MINISYMPOSIUM ON OF SPEED LIGHT—PART I

HAS THE SPEED OF LIGHT DECAYED RECENTLY? — PAPER 1

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Received 25 November 1987 Revised 26 January 1988

Abstract

The hypothesis that the speed of light has decayed, presented by Trevor Norman and Barry Setterfield in The Atomic Constants, Light, and Time is shown to be unsupported by an objective analysis of the actual historic measurements of the speed of light given in that report. The implications of the hypothesis for radioactivity and radiocarbon dating are shown to be unacceptable.

Introduction

This critique is the result of an intensive investigation into *The Atomic Constants, Light, and Time,* by Trevor Norman and Barry Setterfield (1987). In that report, the authors advance the hypothesis that the speed of light, c, has been decreasing in the past. This critique presents my reasons for rejecting the above hypothesis. Though the analysis provided here is not exhaustive, I believe it is sufficient to show that general support by the creationist community, of the decay of the speed of light hypothesis, is not warranted by the data upon which the hypothesis rests.

The supplement to the technical report, *Geological Time and Scriptural Chronology,* by Barry Setterfield is not critiqued. However, since this supplement is based on inferences from the technical report, rejection of the decay of the speed of light hypothesis leaves the supplement without a logical basis.

A superficial reading of the technical report and supplement is sufficient to produce an uneasy feeling about the decay of c hypothesis advocated by Norman and Setterfield for two reasons. First, creationists have historically argued that the fundamental physical constants show a remarkable degree of fine tuning and that such precision is clear evidence for special creation. For example, the hydrogen atom could not exist if the mass of the proton was just 0.2% greater (DeYoung, 1985). This historic position stands in sharp contrast to the hypothesis advanced by Norman and Setterfield, which claims that the speed of light has decreased by roughly a factor of 10 million since the creation of the universe.

Secondly, the proposed decay of c shows a very improbable behavior. According to Norman and Setterfield's theory, the rate of decay of c was greater than 1 km/s/year for the entire span of time from creation until about 1920 A.D. Experimental precision has been such that a decay rate of 1 km/s/year would have been easily and unquestionably verified from about 1950 A.D. onward. It seems a remarkable coincidence that direct experimental corroboration of the theory, open for thousands of years as a theoretical possibility, should have eluded us by just 30 years.

This coincidence, coupled with a knowledge of the normal behavior of physical measurements, strongly recommends serious consideration of the alternate hypothesis that c has been constant.

Norman and Setterfield's Data Analysis

The text, *Data Reduction and Error Analysis for the Physical Sciences,* by Philip R. Bevington begins:

> It is a well-established rule of scientific investigation that the first time an experiment is performed the results bear all too little resemblance to the "truth" being sought. As the experiment is repeated, with successive refinements of technique and method, the results gradually and asymptotically approach what we may accept with some confidence to be a reliable description of events. (1969, p. 1)

Thus, any gradual and asymptotic approach of c to its present-day value, which Norman and Setterfield find in the data, needs to be carefully scrutinized to determine if the effect is due to real, physical changes in the structure of the universe, or if it is merely the result of the normal behavior of physical measurement.

It is also well known that a given body of data can be easily, inadvertently manipulated, due to subjective bias and statistical analysis misapplied in such a way as to yield unwarranted conclusions. The best way to avoid this problem in the current context is to treat the entire data set (consisting of 163 values for the speed of light derived from 16 different experimental methods spanning the past 300 years; tabulated in nine groups in *The Atomic Constants, Light, and Time)* as a whole. This minimizes the number of subjective decisions which must be made and enhances the possibility of discerning any real overall trend in the value of c, since it is very unlikely that 16 different experimental methods would all accidentally and independently conform to the same mathematical equation describing c decay, if c was constant.

Unfortunately, the authors of the report spend most of the time, which is devoted to c data considerations, discussing and analyzing the data in separate, small groups for any c decay trend within the group. They do, in one place, however, consider the whole body of data collectively. In this one instance, they use the standard, non-weighted least squares technique to fit the best straight line to the data, and conclude: "When all 163 values involving 16 different experimental methods are used, the linear fit to the data gives a decay of 38 km/s per year." (p. 25)

Actually, giving the computed decay rate without an estimate of the precision to which that rate has been determined is not very helpful. A decay rate of 38 km/s/year with an uncertainty of ± 100 km/s/year would not support the theory of c decay very well, whereas a decay rate of 38 km/s/year with an uncertainty of ± 1 km/s/year would seem quite convincing.

