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# Further Towards a Critical Examination of Setterfield's Hypothesis

VIVIAN E. BOUNDS

The author's earlier article[1] was mainly concerned to check the references and figures which Barry Setterfield had given in the article[2] in which he first argued that 'light has slowed down exponentially since the time of the creation.'[3] I found that Setterfield's Tables 1 and 2 in that initial article were deficient in a variety of ways. After a similar check of Setterfield's work, Holt has similarly concluded that there were *'significant discrepancies between his articles and his references'* and that he was not *'thorough in his examination of the literature, rarely going to the original sources'*.[4]

## GOLDSTEIN'S WORK

The other main point which I made in my article, apart from the deficiency of Setterfield's evidence, was that Goldstein, Trasco, and Ogburn's analysis[5] of the data on which Roemer based his 1675 value for c confirms that c did not differ by 0.5% in 1668 to 1678 from the current value,[7] and, as Fackerell explained,

> *'If there was no variation from the current value of c back in 1668 to 1678, when the deviation from the current value would have been largest, then there are very good grounds for rejecting* (Setterfield's) *theory.'*[8]

In his replies[9,10] to Fackerell's article and to my article, Setterfield raised an important question with regard to Goldstein *et al.'s* method. As Setterfield said,[11] I had assumed that the actual phase of Io was projected back over 300 years,[12] but as Setterfield pointed out,[13] this was not the case. Goldstein *et al.* had said in their article that they found the mean daily motion of Io empirically[14] (i.e. by trial and error). Indeed, there were a number of constants (viz. the derived constants (those not marked with superscript 'a') listed in their Table III) which they found empirically. That is, they adjusted these derived constants so that the root-mean-square difference between the calculated and observed times was minimized.[15] The question which Setterfield raised was whether they readjusted these constants as they varied the light travel time. Setterfield claimed that they had failed to do so,[16] but whether or not they had failed to do so depends on what they meant when they said that *'the same calculation was performed with the velocity of light perturbed in five steps of 0.5% in both directions from the value listed in Table III'*.[17] Did they mean, as Setterfield claims, that when they performed 'the same calculation' with the velocity perturbed in five steps of 0.5% in both directions from the value listed in Table III they used the same values for the derived constants as those which they used when they performed the calculation with the velocity of light at the current value? Or did they mean that they allowed the derived constants to change each time they performed 'the same calculation' with the velocity of light perturbed in five steps of 0.5% in both directions?

In order to resolve this question, I wrote to Goldstein in April 1985 asking him if he and his associates had, in fact, adjusted the derived constants each time they performed the calculation. Goldstein replied in February 1986 as follows:

> *'The criticism in your April 1985 letter of our paper*[18] *is well founded. For each value of the velocity of light, one ought to find the best-fitting constants, and the best velocity of light is the one that gives the smallest mean-squared-difference between the mathematical model and the observed times of eclipse. Only the velocity of light was perturbed in our paper.*
>
> *I have now repeated the calculations, this time with a more realistic model and 10 more observations (50 altogether). (Interested readers should see Astronomical Journal, 80.532*[19] *for details of the model and the observations.) This time for each value of the velocity of light, I solve for four constants (Io's mean longitude, mean motion, and the sine and cosine amplitudes of its interaction with Europa) and calculate the residual difference each time.*
>
> *Again it is the light travel time over the earth's mean orbital radius that is being tested. I find that the minimum residuals occur at a light travel time 2.6% lower than the presently accepted value. The formal uncertainty is ±1,8%.'*[20]

On this basis, the velocity of light in 1668 to 1678 was 307,600 ±5,400 km/sec. Therefore, it may no longer be claimed that Goldstein *et al.'s* analysis of Roemer's data confirms that c has been constant through time. However, although this new value is consistent with a past decrease in c, it does not give significantly greater support to Setterfield's hypothesis (which predicts a value of

301,400 km/sec for this time) than to the hypothesis that c has been constant.

## STATISTICAL ANALYSES OF PAST C VALUES

If the question of whether the velocity of light has decreased cannot be resolved on the basis of a single past value for c, can it be resolved by a statistical analysis of all past values for c such as that which Setterfield has attempted? There are at least three possible levels through which such an analysis might proceed. At the first level, a list of past values of c as given in some secondary source would be statistically analysed. At the second level, a full literature search would be carried out, copies of, or access to, all the relevant primary sources would be obtained, and the resulting list of published values would be statistically analysed. At the third level, all published values of c would be critically examined in order to assess their reliability as records of c at the times the various measurements were made. Then, having corrected the published values where necessary and having weighted them (or omitted them altogether) according to their reliability, the statistical analysis could be repeated.

The problem with Setterfield's work is that he has not carried his analysis through at a particular level. On the other hand, he might have referred only to secondary sources if he had given all the values in the secondary sources which he used. On the other hand, he might have corrected or omitted published values if he had critically examined all the published values. However, Setterfield has done neither of these things consistently. It may be asked, therefore, what results are obtained if these things are done consistently. In this paper, I will analyse all the past values of c given in a secondary source and then give what information I have which has not previously been published in Ex Nihilo or the Ex Nihilo Technical Journal about past values of c and about critical examiaations of these values.

