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The Velocity of Light Decay Debate: The Mathematician's Response

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INTRODUCTION

In this paper I will review yet another note concerning the analysis of the speed of light. I will then turn my attention to some long standing matters of debate, upon which I have until now remained deliberately silent. For those who have been following the ebb and flow of the 'c decay' hypothesis and its attendant controversy, I adjure you to take time to reflect. Ask yourself the questions you will find posed below. Perhaps you may see things in a different light.

I will first address the paper by Evered entitled 'Computer Analysis of the Historical Values of the Velocity of Light',[1] and then examine some related issues.

Evered claims that his analysis shows no significant change in the velocity of light in the last 300 years, and notes that this confirms the results of Aardsma,[2] Humphreys,[3] and Brown.[4] Unfortunately, it also confirms the errors made by these gentlemen, which have been pointed out in considerable detail elsewhere.[5] Since I am going to place great emphasis on this point, I will summarise the essential problem for the benefit of those who are unaware of the history of this debate.

My primary contention is, and has always been this: the method advocated by Aardsma, and defended by others, is **clearly and demonstrably wrong**. It is **guaranteed** to give a grossly misleading result when applied to the velocity of light data, or any other data having similar characteristics. By misleading I mean, for example, that a synthetic data set generated by an exact exponential (or polynomial, or sinusoid ...), when analysed using Aardsma's procedure will result in a so-called 'line of best fit' which **clearly** does not even remotely fit the data. By wrong I mean that the method employed is entirely inappropriate. I will return to elaborate this point further, for those who are sceptical of such a bold claim. Before doing so I will return to Evered's paper.

EVERED'S PAPER

After dismissing the linear equation, Evered claims that according to his analysis the best quadratic equation fitting the data would have c *'rising, peaking then decreasing'*. Significance is not obtained, it is claimed. This is simply not in accord with my own extensive analysis, nor that of Hasofer,[6] who states in relation to his second order (quadratic) analysis, *'Here the regression turned out to be highly significant'*. I am unsure how Evered arrived at his regression coefficients, but there is obviously a problem with the procedure. It is therefore not at all surprising that significance was not attained. It may be useful to remark that I have independently confirmed Hasofer's results. Neither his nor my own analysis predicts the behaviour to which Evered refers.

Next Evered turns to a cubic curve, for which he claims significance is attained, and notes that his results agree with Brown and Hasofer. I should perhaps be relieved, but he goes on to point out that, following Brown's method of applying confidence limits to the regression, he finds *'no evidence for the assertion that c has varied statistically . . .'*. I have two comments. Firstly, the coefficient of the cubic term in Evered's analysis differs from Hasofer's by almost an order of magnitude. Secondly, Brown's method has previously been shown to be clearly and simply wrong — as has been pointed out in our earlier publication,[7] and further explained by Montgomery.[8] If you are inclined to think such dismissal is arrogant, perhaps you should first consider Brown's cavalier mutilation of standard statistical equations, and the nature of his defence. This too will be addressed in the second portion of this paper. As will be seen, following Brown's method and agreement with his conclusions is a claim which takes on a rather unfortunate significance.

In Evered's discussion of his results, he appeals to one's sense of the absurd, invalidating equations which would predict scientific nonsense. Whilst I readily agree with this basic approach, I feel very uncomfortable with the philosophy which demands that we ignore the data if it doesn't agree with our model. It has been our intention to rethink our model before we take liberties with the facts.

Evered concludes with reference to error limits, and some coarse measures (for example, the median), which

may be significant, regardless of error margins. In brief, my own analysis has shown that such tests (non-parametric statistics), strongly reject the hypothesis that c has remained constant. Again, I refer those who want to pursue the matter in detail to Setterfield[9] and Montgomery.[10] Finally, Evered asks, *'Where is the claimed evidence that this distribution supports the Setterfield theory?'* It is actually not at all hard to find. A standard analysis of the residuals of the many regressions performed show it plainly and emphatically, with a non-linear decay having residuals which measure an order of magnitude less than a linear decay, which is in turn less than the case of constant c.

