presentation is absolutely correct. I completely agree with you. If I can help you in any way I will."

To conclude it may be of interest to point out one feature of c decay that comes out of general relativity equations. General relativity still holds if (a) the speed of light is independent of the motion of the source and (b) if the value of the speed of light is the same throughout the universe at any given instant. The second condition implies that there is such a thing as absolute time as far as the Universe is concerned, and yet it is needed for relativity! There are three basic quantities in physics-mass, length and time. Planck obtained these quantities in terms of other parameters. Thus we have

$$\text{Planck Mass } M = \sqrt{(h\,c/\,[2\pi G]\,)} \qquad (11)$$

$$\text{Planck Length } L = \sqrt{(h\,G/\,[2\pi c^3]\,)} \qquad (12)$$

$$\text{Planck Time } T = \sqrt{(h\,G/\,[2\pi c^5]\,)} \qquad (13)$$

These quantities are related to c in the following fashion since Planck's constant h is proportional to 1/c, and Newton's gravitational constant is proportional to c^4

$$M \sim 1/c^2 \qquad (14)$$

$$L = \text{Constant} \qquad (15)$$

$$T \sim 1/c \qquad (16)$$

Equation (14) comes as no surprise as the inverse square dependence on c for mass is already formulated in Part 2 (Ex Nihilo, vol. 4, no. 3, 1981, pp 55-81). Equation (15) is a highly desirable result and prevents any 'weird' effects from c decay. But the effect of (16) is startling until it is realised that this is atomic time, time as seen by the atom, not time as seen from our frame of reference (universal). For an observer outside the atom the atom's time intervals are shorter as the speed of light is higher. For us the time intervals are constant. This latter statement is verified by the equations of relativity. In the Swarzschild equations, for example, the time interval t^2 for the universe is always expressed as c^2T^2, where T is the atomic time mentioned above. Accordingly, we may express the relationship between universal time t and atomic time T as

$$t = cT = \text{const};\ t^2 = c^2T^3 = \text{CONSTANT} \qquad (17)$$

In each case the last step followed directly from relation (16). Thus while the atom is affected by c decay, our time is constant.

The result of (17) explains in a very logical way why the atom behaves as it does with c decay. From the atom's point of view it is behaving in an entirely consistent way throughout. As far as the atom is concerned, an electron has always taken the same time interval ΔT to travel once around its orbit. Seen from the outside this time interval has been lengthening and we say that the electron is slowing down, but the atom is oblivious to that and goes on its merry way. As far as the atom is concerned for a radioactive element, it has always taken the same time interval ΔT for half of a lump of its material to decay into daughter products. Seen from the outside we say that interval is getting longer and that as a consequence the half-lives were shorter in the past proportional to 1/c, but that is because the atom's time interval was shorter in proportion to 1/c as seen from (16). The atom on the other hand is completely oblivious to this inconsistency as seen from our point of view and maintains the rightness of its own ways. Again, as seen from the atom, which is where light comes from in the first place, light has always travelled at a completely constant speed, always moving precisely the same distance in the interval of atomic time ΔT. Seen from the Universal time frame we say that light has travelled further in one second many years ago than it does now, but that is only because one of our seconds lasts longer than that of the atom. The atom in all innocence would counter that the distance that light has travelled in one unit of its time, T, has been absolutely constant throughout its history. And so we could go on. From this it is apparent that atomic time is slowing down uniformly throughout the Universe and that all things are governed by an absolute time which is in the Hands of the Eternal Timekeeper.

THE NUMBER OF SPEED OF LIGHT DETERMINATIONS

A question from Mr C. Butel of Pymble, N.S.W., Australia

By chance I looked at at a copy of **Theoretical and Experimental Physics** by Jerrard and McNeill (Chapman and Hall, 1960). Although I quickly decided that its contents were matters that I as a layman would not be able to understand, the following footnote on page 18 came to my attention: —

"Bergstrand (1956) *gives 2.99793 ± 0.00003 x* 10^8 *ml sec as the mean value for c. This figure has been* obtained *by the analysis of the results of several thousand observations made by different* experiments using *different methods."*

Bergstrand E. "Recent Developments and Techniques in the Maintenance of Standards" (H.M.S.O., 1952) **Encyclopedia of Physics,** vol. 24, no. 1 (Berlin: Springer, 1956) (The date 1952 looks strange but is the date given).

