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Letters to the Editor

THE VELOCITY OF LIGHT

Dear Editor,

As I read through Trevor Norman's article 'The Velocity of Light Decay Debate: The Mathematician's Response' in Creation Ex Nihilo Technical Journal, vol. 5(2), 1991, pp. 108-112, I was disappointed to find numerous unfounded charges, principally of a personal nature, being publicly aired, and at a time when, because of legitimate analytical and scientific concerns, Norman and Setterfield's version of the decay of *c* hypothesis is all but dead. In this letter I would like to respond publicly to a few of these charges. Norman's article addresses itself first to Evered, then to Brown, and finally to myself. I will restrict my comments to those sections of Norman's article directed at myself. This in no way implies endorsement of Norman's treatment of Evered or Brown, however; I am presuming these gentlemen will have opportunity to speak for themselves.

The following charges provide a simple summary of Norman's article with respect to myself.

(1) Aardsma has used the wrong mathematical 'model' to analyze the data.
(2) Aardsma has deliberately fabricated results and altered data.
(3) Aardsma has been reckless.
(4) Aardsma has been rude.

The last three charges are clearly not of scientific interest, being obviously only of a personal nature. The first claim could have been scientifically interesting, but even this possibility was spoiled by diatribe and misrepresentation. So it is difficult to see what Norman's article added of scientific interest or merit to the overall discussion of *c* decay. It contained no new data, no graphs, no tables, no new insights of a theoretical nature, and its only equation ($y=x^2$) was mentioned only parenthetically in reference to another's work. Even a casual perusal of Norman's paper alongside the very fine articles by Evered which appear earlier in the same issue reveals a striking contrast. This journal professes to be *'devoted, to the presentation and discussion of technical aspects of the sciences'* (see the inside front cover). Surely the readers of this journal deserve some sort of explanation from its editor of how such a scientifically and technically impoverished paper came to be published in its articles section.

This question aside, let me begin to deal with Norman's charges by clearly stating my categorical denial of each and every one of them.

I have not used the wrong 'model'. My analysis never even attempted to 'model' the data. In my analysis I was only ever interested in the question, *'Does this data set display a statistically significant decay trend?'* I was interested in this question because of Norman and Setterfield's strong claims in The Atomic Constants, Light and Time[1] that it did display such a trend. Norman's charge that I *'have completely lost sight of the objective, which is to find an appropriate model that fits the data'* is completely out of line. This is not the objective at this stage of the analysis, and never was. We must first answer the question, *'Does this data set display a statistically significant decay trend?'* before we can move on to the question which modelling seeks to address, *'What is the most probable mathematical form of the decay?'*

Norman and Setterfield's development in The Atomic Constants, Light and Time shows very clearly that they are aware of this. Note that Norman and Setterfield use a least squares linear fit on several occasions (e.g., *'When all 163 values involving 16 different methods are used, the linear fit to the data gives a decay of 38 Km/s per year'*,[2] and *'A least squares linear fit indicates a decay of 2.79 Km/s per year . . .')*.[3] They use the results of these linear regressions, not in an effort *'to find an appropriate model that fits the data'*, but to support their claim that the data exhibits a decay trend. For example, *'Thus 16 different methods of measurement by almost 50 different instruments all exhibit the decay trend'*.[4] Not until much later, near the end of their discussion,[5] after they have made the case as forcefully as they are able that a decay trend really does exist in the data, do we find them trying out different mathematical models on the data.

To refresh everyone's memories, so as not to be led astray, the historical flow of this small but important section of the debate is as follows: In The Atomic Constants, Light and Time Norman and Setterfield fit unweighted linear equations to their data and claimed that these clearly revealed a decay of *c* trend. I objected to the claim[6] on the grounds that an unweighted fit is not appropriate when the degree of certainty with which the various data points have been determined range over eight orders of magnitude. Bowden,[7] working from an artificially chosen, mathematically determined data set, proposed a counter example which he felt showed that a weighted fit obscured the true decay trend in this artificial data. I showed[8] that a weighted fit actually emphasized the decay trend in this artificial data set. Now Norman would have us believe that it is not decay trends but 'models' which have been the true objects of interest all along!

Norman has attempted to overturn my claim that it is necessary to use a weighted analysis when analyzing their data set for a trend by advancing another counter example. He applies a weighted linear regression to an artificially chosen, mathematically determined data set which he is confident does not display any long-term decay trend. He claims to have found that *'Aardsma's method'* yields a statistically significant slope in this case, thus indicating the presence of a trend where none exists. He concludes *'this sort of analysis is nothing short of laughable'*.