If Evered is concerned with the matter of distributions (I use the term in its mathematical sense), then I would ask, 'Where is the analysis of the distribution of residuals?' If indeed the c data is random, then the residuals after regression should exhibit a zero mean value. Furthermore, there should be no correlation between the residuals and time. Any evidence to the contrary is a clear indication that the wrong regression model has been employed. This turns out to be **exactly** what is found in the case of the linear model applied to the c data.

Finally, I would dispute Evered's claim that a degree three polynomial provides the line of best fit. It is certainly a vast improvement on a linear fit, but the plain fact is that there are other curves which fit the data even better still. But this is minor nit-picking. There remain more basic issues to be addressed, and at this point I must turn my attention away from Dr Evered.

THE OTHER CRITICS

I promised earlier to elaborate concerning the fundamental flaw in the results previously reported by several other critics. I will concern myself with the works of Brown and Aardsma, the two most prolific 'constant c' protagonists who have dealt at length with the c data analysis.

Firstly, the matter of Brown's analysis — the point of contention being a missing factor of √N. The debate over the √N factor is summarised for your consideration.

1988. Brown[11] disputes our analysis, claiming that the √N factor should be omitted in applying the Student *t*-statistic. He notes, referring to our original analysis,

> *'They divide by SQRT(N) to obtain a predictor for mean values'*[12]

Having noted our intention, Brown then proceeds with his own analysis of mean values, yet still fails to appreciate that it is precisely because the analysis involved mean values that the √N factor is required. In the same volume of the **Creation Research Society Quarterly,** we pointed out the error,[13] including references to statistics textbooks which explain the correct application of the *t*-statistic to situations involving tests of sample means.

Brown's only defence:

> *'My objective is to evaluate their conclusions from the perspective of a broad view of each data set they use.'*[14]

I would suggest that Brown's view is perhaps a little too broad!

1989. Brown[15] adds to his defence with . . .

> ... *'At that time we agreed on wording that was considered to make my use of the data adequately clear.'* . . .[16]

Do these replies contain substance or evasion? Can one possibly find Brown's case 'adequately clear'?

1990. Montgomery[17] clearly points out the source of the contention, explicitly gives the equation which Brown ought to have used, and explains why it is appropriate.

Brown replies, in response to Montgomery's clear and specific explanation:

> *'My position in that analysis was to advocate only views which were consistent with a sound unbiased data evaluation such as may readily be made from the Aardsma and Humphreys Figure 1 plots.'*[18]

The simple fact is this: Brown's analysis is fatally flawed — it is wrong, he knows it is wrong, and rather than manfully retract, it appears he can only throw up pathetic, futile smoke screens like the above quote to protect the credibility of his several critiques, and of those who have followed both his erroneous methods and conclusions.

The √N factor appears in the equation for the calculation of a statistic having Student's *t* distribution because we are concerned with the **sampling distribution** of $\bar{x}$, the sample mean. To understand this, one need only consult an undergraduate statistics text. The actual derivation of the correct formulae is quite simple, requiring only basic proficiency in the use of random variables and expectations.

If I am wrong on this point, then why, in some four years of debate, have I **never** received an intelligible reply on this basic issue?

The bottom line of this debacle is this: Brown's calculations are quantitatively wrong, his confidence intervals are wrong, and no amount of intellectual huffing and puffing can save his conclusions from being, well ... wrong.

Now I will turn to my staunchest critic, Dr Aardsma, who has shown a particular flair for bad data analysis, and subsequent defence of that analysis. This is a strong claim, so I urge you to consider the following paragraphs carefully.