I have in mind your reply to P. Daniel **(Ex Nihilo,** vol. 6, no. 2, p. 36, 1983) and while the above appears to contradict statements made by you, I can see that the authors got their information from secondary sources and the reference to "several thousand observations" may have arisen from wrong assumptions by the authors or by the authors of the secondary sources.

Would you care to comment on this?

Barry Setterfield replies...

Thank you for the reference that you have given for the following enquiry. The statement that the figure has been "obtained by the analysis of. . .several thousand observations. . ." needs clarification in order to avoid the misunderstanding that you and others have about my comment to P. Daniel in **Ex Nihilo,** vol. 6, no. 2, November 1983.

The way that a determination is done is this. The equipment is set up and tested by making a series of observations. A preliminary result was often issued at this point. The equipment was then adjusted and refined to overcome possible errors and the main run of the experiment was done. In this main run it was often the case that upwards of a thousand separate observations were made. All these observations were then plotted and mathematically reduced in accord with known procedures, allowances being made for possible experimental errors, etc. Using this technique the final value was then issued as the best probable determination from that series of experiments. This was the final, mathematically reduced, determination that comprises the final published value that this series of experiments has produced. There may be thousands of individual measurements in order to get the best final determination at the particular date concerned, but they combine to give just one best determination or value at the given date (usually taken as the mid-point of the experiments). It is these "thousands of observations" that is being referred to here and that confuses many. However, these thousands of observations have only produced 3 or 4 final results of determinations which are the published figures. It is these final published figures that are the important ones in the c decay research. It is futile to go back over the individual observations again and re-reduce them. That has already been done on the spot **by** the experimenters who knew their equipment far better than we do at this point in time and so knew just what problems to correct for. What CAN be done is to check the reducing procedure to see whether or not THAT was done correctly, and in fact Professor R.T. Birge frequently did this with all determinations, but that is a different question.

I remember well that the above procedure was the one adopted while I was at University in the determinations that we did of the various physical quantities. When determining the value of the gravitational constant G for example, the lecturer would have been horrified if we came to him with a value based on just one experiment. The procedure was to do as many experiments as possible to get as many data points as possible on the day concerned and then plot them and reduce them to get the best possible value from that series of experiments. This is what has been done in the case of the speed of light, and the number of final determinations thus made was limited, even though the individual points that were used to finally give those values were numbered in thousands. Unless this is firmly understood, confusion will result.

Questions from Mr R. Holt of Lee's Summit, Missouri, U.S.A.

Mr Setterfield has undoubtedly worked a lot on his speed of light articles[1,3], but I find significant discrepancies between his articles and his references. These discrepancies cause me to question his work and I believe your readers deserve his explanation.

One should realize that any value of c given by an experimenter is rarely, if ever, based on one single measurement. Upon reading some of Mr Setterfield's references, I find that there has been thousands of measurements of the speed of light. In **The Velocity of Light and Radio Waves** by Froome and Essen, often cited by Mr Setterfield, I find: "Cornu (1874). . .made a series of over 600 measurements. . .", page 5; "Michelson (1880). . .average value of 100 determinations. . .", page 6; Michelson, Pease and Pearson (1935), "There were 233 series of observations each series consisting of about 10 individual measurements", page 33; Karolus and Mittelstaedt (1928), "Altogether 755 measurements were made. . .", page 36; Anderson (1937-1941), "Nearly 3000 measurements were made. . .", page 39; Huttel (1940), "The total number of results recorded was 135. . p a g e 40; Rosa and Dorsey made at least 869 measurements, page 44. Checking some of the original publications I found Anderson[4] had made 2895 measurements, and Michelson, Pease and Pearson had almost 3000 determinations based on over 22,000 individual

measurements.[5] This alone makes a total of about 3000 determinations of the speed of light based on over 26,000 individual measurements. For Mr Setterfield to say "there were not thousands of measurements"[6] (or determinations], independent of his opinion of them, is inaccurate and misleading.

