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Towards A Critical Examination Of The Historical Basis Of The Idea That Light Has Slowed Down

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Barry Setterfield's arguments that "light has slowed down exponentially since the time of creation"[1] require the most careful consideration, since this idea challenges what seems to me to be the clearest argument for an old universe. That is, assuming that the galaxies are as far away as they appear to be, and assuming that light has travelled from them at a constant speed, then light from some of those galaxies has taken thousands of millions of years to reach the earth.

There are, as I see it, two arguments in his Part 1. The major argument is that a list of values of the velocity of light (c) obtained over three centuries shows that c has decreased.[2] A minor supporting argument is that a decrease in the value of c with time "explains some otherwise unexplainable facts".[3] However, the latter argument does not need to be considered until it has been shown that there are observations supporting the idea that c decreases with time. Therefore I will proceed immediately to an examination of the values in Setterfield's list.

Although Setterfield claimed that he had grouped the values in Table 1, Part 1, "according to method of measurement so that the systematic trend is not masked by different means of measurement with different built-in biases"[4], the values in the group titled "optical methods" were, in fact, obtained by four different methods of measurement. Roemer measured apparent variations in the period of one of Jupiter's moons; Bradley measured the so-called aberration of light from the stars; Fizeau, Cornu, Young and Forbes, and Perrotin used apparatus involving a toothed wheel; and Foucault, Michelson, and Newcomb[5] used apparatus involving a rotating mirror.

ROEMER'S OBSERVATIONS

The letters by Cadusch and Tap[6] indicated that different values have been ascribed to Roemer. In fact, as Boyer documents, "an almost incredible variety of figures" have been ascribed to Roemer, ranging from 193,120 to 327,000 km/sec.[7] However, facsimiles of the original 1676 paper (in French and English translation) in which Roemer's determination of c was published were reproduced in **Isis** in 1940.[8] In this paper Roemer concluded that light requires about 22 minutes to traverse the orbit of the earth. Because this distance was not well known Roemer did not even calculate a value for c, except to remark that light requires less than a second of time to traverse 3,000 leagues (i.e. the diameter of the earth).[9] However, assuming that the earth's orbit has not significantly altered since 1676, a value for c at that time can be calculated on the basis of Roemer's conclusion:

$$3.0 \times 10^8 \text{ km}/22 \times 60 \text{ sec} = 230{,}000 \text{ km/sec.}$$

Where, then, did Setterfield get his value of 301,300 ± 200 km/sec? In his own words, "**Sky and Telescope, 45** (1973), p353 gave Roemer's 1675 value after reworking a selection of his data. The result was 0.5% above the current value, i.e. 301,300 (= 299,792 x 1.005). Froome and Essen placed it higher. The minimum value was used."[10] The reference to **Sky and Telescope** is to a short item in its "News Notes". Having referred to the existence of a manuscript in Roemer's handwriting listing the timings of the eclipses of Jupiter's first satellite which he made with Picard at Paris Observatory from 1668 to 1678,[11] the item briefly reports concerning an article by Goldstein, Trasco and Ogburn: "From the Roemer list, they selected 40 timings of disappearances and reappearances of Jupiter's satellite Io. Using modern orbital data for the earth, Jupiter, and Io, they compared the observed times with those computed for a number of trial values of the speed of light. The Virginia scientists report in the **Astronomical Journal:** 'We conclude that the velocity of light did not differ by 0.5% in 1668 to 1678 from the current value.' " Unfortunately, Setterfield did not consult the original article or he would not have misinterpreted the conclusion as quoted here. In fact, Goldstein et al. concluded that c did not differ by 0.5% **above or below** the current value. They compared the observed times with those computed "with the velocity of light perturbed in five steps of 0.5% in both directions from the current value". Of these eleven values for c the best fit occurs when c is assumed to have been identical to the current value.[12]

Setterfield's comment that "Froome and Essen placed (Roemer's value) higher (than 301,300)" suggests that they used the same method as Goldstein et al. However, what they wrote was this: "Delambre (1790) and Glasenapp (1874) obtained values of 986 sec and 1001.6 sec for the time lag (i.e. compared to Roemer's 1320 sec) and the mean of these values together with the present day value for the diameter of the earth's orbit (2.99 x 10^8 km) gives a value for c of 303,000 km/sec with an uncertainty of about 2,000 km/sec."[13] (Incidentally, this calculation is mistaken. The figure should be 301,000 ± 2,000 km/sec). Froome and Essen are not, here, seeking to establish what value Roemer could have obtained from his data, by taking the mean of values which Delambre and Glasenapp obtained from Roemer's data. Roemer's manuscript was not discovered until 1913.[14] Rather, Froome and Essen are seeking to show the kind of value which may be obtained using Roemer's method, and so they refer to values obtained independently in 1790 and 1874 by Delambre and Glasenapp.

BRADLEY'S OBSERVATIONS

Different values have been ascribed to Bradley as well as to Roemer.[15] However, a facsimile of Bradley's original 1728 paper has also been published, in **Isis** in 1931.[16] Setterfield's value for Bradley came from Froome and Essen[17], and Froome and Essen refer to Bradley's original article. Bradley was the first to show that one very small apparent movement of the stars over a twelve-month period can be explained in terms of the earth's motion around the sun if it is assumed that light has a definite velocity. By measuring this "aberration" θ, the ratio of the earth's orbital velocity to the velocity of light may be calculated, i.e. tan $\theta = v_e/c$.[18] Froome and Essen write that "from the results of his observations (Bradley) deduced that the angle of aberration was 20.2" and that the ratio of the velocity of light to the velocity of the earth's motion in its orbit was therefore 10,210 to 1. This gives a value for c of 301,000 km/sec".[19] However, given these figures and the value of 2.99 x 10^8 km given earlier by Froome and Essen for the diameter of the earth's orbit:

$$c = 10{,}210\, v_e = 10{,}210 \frac{\pi d_e}{T_e}$$

$$= \frac{10{,}210 \times 3.1416 \times 2.99 \times 10^8}{365.25 \times 24 \times 60 \times 60} \text{ km/sec}$$

$$= 304{,}000 \text{ km/sec}$$

Once again, it seems that Froome and Essen's calculation is mistaken, and Setterfield's confident assertion that "their assessment is reliable"[20] is, at this point, unfounded.

It may also be noted that, at the place in his article where he gave these figures of 20.2" and 10,210 to 1, Bradley said that it would follow from these figures "that light moves, or is propagated as far as from the sun to the earth in 8' 12"."[21]

$$\text{i.e. } \frac{C}{V_e} = 10{,}210 \qquad V_e = \frac{2\pi r_e}{T_e} \text{ and } t = \frac{r_e}{C}$$

$$\therefore\ t = \frac{T_e}{10{,}210 \times 2\pi} = 8 \text{ min } 12 \text{ sec}$$

This brings me to the important question of the limits of error of this value. To begin with, the figures of 20.2", 10,210 to 1, and 8' 12" are not, in fact, Bradley's published value for c. They are merely part of a sample calculation of c based on his observations of one star, γ Draconis.[2] It is also important to realize that the figure of 20.2" does not represent an observation accurate to a tenth of a second, but is in fact half of 40.4" which was found by calculation from the figure of 39".[23] It is this figure of 39" which indicates the accuracy of his observations, although in this figure of 39", as in all the observations to be mentioned, Bradley allowed for changes in the positions of the stars due to the precession of the equinoxes and the nutation of the earth.[24] (The need to allow for the latter was not established by Bradley until some years later.) Another indication of the accuracy of his

Table 1.

