!

Evered claims that there ought to be mostly positive residuals in the early data followed by negative residuals in the latter data. This is clearly correct. He then claims that *'you just do not find* (it) *in this data set.'*[29]

Evered's problems here are many and I beg the reader to be patient while I explain. In order to do a Run test or an MSSD test one must first have ordered data. Each datum must have a unique date, otherwise there is no unique order and no unique statistical result. There is nothing in Evered's paper that indicates the order he used. If he used Hasofer's order, then his statistic is biased as the data is clearly arranged with the highest value first when the dates are the same. In my tests I averaged data with identical dates. This not only gives a unique order but also it reduces those data which are reworkings back to a single value so that they are not over-represented. A primary assumption of the test is that there is an equal probability that each datum is above or below the average. When a datum such as Cornu 1874.8 is reworked this assumption is not likely true. It is more likely the reworking will be close to the original and less likely to be on the other side of the average.

The next point is not so important. The textbook application of the Run test uses the average rather than the median. The formulae for evaluating the expected runs and standard deviation provide for an uneven number of positive and negative residuals. Evered ought to have made this point clear.

Now I do not suggest that the median or 50th percentile value cannot be used in the Run test. Considering the volatility of the average when data is edited, the wide dispersion of outlier data and the extreme relative precision of the post-1945 data, the median Run test is a very innovative way of reducing the effects of outlier data.

Evered's last problem is a major one. The median value of his data is not 299,790 as stated by Evered, but 299,792.7 for the 162 data and 299,793.075 for all the data averaged by date. This significantly affects the results of the test. The proper application is seen in Table 1. Instead of finishing the Run test with a long run of positive residuals (Evered's *b*'s) it finishes with a string of 13 negative ones, which is just what Evered states is needed to confirm the legitimacy of the Run statistic results. Table 2 contains the Run test on averaged data with medians and averages. The results for the medians are much less significant than those of the means, yet still well above the 99% confidence level. Note also that the early data contain 38 positive residuals (Evered's *a*'s) out of 54. The remaining 16 negatives contain mostly data rejected by experimenters or outlier values of the EMU/ESU method. Thus the test properly applied with the correct median or average values yields results which are indicative of trend. Evered's conclusions are clearly false.

The lack of significance of the t-test is not such a problem as Evered pretends. The t-test tests the average value of a data set against the accepted value. It does not test for trend directly. It would be reasonable considering the results of the MSSD and Run tests that there exists a trend, but that some of the data is systematically low. In particular Kerr Cell and 20th century aberration values are low and not consistent with other data in the same era. Also, EMU/ESU data have an average value of 297,222 km/sec. By mixing methods the above average and below average values cancel each other leaving a statistically insignificant deviation from the accepted value.

Before proceeding to look at these results, I want to comment on Evered's coefficient of variation which is one of the few relevant statistics that he does offer to the reader. The coefficient of variation is the standard error converted to a percentage of the value measured. Evered's table clearly shows that the electronic post-1945 data have superior precision by a wide margin. But if the coefficient of variation is such a clear indicator of the wide variation in precision of the data, why does he insist on mixing such diverse data? The t-test on the post-1945 data rejects constancy of the current value of *c* at the 99% confidence level and a least squares fit yields a slope significant at the 99% confidence level. In addition the Run test applied with the median value is significant at the 99% confidence level also. Why, with such strong results from the most precise data, is Evered so insistent on mixing it with inferior data?

ERROR BARS

Evered's comments on the error bar analyses are indeed unfortunate for they are not totally accurate, nor are his innuendoes true. The early aberration values are absent from the error bar table, but so is the 1881.8 Newcomb value and the 1901.4 Perrotin value. Why? The reason these are missing is that the analysis was done from Table 2 (best 57 values) of Norman and Setterfield's report, not on all the data. I apologize to my readers that this was not explicitly stated in my paper. I confirm that all values omitted deliberately from Table 2 are clearly stated and the reader can make his own judgment as to the meaning and usefulness of the results. Evered cannot be faulted for failing to read my mind rather than the text. However, he is more than a little eager to use this miscommunication to suggest deceit or at least mischievous bias.

