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The Age of the Astronomical Universe — A Reply

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Professor Edward Fackerell's article on 'The Age of the Astronomical Universe' (**Interchange,** no. 33, 1984, pp. 56-65; and this volume) contains some valid points in its summary of the approach adopted by some when confronted by the immense size of the universe and the problem this presents to those holding to a young age for the cosmos. His analysis and conclusion that the notion of light being created 'on its way with the appearance of age' is not in harmony with either the character of God or of the physical facts of astronomy is very cogent. Additionally, the idea proposed by Moon and Spencer that light travels along Riemannian surfaces (as their means of avoiding Einsteinian relativity), and so traverses the universe in a period of 16 years, is rightly criticised since the double image of every object that this view necessitates just does not occur. The criticism of Slusher's work on radiometric dating is generally valid as the radiometric 'ages' are largely internally consistent. What could have been pointed out, however, is that these internal consistencies are still retained with a decaying speed of light which, at the same time as producing these vast radiometric 'ages', also means that the actual age of the sample may be quite small, only thousands instead of billions of years.

Fackerell is almost certainly correct in stating that a decay in the speed of light "would have revolutionary consequences for currently held physical theory", quite apart from its profound theological implications. Hence I am surprised that he did not check on the most recent results instead of the rough progress reports as many others have done, before publishing a critique. He would then have had the advantage of seeing a fuller development of this work which nullifies much of his criticism.

C DECAY NOT TAKEN SERIOUSLY?

After his introductory comment, Fackerell makes some interesting remarks in the paragraph headed "Setterfield's View that the Speed of Light has been Decreasing". He begins by stating that "such claims have not been taken very seriously". He is obviously unaware of a comment in a serious scientific discussion just recently. In an article entitled "Time to Remeasure the Metre", in which the length of the metre was proposed to be fixed by the supposed constant speed of light, the following comment was made.[1] "But many scientists have speculated that the speed of light might be changing over the lifetime of the Universe. . ." and ". . .it is still possible that the speed of light may vary. . .". Comment then centred on what would be noticed with such a variation under this new definition of the metre. Under these conditions, it would seem that some, at least, in the scientific community have seriously considered such a possibility.

EXPERIMENTAL EVIDENCE FOR C DECAY

In the same paragraph Fackerell states that until the second world war, "the errors in measurement of the speed of light were too big to permit anything to be said about systematic trends". However, even just using the results of Michelson alone the situation is clear. The results of his experiments are given in Table 1.

Table 1. Michelson's Values of C

Date	Value of C (km/sec.)
1879.5	299,910 ± 50
1882.8	299,853 ± 60
1924.0	299,802 ± 30
1926.5	299,796 ± 4

All told there is a systematic drop with each experiment that amounts to 114 km/sec over a period of 47 years. Notice that the drop is almost twice the maximum probable error in any of the experiments (± 60 km/sec). On the theory of probability, if c were constant, then the results should be more or less randomly scattered about a constant value. This is not the case from Table 1. Indeed, Newcomb in 1882.7 obtained a value of c within a few km/sec of Michelson's providing a valuable independent cross check which at the same time suggests that the error margin of that value should be drastically reduced, thus emphasising the reality of the drop in c. Dr Peter Cadusch, who prefers not to accept c decay, was forced to admit that[2] these "determinations all appear to be higher than the currently accepted value. . .despite extensive re-working and re-analysis". This is a reluctant testimony that the measured value of c has indeed dropped with time.

C DECAY COMMENT IN "NATURE"

It may be wondered why, if c is dropping with time, those doing the experiments back in the 1920s and 30s did not comment on it. They did! That was my first surprise in a search of the literature on the subject. Back then every determination of c gave a lower value than the preceding one and scientists were talking about it.

In **Nature,** April 4, 1931, p. 522, after listing the last four measurements (by Perrotin, Michelson, Karolus and Mittelstaedt), an astronomer, de Bray, noting that these values too were systematically dropping asked,

"If the velocity of light is constant, how is it that, INVARIABLY (emphasis his), new determinations give values which are lower than the last one obtained? The graph. . .is frankly oblique *to the axis of time. There are twenty-two* coincidences in *favour of a decrease in the velocity of light, while there is not a single one against it."*

Then he went on to tackle the root of the problem in accepting the experimental results by saying,

"Vrkljan has shown *that a decrease in the velocity of light is not in contradiction with the general theory of relativity."*

Again in **Nature,** March 1934, p. 464 the values of c obtained by measurements in 1924, 1926, 1928 and 1933 were listed, again showing decrease, with the comment that

"No *physicist looking at the above Table (of c values) can but admit that the alleged constancy of the velocity of light is absolutely* unsupported by *the evidence of* observation."