Star	Apparent movement due to aberration	Calculated angle of aberration	Summary of any comments
α Persei Bayero	23"	40.2"	dependable
α Cassiopae	34"	40.8"	dependable
β Draconis	39"	40.2"	dependable
Capella	16"	40"	not so dependable
35th Camelopard Hevelii in Flamsteed's catalogue	19"	40.2"	
η Ursae Majoris Bayero	36"	40.4"	
τ Persei Bayero	25"	41"	estimate

observations is provided by his own comment concerning the accuracy of his telescope: "from all the trials I have yet made, I am very well satisfied, that when it is carefully rectified, its situation may be securely depended upon to half a second".[25]

Having given the sample calculation mentioned, Bradley went on to give his observations of a number of other stars.[26] (See Table 1. The last three items in this table were given to show that the same result was obtained regardless of the star's magnitude.)

He then concluded, "I have compared the observations of several other stars (i.e. other than those in Table 1), and they all conspire to prove that the maximum (i.e. the greatest alteration of declination of a star in the pole of the ecliptic) is about 40" or 41". I will therefore suppose that it is 40.5" or (which amounts to the same) that light moves, or is propagated as far as from the sun to us in 8' 13". The near agreement which I met with among my observations induces me to think, that the maximum (as I have here fixed it) cannot differ so much as a second from the truth, and therefore it is probable that the time which light spends in passing from the sun to us, may be determined by these observations within 5" or 10". . .".[27] Bradley's published value was, therefore, 303,000 ± 6,000 km/sec.

THE OTHER VALUES

With regard to the other values given by Setterfield in Tables 1 and 2 in Part 1 of his article, I have not yet examined the original articles in which they were published. All that I can do here is to examine the values given by Setterfield and the remarks he makes about them on the basis of secondary material.

The omission of Fizeau's 1849 value of 315,300 km/sec[28] has already been noted by Morton, Cadusch and Tap.[29] In his replies, Setterfield refers to "Fizeau's 1855 measurements"[30] as well as to "the Fizeau value of 1849".[31] These "1855 measurements" (which Morton referred to without ascribing them to Fizeau) are part of a list published in the first of Setterfield's sources for Tables 1 and 2, a short item headed "The Velocity of Light" in the "Science News" supplement to **Science,** Sept. 30, 1927. As Setterfield says, the author of this item was quoting M.E.J. Gheury de Bray from **L'Astronomie.** (An exact reference was not given.) The first three items in this list are: 1849, 313,300; 1855, 298,000; 1855, 305,650. I have not been able to find out if Gheury de Bray gave references for these 1855 values when he wrote in **L'Astronomie.** (This serial is not held by any Victorian library listed in **Scientific Serials in Australian Libraries.)** However, Gheury de Bray omitted them from the table which he published in **Nature** in 1927 and again in **Isis** in 1936. Other lists agree in only ascribing an 1849 value of 315,000 km/sec to Fizeau.

The omission of Foucault's 1862 value of 298,000 ± 500 km/sec[32] has already been noted by Morton, Cadusch and Tap.

The next two values which Setterfield lists after Roemer and Bradley are an unnamed 1871 value of 300,400 ± 200 km/sec and "Cornu-Helmert 1874.8 299,990" km/sec. The 1871 value comes from the 1927 **Science** news item and the 1874.8 value comes from Birge's 1934 letter to **Nature** in which he was quoting from Gheury de Bray's 1927 article in **Nature.** (Incidentally, the " ± 200" was with the second value, not the first.) In fact, these are two different values for the same 1874 experiment by Cornu. The value Cornu published was 300,400 ± 300 km/sec and "Cornu-Helmert" is Gheury de Bray's shorthand for Helmert's discussion of Cornu's results. Another discussion of Cornu's observations by Dorsey gave a result of 299,900 km/sec.[33] As Froome and Essen remark, the difference between the values obtained by Cornu and Dorsey illustrates how the averaging of widely scattered observations can lead to quite different results.[34]

Cornu's 1874 experiment was, in fact, his second experiment. The result of an earlier experiment in

Table 2.

1941	Anderson	Kerr cell	299,776 ± 14[44]
1947	Smith, Franklin and Whiting	Radar (in air)	299,695 ±50 (~ 299,780 ± 50)[45]
1947	Jones	Radar (in air)	299,687 ± 25 (~ 299,780 ± 25)
1949	Jones and Cornford	Radar (in air)	299,701 ± 25 (~ 299,790 ± 25)
1950	McKinley	Quartz modulator	299,780 ± 70
1950	Houstoun	Quartz modulator	299,775 ± 9
1950	Hansen and Bol	Cavity resonator	299,789.3 ± 0.8[46]
1952	Rank, Ruth and Vanden Sluis	Spectral lines	299,776 ± 6[46]
1954	Rank, Shearer and Wiggins	Spectral lines	299,789.8 ± 3

1872 was given by Cornu as 298,500 ± 300 km/sec, but Cornu himself rejected this value.[35]

With regard to Michelson's 1878-9 experiments, Michelson gave a value of 300,140 ± 300 for his 1878 observations and a corrected value of 299,910 ± 50 for his 1879 observations.[36] Gheury de Bray says that the first value was discarded by Michelson because the second value was more accurate.[37]

Similarly, with regard to Newcomb's 1880-2 series of experiments, Newcomb gave values of 299,627 km/sec (in air) (299,720 km/sec in vacuum[38]) for his 1880 observations, 299,694 km/sec (in air) (299,780 km/sec in vacuum) for his 1881 observations, and 299,860 ± 30 for his 1882 observations.[39] However, Newcomb believed that his result "should depend entirely on the measures of 1882".[40]

Michelson gave the value 299,853 ± 60km/sec for his 1882 experiment. The "1885 299,940" value which Setterfield lists is from the 1927 **Science** news item, but is not mentioned by Gheury de Bray in his later articles.

Perrotin carried out two experiments in 1900 and 1902 over different bases and published two values for each: 299,900 ± 80 and 300,032 ± 215 for the first, and 299,860 ± 80 and 299,901 ± 84 for the second.[41] Only the last of these is given in Birge's 1934 letter which was Setterfield's source for this value. The "1902.8 299,895" and "1906 299,880" values are, again, from the 1927 **Science** news item but, again, are not mentioned by Gheury de Bray in his later articles. Setterfield obtained the values from 1924 to 1933 from Gheury de Bray and Birge's 1934 letters to **Nature.** These may be compared with those given for this period by Froome and Essen:[42]

1924 Michelson	299,802	±30
1926 Michelson	299,796	± 4
1928 Karolus and Mittelstaedt	299,778	±20
1935 Michelson, Pease and Pearson	299,774	±11

The values from 1937 to 1967 in Tables 1 and 2 are from Froome and Essen[43], except those obtained by radio techniques which are from **Ham Radio,** Jan., 1980. I have been unable to check the latter values because the R.M.I.T. library only has **Ham Radio** to 1979 and the only other libraries listed in **Scientific Serials in Australian Libraries** which have this serial are in South Australia. However, with regard to the values from Froome and Essen there are various slight changes to many of the limits of error and various omissions. The former do not appear to be significant but the latter may be worth noting (see Table 2).