It isn't necessary to check Norman's math here, for whether or not his math is correct, his logic is wrong. Norman and Setterfield presented a data set consisting of 163 historic *c* measurements which they claimed showed a clear decay trend. I proposed that a weighted linear regression could be used to test this claim as follows: if the fitted slope was less than three times the estimated uncertainty in the slope away from a slope of zero then Norman and Setterfield's claim of a clear decay trend was not valid. I made no claims about the ability of this method to accurately confirm the absence of a trend in a data set (which is what Norman's counter example tests), since that is not what we are interested in here. We are only interested in whether we can confirm the presence of a trend in Norman and Setterfield's data set. To conclude that this method is of no value in testing this claimed presence of a trend because it fails to confirm a known absence of a trend in another data set is simply illogical.

I have not deliberately fabricated results or altered data in this or any other analysis. Such a charge is very grievous to me, for I have always put great emphasis on the imperative of being simply honest in scientific pursuits (as well, of course, as in all personal matters).

It is somewhat frustrating trying to deal with this charge, as Norman gives absolutely no specifics. Norman states that he has found *'anomalies' in my* results; that one result is *'clearly bogus';* that I have included a *'substitute value'* in my publication; that *'Aardsma's own papers contain numerical errors';* and that I have mischievously mixed *'two alternative forms'* from Bevington. I should very much like to get to the bottom of these claims — if I have inadvertently made a mistake I would like to know about it so I can correct it — but I don't know where to begin to look. What *'anomalies'*, which result is *'clearly bogus'*, which is the *'substitute value'*, what *'numerical errors'*, and which *'alternative forms'?*

Norman seems to hang the validity of these assertions upon what he believes to be an inevitable computer *'crash'* of my regression program if unaltered data is used. But my computer program never *'mysteriously failed'*, and I can see no reason why it should have. I find it somewhat incredible that Norman, a *'manager for all computer systems in a university department of computer science'*, would make such a sweeping claim. Surely, he must know that not all computers are identical. The fact that his computer program experienced an *'abnormal termination'*, does not mean that mine would also do so. The most probable cause of an *'abnormal termination'* in the present case would be a lack of significant digits and/ or insufficient floating point range. Normally, for example, FORTRAN works with just seven or eight digits in single precision floating point operations, and has a range of only about $\pm10^{38}$ on many machines. But I didn't do these calculations in FORTRAN. At the time I did this work the only computer hardware which the ICR Graduate School owned was a somewhat antiquated HP 9825 desktop computer which employed the HPL programming language. Now this computer had a number of limitations (it wasn't very fast, had no disk storage, and it couldn't handle very large computational problems), but it did have this advantage — it worked with 12 digits, and had a calculation range of about $\pm10^{512}$! So it is clear that this machine would hum along merrily after many other computers had exceeded their calculation range and terminated abnormally. Furthermore, there are programming measures one can take to avoid exceeding the computational range of a computer, such as rearranging the order in which specific parts of the calculation are performed. So there is no way Norman could know that *'abnormal termination'* of my regression program was *'guaranteed'*, unless he had used comparable hardware (i.e. an HP 9825) running comparable software (i.e. the regression program I wrote). To my knowledge, he had neither.

I would ask Norman to try checking my analysis on an HP 9825 to see whether he still encounters an *'abnormal termination'* or *'floatingpoint exception'*. If he does (and I don't think he will) then we might move to the next step of comparing our software and our input data. But it looks very much to me at this stage that Norman has levelled charges of corrupt and inappropriate behavior at me based upon nothing more than false assumptions.

The charge of *'outright recklessness'* in the handling of the *c* data is also not warranted. My study of Norman and Setterfield's The Atomic Constants, Light and Time was not undertaken at my own initiative, but at the request of ICR President, Dr Henry Morris. I worked full-time on The Atomic Constants, Light and Time for about a month at ICR, beginning in early October of 1987, before I began to write anything. I entered this study positively disposed toward the idea of *c* decay, and emerged negatively disposed. I wrote up the results of my investigation and submitted them to the ICR faculty as well as peers external to ICR for their evaluation and comments, which took a further few months. The final analysis only became public subsequent to results of this self-imposed peer review, about one-half year later, through the May 1988 issue of the ICR Impact series, and the June 1988 issue of the Creation Research Society Quarterly. I have tried to proceed rationally, methodically, and honestly throughout this affair.