Now the reasons for omitting the Pease-Pearson result is that the values for *c* fluctuated with the tides because of unstable soil conditions surrounding the equipment.[30] The Kerr Cell results are very consistent among themselves, but are inconsistent with all the best data between 1875–1925, as well as the post-1945 data. The Kerr Cell technique, which is the electronic equivalent to the toothed wheel, was upgraded with the introduction of the geodimeter. The geodimeter results are consistently 10 km/sec higher. For these reasons, I suspect strongly that the method has a systematic error.

There are some, of course, for whom suspicions are not enough. Examine Table 3 which contains all data with error bars 90 km/sec and less, including aberration values, the Pease/Pearson datum and Kerr Cell results. There are 56 values. The t-test on the first line is not significant. In this Evered is correct. Evered, however, is not correct in stating that '*this result is a much truer picture of the real situation and indicates no significant change from "c now"*.'[31] From Table 3 it is plainly evident that with the exception of those ranges strongly influenced by Kerr Cell results and 20th century aberration values, the averages are higher and significant at the 95% confidence level. The 10km/sec group is not favourable to Setterfield but it, too, fails to support the current value of c, since its 5% confidence level is a 95% confidence level that *c* is below the current value! This result indicates that the data may have special systematic error problems as suggested earlier.

The outsiders, the values not containing the current value of *c* in their standard error bars, show that the higher values predominate, with the normal approximation of the binomial distribution failing to reach the 95% confidence level only once! Evered's criticisms are shown to have almost no effect on my conclusions.

There is one other major omission in Evered's discussion. I mention three observational evidences[32] which variable *c* could explain which constant-*c*-physics cannot — the supernovae remnants, the similar shapes of galaxies and the diffusion rates of radioactive decay by-products in zircon crystals. Not one attempt has been made by constant *c* proponents to explain this evidence.

	DATE	AVERAGE *c* VALUE BY YEAR	RESIDUALS	NEGATIVE RESIDUALS	POSITIVE RESIDUALS	RUNS
1	1693.0	352000.0000	52,206.9	0	1	1
2	1727.0	303430.0000	3,636.9	0	1	1
3	1738.0	303320.0000	3,526.9	0	1	1
4	1759.0	303440.0000	3,646.9	0	1	1
5	1771.0	302220.0000	2,426.9	0	1	1
6	1783.0	300460.0000	666.9	0	1	1
7	1841.0	300305.0000	511.9	0	1	1
8	1843.0	299890.0000	96.9	0	1	1
9	1849.5	314300.0000	14,506.9	0	1	1
10	1855.0	301825.0000	2,031.9	0	1	1
11	1856.0	3107000.000	10,906.9	0	1	1
12	1858.0	299800.0000	6.9	0	1	1
13	1861.0	300050.0000	256.9	0	1	1
14	1862.8	298000.0000	-1,793.1	1	0	2
15	1864.5	299870.0000	76.9	0	1	3
16	1866.5	301050.0000	1,256.9	0	1	3
17	1868.0	291720.0000	-8,073.1	1	0	4
18	1869.0	280900.0000	-18,893.1	1	0	4
19	1870.0	299980.0000	186.9	0	1	5
20	1872.0	298500.0000	-1,293.1	1	0	6
21	1873.0	299580.0000	-213.1	1	0	6
22	1874.0	289700.0000	-10,093.1	1	0	6
23	1874.8	300126.6667	333.6	0	1	7
24	1876.5	299921.0000	127.9	0	1	7
25	1878.0	300140.0000	346.9	0	1	7
26	1879.0	297033.3333	-2,759.7	1	0	8
27	1879.5	299675.0000	-118.1	1	0	8
28	1880.0	298441.0000	-1,352.1	1	0	8
29	1880.5	299480.0000	-313.1	1	0	8
30	1881.0	299000.0000	-793.1	1	0	8
31	1881.8	299810.0000	16.9	0	1	9
32	1882.0	287000.0000	-12,793.1	1	0	10
33	1882.7	299860.0000	66.9	0	1	11
34	1882.8	299853.0000	59.9	0	1	11
35	1883.0	298125.0000	-1,668.1	1	0	12
36	1884.0	301880.0000	2,086.9	0	1	13
37	1886.0	301500.0000	1,706.9	0	1	13
38	1887.0	300570.0000	776.9	0	1	13
39	1888.0	292000.0000	-7,793.1	1	0	14
40	1889.0	300250.0000	456.9	0	1	15
41	1889.5	300066.6667	273.6	0	1	15
42	1890.0	299828.5000	35.4	0	1	15
43	1890.5	300560.0000	766.9	0	1	15
44	1891.0	301560.0000	1,766.9	0	1	15
45	1891.5	299964.0000	170.9	0	1	15
46	1892.0	299130.0000	-663.1	1	0	16
47	1892.5	300090.0000	296.9	0	1	17
48	1893.0	298610.0000	-1,183.1	1	0	18
49	1894.5	300430.0000	636.9	0	1	19
50	1895.0	300300.0000	506.9	0	1	19
51	1896.0	300170.0000	376.9	0	1	19
52	1896.5	300170.0000	376.9	0	1	19
53	1897.0	299983.3333	190.3	0	1	19
54	1898.0	301160.0000	1,366.9	0	1	19
55	1898.5	299775.0000	-18.1	1	0	20
56	1899.0	300036.6667	243.6	0	1	21
57	1900.4	299966.0000	172.9	0	1	21
58	1900.5	299480.0000	-313.1	1	0	22
59	1901.4	299880.0000	86.9	0	1	23
60	1901.5	299490.0000	-303.1	1	0	24
61	1902.4	299880.5000	87.4	0	1	25
62	1903.0	299360.0000	-433.1	1	0	26
63	1904.5	300250.0000	456.9	0	1	27
64	1905.0	299895.0000	101.9	0	1	27