A PRIMA FACIE CASE?

It may seem that many of my criticisms throughout this paper have been minor. However, if an idea as fundamental as that which Setterfield has proposed is ever to be widely accepted, the evidence which supports it must be reliable. This is why I have checked Setterfield's references and figures and why I have begun to examine the relevant primary sources. So far as my examination has gone I have found that Setterfield's Tables 1 and 2 are deficient in a variety of ways. On this basis alone his conclusions may be rejected.

However, this does not mean that his attempt to bring actual observations to bear on a question of such fundamental importance may be ignored. The possibility that the question may be resolved along these lines needs to be thoroughly investigated. It perhaps needs to be emphasized that this investigation will not necessarily resolve the question. That is, the historical evidence which is available may be insufficient to allow a conclusion one way or the other. What kind of evidence would be sufficient? Suppose that a reliable list of all published determinations of c had been obtained. Suppose further that a statistical analysis of the values in this list favoured the idea that c has decreased. This would provide a prima facie case for a decrease in c but it would not be conclusive. It would only show the need for a full critical study of the question. Such a study would begin with a critical examination of all the published values of c in order to assess their reliability as records of c at the times the various experiments were performed. Then, having corrected the published values where necessary and having weighted them (or omitted them altogether) according to their reliability, the statistical analysis of the values could be repeated.

Setterfield himself made some explicit and implicit judgements concerning the reliability of some of the published values when he substituted the result of a modern analysis of Roemer's data for Roemer's published value and when he omitted some published values.

However, on the one hand, this prevented his article from being a fully consistent presentation of prima facie evidence for a decrease in c. On the other hand, his very cursory explanations (if any) of these changes in the list of published values (see Appendix 1) do not amount to anything like a full critical study. Furthermore, the one change he made to the list of published values which appears to have adequate support (viz. the substitution of Goldstein, Trasco and Ogburn's analysis of Roemer's data for Roemer's published value) introduces a value which confirms that c has been constant through time.

BIBLIOGRAPHY

(i) Primary Sources (1671 to 1902)

Roemer, Ole, "Demonstration touchant le mouvement de la lumiere", **Journal des Scavans.** 7 Dec, 1676, pp. 233-6 (reproduced in I.B. Cohen, op. cit., as facsimile XIX); translated as "A demonstration concerning the motion of light", **Philosophical Transactions, 7** (1677) June 25, pp. 893-4 (reproduced in I.B. Cohen, op. cit., as facsimile XX).

Roemer, Ole, manuscript folio, reproduced in Kirstine Meyer, "Om Ole Romers Opdagelse af Lysets Toven", **Kingelige Danske Videnskabernes Selskabs Skrifter (Naturvidenskabelig of mathematisk afdeling), Series 7, 7** (1915) pp. 106-45; a page of this manuscript containing a list of the eclipses of the first satellite of Jupiter for the years 1668-1677 is reproduced in I.B. Cohen, op. cit., as facsimile XXI.

Bradley, James, "An account of a new discovered motion of the fixed stars", **Philosophical Transactions, 35** (1729) no. 406, pp. 637-61; reproduced in Sarton, op. cit., as facsimile XII.

Fizeau, H.L., **Comptes rendus des seances de l'Academie des Sciences, Paris, 29** (1849) 90, 132.

Foucault, J.L., **Comptes rendus . . ., 55** (1862) 501, 792.

Cornu, A., **Comptes rendus . . ., 79** (1874) 1361.

Cornu, A., **Journal de l'Ecole polytechnique, 27** (1874) (44) 133.

Cornu, A., **Annales de l'Observatoire de Paris, 13** (1876) A293.

Michelson, A.A., **Nature, 21** (1879) 94, 120, 226.

Michelson, A.A., **Astronomical Papers for the American Ephemeris and Nautical Almanac, 1** part 3 (1880) 109; 2 part 4 (1891) 231.

Young, J. & **Forbes,** G., **Proceedings of the Royal Society,** 32 (1881) 247.

Young, J. & **Forbes,** G., **Philosophical Transactions, 173** (1882) part 1, p. 231.

Newcomb, S., Astronomical Papers for the American Ephemeris and Nautical Almanac, 2 part 3 (1891) 107.

apers for the American Ephemeris and Nautical Almanac, 2 part 3 (1891) 107.

Perrotin, H,, **Comptes rendus . . ., 131** (1900) 731; **135** (1902) 881.

Perrotin, H., **Annales de l'Observatoire de Nice, 11** (1908).

(ii) Secondary Sources

Birge, Raymond T., "The velocity of light" (Letters to the Editor), **Nature, 134 (1934) 771-2.**

Boyer, Carl B., "Early estimates of the velocity of light", **Isis, 33 (1941) 24-40.** (Roemer and Bradley).

Cohen. I. Bernard, "Roemer and the first determination of the velocity of light **(1676)", Isis, 31 (1939-40) 327-79.**

Dorsey, N. Ernest, "The velocity of light", **Transactions of the American Philosophical Society, 34 (1944) 1-110.** (This volume is not in any Victorian library listed in **Scientific Serials in Australian Libraries,** but in a bibliographical note in **Isis, 36 (1945-6) 53.** George Sarton describes it as an "elaborate study of all the determinations of the velocity of light from the time of Fizeau **(1849)** to ours" and notes that "the author concludes against Gheury de Bray **(Isis, 25, 437-48, 1936)** that there is no secular variation".)

Froome, K.D., & **Essen,** L., "The Velocity of Light and Radio Waves", Academic Press, London, **1969).**

Gheury de Bray, M.E.J., "The velocity of light", **Nature, 120 (1927) 602-4.**

Gheury de Bray, M.E.J., "The velocity of light", (Letters to the Editor), **Nature, 133 (1934) 464.**

Gheury de Bray, M.E.J., "The velocity of light: history of its determination from **1849** to **1933", Isis, 25 (1936) 437-48.**

Goldstein, S.J., Jr, **Trasco,** J.D., & **Ogburn,** T.J., III, "On the velocity of light three centuries ago", **Astronomical Journal, 78 (1973) 122-5.**

Sarton, George, "Discovery of the aberration of light", **Isis, 16 (1931) 233-65.**

Science, "The velocity of light" (Science News), **66 (1927)** supplements.

Sky and Telescope, "Velocity of light **300** years ago" (News Notes), **45 (1973) 353-4.**

NOTES

Details of works referred to are given in the bibliography.

1. **Ex Nihilo,** vol. 4, no. 1, p. 39.
2. ibid.
3. ibid., p. 44.
4. ibid., p. 45.
5. E. Richard Cohen, Kenneth M. Crowe, Jesse W. M. Dumond, "The Fundamental Constants of Physics", (Interscience Publishers Inc., New York, 1957), p. 106, say Cornu also used this method, but Gheury de Bray (1927) indicates that he only used a toothed wheel.
6. **Ex Nihilo,** vol. 4, no. 4, pp. 81–4.
7. Boyer, p. 26.
8. Cohen, pp. 374–8.
9. Cf. Boyer, pp. 27f.
10. **Ex Nihilo,** vol. 4, no. 1, p. 45.
11. Roemer, manuscript folio.
12. Goldstein et al., p. 125.
13. Froome and Essen, p. 1.
14. Cohen, p. 349.
15. (i) Boyer, (ii) Barry Tap, letter, **Ex Nihilo,** vol. 4, no. 4, p. 83.
16. Sarton, pp. 240–65.
17. **Ex Nihilo,** vol. 4, no. 1, p. 45.
18. See Bradley, Froome & Essen, and Edwin Edser, "Light for Students", (Macmillian and Co. Limited, London, 1902), pp. 221f.
19. Froome & Essen, p. 3.
20. **Ex Nihilo,** vol. 4, no. 4, p. 87.
21. Bradley, p. 653.
22. ibid., pp. 652–3.
23. ibid., p. 652.