One of the greatest traps for the casual data analyst is to use the wrong **model.** Virtually all statistical methods start by making certain assumptions about the data, and even ultimately about the process which gave rise to the data. Next, a model is chosen by which we hope to characterise the data. If we chose correctly, and our previous assumptions can be justified, we can have some

measure of confidence that the parameters of the process or phenomenon we are measuring will be reflected in the statistics computed from the data. If we fail, our results will quite likely be meaningless.

In the context of analysis of velocity of light data, this takes on a dramatic significance. Firstly, note the enormous variation in the accuracy of measurements. They range from some hundreds or even thousands down to fractions of kilometres per second. Such a range of variability in one data set is atypical, to say the least. When the usual practice of squaring the standard error to obtain weights is employed, we end up with a truly absurd situation in which weights vary by some eight orders of magnitude!

A cautious researcher should already be very wary of how he treats this strange collection of data. Add to this, secondly, the fact that the data are not uniformly distributed in time (the early measurements were rather sparse, the recent data much less so), and we have violated most assumptions upon which the theory of least squares regression rests! I haven't yet mentioned the dilemma the numerical analyst is in by this stage, as the problem of solving for unknown coefficients has by now become extremely ill-conditioned. For the non-mathematical readers, basically that means that very small errors in a computer's representation of the numbers may cause very large changes in the computed results.

It may be of interest to know that I have verified our results (and also found anomalies in the results of Aardsma[19]) by carrying out the required calculations using **exact** arithmetic. In fact, one of the results reported by Aardsma[20] is clearly bogus, as the calculations become so awkward (due to the problem of grossly disparate weights), that abnormal termination (that is, what is commonly called a 'crash') of the regression program is **guaranteed,** unless extraordinary measures such as exact computation are taken.

I have often pondered what Aardsma did when his chosen weighted regression program mysteriously failed. In view of the highly conspicuous behaviour of the program, it seems unbelievable that the inclusion of a substitute value in his publication was merely due to carelessness, oversight or even typographic error. In fact, the erroneous value itself provides the answer (as I will show later), and suggests a more deliberate evasion of the problem. The latent numerical minefield referred to above had been triggered. Aardsma must have completely ignored the warning signs implied by his program failure.

In much less delicate circumstances, it is still vitally important to select the right model. In this case, the models which come to mind are the constant, the linear, and also several non-linear equations (which includes polynomials, exponentials, etc.). Even if we had a 'respectable' data set, forcing it into a shape which it plainly does not fit is simply bad analysis. Given the nature of the c data, it is outright recklessness. That is not to say one ought not to try ail the alternatives. In the absence of external information, that may be the only way to determine the appropriate model! What I find irresponsible is the insistence of some who believe that they can 'prove' something by only considering results from a clearly inappropriate model. Such has been the case in the debate over the claimed constant-c versus decaying-c debate.

A CHALLENGE

The crux of the matter is this: the c data is patently non-linear, as is the distribution of points along the time-axis, and the error margins vary enormously from one end of the data to the other. **I challenge anyone to demonstrate a weighted linear regression procedure which performs better than similar unweighted analysis on some known data with similar characteristics.** By 'better' I simply mean that the predicted values should consistently be closer to the data points. Of course the fit will be crude, since we are deliberately using the wrong model. Even so, it should be clear which method — weighted or unweighted — gives the better approximation.

As a test case, let us choose the equation $y=x^2$, with data points taken at x=0.1, 0.2, 0.5, 1, 2, 4, 8, 16, and let the respective error margins be assigned as $e=x^2$ also. If you will publish your graph in the next issue of this journal, I promise to likewise publish the graph produced by the same computer program which was used in our analysis of c-data. We could then let the readers decide which method gives a result most representative of the original data.

Some readers may have seen a similar scenario in a short article by Bowden,[21] who clarified graphically the problem which had previously been brought to Aardsma's attention by Setterfield.[22] Aardsma was given the opportunity to reply in the same issue of the **Creation Research Society Quarterly**,[23] and he proceeded to 'prove' that his weighted, linear regression is not only 'proper', but even that the weighted analysis of synthetic non-linear data is better in some sense . . .