65	1906.0	299781.5000	-11.6	1	0	28
66	1906.5	299650.0000	-143.1	1	0	28
67	1907.0	299610.0000	-183.1	1	0	28
68	1907.5	299610.0000	-183.1	1	0	28
69	1908.0	299630.0000	-163.1	1	0	28
70	1908.5	299435.0000	-358.1	1	0	28
71	1909.0	299440.0000	-353.1	1	0	28
72	1909.5	299670.0000	-123.1	1	0	28
73	1910.0	299710.0000	-83.1	1	0	28
74	1914.0	299640.0000	-153.1	1	0	28
75	1916.0	299520.0000	-273.1	1	0	28
76	1922.0	299550.0000	-243.1	1	0	28
77	1923.0	299795.0000	1.9	0	1	29
78	1923.5	299760.0000	-33.1	1	0	30
79	1924.6	299802.0000	8.9	0	1	31
80	1926.5	299734.0000	-59.1	1	0	32
81	1928.0	299938.0000	144.9	0	1	33
82	1930.5	299630.0000	-163.1	1	0	34
83	1932.5	299774.0000	-19.1	1	0	34
84	1933.0	300420.0000	626.9	0	1	35
85	1935.0	299570.0000	-223.1	1	0	36
86	1935.5	299920.0000	126.9	0	1	37
87	1936.8	299771.0000	-22.1	1	0	38
88	1937.0	299771.0000	-22.1	1	0	38
89	1940.0	299776.0000	-17.1	1	0	38
90	1947.0	299795.0000	1.9	0	1	39
91	1949.0	299794.2000	1.1	0	1	39
92	1950.0	299793.3000	0.2	0	1	39
93	1951.0	299793.3000	0.2	0	1	39
94	1953.0	299792.8500	-0.2	1	0	40
95	1954.0	299793.9250	0.9	0	1	41
96	1955.0	299792.2000	-0.9	1	0	42
97	1956.0	299792.4200	-0.7	1	0	42
98	1957.0	299792.6000	-0.5	1	0	42
99	1958.0	299792.5000	-0.6	1	0	42
100	1960.0	299792.6000	-0.5	1	0	42
101	1966.0	299792.4400	-0.6	1	0	42
102	1967.0	299792.5300	-0.5	1	0	42
103	1972.0	299792.4610	-0.6	1	0	42
104	1973.0	299792.4577	-0.6	1	0	42
105	1974.0	299792.4590	-0.6	1	0	42
106	1978.0	299792.4588	-0.6	1	0	42
107	1979.0	299792.4581	-0.6	1	0	42
108	1983.0	299792.4586	-0.6	1	0	42
			37,060.68	54	54	
All Data (1693–1983)		Expected Runs	55.00			
Median 299793.0750		Standard Deviation	5.17			
		Run Test	-2.51			
		Confidence Level	99			