The correspondence on this issue in **Nature** alone totalled seven articles from 1931 to 1934. F.K. Edmondson of the Lowell Observatory, Flagstaff, Arizona concluded his article on a discussion of possible decay equations with the comment[3],

"With *the* decrease *(in the speed of light) with time, it is to be* hoped *that those who have been performing velocity experiments will* continue *their work until this matter (of which decay curve is best) is settled".*

This stream of correspondence about the measured decay in c was finally closed in **Nature** by Prof. R.T. Birge who was well aware of the consistent drop in the measured value with time. After discussing the various decay formulae presented, Birge finally concluded that c could not be changing, NOT because of the Relativity issue which had been settled, but because it required "the value of every atomic frequency to be changing. Such a variation is most improbable. . .". As it turned out, before I had even seen Birge's article, the theory of the effect of c decay on the atom had been worked out in accord with known conservation laws, and it turned out that what Birge had considered "most improbable" was in fact occurring. The rate of travel of the electrons in their orbits is proportional to c; consequently the frequency of emitted light is proportional to c and the wavelength is constant. In addition it means that radioactive decay processes are also proportional to c. Birge's objection is thus completely over-ruled and the whole question should again be investigated.

C DECAY NOT DUE TO SYSTEMATIC ERRORS

Lest it be thought that the above-mentioned decay is merely a trend due to the elimination of a systematic error, several points should be made. First, a variety of instruments were used which measured the drop. If it were an instrumental effect the results from different instruments would be scattered around a horizontal line. Such is not the case. Again, for instrumentation that DOES have a known systematic error, the drop is STILL recorded but shifted into another velocity range. Thus for those using the Kerr Cell that employed a detection tube in which everything hinged on the passage of electrons which obviously travel slower than light, it might be surmised that the value of c so obtained would be lower than by other means. This was in fact found to be the case. Yet even in this lower range the decay is still evident, as shown by the Kerr Cell results in Table 2.

Table 2. Kerr Cell Results

Experimenter	Year	Value of C
Mittelstaedt	1928	299,778 km/sec.
Anderson	1937	299,771 km/sec.
Huttel	1940	299,768 km/sec.

Finally, lest it be said that it was simply error in observation, it should be pointed out that the average observational error from all the measurements is just 1.6% of the observed decay. Under these conditions it can be stated that the decay in c is NOT due to be observational error or equipment limitation. Sophisticated instrumentation was still picking up the decay throughout the 1950s, but since 1960 the decay appears to have ceased: that is a fact of observation. However, back in the 1920s and 30s when the decay was STILL occurring there WAS a discussion about the implications THEN.

THE EVIDENCE FROM STATISTICS

Strangely enough Fackerell seems to regard the fact that the data from post-1870 plainly shows a drop in c with time as "a false claim". He obviously has not heard of the independent analysis at Newcastle University that stated[5]: "any two-stage curve fit gives highly significant improvement over the assumption of a constant c value. Residuals reduced from 22,000 to under 2,000". This is statistical proof that c has not been constant. In support of his contention, however, Fackerell quotes R.T. Birge on this issue.[6] Nevertheless, Fackerell's quote is only part of the story. Birge himself explicitly states[7] that "these older results are ENTIRELY CONSISTENT AMONG THEMSELVES, but their average is NEARLY 100 km/sec GREATER than that given by the eight most recent results" (emphasis added). Birge then used a statistical weighting procedure and came up with the statement that Fackerell quoted, as well as colouring the argument with his philosophy of science. Birge admits in that article that a better method than the weighting procedure is a least squares linear fit to the data, and that result produced an average decay of about 1 km/sec per year. However, using a TI programmable calculator, and the 13 data points that Birge gives, the result seems as if it should be 2.44 km/sec per year. In any case a decay in c is still apparent. In view of the controversy that was still raging on the decay in c, and in view of the fact that this was the second last published article on the subject, one may be permitted to speculate on the significance of Birge's opening comment, relating as it does specifically to the measurement of c. "This paper is being written on request — and at this time on request". Pressure perhaps?