## ANALYSIS OF FROOME AND ESSEN'S C VALUES

First, what result is obtained from an analysis of all the values of c given in Setterfield's main secondary source, EC. D. Froome and L. Essen, The Velocity of Light and Radio Waves (Academic Press, London, 1969). Froome and Essen summarise the values which have been obtained for c in four tables.[21] In my Table 1 I have collated all these values, with the following exceptions. First, I have adjusted the three asterisked radar values in their Table 1, chapter 11, from values in air to values in vacuum by multiplying by 1.0003, the refractive index of air. Second, I have given only the 1958 correction as Froome's 1954 value and the 1967 correction as Karolus's 1966 value.

Now it cannot be said of the values in my Table 1, as Setterfield says of the values in his Table 1, that 'the drop in c is very evident.'[22,23] What is evident, if anything, is that all the values are close to 300,000 km/sec. A closer examination shows that the range of values has decreased over the years, so that it is possible to select values which show a decrease over the years. However, if all the values in each successively smaller range are considered (see Table 2), the mid-point of each range and the median of the values within each range are found to be close to the present value. If anything, this examination shows that c has gradually increased!

Although Setterfield claimed that *'the drop in c is very evident'* he did not claim that there was always a decrease from one value to the next. However, he did claim that when

> *'the values in* (his) *Table 1 were grouped according to their method of measurement so that the general downward trend was not masked by different means of measurement with different built-in biases . . . the value of c was still obviously decreasing with time'* *'without exception.'*[24]

This is a reasonable procedure, but when it is applied to Froome and Essen's values (see Table 3) there are exceptions. Indeed, the only means of measurement which have, according to the values in Froome and Essen, given values in which there is always a decrease from one value to the next are the geodimeter, quartz modulator, and tellurometer.[25]

Even if there is no obvious decrease in the values as a whole or in the values given by each particular means of measurement, what of Setterfield's claim that statistical analysis of past values for c supports his hypothesis?[26] The problem with Setterfield's work at this point is that his method of analysis, using the coefficient of determination, $r^2$, does not allow a comparison to be made between his hypothesis and the hypothesis that the velocity of light has been constant. He says that, for the coefficient of determination,

$$r^2 = \frac{\Sigma(\hat{y} - \bar{y})^2}{\Sigma(y_i - \bar{y})^2}$$

where $\hat{y}$ = predicted value, $\bar{y}$ = mean value, and $y_i$ = observed value.[27] This may be all right when the predicted value varies, but when the predicted value is constant,

$$\Sigma(\hat{y} - \bar{y})^2 = 0$$

and $r^2$ is indeterminate. Therefore, instead of comparing predicted values and observed values by calculating the coefficient of determination, I have calculated the root-mean-square, that is, square root of the mean of the squares, of the differences between the predicted values and the observed values, for various groups of values (see Table 4). The predicted values adopted for Setterfield's hypothesis are given in Table 1, and the predicted value adopted for the hypothesis that c has been constant is 299,792.5 km/sec.

These results show that, with regard to the values listed by Froome and Essen, the hypothesis that c has been constant is better supported than Setterfield's hypothesis, firstly, by all values, secondly, by the values in each successive period, and thirdly, by eight out of the twelve methods of determining c for which there are more than one value. The only groups of values which provide better support for Setterfield's hypothesis are those obtained by the toothed wheel method, by Michelson, by geodimeter, by radio interferometer, and by tellurometer.

The above analysis of all the values listed in Froome and Essen's tables is only the first level at which an analysis of all past values for c may proceed. In order to proceed to the second level of analysis, a full literature search would need to be carried out. In my first paper I took some steps towards this by checking Setterfield's