The least squares technique provides an objective estimate of the uncertainty in the decay rate determination. However, this number does not appear to be

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included in the report. I have repeated the calculation using the 163 data points and obtained the result, 38 ± 8 km / s / year.

If this was the end of the matter it would provide powerful evidence in favor of the c decay hypothesis. Unfortunately, even a cursory glance at the data reveals that the above analysis is inappropriate for the given data set hence, Norman and Setterfield's conclusions are not valid.

The Data Reanalyzed

The Atomic Constants, Light, and Time tabulates 163 values for the speed of light derived from measurements spanning the past 300 years. A graph of this entire data set does not appear in the report, but is shown in Figure 1. This figure displays the percent difference between the measured value of c and the modern value for the speed of light given in the report of 299,792.458 km / s. The vertical lines on some of the data points are error bars which express the range of uncertainty in the measurement which was reported by the researcher. These are normally reported in such a way that there is roughly a 68% chance that the error bars encompass the true value, assuming there are no undetected systematic errors in the measurement.

The range of uncertainty was not reported for many of the earliest measurements, so some of the data points are plotted without error bars. Most of the data points after 1850 do have error bars but they are too small, in most cases, to be seen on the scale of the graph. The relatively few data points between 1850 and 1900 which have very large bars result from two indirect methods of measuring c which yield low-precision results.

In a non-weighted least squares fit, every data point has equal weight in determining where the best fit straight line should be drawn through the data. For a data set consisting of measurements having error bars of varying lengths, it is not appropriate to give every data point equal weight. It is standard practice to weight the data points in inverse proportion to the size of their error bars. That is, data points with large error bars (greater uncertainty) have less impact on where the best fit straight line would be drawn than do data points with small error bars. This is especially important for the current data set since the reported error bars range from ± 20,000 km / s to ± 0.0003 km / s, spanning roughly eight orders of magnitude.

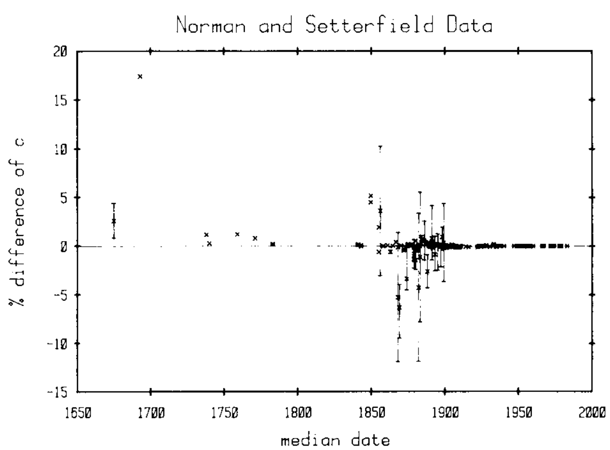

Figure 1. The 163 data points used by Norman and Setterfield shown as percent deviation of c from the modern value (i.e. δc = ((c - 299,792.458)/299,792.458) x 100) versus the median date of observation.

Before proceeding to the proper weighted computation of the straight line which best fits the data, it is necessary that all data points be supplied with error bars since the original researchers have not always supplied an error estimate. This can be done in a fairly accurate and objective fashion by calculating the standard deviation of a group of neighboring data points from the given data set which were obtained using the same experimental method of measurement. The absolute lengths of the error bars do not need to be known very accurately. Even if the estimates of the standard deviation are off from the true error bar length by a factor of two or three, the resultant weighted fit will be a vastly superior representation of the true nature of the data than was the unweighted fit which treated all data points equally, even though they differed in precision by orders of magnitude.

Only 28 of the 163 data points tabulated were given without error estimates. Of these, the first one needs to be dealt with separately, in detail, before the others are discussed briefly. This is the data point in the upper left hand corner of Figure 1. It is attributed to uncorrected observations of the Roemer type, by Cassini, in 1693. To obtain the speed of light by this method, the Earth's orbital radius is divided by the measured time of transit of that radius by light (about 8 minutes, 20 seconds, today). The following quote from Norman and Setterfield is illuminating:

> Observations by Cassini (1693 and 1736) gave the orbit radius delay as 7 minutes 5 seconds. Roemer in 1675 gave it as 11 minutes from selected observations. Halley in 1694 noted that Roemer's 1675 figure for the time delay was too large while Cassini's was too small. (p. 11).