24. ibid., pp. 651–2.
25. ibid., p. 643.
26. ibid., pp. 654–5.
27. ibid., p. 655.
28. Gheury de Bray, Nature (1927), p. 405; cf. Froome & Essen.
29. Ex Nihilo, vol. 4, no. 4, pp. 77, 82, 83.
30. ibid., p. 79.
31. ibid., p. 85.
32. Gheury de Bray (1927), Froome & Essen.
33. Froome & Essen, p. 6; cf. C.R. O'Dell, "The velocity of light'', Astronomical Society of the Pacific: Leaflets, 9 (1966) no. 402 (Dec. 1962), p. 8.
34. Froome & Essen, p. 6.
35. Gheury de Bray (1927), p. 603; cf. Froome & Essen, pp. 5, 10.
36. Gheury de Bray (1927), p. 603; Froome & Essen.
37. Gheury de Bray (1927), p. 604.
38. I have obtained values in vacuum from values in air by assuming 1.0003 for the refractive index of air. See Froome & Essen for detailed discussion of the complexities involved in making this conversion accurately.
39. Gheury de Bray (1927), p. 603.
40. Newcomb (1891), p. 201; quoted ibid., p. 604.
41. Gheury de Bray (1927), p. 604.
42. Froome & Essen, p. 49.
43. ibid., pp. 49, 136–7.
44. Noted by Morton, Ex Nihilo, vol. 4, no. 4, p. 77. The value which Setterfield gives as "Anderson 1937–41" Froome & Essen give as "Anderson 1937".
45. See note 38.
46. Noted by Morton, loc. cit.

Appendix 1. Setterfield's explanations for the omission of published values of c.

In appendix 1 of Part 1 of his article, Setterfield notes that two values were omitted because they "were obviously anomalous, one far too high, the other far too low by all standards and de Bray comments that the apparatus appears to be faulty".[1] Setterfield also notes that he has not included the results of indirect measurements of c such as parallel wires or the ratio of electromagnetic to electrostatic units.[2] These results were omitted because results separated by a few years varied by thousands of kilometres per second and because the methods by which they were obtained involved other constants and used invalid assumptions.[3] Faced with Morton's criticism that "any data, if properly edited, can support nearly any theory,"[4] Setterfield realized that it is not good enough to say that values were omitted because they were "anamalous" or varied too much. Consequently, in his reply to Morton he emphasizes the second of his original reasons for omitting some values, viz. that they were obtained by faulty equipment or unreliable methods.[5]

With regard to Fizeau's 1849 value, Setterfield claims[6] that this has been criticized by Sanders and by Cohen et al. I assume that the first reference is to "The Velocity of Light" by John Howard Sanders. The only thing I can find in this which looks like criticism is the statement that "the precision of the experiment depends upon the ability of the observer".[7] With regard to Cohen et al., Setterfield says they point out that "the 'experimental conditions of the experiment' left much to be desired and 'were later improved by others with more accurate results.' "[8] (In fact, the words "left much to be desired" are Setterfield's addition, and "by others" is Setterfield's editorial gloss for "by Cornu and by Young and Forbes.")[9] These statements are hardly grounds for Setterfield's claim that Fizeau's equipment was "faulty".[10]

With regard to Foucault's 1862 value, Setterfield claims that this "has been criticized by de Bray and more recently by Edser as being inaccurate due to a problem that existed in the micrometer eyepiece that was used".[11] I assume that the reference to Gheury de Bray is to the report in the 1927 **Science** news item of what he had written in **L'Astronomie.** The relevant statement, which is not even a quotation, is that the "1855 298,000" km/sec result was made "with apparatus which may have been faulty".[12] In a later article, Gheury de Bray says that the unreliability of Foucault's determination "may be estimated from the fact that the deflexion from which it was deduced was only 0.7 millimetres".[13] The reference to Edser seems to come from Tap's letter, in which Foucault's "1860" result is noted as being "possibly inaccurate due to limitations of micrometer eye piece", with a reference to "Edser 1946".[14] Edwin Edser's "Light for Students" was, in fact, first published in 1902. I have not been able to consult later editions, but in the first edition Edser noted, as Gheury de Bray did, that the deflection of the image was only 0.7 mm. However, he went on to say that "the perfection of the design of the optical arrangements" allowed the micrometer eye-piece to be set to within 0.005 mm, so that the error of measurement amounted to 1/300, or at most 1/150.[15] If these comments are accepted, they show that the observational error ascribable to Foucault's value is comparatively large. However, they do not show that systematic error may be ascribed to his value because his equipment was "faulty", as Setterfield claims.[16]

With regard to later values which Setterfield omitted, it may seem that most of these have been obtained by methods which Setterfield has omitted, perhaps because he considered them untrustworthy. Certainly, the omission of the 1950 McKinley and Houstoun values is probably justified given the large spread (500 and 180 km/sec respectively) of the observations on which these values were based.[17] Also, the limits of error of the radar results are comparatively large, and the conversion of values for c in air to values for c in vacuum is always somewhat problematical. However, Setterfield included one radar value in Table 2 — the 1951 Aslakson value of 299,792.4 ± 2.4 (Setterfield actually gave limits of error of ± 5.5 which were derived from the spread of results of 11).[18] The omission of the two spectral lines values is the most serious of these omissions, especially since Setterfield included in Table 2 the 1955 value of 299,792 ± 6 obtained by Plyler, Blaine and Cannon by this method.

NOTES

1. **Ex Nihilo,** vol. **4,** no. 1, p. 45.
2. See K.D. Froome, L. Essen, "The Velocity of Light and Radio Waves", (Academic Press, London, 1969), ch. 1.
3. **Ex Nihilo,** vol. **4,** no. 1, p.45.
4. **Ex Nihilo,** vol. **4,** no. 4, p.77.
5. ibid., p. 79.
6. ibid.
7. J.H. Sanders, "Velocity of Light", (Pergamon Press, Oxford, 1965), p. 5.
8. **Ex Nihilo,** vol. **4,** no. 4, p. 79.
9. Cf. E. Richard Cohen, Kenneth M. Crowe, Jesse W.M. Dumond, "The Fundamental Constants of Physics", (Interscience Publishers, Inc., New York, 1957), p. 106.
10. **Ex Nihilo,** vol. **4,** no. 4, p. 85.
11. ibid., p. 79.
12. "The velocity of light" (Science News), **Science,** 66 (1927) supplements.
13. M.E.J. Gheury de Bray, "The velocity of light", **Nature,** 120, p. 603.
14. **Ex Nihilo,** vol. **4,** no. 4, p. 83.
15. Edwin Edser, "Light for Students", (Macmillan and Co. Limited, London, 1902), p. 228.
16. **Ex Nihilo,** vol. **4,** no. 4, p. 85.
17. Froome & Essen, op. cit., p. 137.
18. ibid.