Concerning the determination of the speed of light by Roemer (1675] Mr Setterfield has inaccurately interpreted his reference in **Sky and Telescope**.[7] This article in **Sky and Telescope** is a brief summary of a paper in the **Astronomical Journal** that says: "Our approach is to compare each of the observations with predictions based on modern orbits for each (moon of Jupiter] and for Jupiter, and on a modern radius for Jupiter. The speed of light is then determined by a least squares fit between the observed and predicted eclipses. . . .The best fit occurs at zero where the light travel time is identical to the currently accepted value. Guided by numerous repetitions of the experiment with different constants and with some of the observations deleted, we estimate that the difference between light travel time three hundred years ago and today's value is less than 0.5%. We conclude that the velocity of light did not differ by 0.5% in 1668 to 1678 from the current value."[8] Contrary to what Mr Setterfield has written, it obviously does not "conclude that the best possible result was still 0.5% above the present value".[9] Based on this work, the best analysis of Roemer's data, one can only say that c = 299,792.5 ± < 1,499 km/sec in 1675. Arguments to reduce the uncertainty based on Roemer's measurements made to the second or the present accuracy of earth's orbit are without merit. Therefore the value given in Setterfield's article of c = 301,300 ± 200 km/sec is not only erroneous, but entirely unsupported by his references and contrary to the actual data.

Should one prefer the travel time for light to cross earth's orbit estimated by Roemer, it is about 22 minutes.[10] This gives a value of c = 227,000 km/sec — even much slower yet.[11] Roemer's data does not support Mr Setterfield's contentions.

The speed of light based on Bradley is dependent upon very precise and accurate positioning of Bradley's telescopes. Bradley gave an angle of aberration due to earth's motion of 20.2 seconds of an arc[12] when measured over a year's time. An error of approximately 0.3 seconds in measuring this very small angle would give the present value of c.

The speed of light given for 1871.0 by Mr Setterfield is taken from Cornu cited by Gheury de Bray.[13] In response to criticism[14] for misquoting the original authors, Gheury de Bray corrects himself.[15] The date is now 1872.0, the value is c = 298,500 ± 300 km/sec and this result is rejected by Cornu as being doubtful. However, it appears he was disposed toward a higher value of c. When later measurements indicated perhaps a higher value, he questioned his earlier work.[16] A thorough evaluation of Cornu's 1872 work by Dorsey shows these measurements should not be totally ignored and that the data indicates c lies within the range 296,500 to 300,500 km/sec centered on 298,500 km/sec.[17] The final result of Cornu's work, which continued through 1874 and Setterfield lists at 1874.8, indicates c lies between 299,300 and 300,500 km/sec centred at 299,900 km/sec.[18] It should be noted that not one of Cornu's assumptions used to obtain the accuracy he wanted and indicated were valid.[18]

Thus far I have addressed only the earliest measurements that Mr Setterfield claims supports a decay in the velocity of light. In my investigations so far I am disappointed that Mr Setterfield is not critical or thorough in his examination of the literature rarely going to the original sources, missing the thousands of individual measurements, and reaching conclusions not supported by the data. If Mr Setterfield really believes that c is or was decaying, I strongly suggest he go back to the thousands of measurements done at different years, months and days treating each as a data point since they are unique in time, and repeat his modeling. His present procedure, as it stands, is certainly erroneous and has the appearance of a selective selection of data. For those who want critiques reaching opposite conclusions to Mr Setterfield's, I refer you to the literature.[16,19,20]

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9. B. Setterfield, **Ex Nihilo,** vol. 1, no. 1, Int. Ed., 1982, p. 62.
10. O. Roemer (1676), J. Scavans, reprinted in **Isis,** vol. 31, 1939, p. 327-, see especially pp. 328, 346-8.
11. Calculation based on a 1.4797 x 10^8 km diameter of earth's orbit taken from the **CRC Handbook of Chemistry and Physics,** 1977-1978, p. F-176.
12. K.D. Froome and L. Essen, **The Velocity of Light and Radio Waves,** Academic Press, London, N.Y., 1969, p. 3.
13. M.E.J. Gheury de Bray, quoted in **Science,** vol. 66, sup. p. X, September 30, 1927, taken from **L'Astronomie,** see also