Since Roemer and Cassini's results used the same method, separated by only 18 years, it is appropriate to use these two time observations to calculate the sample standard deviation. The result is an uncertainty of ± 166 s in the two measurements. This yields error bars for Cassini's determination of c of ± 138,000 km / s (or about ± 39%).

This is much larger than the error bars shown on Roemer's c determination shown at 1675, in Figure 1. The reason for this difference is that Norman and Setterfield have chosen to use a reworked or "corrected" value for Roemer's c determination and an uncorrected value for Cassini's. It is peculiar that Norman and Setterfield were content to use an uncorrected value for Cassini considering the comments by the eminent and talented Halley. It is also unfortunate*, since this single, anomalous point is responsible for most of the apparent 38 km / s / year decay, which they report.

*I say "unfortunate" because Norman and Setterfield have at this point, been misled into believing that the historical data strongly supports their hypothesis (i.e. shows a decay of 38 ± 8 km / s / year) mainly because of this one data point. Had they removed this single data point and analyzed the remaining 162 data points in the same way, they would have found an apparent c decay rate of only 11 ± 6 km / s / year. This result would have suggested that support for the hypothesis from the historical data is marginal, at best. (Of course, even this conclusion would not have been valid, since an unweighted fit is not appropriate for this data set.)

The 1740 determination attributed to Bradley was also a special case. For this data point, it was clearly most appropriate to calculate the standard deviation of the data from which it was obtained as an average (i.e., Table 2 of Norman and Setterfield less Busch's re-workings).

The remaining error bars were determined by calculating the standard deviation of a neighboring group of data points obtained using the same method of c measurement. These were calculated from the given values of c directly. This involved the use of three to eight neighboring data points, always spanning a time interval of less than 20 years.

When the entire data set of 163 points, complete with error bars was analyzed, using the standard, weighted, linear least squares method, the decay of c was determined to be:

$$\text{decay of c} = 0.0000140 \pm 0.0000596 \text{ km / s / year.}$$

This result shows plainly that there is no discernable decay trend in the data set presented by Norman and Setterfield.

Norman and Setterfield call into question the validity of the post 1972 data points in the technical report, as follows:

> It is significant that the eight laser values from 1972 to 1983 were using the atomic time and frequency standards. It is therefore inevitable that the constancy of c in the atomic time frame will be reflected in the measurements. It is for this reason that no statistically significant trend was revealed in that period. (p. 45)

I have analyzed the data set exclusive of these suspect data points, since they could possibly obscure an earlier decay trend in the c data by their constancy and very high precision. Using the same weighted linear least squares technique, the result is:

$$\text{decay of c} = 0.0101 \pm 0.0068 \text{ km / s / year.}$$

Thus, there is still no discernible, statistically significant decay trend.

The conclusion is inescapable, that the c data upon which the c decay hypothesis rests, when analyzed objectively and in an appropriate fashion, does not support the hypothesis. This should be sufficient to cause the c decay hypothesis to be dropped from the repertoire of creationist answers to difficult questions. However, those who would like to preserve the hypothesis of c decay will probably point out that I have not shown the hypothesis to be false, but merely that it has no basis of support in the actual c measurements. Is it possible that the decay of c hypothesis advanced by Norman and Setterfield may still be right?

In my opinion, a look at the implications of the theory for radiocarbon dating alone is sufficient to eliminate this possibility.

The Implications

Much of the impetus behind the decay of c hypothesis stems from its implications which are generally viewed as favorable to the young earth creationist position. In particular, the knotty problem of starlight from distant galaxies seems to be solved in a straightforward, naturalistic manner. The universe, however, is a very intricate machine and it is not possible to modify the speed of light without a host of ensuing changes in other areas. Indeed, the implications of the theory have been extended far beyond the starlight problem to bring about an apparent coup for creationists in all areas. In the words of one creationist writer:

> Can one simple scientific assumption radically alter the dimensions of the age of the universe, eradicate the 'Big Bang,' sink the Nebular hypothesis, undermine geological Uniformitarianism, and destroy Darwinism all at the same time? (Montgomery, 1987)

One of the implications of the decay of c hypothesis advocated by Norman and Setterfield is that of its effect on radioactive decay. According to Norman and Setterfield, the decay of c is just a symptom of a more fundamental change which has been occurring in the basic structure of the universe since creation. This change is that time as measured by atomic clocks has been different from time as measured astronomically (called dynamical time). Norman and Setterfield state it this way:

> As a consequence, it would seem that the further back in the past we go, the more quickly the atomic clock ticked. It therefore registers a systematically old date when compared with the dynamical standard.
>
> All forms of dating by the atomic clock are subject to this effect. This includes radiometric dating whether it be the uranium / lead, thorium / lead, lead / lead, rubidium / strontium, potassium / argon, carbon 14 or any other. (pp. 82, 83)

The overall effect is that 18 billion years of atomic time can be rescaled to fit within less than 10 thousand dynamical years. Apparent ages of billions of years, as deduced from radiometric decay dating techniques, are thus posited to be due to the more rapid rate of decay of atoms in the past.

It is apparent that a past, accelerated radiometric decay rate presents serious problems of excess energy release via radioactive decay. For example, a solid outer crust for the Earth would not be possible and no life would be able to survive the intense radiation fields resulting from the postulated increase in decay rate. The authors have anticipated this problem. They suggest that though the number of gamma ray photons resulting from radioactive decay would have been 10 times greater when c was 10 times larger and though each photon would have had the same energy as a photon from such a decay today, still: ". . . it requires 10 radioactive atoms to decay in unit time back then to give the same total flux, S, or intensity, equal to that now." (p. 56)

The argument advanced by Norman and Setterfield to support this conclusion seems to me to be unnecessarily abstruse. Simply stated, all of the energy which is released from the radioactive decay of an atom must eventually show up somewhere if energy is to be conserved. For the decay of uranium in the earth, for example, the energy released by the decay of a single atom is ultimately all converted to heat energy in the Earth. If the decay rate was 10 times higher, the heat production in the Earth would also be 10 times higher. Similarly the energy deposited in living tissues by 10 times higher radiation fields would be 10 times higher. These conclusions are only dependent on the conservation of energy; the details of energy transport are irrelevant.

This is an enormous problem for the decay of c hypothesis, when it is recognized that the particular behavior of c which is advocated by Norman and Setterfield does not imply a rate of decay merely 10 times higher in the past, but over a million times higher for all of Earth history prior to about 3500 B.C. It is very difficult, if not impossible, to imagine how such a huge increase in the energy released by radioactive decay would be tolerable to life.

Radiocarbon

The implications for radiocarbon dating have not been anticipated in the report, but are no less serious than the problems pointed out above. According to the report the following mathematical equation for the speed of light at any dynamical time, T, can be used:

$$c = a + b \times T^2 + d \times T^8 \tag{1}$$

where a = 299792 km / s, b = 0.01866 km / s year^2, d = 3.8 x 10^{-19} km / s year^8 and T = (1961- t) year where t is the year A.D. Norman and Setterfield have no qualms about applying this equation to periods of time well outside the several-hundred-year span of the data from which it was derived, extrapolating it back to the origin of the universe. The extrapolation of this equation is much less severe and, hence, much more justifiable in the case of radiocarbon since we are dealing with relatively recent atomic times.

Equation 1 can be used to produce a formula for calculating atomic time, τ, given the dynamical time, T, and vice versa, as follows:

$$\tau = \int_{T=0}^{T} \frac{c(T)}{c_0} dT, \qquad c_0 = c(T = 0) = a \tag{2}$$

thus,

$$\tau = T + \frac{b}{3a} T^3 + \frac{d}{9a} T^9 \tag{3}$$

(This equation is not given in the report but checks with the rescaled dates given by Setterfield in the supplement.)

Most creationists would agree that the results of radiocarbon dating are valid, at least back to the time of Christ. The Dead Sea Scrolls, for example, can be dated paleographically, and by inference from excavations at Khirbet Qumran to between 135 B.C. and 68 A.D.; cloth associated with the scrolls was radiocarbon dated to 20 B.C. ± 200 years (Yamauchi, 1975). According to Norman and Setterfield's theory, however, radiocarbon is not valid in this period since the decay constant was changing as c decreased. As a result of their theory, the above agreement between the different methods of dating the Dead Sea Scrolls must be regarded as fortuitous; the radiocarbon date, 20 B.C. (i.e., $\tau \simeq 2000$ years), corresponds to a "true" dynamical date for the Dead Sea Scrolls of about 800 A.D. when rescaled according to equation 3.