THE ROEMER VALUE

1. Incorrect Error Limits

One of Fackerell's reasons for rejecting the claim that c has decayed is that the value given to Roemer's determination in 1675 "is seriously in error". As has been pointed out elsewhere, the value given was the attempted harmonisation of Roemer's value given by Froome and Essen[8] and Goldstein et al[9] by taking the lower limit of the former and the upper limit of the latter. As Fackerell points out, Froome and Essen made "a careless error", and hence the exercise is rendered useless. However, it has been pointed out by someone highly critical of c decay[10] that there are two errors in the Goldstein et al paper about Roemer's value that Fackerell himself has apparently overlooked. These two problems with Goldstein et al's paper totally invalidate Fackerell's comments on Roemer's value and the outcome actually supports the c decay proposition. Goldstein claimed that his re-working of Roemer's observations gave a value of c that did not differ by 0.5% above or below the current value. One does not have to go beyond the introductory statement in that Goldstein et al. paper to see that there is an immediate problem. Their model permitted a calculation of Io's eclipse times for any given position of the Earth — Jupiter — Io system. Their method results in a root mean square (rms) deviation of actually observed times, compared with those calculated from the model, of 118 seconds. In other words, the model times and Roemer's times disagreed by upwards of 2 whole minutes! Fackerell should have noticed this as his appeal to re-working of Roemer's results by Goldstein et al. With a stated error margin of 0.5% to confirm that the current value of c held back in 1675 is thus negated. An rms error of 118 seconds in about the 1000 seconds, that is, the observed delay across the diameter of the earth's orbit is an 11.8% error which is equivalent to ± 35,000 km/sec in value of c. The decay in c is very liberally covered by this error margin.

2. Goldstein's Incorrect Procedure

However, there is a further problem. It takes the form of an unfortunate conceptual blunder and a mislabelled Table (Table IV). To understand the problem, let us define the following quantities. T_o = times of observed events of Io; T_a = calculated times of events; T_f = calculated times of observations = $(T_a + D)$ where D = calculated delay time due to light travel. Goldstein et al. adjusted the delay times to minimise the sum of $(T_o - T_f)^2$. Their error arose as follows. The calculated times of events T_a were adjusted to an "empirical" initial point. But the ACTUAL PHASE of Io was NOT projected back over 300 years in absolute time. That would have involved knowing the orbital period of this satellite of Jupiter to an accuracy better than 1 part per billion. Instead, the adjustment was accomplished by setting the sum of (T_o - T_f) to zero. THEY FAILED TO READJUST THE PHASE AS THEY VARIED D. Hence, instead of adjusting c to account for the VARIATION in the period of Io, they were adjusting the AVERAGE time of observation to agree with the average predicted time of observation. This procedure gives them right back again the **same value of c** that they used to adjust the initial phase of Io. In addition, the data given in their Table are really the predicted minus the observed times. Lew Mammel Jr at AT and T Bell Laboratories has pointed out with surprise (he was criticising c decay)[10] that when the correct procedure is adopted (which involves subtracting the average delay time D_o from each D and following through the maths) he had to SUBTRACT 6% of the nominal delay time FOR EACH DATUM to get the best fit. The delay times were therefore being REDUCED by 6% meaning that the value of c was 6% higher than now or of the order of **317,700** km/sec. The expected error is 8.6%. He points out that adjustment to the longitude of the ascending node of Io with respect to the plane of Jupiter's orbit will reduce the value and error somewhat but the final calculation should be possible once this quantity is known for the period around **1670.** Nevertheless the value of c will be significantly higher than the present. Conclusion: Roemer's data when re-worked correctly by the Goldstein method shows c was somewhat ABOVE the present value and so is completely consistent with a decay in c with time.

3. Other Results

By way of confirmation of this result it should be noted that Delambre, from an immense number of observations of eclipses of Jupiter's satellite in the **150** years to **1809** fixed the delay across the radius of the earth's orbit as **493.2** seconds.[11] Using the standard value of **1.496** x $\mathbf{10^8}$ km for this radius we obtain c = **303,300** km/sec. Again in **1875** Glasenapp[12] of Pulkova reviewed all available data on eclipses of Io between **1848** and **1870** and obtained results between **496** and **501** seconds delay across the earth orbit radius, with an average of **498.5** seconds. This latter result gives a value for c of **300,100** km/sec. More recently, Sampson[13] in **1909** derived a value of **498.64** seconds from his own reading of Harvard observations while the Harvard readings themselves gave a value of **498.79** ± **0.02** seconds, the difficulty being the inequalities of Jupiter's surface preventing a more exact determination. Thus Sampson's value of c becomes **300,016** km/sec while the official Harvard records give **299,925** km/sec for the same epoch. These values by the Roemer method are thus in accord with the other experimental values that indicate a decay in c. The Roemer method values are given in Table **3.**

Again, these figures would seem to support the basic downward trend that the other experimental determinations of c indicate. Certainly there is nothing inconsistent with such a proposition. Fackerell's appeal to the Roemer value of Goldstein et al in order to dismiss any claim for c decay is therefore misleading as Goldstein's methodology will always give rise to the assumed value for c which invalidates the exercise, as does their quoted error limit. The re-worked Goldstein method actually supports the c decay contention.