| Date | Published value (km/sec.) | Predicted value (km/sec.) | Date | Published value (km/sec.) | Predicted value (km/sec.) |
|---|---|---|---|---|---|
| 1676 | 214,000 | 301,400 | 1926 | 299,796 | 299,815 |
| 1726 | 301,000 | 300,900 | 1928 | 299,778 | 299,813 |
| 1849 | 315,000 | 300,045 | 1935 | 299,774 | 299,805 |
| 1857 | 310,800 | 300,005 | 1937 | 299,771 | 299,803 |
| 1862 | 298,000 | 299,983 | 1940 | 299,768 | 299,800 |
| 1868 | 284,300 | 299,963 | 1941 | 299,776 | 299,799 |
| 1869 | 280,900 | 299,960 | 1947 | 299,792 | 299,795.8 |
| 1872 | 298,500 | 299,950 | 1947 | 299,785 | 299,795.8 |
| 1874 | 300,400 | 299,943 | 1947 | 299,777 | 299,795.8 |
| 1874 | 289,700 | 299,943 | 1949 | 299,792.4 | 299,794.9 |
| 1878 | 300,140 | 299,930 | 1949 | 299,796 | 299,794.9 |
| 1879 | 299,910 | 299,927 | 1949 | 299,791 | 299,794.9 |
| 1879 | 296,100 | 299,927 | 1950 | 299,792.5 | 299,794.4 |
| 1880 | 295,600 | 299,923 | 1950 | 299,793.1 | 299,794.4 |
| 1882 | 299,810 | 299,917 | 1950 | 299,780 | 299,794.4 |
| 1882 | 299,853 | 299,917 | 1950 | 299,775 | 299,794.4 |
| 1883 | 296,400 | 299,913 | 1950 | 299,789.3 | 299,794.4 |
| 1884 | 302,000 | 299,910 | 1951 | 299,793.1 | 299,794.1 |
| 1888 | 301,000 | 299,897 | 1951 | 299,794.2 | 299,794.1 |
| 1889 | 300,500 | 299,893 | 1951 | 299,792.6 | 299,794.1 |
| 1889 | 300,090 | 299,893 | 1952 | 299,776 | 299,793.7 |
| 1890 | 299,690 | 299,890 | 1954 | 299,792.75 | 299,793.2 |
| 1891 | 301,010 | 299,888 | 1954 | 299,789.8 | 299.793.2 |
| 1891 | 297,600 | 299,888 | 1954 | 299,795.1 | 299,793.2 |
| 1892 | 299,220 | 299,885 | 1955 | 299,792.4 | 299,792.9 |
| 1895 | 300,300 | 299,878 | 1955 | 299,792 | 299,792.9 |
| 1897 | 300,190 | 299,873 | 1956 | 299,792.9 | 299,792.8 |
| 1897 | 299,700 | 299,873 | 1956 | 299,792.7 | 299,792.8 |
| 1898 | 299,870 | 299,870 | 1956 | 299,791.9 | 299,792.8 |
| 1899 | 301,000 | 299,868 | 1956 | 299,792.4 | 299,792.8 |
| 1899 | 299,100 | 299,868 | 1956 | 299,792.2 | 299,792.8 |
| 1907 | 299,788 | 299,848 | 1957 | 299,792.6 | 299,792.6 |
| 1908 | 299,901 | 299,846 | 1958 | 299,792.5 | 299,792.5 |
| 1923 | 299,795 | 299,819 | 1966 | 299,792.44 | 299,792.5 |
| 1924 | 299,802 | 299,818 | 1967 | 299,792.56 | 299,792.5 |

***Table 1.*** *PUBLISHED VALUES FOR THE VELOCITY OF LIGHT GIVEN BY FROOME AND ESSEN ARRANGED IN SEQUENCE ACCORDING TO DATE WITH VALUES PREDICTED BY SETTERFIELD'S HYPOTHESIS FOR EACH DATE.*

| Period | Mid-Point (km/sec.) | Range (km/sec.) | Median (km/sec.) |
|---|---|---|---|
| 1726–1874 | 298,000 | ±17,000 | 298,500 |
| 1878–1908 | 298,800 | ± 3,200 | 299,870 |
| 1923–1954 | 299,785 | ± 17 | 299,791.5 |
| 1955–1967 | 299,792.4 | ± 0.5 | 299,792.44 |

***Table 2.*** *RANGE, MID-POINT, AND MEDIAN OF PUBLISHED VALUES IN TABLE 1 GROUPED IN SUCCESSIVE PERIODS.*

references, by listing some of the primary sources mentioned in the secondary sources which I consulted, and by referring to relevant secondary sources. Since then I have done little else towards this, but what little else I have I offer here.

## ROEMER AND BRADLEY-TYPE EXPERIMENTS

In his replies to Fackerell's paper and to my first paper, Setterfield presented some new information concerning past values of c obtained by Roemer-type experiments and Bradley-type experiments.[28] I have only checked one of his sources for these values, viz. the article in Nature, May 13, 1886, entitled, 'The Velocity of Light'. According to an introductory editorial note, this article was a reproduction of an historical notice with which Newcomb commenced a recently published reinvestigation of the velocity of light.[29]

With regard to the time required for light to pass over a distance equal to the radius of the earth's orbit as found from observations of the eclipses of the first satellite of Jupiter, Newcomb says,

> *'In 1809 it was fixed by Delambre at 493.2s., from an immense number of observations of eclipses of Jupiter's satellites during the previous 150 years.'*[30]

Newcomb also writes,

> *'In 1875, Glasenapp, then of Pulkowa, from a discussion of all available eclipses of Jupiter's first satellite between 1848 and 1870, showed that results between 496s. and 501s. could be obtained from different classes of these observations by different hypothesis.'*[31]

Therefore, Setterfield has accurately quoted these results as quoted by Newcomb, but the information which he omitted concerning Glasenapp's results, viz. that he obtained them *'from different classes of these observations by different hypothesis'*, casts doubts on whether these results may be averaged as Setterfield does.[32]