The difficulties intensify further backwards in time. Consider the deck board obtained from the Egyptian funerary ship of Sesostris III. This specimen is dated archaeologically to between 1845 and 1831 B.C. and yields an uncorrected radiocarbon date only a few percent younger than this (Save-Soderbergh and Olsson, 1970). If we accept the radiocarbon date as approximately correct, (i.e. $\tau \simeq 3700$ years) then the "true" archaeological age, according to Norman and Setterfield's theory is about 1370 years old, or 600 A.D. If, however, radiocarbon has somehow been disturbed so that it cannot be used for even approximate dating back to 1845 B.C. and the archaeological date is accepted as approximately correct (i.e. T $\simeq$3800 years), then we have an even greater problem. A dynamic age of 3800 years yields an atomic age of about 23 million years. This corresponds to over 4000 half-lives for radiocarbon decay. Thus, there must have been greater than 2^{4000} (or 10^{1204}) times more radiocarbon in this deck board when its tissues were living than is measured in it today. This condition cannot be met even if it is assumed that every atom in the whole board was originally radiocarbon!

Conclusion

The hypothesis of a decaying speed of light developed by Norman and Setterfield in *The Atomic Constants, Light, and Time* is not supported by the experimental measurements of the speed of light for the past 300 years presented in that report. Furthermore, the implications of the theory are unacceptable for radioactive decay, in general, and radiocarbon dating, in particular. In the absence of positive, historical, experimental evidence for the decay of c, and in the face of the unacceptable implications of the theory, the hypothesis must be rejected.

N. Ernest Dorsey, writing on this same topic of c decay in 1944, noted that trends in the measurements prior to his writing were explained by many:

> . . . as probably arising in large part from two all but universally acting causes: (1) the observer's exaggerated opinion of the accuracy of his own work, and (2) his inability to avoid being influenced in some measure by his preconceived opinion as to what he should find. (p. 2)

In my opinion, the problem of subjective bias in scientific endeavor is likely responsible for the current widespread scientific consensus in favor of macroevolution, despite the lack of support for that hypothesis in the actual data. But it will hardly do to point out slivers of this nature in the anti-creationists' eyes, if we are content to have logs in our own eyes.

Acknowledgements

The author thanks Lambert T. Dolphin for aid in clarifying some aspects of Norman and Setterfield's technical report. The assistance of Institute for Creation Research graduate students Dave Rush (who obtained a host of library materials used in the production of this paper) and Max Chappell (who offered a helpful computation suggestion) is greatly appreciated. Mr. David Unfred provided a valuable, independent appraisal of the statistical analysis given in *The Atomic Constants, Light, and Time.*

References

Bevington, Philip R. 1969 Data reduction and error analysis for the physical sciences. McGraw-Hill. New York.

DeYoung, Donald B. 1985. Design in nature: the anthropic principle. *Impact* (no. 149). Institute for Creation Research.

Dorsey, N. Ernest. 1944. The velocity of light. *Transactions of the American Philosophical Society*. 34 (Part 1):1-110.
Montgomery, Alan. 1987. New light on an old problem. *Feature* (no. 4). Creation Science Association of Ontario.
Norman, Trevor and Barry Setterfield. 1987. The atomic constants, light, and time. SRI International Invited Research Report. Menlo Park, CA.
Save-Soderbergh, T. and I. U. Olsson. 1970. C14 dating and Egyptian chronology in Olsson, I.U. (editor) Radiocarbon variations and absolute chronology (Nobel Symposium 12). John Wiley & Sons. New York.
Yamauchi, Edwin M. 1975. Dead Sea Scrolls. Wycliffe Bible Encyclopedia (volume 1). Moody Press. Chicago. pp. 434-42.

HAS THE SPEED OF LIGHT DECAYED RECENTLY? — PAPER 2

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Received 30 November 1987 Revised 16 December 1987

Abstract

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

Introduction

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

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

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

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A Disturbing Quote

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

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

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

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

planets in their orbits constant. This construction of his mind makes a clock which ticks independently of changes in c, in contrast to electromagnetic clocks. Nowhere in Setterfield's letter or book does he say explicitly how all this applies to my example, which has nothing to do with gravitation. His foray into new realms of field theory is accomplished with stunning brevity upon a single page, 44. To the nonspecialist who does not speak the language of physics, all this may appear impressive. But speakers of the language can recognize that without clear and precise explanations of all the new terms, the theory is unintelligible: "So also you, unless you utter by the tongue speech that is clear, how will it be known what is spoken? For you will be speaking into the air" [l Corinthians 14:9].