Appendix 2. Setterfield's references to others who have noticed a decrease in c.

In this appendix, I want to examine Setterfield's references[1] to people who have noticed a decrease in c and who have even calculated the rate of change of c. The importance of these statements which have been made by others is that they appear to lend independent support to the idea that c has been decreasing with time and, hence, to the idea of a young universe. (i) Gheury de Bray says that he first pointed out the "fact" that c was decreasing by 4 km/sec/year in 1924.[2] In his search for evidence to support this idea he discovered that existing tables of determinations of the velocity of light were either inaccurate or incomplete.[3] He set about to remedy this lack in the literature and sent the results of his research to no less than five different journals in three different languages! His historiographical work was of real, but minor, value and was, not surprisingly, generally noticed. However, his opinion that c is decreasing was no more than an uncritical guess.

(ii) I have not been able to examine the letter and article in **Ham Radio** which remark on a change in c since, as mentioned before, this material is not available in Victorian libraries listed in **Scientific Serials in Australian Libraries.**

(iii) Setterfield writes, "In the October 1975 issue of **Scientific American,** p. 120, C.L. Stong questioned if c might change with time as science has failed to get a consistently accurate value". In fact, the reference is to a regular section of **Scientific American** entitled "The Amateur Scientist" which is conducted by C.L. Stong. In the October 1975 issue this section is sub-titled, "An amateur's version of A.A. Michelson's apparatus for measuring the speed of light" and contains a long letter from Sam Epstein, a chemist, describing his experiment. For the record, he obtained a value of 300,318 km/sec. In his concluding comments, Epstein (not Stong) wrote as follows: "Investigators have been attempting to determine the value of c ... for more that 300 years. Its determination to within a few metres per second still eludes the best experiments. In fact, it has been suggested in recent years that nature does not impose an absolutely fixed speed limit on the universe. Does c change with the passage of time?" An unsubstantiated question of an amateur scientist is surely not worth noticing in a serious discussion.

(iv) Setterfield writes, "Pease and Pearson's equipment was sufficiently sensitive to note a change in c over the period (about 1.5 years) of the experiment." In the context of Setterfield's article and without any other explanation of its inclusion, this implies that a steady downward change was noticed. In fact, what the experimenters noticed were fluctuations.[4]

(v) Setterfield writes, "Froome and Essen commenting on the downward trend said that they could give no reason 'to account for these discrepant results'." I took this to mean that Froome and Essen were commenting on a trend evident over a number of different experiments. In fact, the discrepant results referred to are the fluctuations noticed by Pease and Pearson. With regard to these, Froome and Essen point out that "it was the opinion of the authors (i.e. Pease and Pearson, not Froome and Essen) that (experimental errors) could not account for the discrepant results which were obtained."[5]

NOTES

1. **Ex Nihilo,** vol. **4,** no. 1, pp. 39, 45.
2. M.E.J. Gheury de Bray, "The velocity of light" (Letters to the

Editor), Nature, 133 (1934) 464.
3. M.E.J. Gheury de Bray, Nature, 120 (1927) 404, 602.
4. Ex Nihilo, vol. 4, no. 4, p. 85.
5. K.D. Froome, L. Essen, "The Velocity of Light and Radio Waves", (Academic Press, London, 1969), p. 33.

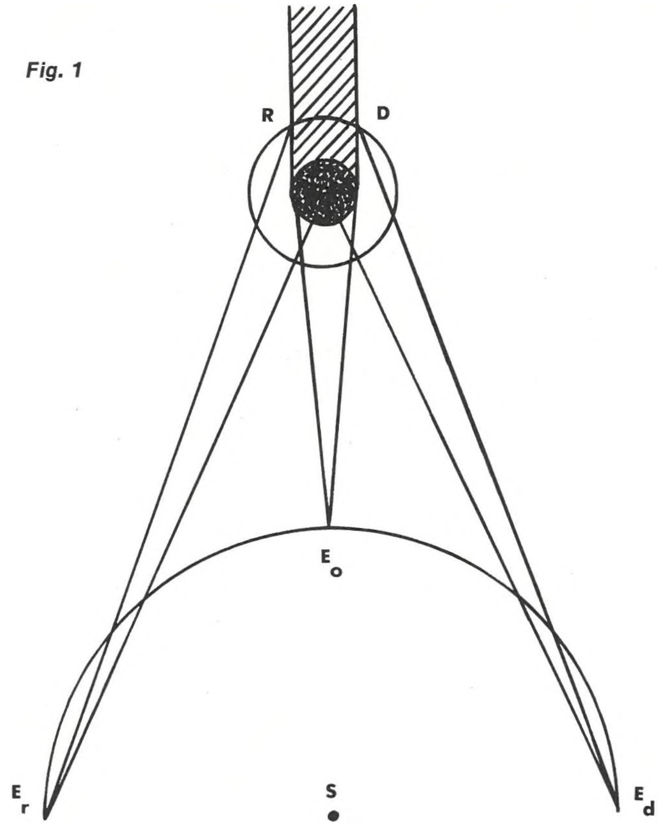
Appendix 3. Roemer's determination of c.

In his reply to Cadusch and Tap, Setterfield defends the value 301,300 ± 200 km/sec for Roemer. He evidently believes that the main thing limiting the accuracy with which Roemer's value could be given has been the accuracy with which the earth's orbit has been known. In fact, it is the timing which is the limiting factor. His comments that "Delambre in 1790 and Martin and Connor in 1951 quoted Roemer's timing identically to the second" and that "the given limits of error allow Roemer's timing to be out by the best part of a second either way"[1] betrays a lack of insight into Roemer's method. (Wherever Delambre and Martin and Connor[2] got their figure of 986 sec from they were not quoting Roemer's timing.) The inadequacy of Setterfield's discussion at this point is not inexcusable, since all references to Roemer's value that I have seen so far either avoid the question of how he obtained his value or suggest an answer which has little relation to what Roemer actually said he did or could have done! However, from a reading of the original papers together with a consideration of the earth's positions in its orbit on the dates of Io used by Roemer and a consideration of how accurately the eclipses may actually be timed, I believe that it is possible to understand what Roemer's method was. This method I will now try to describe.

Four of Jupiter's satellites are easily observable with a small telescope. The innermost of these is called "Io". In Fig. 1 the distances are not drawn to scale in order to illustrate how the disappearance of Io into the shadow cast by Jupiter at D can only be observed as the earth approaches Jupiter from E_d to E_o. Similarly, the reappearance of Io out of the shadow at R can only be observed as the earth moves away from Jupiter from E_o to E_r. When the earth is directly between Jupiter and the sun at E_o, that is, when the earth and Jupiter are in "opposition", Io's disappearance into and reappearance from the shadow are hidden by Jupiter.[3]

Suppose E_1 – E_4 in Fig. 2 are various positions of the earth when eclipses of Io are observed at the times e_1 – e_4. (Although earth's orbit is an ellipse it will be considered here to be a circle.) Suppose J_1E_1 – J_4E_4 is light from Io to the earth when the earth is at the positions E_1 - E_4. (Since the distance to Jupiter is great compared to the radius of the earth's orbit and Jupiter moves slowly compared to the earth, the lines J_1E_1 – J_4E_4 will be considered to be parallel.) Now, as