QUESTIONING THE DATA

Fackerell states that "there is serious reason to question" that the data values used for c are correct, and calls on the Bounds' discussion (this volume) to back up this point. A detailed reply to Bounds follows his discussion in this volume. In summary let it be said that the values of c that Bounds' scholarship produced are the same as presented in the International Edition of **Ex Nihilo,** vol. **1,** no. **1, 1982** (which Bounds has not seen) except for some Birge corrected values. There are a total of five values omitted compared to Bounds' listing of c values, three of which were by the same experimenter (Perrotin), whose values were confused in the literature sources, and one value was a re-working by Dorsey. There were only two disputed values — Roemer's which has been dealt with as above, and Bradley's. In my reply to Bounds, a Table was presented giving ten values of c obtained by the Bradley method, most from the Pulkova Observatory, covering the period from **1740** to **1935.** This method has maintained approximately the same instrumental error throughout, and consequently the decay in c that these values show (about 6 km/sec per year in a linear fit to the

Table 3. Roemer — Type Experiments

Authority	Date	Value of C (km/sec.)	Comment
Roemer	1675	317,700	Using corrected Goldstein method
Delambre	1734	303,300	Median date 75 years
Glasenapp	1859	300,100	Median date 11 years
Sampson	1909	300,016	Reduction of Harvard values
Harvard	1909	299,925 ± 13	Official reductions at Harvard.

data) cannot be attributed to equipment improvement or a reduction in errors of measurement. One may argue about one value obtained by this method, but when a number of comparably accurate measurements indicate a decay in c over a period of 200 years using the same method, then the basis for the argument tends to evaporate. In any case, when Bounds' additional values are included the decay trend is still unaffected. Under these circumstances, Fackerell's excuse for rejecting c decay on the basis of bad data is totally nullified. He cannot base his argument on the one value of Roemer and Picard, nor can he ignore the testimony of over fifty other determinations that support the contention that c has decayed.

PROBLEMS WITH THE COEFFICIENT OF DETERMINATION r^2

Fackerell spends some time in a consideration of the value obtained for the coefficient of determination, r^2. With the observational data showing a systematic drop in c it became important to discover what form this decay was following. A series of curves were fitted to the data, with r^2 telling us how good a fit was obtained. If $r^2 = 1$ all data points are on the curve. There was only one curve which fitted all three data sets satisfactorily as explained below. This final curve has the form $c = A \operatorname{cosec}^2 kt$, but the linearised form used initially was the log sine curve referred to by Fackerell. This finally accepted curve has an r^2 value of 0.986 for the fit to the c data alone as Fackerell correctly points out. There have been a number of programming problems which resulted in the incorrect values that Fackerell mentions being published. Yes, an embarrassing error did occur in the early r^2 computations. However, to put this in its correct perspective, it should be noted that it is only affecting the value of r^2 in the second decimal place. This cannot therefore be used as a reason for rejecting c decay. In many applications of this procedure it is considered a good result to obtain a value for r^2 of 0.8. In this case the value has dropped from 0.99 down to 0.98. Fackerell's discussion on this point is therefore of somewhat less significance than may at first appear.

r^2 AND THE ORIGIN DATE

Fackerell has pointed out that Table 2 on p. 43 of **Ex Nihilo,** vol. 5, no. 3, 1983 is totally spurious. Again he is correct, but the error was pointed out by Professor Kenyon a year ago and the problem rectified. A report appeared on this in **Ex Nihilo,** vol. 7, no. 1, 1984, p. 42. Under the new programming the oscillation in r^2 (caused by a cyclical misreading of the order of columns of figures by the computer) disappears. A virtually constant value of r^2 is obtained as the origin is shifted back in time, but only as the power of the cosec function is increased beyond 2. Thus for a 5000 BC origin date the value of r^2 is still 0.98588, but the power of the cosec function becomes 2.5817. As pointed out in the **Ex Nihilo** article, which presumably Fackerell still has not seen, there is an observational constraint imposed upon the power of the cosec function so that it must lie between 2.22 and 1.83 which in turn limits the origin date to between about 4500 BC and 3900 BC. Professor Dean Kenyon of San Francisco University, in conjunction with Professor David Meredith of the same University's Department of Mathematics, had this to say after running their own computer check[14] on our new figures: "we are now in agreement on the statistical analysis of the cosec curve ($r^2 = 0.986$). I have run origin and exponent scans on our computer and find that the most probable value of the exponent is 2.0 as you say (that is, cosec^2). The most probable values for the origin are 4166.7 to 4169 BC, in very close agreement with your new value". Since nothing has changed very much, either in the value of r^2 or in the origin date, then Fackerell's rejection of the work on this basis cannot be sustained.