With regard to the constant of aberration Newcomb says,

> *'Struve's value, 20".445, determined in 1845 from observations with the prime vertical transit of Pulkowa, has been the standard up to the present time. The recent determination of Nyrén, being founded on a much longer series of observations than those made by Struve, and including determinations with several instruments, must be regarded as a standard at present. His result is: Definitive value of the constant of aberration = 20".492 ± 0".006.'*[33]

Again, Setterfield has accurately quoted these results. The limits of error which he gives for Struve were, presumably, given by Romanskaya.[34]

While all this is of interest, it is of little value unless these values can be checked and critically examined on the basis of primary sources. Cornu gives the same value for the *'temps moyen'* (i.e. mean time) which Delambre obtained from *'la discussion de mille éclipses'*.[35] Cornu gives as his source. Tables écliptiques des satellites de Jupiter, Introduction, p. vii, but Newcomb says that *'not a trace of Delambre's investigation remains in print, and probably not in manuscript'*[36] since Newcomb *'could find no remains of this investigation among Delambre's papers at the Paris Observatory'*.[37] Newcomb concludes that, given this, *'it is impossible to subject Delambre's investigation to any discussion'*.[38] Froome and Essen give a date of 1790 and a value of 986 sec., but do not give their source.[39]

With regard to his source for Glasenapp's value, Newcomb says,

> *'This paper of Glasenapp's was published only in the Russian language as an inaugural dissertation, and in consequence has never become generally known.'*[40]

Froome and Essen give a date of 1874 and a value of 1001.6 sec., but again do not give a source.[41]

Cornu gives the same value for Struve[42] as Newcomb does, but neither give a source for this value. Newcomb gives as his source for Nyrén's values, Mémoires de l'Académie Impériale des Sciences de St. Pétersbourg, vii. serie, tome xxxi. No. 9.[43]

As well as asking about the primary sources for Setterfield's new values, in order that they may be checked and critically examined, it must also be asked whether any other values of c have been obtained by Roemer-type experiments and Bradley-type experiments. Indeed, there must have been many observations of Jupiter's satellites and of the aberration of light since Roemer and Bradley. For example, Houstoun in a work published in 1930, says,

> *'The value for the aberration constant adopted at*

| Method | Date | Author | Value (km/sec.) | Limits of error (km/sec.) |
|---|---|---|---|---|
| Jupiter's satellites | 1676 | Roemer | 214,000 | |
| Aberration of stars | 1726 | Bradley | 301,000 | |
| Toothed wheel | 1849 | Fizeau | 315,000 | |
| | 1872 | Cornu | 298,500 | ±900 |
| | *1874 | Cornu | 300,400 | ±800 |
| | 1908 | Perrotin and Prim | 299,901 | ± 84 |
| Ratio of electromagnetic to electrostatic units | 1857 | Weber and Kohlrausch | 310,800 | |
| | 1868 | Maxwell | 284,300 | |
| | 1869 | Thomson and King | 280,900 | |
| | 1874 | McKichan | 289,700 | |
| | 1879 | Ayrton and Perry | 296,100 | |
| | 1880 | Shida | 295,600 | |
| | 1883 | Thomson, J. J. | 296,400 | |
| | 1874 | Klemencic | 302,000 | |
| | 1888 | Himstedt | 301,000 | |
| | 1889 | Thomson, W. | 300,500 | |
| | 1889 | Rosa | 300,090 | |
| | 1890 | Thomson, J. J. and Searle | 299,090 | |
| | 1891 | Pellat | 301,010 | |
| | 1892 | Abraham | 299,220 | |
| | 1897 | Hurmuzescu | 300,190 | |
| | 1898 | Perot and Fabry | 299,870 | |
| | 1899 | Lodge and Glazebrook | 301,000 | |
| | 1907 | Rosa and Dorsey | 299,788 | ± 30 |
| Deflection of light by rotating mirror | 1862 | Foucault | 298,000 | ±500 |
| | 1878 | Michelson | 300,140 | ±700 |
| | 1879 | Michelson | 299,910 | ± 50 |
| | 1882 | Newcomb | 299,810 | ± 30 |
| | 1882 | Michelson | 299,853 | ± 60 |
| | 1924 | Michelson | 299,802 | ± 30 |
| | 1926 | Michelson | 299,796 | ± 4 |
| | 1935 | Michelson, Pease, and Pearson | 299,774 | ± 11 |
| Lecher wires | 1891 | Blondlot | 297,600 | |
| | 1895 | Trowbridge and Duane | 300,300 | |
| | 1897 | Saunders | 299,700 | |
| | 1923 | Mercier | 299,795 | ± 30 |
| Free space | 1899 | MacLean | 299,100 | |
| Kerr cell | 1928 | Karolus and Mittelstaedt | 299,778 | ± 20 |