Fig. 1



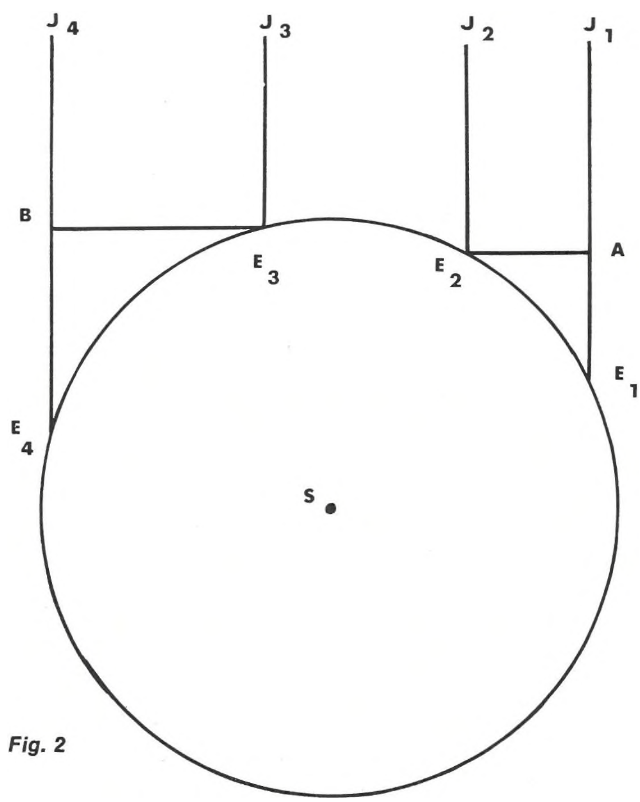


Fig. 2

the earth moves from E_1 to E_2, light from Io takes AE_1/c sec **less** to reach the earth at E_2 than at E_1. Suppose that n_1 eclipses have occurred between e_1 and e_2. The mean period between these eclipses $(e_2 - e_1)/n_1$ is the mean period between all eclipses, T_i (i.e. the mean period of Io's orbit about Jupiter) minus AE_1/cn_1. Similarly, as the earth moves from E_3 to E_4, light from Io takes BE_4/c sec **more** to reach the earth at E_4 than at E_3. Suppose that n_2 eclipses have occurred between e_3 and e_4. The mean period between these eclipses $(e_4 - e_3)/n_2$ is the mean period between all eclipses plus BE_4/cn_2.

ie. $$\frac{e_2 - e_1}{n_1} = T_i - \frac{AE_1}{Cn_1}$$

and $$\frac{e_4 - e_3}{n_2} = T_i + \frac{BE_4}{Cn_2}$$

$$\therefore c\left(\frac{e_4 - e_3}{n_2} - \frac{e_2 - e_1}{n_1}\right) = \frac{AE_1}{n_1} + \frac{BE_4}{n_2} \qquad \text{................ (1)}$$

Now, both AE_1/n_1 and BE_4/n_2 are approximately equal to the diameter of the earth's orbit divided by the number of eclipses of Io (i.e. the number of orbits of Io about Jupiter) as the earth moves through half its orbit. Taking the radius of earth's orbit, r, as 1.496×10^8 km, and the mean period of revolution of Io, T_i, as 1.7698 days[4]:

$$\frac{AE_1}{n_1} \simeq \frac{BE_4}{n_2} \simeq \frac{2r}{0.5\ T_e/T_i}$$

$$= \frac{2 \times 1.496 \times 10^8 \text{km} \times 1.7698 \text{ days}}{0.5 \times 365.25 \text{ days}}$$

$$= 2.9 \times 10^6 \text{ km}$$

Substituting this value for AE_1/n_1 and BE_4/n_2 in equation (1):

$$c\left(\frac{e_4 - c_3}{n_2} - \frac{e_2 - e_1}{n_1}\right) = 5.8 \times 10^6 \text{ km} \qquad \text{................ (2)}$$

This brings me to a consideration of the actual observations on which Roemer based his value. These consist simply of times for the disappearances and reappearances of Io, and these times may be substituted for e_1 - e_4 in equation (2). Since Io passes through a distance equal to its 3,650 km diameter in 3.5 minutes it is possible to observe the time when an eclipse begins or ends within a small fraction of this time.[5] McMillan and Kirszenberg describe what is seen in a small telescope as follows: "(Io) appears initially as a 6th-magnitude star, which fades with increasing rapidity for a minute or two until it become a very faint point of light and disappears. An experienced observer can time the **last speck** to an accuracy of about ± 10 seconds, if conditions are favourable. At a reappearance of (Io), the same phenomena occur in reverse order, and the observer times the **first speck** of light of the brightening satellite".[6] A manuscript of Roemer's lists times for "immersions" and "emersions" of Io.[7] There are over 50 observations, the first made on 22 October, 1668, the last made on 6 January, 1678. However, only the observations for 1672 and 1673 are available in sufficient numbers to enable an estimate of c using equation (2). Indeed, Roemer said in a letter to Huygens that these 1672-3 observations were those he used to obtain his figure of 22 minutes.[8] Goldstein et al. agree with Mayer and Cohen that Roemer's observations were in apparent solar time.[9] A distinction is made in astronomy between apparent solar time and mean solar time because the earth's orbit is an ellipse and the times when the sun is directly north or south are slightly more or less than 24 hours apart. In order that the times of observations made at different points in the earth's orbit may be accurately compared, the apparent solar time is adjusted by means of the so-called "equation of time" to give the mean solar time. Goldstein et al. make this adjustment to 40 observations they chose from Roemer's list by applying the equation of time obtained from Cassini's 1730 tables for the day and year of the observation.[10] Table 3 gives the observations for 1672-3 (column 1) for which adjustments were available from Goldstein et al. (column 2).

Figures 3 and 4 show the positions of the earth and the approximate direction of Jupiter on the days of the listed observations.

The calculations in columns three to six of Table 3 confirm that these observations had an accuracy of no more than ± 10 seconds, and were certainly not accurate to within ± 1 second, as Setterfield suggests. The third column gives the difference between the mean solar times (e_n and e_{n+1}) of one observed eclipse and the next. In column four this difference is divided by the mean period of revolution of Io (TJ, already given above as 1.7698 days. This quotient is taken to the nearest whole number to find the number of eclipses (N_n between the two observed eclipses. In column five, the difference from column three is divided by N_n to find the mean period of the eclipses between the two observed eclipses. If the observed times of the eclipses were accurate, this calculated mean period would slowly decrease or increase as earth was approaching or moving away from opposition with Jupiter. That is, in terms of Fig. 2, AE_1/n_1 decreases as E_1 and E_2 are closer to E_o and BE_4/n_2 increases as E_3 and E_4 are further from E_o (cf. Figs. 3 and 4). As may be seen from column six, this was far from the