Gheury de Bray, **Nature,** vol. 120, September 17, 1927, pp. 404-5.
14. **Nature,** vol. 120, October 22, 1927, p. 594.
15. M.E.J. Gheury de Bray, **Nature,** vol. 120, October 22, 1927, pp. 602-4.
16. N.E. Dorsey, **Transactions of the American Philosophical Society,** vol. 34, part 1, October 1944, see footnotes on pp. 15-6.
17. ibid., pp. 15-21, 85-87.
18. ibid., pp. 85-6.
19. R.T. Birge, **Reports on Progress in Physics: The Physical Society of London,** vol. 8, 1941, pp. 92-101.
20. R.J. Kennedy, **Nature,** vol. 130, August 20, 1932, p. 277.

Barry Setterfield comments. . .

Your thinking seems to be rather confused on the difference between "measurements" and "determinations". You quite correctly state that "any value of c given by an experimenter is rarely, if ever, based on one single experiment". You quite correctly note that there have been thousands of individual measurements made of c, but then you completely confuse the issue by stating that it is misleading to say there were not thousands of measurements (or determinations). I admit part of the confusion was caused by me using the word "measurement" in my quoted reply when I should have used "determination", but you are making an error by saying that I have been "missing the thousands of individual measurements" and should "go back and treat each as a data point. . .". This is what the final determination is all about. A series of perhaps 1000 measurements is made of a physical quantity, the results are plotted and mathematically reduced, allowance being made for possible experimental errors, and from this procedure a final value is issued, the determination of that physical quantity at that point in time. You should be well aware of this. What you are asking me to do is to repeat the mathematical reduction procedure of each experimenter and re-issue a new value for their work. Certainly not! They were in far better position that I to take account of equipment limitations, to know which experiments were the most valid, and hence to use the best reduction procedure. The experimenters have issued their final determination for their series of experiments and that value is the one that stands in the record books, that is quoted and used in subsequent discussions. This is not selection of data, this is simply standard scientific procedure. It is these finally issued determinations that have been used as data points in the c decay work and are the ones always quoted in any discussions of the value of c — not some individual aberrant value that may have occurred during the experiment series.

Concerning the Bradley value, I would point you to my reply to Bounds (this volume) in which there are listed over 50 determinations of the speed of light using this technique. I would emphasise that the error in each determination is about the same, that the list covers almost two centuries of observations, and despite some inevitable scatter in the results due to the problems with the method, the decay in c with time is quite evident. You may argue one way or another about an individual determination, but the whole suite of results by this method supports the basic contention that c has decayed.

Concerning the Roemer value I refer you to my replies to Fackerell and Bounds (this volume). However, you are under a misapprehension concerning my procedure. If you checked the reference (2) against that value in the initial article you would have noticed that BOTH Froome and Essen AND **Sky and Telescope** were used in obtaining the value listed. It was NOT a direct use of the Goldstein et al result. What was done was to take the Froome and Essen value of 303,000 km/sec with its error margin of 2,000 km/sec and the error limit of the Goldstein et al re-working of 1,500 km/sec above the present value and reconcile the two authorities by taking the common ground of 301,000. This is not erroneous, nor is it unsupported by the references. However, a fuller treatment of this value is in order and it appears in my replies to Fackerell and Bounds.

Concerning Cornu's values, the determination listed against 1871 was in fact one of the two quoted against the 1874.8 date **(Nature,** vol. 120, p. 603). The value rejected by Cornu in 1872 was not included in the final listing because of its doubtful nature. Additionally, the value adopted as the central one in the final determination was the one included against Cornu-Helmert. However, all the published values were included in the final analysis of all c values, along with all the re-worked values for Roemer's data etc., and all the values that Cornu and others had rejected as being "inaccurate" or "doubtful". As pointed out in the concluding paragraph of the article in **Ex Nihilo,** vol. 1, no. 3, Int. Ed., p. 45, January 1983 the result was that a higher rate of decay for c for all curves was indicated.