| Method | Date | Author | Value (km/sec.) | Limits of error (km/sec.) |
|---|---|---|---|---|
| | 1937 | Anderson | 299,771 | ± 12 |
| | 1940 | Hüttel | 299,768 | ± 10 |
| | 1941 | Anderson | 299,776 | ± 14 |
| Cavity resonator | | | | |
| | 1947 | Essen and Gordon-Smith | 299,792 | ± 3 |
| | 1950 | Essen | 299,792.5 | ± 1 |
| | 1950 | Hansen and Bol | 299,789.3 | ± 0.8 |
| Radar | 1947 | Smith, Franklin and Whiting | 299,785 | ± 50 |
| | 1947 | Jones | 299,777 | ± 25 |
| | 1949 | Aslakson | 299,792.4 | ± 2.4 |
| | 1949 | Jones and Cornford | 299,791 | ± 25 |
| | 1951 | Aslakson | 299,794.2 | ± 1.4 |
| Geodimeter | 1949 | Bergstrand | 299,796 | ± 2 |
| | 1950 | Bergstrand | 299,793.1 | ± 0.26 |
| | 1951 | Bergstrand | 299,793.1 | ± 0.4 |
| | 1955 | Schöldström | 299,792.4 | ± 0.4 |
| | 1956 | Edge | 299,792.4 | ± 0.11 |
| | 1956 | Edge | 299,792.2 | ± 0.13 |
| Quartz modulator | 1950 | McKinley | 299,780 | ± 70 |
| | 1950 | Houstoun | 299,775 | ± 9 |
| Radio interferometer | 1951 | Froome | 299,792.6 | ± 0.7 |
| | 1954 | Froome | 299,792.75 | ± 0.3 |
| | 1954 | Florman | 299,795.1 | ± 3.1 |
| | 1958 | Froome | 299,792.5 | ± 0.1 |
| Spectral lines | 1952 | Rank, Ruth and Vanden Sluis | 299,776 | ± 6 |
| | 1954 | Rank, Shearer and Wiggins | 299,789.8 | ± 3 |
| | 1955 | Plyler, Blaine and Cannon | 299,792 | ± 6 |
| | 1956 | Rank, Bennett and Bennett | 299,791.9 | ± 2 |
| Tellurometer | 1956 | Wadley | 299,792.9 | ± 2.0 |
| | 1956 | Wadley | 299,792.7 | ± 2.0 |
| | 1957 | Wadley | 299,792.6 | ± 1.2 |
| Modulated light beam | 1966 | Karolus | 299,792.44 | ± 0.2 |

***Table 3.*** *VALUES IN TABLE 1 ARRANGED ACCORDING TO METHOD OFDETERMINATION.*
*Note: Method of Simkin, Lukin, Sikora, and Strelenskii's 1967 value not specified.*
**Method of Cornu's 1874 value specified as 'Deflection of light' in Froome and Essen's Table 1 (p. 10) but, correctly, as 'toothed wheels' in their text (p. 5)*

| Group of published values | Number of values in group (n) | $\sqrt{\frac{(v_o - v_p)^2}{n}}$ | |
|---|---|---|---|
| | | $v_p$ = 299,792.5 km/sec. | $v_p$ = predicted value listed in Table 1. |
| All values (1676–1967) | 70 | 11,010 | 11,190 |
| 1726–1874 | 9 | 10,842 | 10,853 |
| 1878–1908 | 23 | 1,599 | 1,627 |
| 1923–1954 | 26 | 10.9 | 16.9 |
| 1955–1967 | 11 | 0.29 | 0.47 |
| Toothed wheel | 4 | 7,640 | 7,520 |
| Ratio of units | 18 | 6,959 | 7,011 |
| Rotating mirror | 8 | 647 | 706 |
| Michelson | 6 | 152 | 91 |
| Lecher wires | 4 | 1,126 | 1,167 |
| Kerr cell | 4 | 20 | 31 |
| Cavity resonator | 3 | 1.9 | 3.8 |
| Radar | 5 | 7.8 | 9.9 |
| Geodimeter | 6 | 1.48 | 0.88 |
| Quartz modulator | 2 | 15.2 | 17.1 |
| Radio interferometer | 4 | 1.31 | 1.23 |
| Spectral lines | 4 | 8.4 | 9.0 |

***Table 4.*** *ROOT-MEAN-SOUARES OF THE DIFFERENCES BETWEEN PUBLISHED VALUES ($V_o$) AS LISTED IN TABLE 1 AND PREDICTED VALUES ($V_p$) FOR VARIOUS GROUPS OF PUBLISHED VALUES AND FOR TWO HYPOTHESES.*

*present as a result of observations is 20".47.*'[44]

This suggests a greater variation in values for the constant of aberration than is shown by the values in Setterfield's list.

## SECONDARY SOURCES

With regard to secondary sources relevant to the analysis of past values of c, the following may be noted:[45]

Cohen, E. R., 1973. Light, Velocity of. **Encyclopaedia Britannica,** 14th edition, 1973 printing, vol. 13, pp. 1129–1133.

Essen, L., **1956.** The velocity of light. **Endeavour,** 15:87–91.

Jaffe, Bernard, 1960. **Michelson and the speed of light.** Reviewed: **Science,** 133:1472–1473 (1961); **Isis,** 53:426–428 (1962) and **Physics Today,** 15(1):72–74 (1962).