CURVE SELECTION - CLARIFYING THE ISSUES

With a reduced value of r^2 compared with the original value it might appear that there are other contenders for a curve of best fit to the data, like Fackerell's parabola. This is not the case because, contrary to his suggestion, the value of the coefficient of determination WAS ONLY ONE BASIS for curve selection. It was a primary test performed on the c data alone to weed out unlikely contenders, but the curves that pass that test, such as Fackerell's parabola, have several other tests that they must pass — again on the basis of observed physical quantities. The first of the two extra sets of data comes from the effect on radioactive decay rates of a higher value for c. The other data set is given by the galaxies: the more distant they are the greater is the degree to which their light is shifted towards the red end of the spectrum. From the Doppler-Michelson formula it becomes apparent that a decay in c will result in just such a red-shift. The observed values of this quantity thus give another data set that must be approximated to by any proposed curve. Let us look at these two tests in a little detail.

1. Radioactive Decay

As mentioned elsewhere, when the speed of light was higher, the rate of movement of atomic particles was faster, and proportional to c. This applies to particles in the nucleus as well as electrons. Since the leaking out of particles from atomic nuclei of radioactive elements by the tunnelling effect is a probability function dependent (in the consideration of c decay) entirely upon the number of hits per second upon the walls of the potential wells of the nuclei, then with a higher rate of movement proportional to c, the number of hits per second will also be proportional to c. Thus the escape of particles from the nuclei is proportional to c, which gives the radioactive decay rate. Thus the half-lives of these elements are proportional to 1/c. Similar arguments apply to both alpha and beta decay rates.

This effect therefore gives us access to a whole new set of data from which certain constraints may be imposed on the values of c in order to account for the observed abundances of the radioactive elements and the radiometric 'ages' that result. For example, if the maximum value of c remains too high for too long, some of the shorter lived naturally-occurring isotopes would have ceased to exist[15], as it is usually assumed that once a radioactive element has gone through ten half-lives there will be virtually no parent product left. The critical element to watch in this case is U-235 which has a half-life of 713 million radiometric years. Eicher in **Geologic Time,** pp. 137-138 points out that the maximum number of half-lives that this shortest lived naturally-occurring radioactive element could have gone through is 7.8, and that assumes that all Pb-207 is radiogenic. This then forms the upper maximum for all radiometric clocks and gives a radiometric age for the Universe of 5.56 billion 'years'. Though this gives a round figure of about 5 billion radiometric years for the age of the Universe, it is customary to assume that the total 'age' is greater and to account for the continuing abundance of U-235 on the basis of nucleogenesis. This is untenable in a c decay situation, however, and the radiometric value must stand as the upper limit. Thus the total integral under whatever curve or line is adopted for c decay must approximate to a maximum of 5 billion radiometric years.

A lower maximum can also be set, remembering that the upper maximum requires most Pb-207 to be radiogenic, which it may not be. The Precambrian continental nuclei, the basement rocks of the geological column on earth, have a cut-off radiometric 'age' of about 1.8 billion years. This is in very close accord with the minimum 'ages' of moon rocks determined by radiometric dating, which are marginally less than 2 billion.[16] Thus we may state that the minimum integral under the decay curve must be about 1.8 billion radiometric years with a maximum of about 5.6 billion. These two figures set constraints on the total time over which c decay has occurred.

2. The Red-Shift

From a full Doppler-Michelson Law it follows that c decay will result in a red-shift. A mathematical examination indicates that for a given red-shift Z, then numerically-

$$1 - \frac{1}{1 + Z} = \frac{n}{t} \qquad (1)$$

where

$$n = c\,(\text{initial})/c\,(\text{now}) \qquad (2)$$

t = total travel time in seconds emitter to observer (3)

The red-shift data give the following points on the curve. The Virgo cluster of galaxies at a distance of about 60 million LY[17] means that the integral under whatever curve or line for c decay that at time t approximates to 60 million years should give a value of c in accord with the observations where and n/t approximate to 0.001. Thus the value of n for the 60 million LY integral should be 0.001 t. Again, the Coma cluster has a typical value for Z of 0.023 and hence from (1) we have n = 0.0225 t for an integral corresponding to a distance of about 250 million LY[18] (see more detail in the discussion of the red-shift in this volume). The most distant galaxy has Z = 4 approximately, so that n = 0.8t. Finally the 3°K background is suspected of being the x-ray background red-shifted by a factor of 1 million.[19] Thus with $Z = 10^6$, n = 0.999999 t and so in the limit we can put n = t approximately as the final upper condition pertaining from observation. In this latter case we have the integral value supplied by the radiometric data. From this data as noted above, the minimum integral from which the decay occurred with the condition n = t is 1.8 billion radiometric years, which is equivalent to 1.8 billion LY. Thus on the curve chosen, the value of t that gives an integral of 1.8 billion 'years' must also be the approximate value of n when t is expressed in seconds and hence c is fixed. The maximum possible value of this integral is 5.6 billion 'years' or LY. Hence the possible range of c is fixed.