> *'In fact, the weighted fit of the test data . . . shows that the decay trend ... is real . . . far more forcefully than the unweighted fit . . .'*[24]

Aardsma concludes rather patronisingly, stating

> *'But Norman and Setterfield's analysis is so clearly flawed and the proper analysis is so simple to do and clear in its results, that . . . there is no alternative but to reject it.'*[25]

If indeed it were so, perhaps I would be on the defensive — but let's do some spot checks ...

Firstly, if the analysis is indeed 'so simple', why is it that Aardsma's own papers contain numerical errors? Let me suggest an answer. In his original critique, Aardsma did us the service of referring to a text by Bevington,[26]

from which it was apparent that he had used Bevington's equations to calculate 'uncertainties' (yet another statistical term, in this case applied to the estimate of the slope of the linear regression line). But Bevington gives two alternative forms, which lead to numerically quite different quantities. This need not necessarily be a problem, as long as we use one method consistently — clearly one should not mix the two forms in the one analysis ...

But that is exactly what Aardsma did! One form, when calculating the uncertainty of the weighted regression, produces a 'floating point exception'. (That's computer jargon for a type of program failure, usually resulting in a 'crash'.) This is somewhat embarrassing. Rather than not report a result — much less account for such behaviour — Aardsma has quoted the value which the alternative form gives!

It may interest the reader to know that I informed Aardsma of this discrepancy in 1988. The error, to my knowledge, has **never** been conceded.

But there is more — **much** more — than this indiscretion to concern us. Aardsma, in defending his cherished weighted linear regression, seems to have created his own petard ...

In Aardsma's (rather contemptuous, I regret to say) reply to Bowden, we see the extent of the problem. The scenario is this: Bowden has presented three cases, in which synthetic data ($y=x^2$) is analysed, hoping to illustrate the ill-effects of weighting. Aardsma's reply is based on one crucial point — we should judge the 'line of best fit' **NOT** by how closely the regression line fits the data, but by a hallowed ratio: the slope relative to the estimate of the uncertainty of that slope. Aardsma, when pressed to concede that his 'fit a straight line at any price' methodology is inappropriate, seems to escape by changing the rules of the game. We have a new yardstick, which he demonstrates with a revised set of synthetic data and weights. Indeed, as stated his data is a better analogue to the speed of light data, in that the errors of his Case 4 are more like that of the capricious c data.

Aardsma's would-be death blow is this: the ratio of slope to uncertainty in his Case 4 is a massive 0.75×10^8! His case is closed, he has 'proved' that weighted linear regression really is okay to apply to non-linear data, as long as you use the right yardstick. I **suspect** that many readers were suitably impressed. I **know** that all who were, were totally mislead.

The problem is found in the definition of the uncertainty which Aardsma has, I suspect, taken from Bevington also. The yardstick Aardsma has chosen for this duel might just as well be made of bubble gum, for the equations in Bevington's text, which I have confirmed give Aardsma's results, are **completely independent of y**! That means we could choose **any** y-values, as long as we retain the same values for the x-ordinate and the same weight, we get the **same uncertainty**. A little experimentation on the computer bears this out graphically. For example, imagine that we take Aardsma's data, but change the sign of alternate y-values, so that our data looks like a rapidly oscillating function of ever increasing amplitude. The y-ordinates are the sequence -1, 4, -9, 16 ... — not a good candidate for linear regression. Yet Aardsma's method gives the same uncertainty, and — by virtue of the weights — almost the same slope to the 'line of best fit'. His hallowed ratio is fantastic, and yet this sort of analysis is nothing short of laughable.