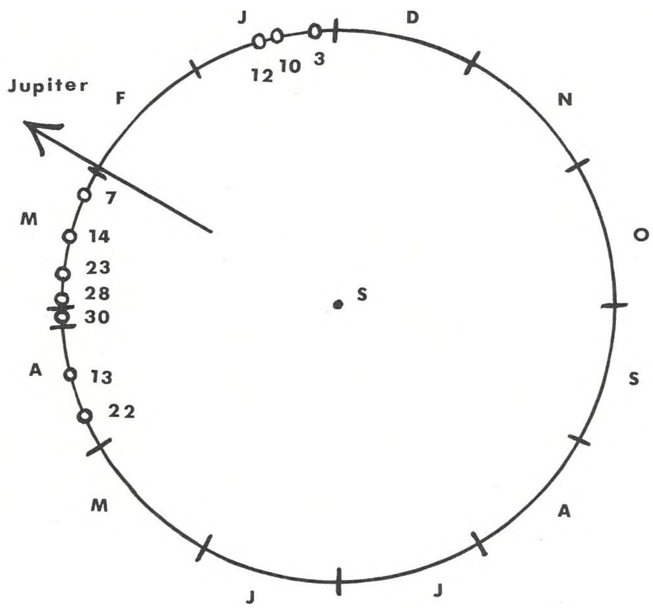


Fig. 3 (1672)

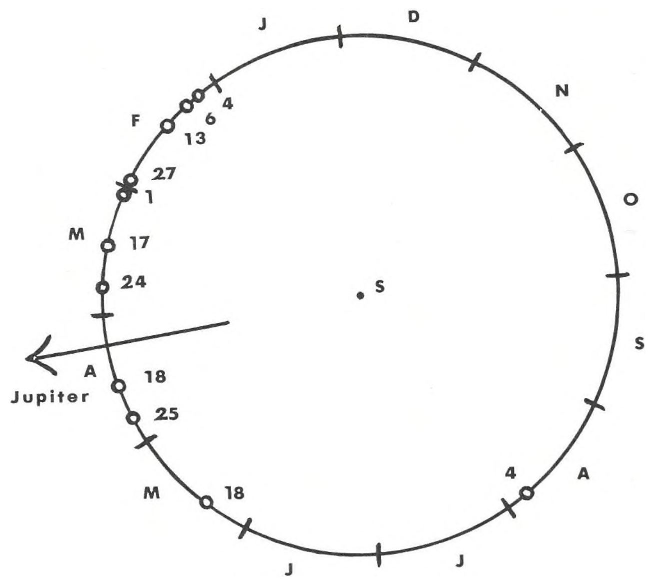


Fig. 4 (1673)

Table 3.

Date	Apparent Solar Time	Mean Solar Time	$e_{n+1} - e_n$	$\frac{e_{n+1} - e_n}{T_i}$	$\frac{e_{n+1} - e_n}{N_n}$	$\frac{e_{n+2} - e_{n+1}}{N_{n+1}} - \frac{e_{n+1} - e_n}{N_n}$
Immersions (Jan. 1672)						x 0.00001
	h m s					
Jan 3	12 42 36	1748.53347	7.07829	3.9995	1.76957	- 24
Jan 10	14 32 14	1755.61176	1.76933	0.9997	1.76933	
Jan 12	8 59 22	1757.38109				
Emersions (Mar. Apr. 1672)						
Mar 7	7 58 25	1812.34008	7.07778	3.9992	1.76945	42
Mar 14	9 52 30	1819.41786	8.84933	5.0002	1.76987	- 7
Mar 23	6 18 14	1828.26719	5.30941	3.0000	1.76980	13
Mar 28	13 45 30	1833.57660	1.76993	1.0001	1.76993	- 3
Mar 30	8 14 46	1835.34653	14.15922	8.0005	1.76990	14
Apr 13	12 8 8	1849.50575	8.85021	5.0007	1.77004	
Apr 22	8 34 28	1858.35596				
Immersions (Feb. Mar. 1673)						
Feb 4	17 31 10	2146.74023	1.77011	1.0002	1.77011	-42
Feb 6	12 0 0	2148.51034	7.07875	3.9997	1.76969	- 15
Feb 13	13 53 20	2155.58909	14.15630	7.9988	1.76954	27
Feb 27	17 40 10	2169.74539	1.76981	1.0000	1.76981	- 13
Mar 1	12 9 1	2171.51520	15.92708	8.9994	1.76968	12
Mar 17	10 28 16	2187.44228	7.07920	4	1.76980	
Mar 24	12 24 30	2194.52148				
Emersions (Apr.—Aug. 1673)						
Apr 18	9 22 0	2219.38970	7.07961	4.0002	1.76990	2
Apr 25	11 18 5	2226.46931	23.00890	13.001	1.76992	8
May 18	11 32 44	2249.47821	77.88018	44.005	1.77000	
Aug 4	8 30 41	2327.35839				

Table 4.

e_1	e_2	n_1	e_3	e_4	n_2	$c(10^5$ km/sec)
1748.53347	1757.38109	5	1812.34008	1858.35596	26	2.1
1748.53347	1757.38109	5	1819.41786	1858.35596	22	1.7
1748.53347	1757.38109	5	1812.34008	1849.50575	21	2.5
2146.74023	2194.52148	27	2219.38970	2327.35839	61	2.2
2146.74023	2187.44228	23	2219.38970	2327.35839	61	2.1
2146.74023	2194.52148	27	2219.38970	2249.47821	17	2.8

case.

Although the individual observations are not very accurate, Roemer realized that by taking e_1 and e_2 as far apart as possible the inaccuracy in the calculated mean period $(e_2 - e_1)/n_1$ is kept as small as possible.

That is, any inaccuracies ($\pm$ x, $\pm$ y) in e_1 and e_2 lead to an inaccuracy in the calculated mean period $(e_2 - e_1)/n_1$ of $(\pm x \pm y)/n_1$. Clearly, as n_1 increases, so the inaccuracy in the calculated mean period tends to decrease. Similar considerations apply to the choice of e_3 and e_4. Nevertheless, there is still a wide range in the values of c obtained by substituting observed values for e_1 - e_4 in equation (2), as the sample results in Table 4 show.

The mean of the sample results in Table 4 (viz. 220,000 km/sec) may be compared with Roemer's conclusion that light requires about 22 minutes to traverse the orbit of the earth[11] (i.e. 3.0×10^8 km/ 22 x 60 sec = 230,000 km/sec). If the close correspondence between these two figures confirms that Roemer reached his conclusion by an averaging of just such results, then it may be concluded from Table 4 that Roemer's published value has limits of error of about $\pm$ 50,000 km/sec. Of course, Roemer never intended to determine c accurately. His achievement was to provide evidence that light is not transmitted instantaneously but has a definite velocity.

Throughout the above discussion I have assumed knowledge of the modern value for the earth's orbit in order to make it clear that this was not the main thing limiting the accuracy of Roemer's value. Even supposing Roemer's method to have been accurate, it is clear from Table 4 that inaccuracies in individual observations meant that a wide range of results ($\pm$ 50,000 km/sec) could be obtained. However, Roemer's method, if it was as I have described, involved certain simplifying assumptions. The most serious of these were that in Fig. 2, the lines J_1E_1 - J_4E_4 are parallel, and AE_1/n_1 and BE_4/n_2 are equal for all positions of E_1 - E_4. For Roemer there was no point in trying to remove these assumptions, even if it were practically possible for him to carry out the necessary calculations. However, in our time there is a point in trying to remove these assumptions (viz. to see whether c was different in Roemer's time) and computers bring the necessary calculations within the range of practical possibility. Given that the positions of the planets and their satellites in Roemer's time can be accurately determined, it is possible to predict when eclipses of Io would have been observed on earth, assuming various values for c, and to compare these with the observations that Roemer used. This is what Goldstein et al. did, and their conclusion was that "the velocity of light did not differ by 0.5% in 1668 to 1678 from the current value."[12]