O'Dell, C. R., 1966. The velocity of light. **Astronomical Society of the Pacific: Leaflets, 9,** No. 402, Dec. 1962, 8pp.

Pla, Cortés, 1948. Sobre la constancia de la velocidad de la luz. **Revista de Ciencias** (Lima), 50:21–26.

Pla, Cortés, 1948. **Velocidad de la luz** y **relatividad.** Reviewed: **Arch. Int. Hist. Sci.,** 1:740–741 (1948)

Also, O'Dell mentions a *'review article (**Handbuch der Physik**, Band XXIV) by Bergstrand who applied small corrections to some of the original results and recalculated the probable errors of observation'*.[46]

The above is all that I have to add towards the second level of analysis. In order to proceed to the third level of analysis a critical examination of all published values would need to be carried out. The only steps I took towards this in my first paper were, firstly, to examine Roemer and Bradley's original papers and attempt to understand the methods by which they obtained their values, and secondly, to check some of Setterfield's explanations for his omission of published values of c. Since the publication of my first paper I have written to Goldstein (as I have already explained), I have reconsidered my description of Roemer's method (see Appendix 2), I have checked some more of Setterfield's explanations for his inclusion and omission of published values of c (see Appendix 1), and I have located a copy of Dorsey's monograph.

## DORSEY'S MONOGRAPH

The copy of Dorsey's monograph which I have located is in the Australian National University Library, in the collection in the J. G. Crawford Building. The whole paper deserves close study, and since it is not generally accessible in Australia, I will give quotations concerning his object, his conclusions with regard to the work of the various experimenters, and his general conclusion, rather than attempting to summarize what he says.

His object: *'As is well known to those acquainted with the several determinations of the velocity of light, the definitive values successively reported—those values which the several observers give as defining or summing up the result of the experimental work being reported—have, in general, decreased monotonously from Cornu's 300.4 megameters per second in 1874 to Anderson's 299.776 in 1940, the monotony being severely broken by the presence of Perrotin and Prim's 299.90 of 1902, between the adjacent values by Michelson — 299.853 in 1882 and 299.802 (first published as 299.820) in 1924. In how far is either this drift or its interruption of physical significance? That is in dispute, some holding one view, and others the opposite. In this paper an answer to that question is sought.*

*The earlier view, still held by most experienced experimental physicists, is that the drift is of no physical significance, and that the break in it is to be sought in the low precision of Cornu's and of Perrotin and Prim's work, and perhaps in some common systematic error, those two determinations having been made by the same method (Fizeau's, not used in any of the others) and largely by the use of the same apparatus, and carried out in the same manner . . .*

*The later view, apparently first published by Gheury de Bray in 1926, is that both the drift and its break are of prime physical significance, indicating that the velocity of light is subject to secular variations, presumably arising from changes in the space medium in which the earth finds itself from time to time.'*[47]

*'In order to evaluate satisfactorily the strength of the foundation on which the suggestion of such a secular variation rests, it is necessary to go behind the inferences of the several experimenters and to see how far those inferences are justified by the experimental work. That demands in each case a study of the method employed, of the means adopted for the realization of the method, of the systematic errors that might be expected to affect the results, of the author's diligence in searching out and eliminating such errors, of the degree of concordance of the observations, and of the procedure by which the author derived his definitive value from the experimental data. In every case it is the objective value of the work that is to be independently appraised.*

*The present paper is a report of such a study . . .'*[48]

Fizeau's work: *'The meagerness of the report makes it impossible to estimate the dubiety of this value [of 315 megameters per second], but it may be expected to be great, this being but a first attempt to use this method.'*[49]

Foucault's work: *'There is nothing that will enable one to estimate the minimum dubiety of his reported result: Velocity of light in air = 298 megameters per second. His estimated uncertainty is 0.5 megam./sec.; it seems to be decidedly too small.'*[50]

Cornu's work of 1872: *'One may conclude that his observations indicate that the velocity of light in vacuum may lie within the range 296.5 to 300.5 megameters per second.'*[51]

Cornu's work of 1874: *'His definitive value is entirely untrustworthy.'* Therefore, *'it becomes necessary to derive another from his observations.'*[52] *'[O]ne may conclude that the velocity of light in a vacuum = 299.9 megameters per second, dubiety at least = ±0.6 megameters per second. That is, the velocity of light in a vacuum seems to lie near or within the range 299.3 to 300.5 megam./sec. If the computed value is not seriously affected by systematic errors, then the velocity probably lies nearer 300 than either 299 or 301, but the data do not justify one in assuming the absence of such errors.'*[53]

Perrotin and Prim's work: *'[Their definitive] value is totally unworthy of confidence.'*[54] *'[N]o statement more specific than the following is justified by the data: It is probable that the velocity of light lies between 298 and 302 megameters per second, and it may be closer to their mean (300) than to either 299 or 301; but the obvious presence of systematic errors of unidentified origin throws serious doubt on the validity of taking the mean as the best representation of the whole. The dubiety arising from discordance alone is at least 0.6 megam./sec.'*[55]