General Remarks

There is much work behind this book; it took much time and effort to dig up the many details which it contains. Yet for all that, it seems incomplete and unpolished, as if done in haste. The list of 377 references is very redundant, with many entries repeated three or more times. For example, references 12, 26, and 96 are identical without so much as an *op. cit.* There are many other such redundancies, which tend to give the reader a mistaken impression that nearly 400 books and articles back up the monograph, instead of much fewer. The statistical analysis is very simple and does not appear to take advantage of the more advanced techniques now available. The exposition does not guide the reader as clearly through the wilderness of details as, for example, Dorsey's treatise does [Dorsey, 1945]. The scarcity of figures (four, as compared to Dorsey's twelve) obscures things. In particular, there is no graph such as my Figure 1, showing *all* the data from 1668 to now; the absence makes it difficult for the reader to form his own opinion about the alleged decay.

Conclusion

Statistical analysis is a tricky art; it is easy to fool oneself and others with it. Consequently a study of this sort needs to apply the very best analytical techniques. In my opinion, the minimum acceptable analysis must do the following: (1) Use the correct Roemer point, (2) Correct or exclude the Cassini point, (3) Give each point a weight proportional to its estimated accuracy and (4) Treat all the data from the different experimental methods together with as many points as possible. Norman and Setterfield have not done these things in their published work. When the minimum standards are applied, it appears that no statistically significant decay in the speed of light can be found.

The authors' practice of separating data into various groups seems to have made their analysis susceptible to distortion from systematic errors. One likely candidate for such error is the psychological bias each experimenter has when selecting data, as Dorsey pointed out. Such a bias would explain both the seeming downward trend within each group and the steady flattening out of the data curve as experimental accuracy improves. Norman and Setterfield offered no alternative explanation for the flattening trend, as far as I can tell.

The theory advanced by the authors starts with the *assumption* of a real decay in the speed of light, and the theory contains several more assumptions which are open to question. The lack of clear definitions of new terms on page 44 makes that part of the theory unintelligible.

The problem with this book is that it is misleading. If one reads it naively, assuming that the authors are presenting their case fairly and objectively, one gets the impression that the speed of light has decayed. However, if one examines it critically, carefully checking out the assumptions and calculations, one arrives at the opposite conclusion. This difference between the book's surface appearance and its deeper nature is already producing controversy among creationists. If it is any help to the reader, let him remember that I have been on both sides of the controversy. Also the reader should know that I actually favor the possibility that c has decayed sometime in the past, so that if anything I should welcome the book. But the underlying facts just do not support the hypothesis. The speed of light may well have decreased either during Creation week, after the Fall, or during the Flood. But after critically examining this book, I can find no statistically significant evidence that c has decayed during the last three centuries.

References

Dorsey, N. E. 1945. The velocity of light. *Transactions of the American Philosophical Society.* 34:1-110.

QUOTE

The naturalists did nothing more than take the old theological concept of providence and degrade it beyond recognition and meaning. They took "predestination" and stripped it to "destination." They had to believe that we were destined to go where we were going because we were parts of that vast system of interlocking causalities and had behind us the irresistible pressure of all previous forces. But they reduced such facts to unintelligibility by rejecting the concepts of Aristotle's first and final causes. It should not be overlooked that the components of the term *providence* signify "prevision" or "foresight." The word thus contains the idea of seeing, of knowing, or recognizing a conscious purpose. But the determinism of the naturalists in literature, in philosophy, and in science is a blind process. The process does not know where it is going; neither does anything outside the process. A good example of this confusion is the statement by a representative modern biologist that the evidence in nature compels us to believe that nature has a purpose. But this is not a conscious purpose, he adds, nor can we ourselves define it, since any final end or purpose is inconceivable. His determination to keep out all religious or metaphysical ideas forces him to talk about purpose which is not purpose (since nothing is aware of it) and of a goal which is not a goal (since it infinitely recedes). The theologians who used to debate "fixed fate, free will, foreknowledge absolute" never got into muddles like this. This encourages the hope that from evolutionism and other forms of naturalism some good metaphysician may yet deliver us.

Curtis, III, George M. and James J. Thompson, Jr. (editors). 1987. Contemporary southern literature in *The Southern Essays of Richard M. Weaver.* Liberty Press. Indianapolis. p. 55-6.