NOTES

1. **Ex Nihilo,** vol. **4,** no. 4, pp. 86–7.
2. S.L. Martin, A.K. Connor, **Basic Physics,** 2 (Whitcombe & Tombs Pty. Ltd., Sydney, 1969), p. 268.
3. Cf. Robert S. McMillan, John D. Kirszenberg, "A modern version of the Ole Roemer experiment", **Sky and Telescope, 44** (1972), p. 300.
4. On a portion of a Roemer manuscript there are computations giving the mean period of revolution for Io as follows: 1671–2 1d 18h 28m 30s, 1672–3 1d 18h 28m 31s. (I. Bernard Cohen, "Roemer and the first determination of the velocity of light (1676)", **Isis, 31** (1939–40), p. 351; cf. McMillan & Kirszenberg, loc. cit., 1d 18h 29m; K.D. Froome, L. Essen, "The Velocity of Light and Radio Waves," (Academic Press, London, 1969), p. 1, 42h 28m; R.W. Ditchburn, "Light", (Blackie & Son Limited, London, 1952), p. 300, 1.75 d)
5. Ditchburn, loc. cit.
6. McMillan & Kirszenberg, loc. cit.
7. Cohen, op. cit., facsimile XXI.
8. Cohen, op. cit., p. 351.
9. S.J. Goldstein Jr, J.D. Trasco, T.J. Ogburn III, "On the velocity of light three centuries ago", **Astronomical Journal, 78** (1973), p. 123.
10. ibid.
11. Cohen, op. cit., pp. 374–8.
12. Goldstein et al., op. cit., p. 125.

Appendix 4. Some general remarks on what is needed for a critical examination of any determination of c.

In order to examine any of the published values of c, three things are needed: first, an understanding of the method used to make relevant measurements; second, records of the actual measurements made;

and third, an understanding of how the published value and its associated limits of error were obtained from the recorded measurements.

The methods used to make relevant measurements cannot be discussed in general terms, except to point out that the determination of a velocity involves, however indirectly, a measurement of distance and a measurement of time. An example of indirect measurements of distance and time is the way the speed of a car may be read directly from a speedometer, the speedometer having previously been calibrated by measurements of other distances and times.

With regard to the records of the measurements made, it is usually appreciated that these should be accompanied by an estimate of their limits of error. What is less often appreciated is that there is an important distinction between observational error and systematic error. To make the discussion more concrete, instruments for measuring distance and time will be considered, although similar considerations apply to instruments measuring other quantities. It is a common convention to call anything which is used to measure distance a “ruler”.

Some “rulers” (e.g. micrometer) do not look like rulers but until recently the basic unit of length was, in fact, the length of a particular bar of metal. This was the standard ruler. Similarly, it is a common convention to call any instrument which measures time a “clock”. Again, some “clocks” do not look like clocks. Until recently the basic unit of time was the time for one rotation of the earth. This was the standard “clock”. Mechanical clocks were used to divide the unit of time into equal parts.

Observational errors arise in various ways. There is the problem of lining up the start of a ruler with one end of any thing being measured, and the similar problem of starting a clock when any event being timed starts. With a ruler there is the problem of deciding which line on the ruler lines up with the other end of the thing being measured. There is a limit of how finely a ruler can be calibrated and often the end of the thing is fuzzy so that the end comes either on a line or between two lines. Suppose the end is on the 256 mm mark and it cannot be said with any certainty that the thing is exactly 256.000 . . .mm long. The measurer indicates the limits of his uncertainty by giving his measurement as 256.0 ± 0.1 mm, or 256.0 ± 0.5 mm, or 256 ± 1 mm, etc., as the case may be. If there is uncertainty about the lining up at the start of the ruler this should be added to the total uncertainty. With a clock, there is the problem of deciding which line on the clock-face the clock-hand was nearest to when the event being timed finished. With a ruler, if the thing being measured is longer than the ruler there is the problem of making a line at the end of the ruler and moving the start of the ruler to that mark, and so on. This problem, at least, does not have an obvious counterpart in the measurement of time.

Systematic errors are quite different to this kind of error. What is at fault is not the use of the measuring instrument, but the measuring instrument itself. A ruler or clock-face may be irregularly calibrated. A clock may run irregularly. A ruler may be longer or shorter than the standard ruler. A clock may run faster or slower than the standard clock.

With regard to the determinations of c, there have not only been these kinds of problems with individual measurements, but the scatter of measurements has often been greater than the estimated limits of error. A scatter like this may be taken to reflect actual variations in the speed of light during the course of an experiment, such as the variations which occur as a result of variations in atmospheric pressure and temperature. However, a scatter like this may also be the result of unsuspected systematic errors, such as the fluctuating base-length in Michelson, Pease and Pearson’s experiment, which Setterfield has mentioned (**Ex Nihilo,** vol. **4,** no. 4, p. 85).

Appendix 5. An alternative proposal for avoiding the conclusion of the light travel-time argument.

Like Setterfield, Harold Slusher accepts that the galaxies are as far away as they appear to be.[1] Unlike Setterfield, he does not question the assumed constancy of the speed of light. However, Slusher attempts to avoid the conclusion of an old universe by means of an idea proposed by Moon and Spencer.[2] This idea is that the stars are where they appear to be in Euclidean space, but their light reaches the earth by travelling on Riemannian surfaces.[3]

Newman and Eckelmann have criticized this idea[4], and my criticism follows theirs except for the details. As I understand the theory of Riemannian geometry, if space were Riemannian with a radius of curvature of five light years, as Moon and Spencer suggest[5], then when light from any star had travelled for 15.71 (i.e. 5π) light years in almost any direction it would return to the star from the opposite direction.[6] Only when it travelled in those comparatively few directions in which it met another star or a planet would it not return to the star. Therefore, an astronomer would be able to see most stars in two exactly opposite directions. If one image of such a star appeared to be x light years away, the other image would appear to be 15.71 - x light years away. A very few stars would be hidden in one or both directions. Considering first the stars, the night sky in this hypothetical universe would not be all that different from the observed night sky, although when observed through a telescope it would be much less thinly populated and when studied closely it would be found that no star was more than 15.71 light years away. However, the night sky in this hypothetical universe would have one very curious feature. There would be an image of the sun in exactly the opposite direction to the day-time sun. This night-time sun would be much less bright than the day-time sun because its light would have travelled almost 15.71 light years, but it would subtend the same angle at the earth as the day-time sun. Clearly, the universe is not like this. If it is a Riemannian space it has a radius of curvature of at least several thousands of millions of light years.[7]

NOTES

1. Harold S. Slusher, "Age of the Cosmos" (ICR Technical Monograph No. 9), Institute for Creation Research, San Diego, 1980, pp. 25–32.
2. Parry Moon, Domina E. Spencer, Journal of the Optical Society of America, Aug. 1953.
3. Slusher, op. cit., pp. 35–7.
4. Robert C. Newman, Herman J. Eckelmann, Jr, "Genesis One and the Origin of the Earth," InterVarsity Press, Downers Grove, Illinois, 1977, pp. 19–20.
5. Slusher, op. cit., p. 36.
6. Cf. David Bergamini and the Editors of Life, "The Universe," Time-Life International (Nederland) N.V., 1964, p. 174.
7. ibid., pp. 182f.