Table 1. *Results of the Run test on all data (1693–1983).*

Why? With all his cries of 'scientific sense', Evered should be impressed by some real scientific observational evidence rather than a few statistically 'biased' tests. Yet he shows not the slightest interest in the most crucial test of any scientific hypothesis — can it explain something previously unexplained?

I will leave Evered's comments on Hasofer's data and analysis to Norman who chose the error bars. The issue is of little interest to me as it affects only my error bar analysis and only in one datum (Delambre) which occurs in only one line. I **do** wish to comment here on Evered's charge of 'scientific nonsense' in regard to the predictions of Hasofer's quadratic function. According to Evered the quadratic *'requires 2.2 billion years to reach the required value of* c *at creation.'*[33] The requirement of variable functions is that the distance travelled in dynamic and atomic time standards be equal. In regard to cosmology 10 billion light years is a generally accepted value. The

DATE	TYPE	CENTRAL VALUE	MSSD	CONFIDENCE LEVEL	RUNS	NORMAL *Z*	CONFIDENCE LEVEL
1693–1983	median	299793.1	1.089	99%	42	-2.51	99.4%
	average	300136.2	1.093	99%	22	-4.75	99.995%
1727–1983	median	299792.9	1.61	95%	42	-2.34	99.0%
	average	299651.5	1.62	95%	35	-2.04	97.9%
1693–1967	median	299794.1	1.089	99%	40	-2.39	99.2%
	average	300156.4	1.093	99%	20	-4.92	99.995%
1727–1967	median	299793.9	1.61	95%	40	-1.78	96.3%
	average	299643.2	1.62	95%	35	-1.62	94.7%
Durban-Watson Table Used		n=100; p-1=1; dL=1.65 at p=0.05 n=100; p-1=1; dL=1.52 at p=0.01					

***Table** 2. MSSD and Run tests on all data.*

integral of the Hasofer function reaches this in 700,000 dynamic years. There is no requirement to reach 5 x 10^{11} c. This value is the one necessary to reach 10 billion light years under the cosec squared function and has nothing to do with Hasofer's. His argument is spurious.

THE COSEC SQUARED FUNCTION

Unsatisfied with such an argument, Evered uses the cosec squared function to predict more 'nonsensical' results. The equation I suggested was the cosec squared function until 1961 (although statistically 1951 or 1981 would fit equally well) and constant thereafter. Ignoring the '*constant thereafter'*, Evered proceeds to suggest that this formula predicts a value of 299,803 in 1990 and 299,815 in 2000. This is not the function I suggested and Evered has attacked yet another straw man.

But Evered continues by pointing out that physical decay processes are described by negative exponential functions. In this Evered is correct. But Evered is wrong in his assumption that *c* variation is a decay process. Initially Setterfield tried to use variable *c* to explain the red-shift. This involves loss of photon energy and hence a decay. However, as early as 1984 Cheesman[34] demonstrated that Setterfield's argument was flawed. I also have argued against this[35] and Osborn[36] brought yet a third argument against this position. Setterfield now acknowledges that no red-shift is implied by his hypothesis and hence no decay. I conclude therefore that 'CDK' is a misnomer and the need for a negative exponential function is obviated. This is why the term *c* decay or 'CDK' has never appeared in my papers.

But why the cosec squared function? It is the only one proposed so far which allows carbon-14 dating to be maintained, a point which Aardsma carelessly overlooks. Why should it stop in 1961 (or 1951 or 1981)? I don't know, but may I suggest one **possibility**?

Suppose the universe is closed so that it has a maximum radius R. Suppose also that the universe is expanding so that it has radius $r(t)$ at time t. The ratio $r(t)/R$ ranges from 0 at time zero to 1 at T when $r(T) = R$. Suppose lastly that c varies as the gravitational acceleration at the outer radius of the universe.