Newcomb's work: *'Newcomb made three distinct series of determination.'*[56] *'[I]t is concluded that Newcomb erred in selecting the result of series 3 as the proper representation of the outcome of his work. All that he was justified in saying was that his results for the velocity of light in vacuo ranged from 299.71 to 299.86 megam./sec., and were obviously affected by systematic errors of unknown sign and magnitude. The presence of such systematic errors makes it improper to present any kind of average of the values found in the three series as being more reliable than the individual value given by any one series.'*[57]

Michelson's work of 1878: *'The data do not justify a statement more exact than this: The observed values range from . . . 297 to 304 megam./sec., and seem to indicate that the correct value probably lies nearer to . . . 300 than to either 299 or 301 megam./sec.'*[58]

Michelson's work of 1879: *'The best he would have been justified in claiming for the work would have been this: Velocity of light in a vacuum—299.9 megam./sec. Dubiety at least—±0.2 megam./sec. That is, the velocity of light in a vacuum may lie between 299.7 and 300.1 megam./sec.'*[59]

Michelson's work of 1882: *'He would not have been justified in claiming more for the work than this:*

| Date of publication | Date of observations | Experimenter | Value for c (km/sec.) | Limits of error (km/sec.) |
|---|---|---|---|---|
| 1874 | 7/8/1872 –31/8/1872 | Cornu | 298,500 | ± 2,000 |
| 1874, 1876 | 9/1874 | Cornu | 299,900 | ± 600 |
| 1878, 1880 | 1878 | Michelson | 300,500 | ± 3,500 |
| 1879, 1880 | 5/6/1879 –2/7/1879 | Michelson | 299,900 | ± 200 |
| 1891 | Series 1: 28/6/1880 –15/4/1881 Series 2: 8/8/1881 –24/9/1881 Series 3: 24/7/1882 –5/9/1882 | Newcomb | 299,780 | ± 80 |
| 1891 | 12/10/1882 | Michelson | 299,850 | ± 250 |
| 1900, 1902, | 1898–1902 | Perrotin and Prim | 300,000 | ± 1,000 |
| 1924 | 4/8/1924 –10/8/1924 | Michelson | 299,800 | ± 70 |
| 1927 | 1924–1926 | Michelson | 299,798 | ± 20 |
| 1928, 1929 | 1925– | Karolus and and Mittelstaedt | 299,778 | ± 20 |
| 1935 | 19/2/1931 –27/2/1933 | Michelson, Pease and Pearson | 299,774 | ± 20 |
| 1937 | 22/6/1936 –5/12/1936 | Anderson | 299,771 | ± 15 |
| 1940 | 1940 | Hüttel | 299,768 | ± 10 |
| 1941 | 21/5/1939 –8/7/1940 | Anderson | 299,776 | ± 14 |

***Table 5.*** *VALUES FOR THE VELOCITY OF LIGHT IN A VACUUM, AND LIMITS OF ERROR OF THESE VALUES, INDICATED, ACCORDING TO DORSEY, BY THE OBSERVATIONS OF A NUMBER OF EXPERIMENTERS.*

*Velocity of light in a vacuum — 299.85 megam./sec. Dubiety at least — 0.25 megam./sec. That is, the velocity of light in a vacuum may lie between 299.6 and 300.1 megameters per second, essentially the same as for the earlier work.*'[60]

Michelson's work of 1924: *'The best he would have been justified in claiming for the work is: Velocity of light in a vacuum — 299.80 megam./sec. Dubiety at least ± 0.07 megam./sec. That is, the velocity of light in a vacuum may lie between 299.73 and 299.87 megameters per second.*'[61]

Michelson's work of 1924–6: *'The best he would have been justified in claiming for the 1924–26 series of determinations is this: Velocity of light in a vacuum — 299,798 km./sec. Dubiety at least ±20 km./sec. That is, the velocity of light in a vacuum may, but does not necessarily, lie between 299.78 and 299.82 megameters per second.*'[62]

Michelson, Pease, and Pearson's work of 1931–3:

*'Perhaps it will be safe to say that the observations indicate that the velocity of light in a vacuum lies between 299,764 and 299,784 km./sec . . .*'[63]

Dorsey also examines Karolus and Mittelstaedt's report of 1929, Anderson's reports of 1937 and 1941, and Hüttel's report of 1940, but does not suggest any change to their published values.[64] It may be noted that he quotes Anderson's 1937 value as 299,771 ± 15 km./sec., not ±12, as Froome and Essen have it.[65]

Dorsey's values are summarized in Table 5.

Dorsey concludes:

*'in view of the uncertainty of the significance of the center values in the first 9 determinations, it is obvious that the data give no indication of any secular change in the velocity of light.*'[66]

## CONCLUSIONS

This conclusion confirms the results of my analysis of the past values of c given by Froome and Essen. From an historical point of view it may be assumed that the scientific discussion in the 1930s and early 1940s about whether c was decreasing was ended, not only by theoretical considerations, as Setterfield assumes,[67] but also, firstly, by Dorsey's monograph and, secondly, by the constancy of the values for c which have been obtained since the 1940s.