3. The Final Result from all Three Data Sets

Now the Fackerell parabola can be made to fit the radiometric data alone, and in so doing the total maximum age of the Universe drops down to less than 1 million years. However, the third test offered by the red-shift data set causes the parabola and all other contenders to drop out: the values of n are nowhere near the experimentally observed values. The final test of n = t is disastrous even using the value set by the radiometric data for the parabola. If we take the lower limit given by n = 0.8 t set by galaxy observations with the origin date for the parabola of about 900,000 years, the value for n becomes 2.3 x 10^{13} which is not nearly approximated to, and even if it were, there would be no radioactive elements left at all. The only curve found to-date to fit all three data sets is the cosec curve. The maximum value for c approximates from the 3° K background and other data points to about 200,000 million times c now, and a consequent total decay time of just over 6000 years. On this basis it should be apparent that THERE IS NO EXTRAPOLATION as BOTH ends of the curve are defined by observation. Fackerell's criticism that the extrapolation is unjustified shows a failure to grasp the situation. He cannot avoid the issue as he tries to by stating that there are a variety of hypotheses for the red-shift. It is plain from the Doppler-Michelson formula that c decay results in a red-shift and hence these observed values must be approximated to by any proposed decay curve. Again, as the time base for the decay is also fixed by the above process, his rejection of the proposal on a time argument is without substance.

RADIOACTIVE HEATING OF THE EARTH

Professor Fackerell seems to have correctly grasped the concept that decay in the speed of light inevitably means that the rate of radioactive decay was faster in the past. He then pronounces this as one of his "very strong reasons" for rejecting the c decay concept. His basis for doing so is (1) the "consequent enormous flux of radioactive particles that would have completely destroyed. . .the cells of living things"; (2) the means of overcoming the problem by placing the radioactive material "deep inside the earth's crust" would "concentrate the distribution of radioactive material" and have the "real possibility that this release of energy could vaporise part of the earth"; and (3) naturally-occurring slow fission reactions such as that which occurred at Oklo in Central West Africa would 'go critical' and "turn into a bomb". This fascinating scenario does not stand up to close examination, however. The problem is dealt with in some detail in the article on c decay and the geological ages in this volume, to which all readers are referred.

1. The "Flux of Radioactive Particles"

Let us summarise the main conclusions reached in that article. Alpha and beta particles have a range that is INDEPENDENT of c. Once stopped, no further damage ensues. All their effects have been quoted as being absorbed by three feet of clay. Shielding by even a moderate cover of sediments would be totally effective, even if these particles were in the near-surface zone, and there are excellent reasons why they were not (see below). X-rays and gamma rays are affected by c decay such that their damage during Creation Week would be equivalent to ultraviolet light today and they would be more readily absorbed. Even if they were in the near-surface zone, which the model indicates is most unlikely, effects would be virtually absent, Neutrons decay into protons and electrons which are readily absorbed in the same way as the alpha and beta particles above. The key here is that the neutron has a half-life today of about 13 minutes. At Creation this becomes about 3.9 x 10^9 seconds, following which the resultant proton and electron are readily absorbed. In addition, the scattering rate would be increased for the neutron before decay, thus trapping them in the vicinity where they were released. With the outlined model, neutrons would prove no problem on the surface of the newly created earth. With these considerations then, Fackerell's "enormous flux of radioactive particles" proves to be virtually non-existent, particularly when shielded by the bulk of the earth as the model proposes.

2. Radioactivity Deep Inside the Earth

Fackerell admits that having the radioactive elements now in the crust put originally deep inside the earth would overcome the surface radioactivity problem, quite apart from the considerations outlined above which minimise the effects even more. Note that the model for the formation of the earth and planets out of the whirlpools as the Universe rapidly expanded at the original high velocity of light, has the elements and minerals accumulating at the centre of the whirlpools on the basis of temperature. The rapid Universal expansion rapidly dropped the temperature of the whirlpools and the solidifying species were thrown to the centre. An examination of the situation confirms that on this approach there will be no radioactive material near the surface originally. Fackerell's fear about the 'grave consequences' of increased concentration of this material in the earth are unfounded. First, a calculation shows that if the radioactive material now in the upper crustal region, where it has become concentrated, were re-distributed throughout the interior there would be 1.877 radioactive atoms in the mantle originally for every 1 radioactive atom that is there now. Secondly, a further calculation shows that at the increased rates of radioactive decay and consequential heating, the interior of the earth will not "vaporise" as the perturbed Professor suggests. Instead the core temperature after about 6000 years will have only attained a temperature of roughly 5000°C, the lower mantle something like 2700°C, and the upper mantle around 1800°C. These are rough calculations but serve to indicate the order of magnitude of the effect. It is of interest to note that these temperatures are basically equivalent to those expected to exist in the earth's interior today from other geophysical data. Professor Fackerell's fear on this issue would thus seem to be groundless upon close examination.