If there are still some who think I have used sleight of hand in the above, then consider another example. Let the data be a perfect linear relationship, with slope chosen to equal the uncertainty. Using Aardsma's Case 4 data, that translates into the almost horizontal line with slope 9.85×10^{-9}. According to Aardsma, our weighted regression line, which of course fits exactly every data point, should be considered inferior to his linear fit to a quadratic, as our ratio of slope to uncertainty is mere unity, rather than the impressive 10^8.

So you see, Aardsma has sacrificed all reasonable interpretation of 'line of best fit' on an altar of bubble gum yardsticks. Aardsma's entire basis for defending his analytical methods is a straw man of stunning proportions. In defending his method of analysis, he has completely lost sight of the objective, which is to find an appropriate model that fits the data.

One final point needs to be emphasized regarding this unfortunate matter. Aardsma's efforts to rescue his work are utterly pointless. It is **NEVER** 'proper analysis' to insist on applying a linear fit to non-linear data, irrespective of whether you weight the data or not, and meanwhile totally ignore the results of non-linear methods. That Aardsma does so is in itself a damning indictment. To do so on the basis of his bogus measure (which I should point out was entirely his choice) serves only to demonstrate his ineptitude in statistical data analysis.

Aardma's case is, like Browns', fatally flawed. Does he know that? May I suggest that you take the time to review the letters published by Aardsma (and Brown *et al.)* over the last two years in the light of this paper. We, that is Setterfield and l, have been subjected to an extraordinary level of personal attack — claims of 'sloppy scholarship', refusing to admit to errors, etc., etc. If Aardsma, Brown *et al.* were so convinced of the scientific validity of their work, as opposed to ours, why has it been necessary to descend to personal vitriol, rather than silence us with straight answers to our objections?

A PLEA

It may seem that I am placing a disproportionate emphasis on this issue. I must disagree. Over the last four years, hundreds of hours of labour have been spent debating the validity or otherwise of the basic data analysis in our monograph.[27] What is more annoying is that the issues I have addressed in this article have been made

known to our critics, yet the same erroneous reviews continue to abound. One wonders how much more could have been achieved had some of our previous antagonists acknowledged the problems of their own analytical methods, which we have tried to point out graciously in the past.

To be fair, there has been some useful exchange in these last four years, but surely the issue of whether or not the data indicate that c has varied is of fundamental importance. I have not even begun to discuss the consequences of c decay, nor do I intend to. That is for the physicists and astronomers to ponder. Even after eleven years of involvement, I happily admit that the issues to come out of this research are far beyond my modest expertise in the physical sciences. I can however, speak with confidence concerning the basic data analysis. As for my credentials, I have majors in both mathematics and computer science. I have lectured for five years to final undergraduate year students on both the theoretical foundations and practice of statistics and data analysis, with particular emphasis on time series. I have held the position of manager for all computer systems in a university department of computer science for the last seven years. In short, I know what my strengths are — and what they are not.

The galling thing is that even though all of the blatant errors I have referred to above were politely pointed out to our more vocal critics **before** their articles were published, giving them time to make corrections, the errant papers were published essentially unchanged. Indeed, some still refuse to retract even the most glaring inaccuracies.

As you will by now have realised, I am basically saying 'Enough!' I have endeavoured to answer the many questions which have been raised *apropos* our analytical methods over the last few years. I believe it is now time for our critics to answer my challenge. It may well be impossible — indeed undesirable — to contain the controversy of the associated physical ramifications of the c decay hypothesis, but it is not so difficult to come to a conclusion regarding the analysis of the raw data, which underpins all else. That is my ambition. If I fail, then I can see little point in prolonged speculation over the more intangible issues. Until this statistical analysis of the data issue is resolved, none of us ought to proceed. I sincerely hope that my present discourse may go some way to putting the issue of the significance of the c data to rest.

Finally, I must beg pardon from Dr Evered. The above is not meant to be an attack upon his paper in particular — far from it. I have merely taken the occasion of reviewing his paper as a stimulus to clear up some long standing contentions.

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