Then

$$c(t) = KGM/r^2(t) \qquad (4)$$

for some K.

Using 3 x 10^8 m/sec as the value at time T

$$C(T) = 5 \times 10^{17}\, GM/R^2 \qquad (5)$$

where M is 10^{53} kg and R is 10^{26} metres.
Now sin θ also ranges from 0 to 1 for θ = 0° to 90°

So let k be a constant such that

$$\sin(kt) = r(t)/R \qquad (6)$$

and

$$\sin(kT) = R/R = 1 \qquad (7)$$

ERROR BAR	NO. OF DATA	AVERAGE	T TEST	CONFIDENCE LEVELS	OUTSIDERS			BINOMIAL TEST	CONFIDENCE LEVEL
					ALL	HIGHS	LOWS		
'=<90	56	299,851.99	0.91	82.0	32	21	11	1.77	96.0
'=<50	40	299,801.94	1.87	96.0	19	14	5	2.06	98.0
'=<10	31	299,790.28	-1.75	4.0	15	10	5	1.29	90.0
'=<5	25	299,793.14	2.50	99.0	11	10	1	2.71	99.7
'=<2.5	22	299,792.89	2.30	97.5	10	9	1	2.53	99.0
'=<1	14	299,792.61	2.22	97.5	7	6	1	1.89	97.0
'=<0.5	12	299,792.62	2.04	97.0	7	6	1	1.89	97.0
'=<0.1	3	299,792.53	2.77	95.0	1	1	0		

Results negate c *constancy in 6 of 8 t-tests at 95% confidence level.*
Results negate c *constancy in 7 of 8 binomial tests at 95% confidence level.*

***Table** 3. Analysis of* c *by error bar and outsiders.*

and

$$r(t) = R \sin (kt) \qquad (8)$$

From (4) and (8) we get

$$\begin{aligned} c(t) &= KGM/R^2 \sin^2 (kt) \qquad (9) \\ &= C(T) \operatorname{cosec}^2 (kt) \end{aligned}$$

In this model the universe ceases to expand at radius R and $c(t)$ is at its minimum. If matter then falls into orbit at this distance the universe will stabilize and no further change in c will be observed. If creationists are to continue this type of research they need some able thinkers who can bypass the skepticism and the ‘it has never been done before’ mentality. Other possibilities may exist, but they will never be found if we listen to Evered, Brown and Aardsma.

RATES OF CHANGE

The rate of change table of Evered is interesting. I wish that he had given the timespan over which these values had been calculated. For, since the rate of change in these ‘constants’ is variable, it is quite important to calculate those changes over the same time period. Since h/e^2 and y' are all post-1945 measurements a more reasonable comparison to c would use the 23 values from Table 7 of Norman and Setterfield’s report. Their rate of change is 6.33 x 10^{-7}, a remarkable close fit to both h/e^2 and y'. This to me should be reason for confirmation not rejection of Setterfield.

As for the e/mc and h/e, their high rates of change are already exhibited in Evered’s diagrams of e/mc^2 and hc. I have already pointed out that such claims do not fit the constancy of c any better than they fit the variable c hypothesis. Does Evered believe that e is increasing, mass-energy is decreasing, or both? Any conventional scientist would maintain the constancy of these by sincerely suggesting such rates of change are due to systematic errors in the data. Now why would the most reasonable explanation in constant c physics not be at least an option in variable c physics? But actually, there is no need to wait for Evered’s answer — the values of h/e are known already to have systematic errors in the pre-1945 data due to bad estimation of the x-ray cut-off point.[37] If the post-1945 data is used to calculate the trend for h/e and q/mc the rate of change values reduce to 1.92 x 10^{-5} and 9.23 x 10^{-6} respectively. Thus the relative rates of change do not exceed 33! Thus the rates of change are remarkably close when corresponding time periods are used. Evered’s conclusions are based on inappropriate comparisons.