Finally, I have been charged with rudeness. More specifically, my failure to concede an alleged error which Norman claims to have pointed out to me in 1988 is called an *'indiscretion'*, my published reply to Bowden is characterized by Norman as *'rather contemptuous'*, and Norman feels he and Setterfield *'have been subjected to an extraordinary level of personal attack'*. I am sincerely sorry for any personal injury I may have caused either Norman or Setterfield in this affair. I did not intend to attack them personally, seem contemptuous, or be indiscrete. It was only their claims, as presented in The Atomic Constants, Light and Time which I meant to attack; and even this was done as a professional and moral responsibility, not for personal pleasure, by any means.

I have no recollection of Norman ever pointing out the supposed error over alternate forms in Bevington prior to reading about it in the last issue of the Creation Ex Nihilo Technical Journal. I can remember no case of any personal communication with Norman at any time in the past, nor have I been able to locate any record of any such communication in my files. I do have a photocopy of an unpublished article by Norman and Setterfield responding to an early draft of my 'Has the Speed of Light Decayed?', dated January 1988, but to my knowledge there was no personal correspondence from Norman or Setterfield accompanying this article. I have looked, once again, in this article to try to shed some light on Norman's claim that he pointed out the alleged misuse of Bevington's mixed forms, but without success. I responded to this article in a personal letter to Setterfield dated March 2, 1988, which I assumed he shared with Norman. I certainly intended no slight or indiscretion.

I am sorry if my published response to Bowden seemed *'rather contemptuous'*. I certainly did not intend it to seem that way. I had opportunity to correspond privately with Bowden over the *c* decay issue before either of us published in the Creation Research Society Quarterly. I felt this correspondence was mutually polite and respectful, and Bowden never gave any indication to the contrary. In fact, he remarked toward the end of our correspondence that we should be able to be frank and *'speak the truth in love'*, which I heartily agree with. No other correspondent has ever suggested that my response to Bowden seemed contemptuous.

Finally, I am surprised that Norman feels he and Setterfield *'have been subjected to an extraordinary level of personal attack'*. Certainly the public record doesn't bear this out. It scarcely needs mentioning, of course, that one can hardly expect to propose any theory with as far reaching implications as that of the decay of *c* without engendering some observations as to the quality of one's scholarship, degree of fair-mindedness and clear-sightedness, credentials, etc. But the comments of this sort relative to Norman and Setterfield that I have seen hardly constitute an *'extraordinary level of personal attack'* or *'personal vitriol'*. I feel, in fact, that Norman and Setterfield have personally been treated extremely well by the creationist community. Recall that Setterfield was invited to be the keynote speaker at the 1986 International Conference on Creationism in Pittsburgh. And men such as Bowden in England, Dolphin (and others) in the U.S., Montgomery in Canada, and Wieland in Australia seem to me to have gone to extraordinary measures to assist Norman and Setterfield in every way possible. I am not aware of any other creationist, making novel scientific claims, who has received such sustained, international support and assistance. Finally, The Atomic Constants, Light and Time has been carefully and thoroughly worked through by practically every qualified, active recent-creation scientist that I can think of. They have in many cases taken of their own time to research Norman and Setterfield's claims, and to write up their findings and see them through to publication for the benefit of others. These scientists have in no way launched a campaign of *'personal vitriol'* against Norman and Setterfield. (What ever would be their motive for doing so?) They have simply assessed the claims of Norman and Setterfield presented in The Atomic Constants, Light and Time in diverse ways, in keeping with their respective scientific backgrounds, . . . and they have found these claims wanting.

Gerald E. Aardsma,
Institute for Creation Research,
San Diego,
California, USA.

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5. Norman and Setterfield, Ref. 1, section VI D, starting on page 51.
6. Aardsma, Gerald, 1988. Has the speed of light decayed recently? — Paper 1. Creation Research Society Quarterly, 25(1):36–40.
7. Bowden, Malcolm, 1989. The speed of light—a critique of Aardsma's statistical method. Creation Research Society Quarterly, 25(4):207–208.
8. Aardsma, Gerald, 1989. Response to Bowden. Creation Research Society Quarterly, 25(4):208–209.