Finally, the comment that Birge and Kennedy reached conclusions opposite to my conclusion that c has decayed shows some lack of perception and failure to trace through the sources. If we take Kennedy's comments first as they appear in Nature, August 20th 1932, p. 277 he points out that a change in c should produce a shift in interference fringes DUE TO A SHIFT IN WAVELENGTH. However, from a conservation of energy approach, a change in c demands A CONSTANT WAVELENGTH. Indeed, Prof. R.T. Birge criticised Kennedy's assertions on just this point **(Nature,** November 17th, 1934, p. 771) and in the same paragraph indicates his own reason

for not accepting decay in c, which carried through to his later article. It was this. If the wavelength is not changing, then the frequency must be changing and consequently "the value of every atomic frequency must be changing (in unison with c). . .Such a variation is most improbable. . .". However, from a conservation of energy approach as applied to the atom, what Birge as his natural reaction considered as most improbable turned out to be the only mathematically valid solution. The atom is affected by the change in c — higher c means faster motion of the atomic particles etc. This had been worked out mathematically even before the opportunity came to check these references. Thus your reasons for referencing Birge and Kennedy as being against c decay are invalid.

MICHELSON'S C MEASUREMENTS AND THE C DECAY CURVE

Comments from Mr A. Sproul, Frenchs Forest, N.S.W., Australia.

Let me make it clear from the start that I don't believe in evolution, nor do I believe in creation science. I do however, firmly believe in a creative Being, which some of us call God, and that science is only really capable of explaining that which our physical nature can comprehend. The nature of God certainly can't be perceived by our physical being alone, and it is for these reasons I find I must object to Barry Setterfield's "treatise" on the decay of the speed of light.

Being in my fourth year of a physics degree, a friend, thinking I would be interested, lent me a copy of Barry Setterfield's article. Rather than finding some new physics within the pages of **Ex Nihilo I** found I was greatly disappointed by the argument put forward.

The article described how Setterfield using a curve-fitting computer program, found that the best **fit** to the 300 years or so of data of the velocity of light was **not** c equals a constant but a curve which decayed with time. This is fine for interpolation of data from between known data values. However, Setterfield extrapolated back in time along his decay curve to obtain some huge value for the speed of light 10,000 years ago. This was then used to "explain" how the Universe was really much younger than the "evolutionists" would have us believe.

I will not enter into the metaphysical argument, but merely point out that Setterfield's basic physical argument is utterly and totally wrong. There is absolutely no mathematical justification for extrapolation in the best of circumstances, let alone extrapolating 10,000 years outside a known range of only 300 years of data!

Apart from this, in his update **(Ex Nihilo,** November 1983), Setterfield correctly quotes Michelson's decreasing measurement of c from 1879-1926 and the ensuing physical debate. What he fails to mention is that Michelson's 1932 measurement with the same method, gave a value

c = 299,774 ± 11 km/sec

which is below the constant value

c = 299,792 km/sec

which Setterfield agrees is the current non-decaying value today.

If this was only a distortion of science it would be bad enough but to mislead non-scientifically trained people concerning God and the nature of His creation with incorrect, pseudo-scientific arguments is far worse.

Barry Setterfield replies...

Allow me to take your final point first. You mention that I have quoted Michelson's values from 1879-1926 which show that c was decaying during that period, but that I fail to mention Michelson's 1932 measurement which was below the present value of c. It is rather unfortunate for those following this debate in Australia that there was an American edition of **Ex Nihilo** put out from Australia in which all these values were totally presented and discussed. You apparently have not had the benefit of viewing that document, which also appears in my current research monograph and is about to be updated. Yes! Michelson, or rather Pease and Pearson, did obtain a value of 299,774 km/sec and this appears in the list of figures on p. 13 of the monograph and is discussed, along with the other "low" values, on p. 20. This result has been the subject of much criticism and a reason for the anomalously low value is apparent. The experiments were conducted along a light path in a 1 mile long evacuated tube with plane mirror reflections giving a total light path of 10 miles. There was one unfortunate aspect of the situation in that the "base line was on very unstable alluvial soil" near Laguna Beach, California. There was an admitted "correlation between fluctuations in the results and tides on the sea coast. . ." which was causing fluctuations in the base length and the measurements made of it. Indeed those base-line measurements were all made during the day while measurements on the rotating mirror were done at