3. Natural Fission Becomes a Bomb?

There is a more detailed discussion of this point in a reply to Robert Gentry in the 'techlets' in this volume. However, in summary it can be stated that Fackerell's suggestion that enhanced decay rates would turn slow reactors (natural or otherwise) into a "bomb" is not in accord with changing c situation. One thing has been forgotten in that suggestion. Atomic particles move at speeds proportional to c.[20] The neutron is no exception. Thus although its energy content is constant, it is moving at a much higher speed. Now as Irvine points out in his article,[21] the key thing to get U-235 to fission is the neutron. He states that "the neutron's effectiveness in inducing fission increases with the length of time it spends near the nucleus, that is, THE LOWER ITS VELOCITY. . . Thus there is a need for moderator TO SLOW DOWN THE NEUTRONS. . ." (emphasis added). With a higher value for c, the neutron velocity will be correspondingly shorter, and the chance of fission accordingly reduced all by a factor of c. If we make an assumption that the production rate of neutrons is proportional to c, then the increased flux of neutrons will still only raise the probability of a fission reaction TO THE SAME LEVEL THAT IT IS TODAY in the same ore body. However, the production rate of neutrons in uranium is largely dependent upon U-235 fission anyway and so the chance of 'going critical' is actually even less in the past than it is today. For the calculations associated with the Oklo natural reactor it means that even larger quantities of water would have to have been present as a moderator than has hitherto been assumed.[22] Contrary to Fackerell's suggestion then, the natural fission reactor at Oklo is in good accord with the c decay situation.

THE PHYSICAL CONSTANTS

Fackerell uses the Oklo reactor to show that there are "stringent limits on the variation. . .in the fundamental constants of physics". Actually what he is showing is that there are very stringent limits to the law of conservation of energy. This law is upheld in the approach to c decay and as the variation of the constants is encompassed within this law, then no disagreement is expected. For example, the quoted article by Irvine comes to this conclusion on the basis of the resonance energy of samarium-149 changing by less than 0.01 eV, which in turn translates into a limit on the variation of neutron resonance energy and hence the coupling.[23] Another example of the problems with that method of determining changes in the constants is the situation with the test proposed by Bahcall and Salpeter to measure the variability of h in space.[24] What was actually measured was the hc ratio in this and other experiments, and because no change was noticed in that, some concluded that h did not change. Wesson[25] was among those to point out that h and c might each be varying in such a way that their variations mutually cancel. This is the situation pertaining on a conservation of energy approach. However, when individual constants are measured rather than in combinations associated with energy, it is then that their variability is detected. In some instances combinations of constants have mutually-cancelling c-dependent terms, in other instances they do not (see reference 26). When these individual constants and groupings of constants are considered, and their measured values tabulated, EACH ONE IS BEHAVING IN ACCORD WITH C DECAY THEORY. The Rydberg constant has three c-dependent terms that mutually cancel in theory. In practice the measured value of the Rydberg constant is shown by experiment to be constant to six significant figures. This is important because these measurements were made over the same period as the c decay experiments. One set of values (Rydberg constant) is stable to six figures whereas the other set of c measurements is varying from the third and fourth figures. This is the situation with the other constants. If there were no c variation and no connection with c these other constants might all be expected to have values varying on either side of a fixed datum. Instead, each one is behaving in the manner that c decay theory indicates. If these physical quantities have really been constant, as Fackerell prefers to believe, he must explain why there are thirteen coincidences in support of c decay theory and not one against it. As a solid state physicist at the University of N.S.W. stated,[27]

"the c decay curve correlates harmoniously with all the other observed values of the physical constants. The possibility of these theoretical predictions matching the behaviour of thirteen physical constants is 0.6 of a chance in a million. This very strongly suggests that the theory is correct". Though the "probable error" argument that Fackerell uses may apply in some instances to allow his preferred position, what he fails to state is that the same probable errors in no way deny the c decay position either, and this latter position is backed up by the additional observational evidence that gives rise to the thirteen 'coincidences'. This cannot therefore be used as an argument to avoid the c decay proposition.