SUMMARY AND CONCLUSIONS

The c-data have been analyzed by method with trend lines, t-tests, MSSD and Run tests, by error bars with t-tests, by outsiders and by 300 km/sec steps from the current value with binomial tests; t-tests, MSSD and Run tests have been performed on the whole 162 data. ‘Ups’ and ‘downs’ have been analyzed on the whole 162 data. The c-dependent data have also been subjected to t-tests, trend lines, MSSD and Run tests. With one exception each subgroup rejects the constancy of c at the 95% confidence level in at least 50% of the tests! How does Evered respond to the depth and the consistency of the pattern of these results? He declares

> *‘The evidence against a recent decrease in* c *is overwhelming . . .’*[38] and
> *‘decrease in* c *exists only in the minds of those advocating the theory.’*[39]

His abstract asserts that,

DATA	TEST	CONFIDENCE LEVEL	RESULTS
c by method	t	95.0	constancy at *c* now rejected in 3/6
	MSSD	95.0	constancy rejected in 6/7
	Run	95.0	constancy rejected in 2/2
	Trend line	95.0	constancy rejected in 4/6
c-dependent	t	99.0	constancy at accepted value rejected 3/5
	MSSD	97.5	constancy rejected in 5/5
	Run	95.0	constancy rejected in 4/4
	Trend line	95.0	constancy rejected in 4/5
c by 300km/sec range	Binomial	95.0	normal distribution rejected in 13/13
c by error bars =<90	t	96.0	constancy at *c* now rejected in 6/8
	Binomial	96.0	normal distribution rejected in 6/7
'Ups' and 'Downs'	Binomial	95.0	normal distribution rejected in 0/1
c averaged by date	MSSD	95.0	constancy rejected in 8/8
	Run	95.0	constancy rejected in 7/8
Setterfield hypothesis supported at 95% confidence level in 71 of 85 tests. Setterfield hypothesis supported at 90% confidence level in 80 of 85 tests.			

Table 4. *Summary of tests of the Setterfield hypothesis.*

> *'in view of the huge amount of evidence against this theory and virtually none for it, . . . the whole notion should be dropped ...'*[40]

Based on faulty applications of the MSSD and Run tests with the wrong median he writes *'I repeat, there is no trend.'*[41] These conclusions are grossly overstated to say the least.

There are many other blunders contained in Evered's paper — too many to examine here. It is far easier to acknowledge the few statements which are correct.

(1) Van Flandern's results are not supported by radar ranging experiments; although this does not clarify which one is right.

(2) *hc* and e/mc^2 graphs do not support Setterfield's hypothesis, but neither do they support the constancy of c, *h*, *e* or *m*.

(3) The coefficients of the cosec squared curves can vary widely without affecting the fit, and the extrapolation of this curve can only be done on 'what if' type scenarios and not on any statistically valid basis. This says little about the validity of the function.

(4) The coefficients of variation vary widely. This is not surprising nor necessarily indicative of the constancy of c.

(5) The average value of the Pulkovo data is below *c* now. The data in Norman and Setterfield's graph have an average 88 km/sec above *c* now because they contain the Bradley datum which was not done at Pulkovo.

(6) The t-test on all 162 data is not statistically significant.

Apart from these points I do not find any data, methodology and/or argument that are valid. Table 4 contains a summary of the tests of *c* as constant versus *c* as trending. These tests were done on *c* by method, error bar size, range, distribution and on all data combined. They total 85 tests, 71 of which support directly or indirectly the trending of *c* over constancy at the 95% confidence level and 80 at the 90% confidence level. Nor are all the remaining five tests favourable to *c* constant at *c* now. The MSSD and Run tests which test the hypothesis directly produce positive results in 33 of 34 tests. The suggestion that this trend is produced by increasing precision is not supported by the error bars analysis. The suggestion that the trend results from a combination of methods and systematic errors is negated by the tests by methodology. The predictions based on decreasing *c* on other atomic-time constants are validated by testing values containing *h* (Plank's constant) and *m* (rest-mass). Nobody has demonstrated any reasonable explanation of these results other than variation of *c* itself.

Evered may be of the opinion, as many are, that *c* is a constant. Most, however, do not have the advantage of seeing these results. Evered is entitled to his opinion and I would not force him to change it on my account. I do not know why he holds this opinion. I only know that it is not the fault of the data. To suggest that these results are not real but only in our 'imagination' is, I maintain, insulting.

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