Dear Editor,

With regard to my paper 'On the Compatibility of Special Relativity with a Decreasing Velocity of Light',[1] Maurie Evered in a 'Letter to the Editor'[2] has asked where I obtained certain information. I am grateful for the opportunity to give the answer. My paper was so worded as to imply that Einstein in 1905 was aware of the famous null result of the Michelson-Morley experiment to detect any motion of the earth relative to the ether. Maurie Evered, on the strength of a quotation from Calder,[3] found this to be an improbable suggestion. However, if only Maurie Evered had read a few lines further on in Calder's book, he would have read the conclusion that Einstein *'must have been aware of the new climate of opinion'*.

Banesh Hoffmann[4] confirms Calder's conclusion. He admits Einstein made no specific mention of the Michelson-Morley experiment, but then quotes Einstein himself in his 1905 paper as talking about *'the unsuccessful attempts to discover any motion of the earth relative to the* (ether)'.

In any case, the point made in my paper stands, namely, that in reviewing the relationship of these events in historical perspective, it's certainly clear that Einstein's work in effect dispensed with the ether assumption in answer to these experimental results, even if they were not his main inspiration, nor explaining them his chief purpose.

I'd be more interested to receive competent comment on the main thesis of my paper. Perhaps I could recap by saying this presented us with a choice:—

either to follow the popular view of Einstein's work as showing that the nature of Time is such that simultaneity is relative (akin to the mutual effects of perspective) as taught by special relativity,

or to insist that the behaviour of clocks is always consistent with common sense, and that this gives the lie to the theory of special relativity as a physical solution, but shows the mathematics of relativity (following Lorentz) to be compatible with Setterfield's *c* decay hypothesis.

Brian D. (not F.) Johnston,
Leigh,
Lancashire,
ENGLAND.

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THE EARLY HISTORY OF MAN

Dear Editor,

Mr Cooper's article 'The Early History of Man — Part 3. The Kings of the Ancient Britons: A Chronology'[1] has come to my notice, and I very much appreciate the theme and conclusion of the same.

However, the definiteness of his assertions in respect of Bible chronology took me by surprise. Examples are:—

(1) *'We know that Eli judged Israel between the years 1115–1075 bc'*
(2) *'We know that Samuel judged Israel for the forty year period between 1075–1035 bc'*
(3) *'Saul was king in Israel between 1030–1010 BC'*
(4) *'Again, we know that David ruled from 1010–970 BC'*
(5) *'. . . Solomon who ruled between the years 970-930 BC'*
(6) *'Ahab* was *king of Israel between 874–853 BC'*
(7) *'Isaiah was active between 740–701 BC'*

Items (4), (5) and (6) indicate that he is quoting Thiele's chronology,[2] which has been demonstrated by Aaronson[3] to be contrary to Scripture on nine counts as follows:

(1) The identity of Pul
(2) Malchut years
(3) Jubilee years
(4) The date of the accession of Hezekiah
(5) Regnal synchronisms between Judah and Israel
(6) The 'third' kingdom of Ephraim
(7) The date of the accession of Jehoram of Judah
(8) The false Tishrei reckoning for Judah
(9) The unnecessary complication of Judah and Israel each using their own system for the chronology of the other's regnal years.

I am intrigued to know the reasons why he assigns a 20 year reign to Saul and why 40 years to the judgeship of Samuel, and on what basis is Isaiah's ministry dated 740-701 BC.

C. L. Prasher,
Brighton,
ENGLAND.

The Author Replies . . .

I am very grateful for the points that Mr Prasher raises, and I note with alarm that in gathering information for the synchronisms that made the construction of the early British chronology possible, I may have inadvertently relied upon a flawed chronology of the biblical kings. I am not in the least familiar with the work of Thiele[4] or its apparent and particular dangers, although I do note that my source[5] for the biblical dates does indeed rely on him. Well done for noticing! (I have to assume, however, lacking entirely the leisure to explore the matter deeply, that your own source is itself without problems, and that Aaronson did not commit a thousand sins of his own in refuting Thiele. I doubt that even Aaronson would claim that his is the very last word on the subject.)

In mitigation, if any is needed, I would plead that my sole interest was in constructing a reasonably accurate chronology for the early British kings (something that no previous scholar has succeeded in doing these four hundred years past, or even bothered to do of late), rather than in raising dust over the somewhat controversial subject of fixing the exact dates for the kings of Israel and Judah, and the points that you raise, whether right or wrong, do not significantly affect the British chronology. Time dictates that I must leave to others already engaged in the field of