SUMMARY OF DISCUSSION

Fackerell began by stating that the notion of c decay has "not been taken very seriously" by the scientific community. This is falsified by a recent discussion in a scientific journal, and by the series in **Nature** in the 1920s and 30s, as well as **Science, Isis, l'Astronomie,** etc. He then moved on to deny any decay on the basis of error margins in the observations. Birge, whom he quotes in defence of this position, admits that the earlier observations are internally consistent and are about 100 km/sec HIGHER than the present value. The better test of the linear fit to the data, acknowledged by Birge, gives a decay of around 2.4 km/sec per year from 1870 to 1940, in good agreement with the consistently dropping results of Michelson. Since the residuals for a decay curve drop to under 2000 compared to 22,000 for an assumed constant c, Fackerell cannot hide behind these statistical arguments to deny observational fact. Appeal to systematic errors is invalidated by the fact that the Kerr Cell and the toothed wheel both pick up the decay, but shifted into a lower range in the case of the former, which registers systematically low, and shifted into a higher range in the case of the latter. Systematic errors in equipment are thus not the cause of the observed decay, they simply shift it into another range of values.

Fackerell's claim that the constancy of c is proven by Roemer's results as reworked by Goldstein et al is nullified by the rms error of about 2 minutes in the observed times compared with that of the model, equivalent to ± 35,000 km/sec in the value of c. Goldstein's incorrect mathematical procedure also gives back whatever value of c was originally assumed. The correction of this error gives actual values of c well above the current value. Confirming this trend is the tabulation of all results by this method from 1675 to 1909. Fackerell's confidence in the Roemer method and its values to justify his constant c position would appear to be totally misplaced.

Fackerell's questioning of the data values on the basis of Bounds' critique is shown to be without substance. Apart from the omission of five values (three from Perrotin) the values listed are the same. With the extra five values included, there is nothing inconsistent with the decay trend. The tabulation of ten values of Bradley-type experiments from 1740 to 1935, with basically the same error margins throughout, still shows a consistent decay. Fackerell's faith in the argument of bad data is thus groundless.

The problems that Fackerell has with the coefficient of determination are shown to be trivial, the changes being only in the second decimal place, changing in fact from an incorrect 0.99 down to a correct 0.98. The shift in the origin date is also marginal. His statement about an extrapolation of the curve is shown to be incorrect as BOTH ends of the curve are defined by observation: the beginning of the decay by the astronomical and geological data, and the end of the decay by the c data. Contrary to his statement that the initial value of the speed of light is "entirely unwarranted", the true situation is that observation alone gives that figure. His argument is with the data.

The fears that Fackerell has about an intense flux of radioactive particles, of the earth being vaporised, and natural fission becoming a bomb are shown to be based on a lack of true understanding of the issues, and thus become nothing more than extravagant statements. In a word, therefore, his basis for the statement that "Setterfield's hypothesis is entirely without foundation" is shown to be largely a result of his own misconceptions of the whole situation. The problem is one of his own making and one which does not exist in actual fact. Let us end on a positive note, however.

FINAL CONCLUSIONS

In view of the foregoing, with all the theoretical and observational evidence, the conclusion is strongly suggested that the speed of light is following a $cosec^2$ decay patterning that commenced approximately 6000 years ago. As a result of this decay it can be stated that the total distance that light has travelled in the 6000 years is approximately 6 billion light years (to use the common unit of astronomical distance). This approximately equals the most recent estimate of the distance to the furthest astronomical objects.[28] In addition the vast radiometric 'ages' of the basement rocks of the earth's geological column and the rocks on the moon are a natural consequence of the high speed of light in the past. These billions of radiometric 'years', based on the assumption of constant decay rates, are accomodated within a 6000 year framework of actual time. At the same time, some anomalies that occur in the radiometric dates, such as the lava that was outpoured in 1800 and 1801 in Hawaii that gave radiometric dates ranging from 169 million up to 3 billion years, that Professor

Fackerell mentioned, are very naturally explained by the c decay model in a way we do not have time to go into here.[29] The nature of, and reason for, the quasars is also an outcome of the c decay model. The difference in character between the two populations of stars and their apparent 'ages' is another feature of the effect of decay and is in accord with Scripture also.[30]

By way of conclusion it might be stated that the evidence of c decay strongly supports a literal interpretation of the Genesis account of Creation. The model points out how the earth, planets and stars came to acquire their present physical characteristics within a basic period of six days[31] in a way which we have not had time to develop here. In addition the vast radiometric and astronomical 'ages' are all accomodated in the 6000 year time-frame that a literal reading of the Biblical genealogies from Adam to Christ requires. This discovery of the decaying speed of light thus harmonises Scripture and science, fact and faith.

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