Setterfield has only been able to reopen the question by uncritically accepting those values which support his hypothesis and by uncritically rejecting those values which do not support his hypothesis. With regard to the values which he has accepted, he has not subjected them to any kind of critical examination and has not even checked them by consulting their primary sources. This applies to the new values from Roemer-type experiments and Bradley-type experiments as much as to the values which he had previously presented. With regard to the values which he has rejected, his reasons for rejecting them have rested mainly on brief comments in secondary sources, and even these comments have often been misrepresented. Setterfield's hypothesis is, therefore, without any adequate foundation, whereas the constancy of the velocity of light through time is confirmed by statistical analysis of all past values of c listed by Froome and Essen and by Dorsey's careful critical examination of the early values of c.

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25. cf. Setterfield, Ref. 2, Table l, p. 39 and Ref. 23, Tables 1 and 7, pp. 54, 63.
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## Appendix 1. Setterfield's explanations for the inclusion and omission of published values of c.

In Appendix 1 of my first paper I checked some of Setterfield's explanations for his omission of some published values of c. In this appendix I check, first, Setterfield's explanations for his omission of values obtained by the method involving the ratio of electromagnetic to electrostatic units and by the method involving radio waves and parallel wires; second, Setterfield's inclusion of a number of values on the basis that they had been checked by Birge; and third, Setterfield's omission of a number of values from some of his analyses on the basis that they involved the use of a Kerr cell.

(i) In Appendix 1 to my first paper I mentioned Setterfield's explanation of his omission of values for c obtained by measuring the wavelength and frequency of a radio wave transmitted along a pair of parallel wires and by measuring the charge on a capacitor in electrostatic and electromagnetic units.[1,2] In the first edition of his paper, he claimed that Froome and Essen pointed out that these methods *'use assumptions which do not appear valid'*.[3] In his reply to Morton's letter and in the second edition of his paper, he said that Froome and Essen pointed out that the standing wires method *'is based upon assumptions that are strictly not true'*, and that he *'echoes Froome and Essen's sentiments on the matter'* when he concludes that the method of comparing electromagnetic to electrostatic units is *'completely unreliable'* or, at least, *'far from satisfactory'*.[4,5]

In fact, Froome and Essen say neither that both methods use assumptions which do not appear valid nor that the standing wires method by itself is based upon assumptions which are not strictly true. What they say is this:

> *'It is now known that many of the assumptions involved in* [Blondlot's] *experiment are not strictly true . . .*'[6]

That is, this comment about assumptions only referred to the first of the standing wires experiments. Later Froome and Essen say,

> *'Both of these methods were improved and great care was taken with the calculation of systematic errors. Presumably it was not appreciated at the time that the refractive index of air for radio waves depended greatly on the humidity and no records appear to have been taken of the precise atmospheric conditions. Even so the results were probably as reliable as those obtained by optical methods.'*[7]

Therefore, there are no clear grounds in Froome and Essen's comments for excluding the values obtained by these other methods from the analysis of all past values of c while including the values obtained by optical methods during the same period.

(ii) In his reply to Cadusch and Tapp and in the second edition of his paper, Setterfield says with regard to the accuracy of the results from 1870 to 1940 that Birge

> *'checked through these early measurements quite thoroughly'* and *'made only minimal changes in the published figures and their errors. It is significant that in each case his corrected figures gave a higher value for c, thus confirming the trend.*'[8]

Assuming that Setterfield is referring to Birge's 1941 article in Reports on Progress in Physics, it is true that Birge checked through a number of values from 1906 to 1940.[9] However, to begin with, it is doubtful whether he could check them *'quite thoroughly'* in only six pages. Then, although his Table 1 (see my Table 6) appears to give *'corrected results'* for the values from 1870 to 1902, Birge had not checked these results. He had only listed *'the five earlier "final declared values", as given by de Bray (1927)' 'in order to consider de Bray's hypothesis of the time variation of c*'.[10] Finally, although Birge's *'corrected results'* were higher, or the same as, Birge's *'original published results'*, when they were higher they were closer to the present value of c in all cases except one. Therefore, it cannot be claimed that Birge's corrected results confirm a downward trend in c.

| Author | Epoch* | Corrected result (km/sec.) | Adopted probable error** (km/sec.) | Original published result (km/sec.) |
|---|---|---|---|---|
| Cornu-Helmert | 1874.8 | 299,990 | 200 | 299,990 |
| Michelson | 1879.5 | 299,910 | 50 | 299,990 |
| Newcomb | 1882.7 | 299,860 | 30 | 299,860 |
| Michelson | 1882.8 | 299,853 | 60 | 299,853 |
| Perrotin | 1902.4 | 299,901 | 84 | 299,901 |
| Rosa-Dorsey | 1906.0 | 299,784 | 10 | 299,710 |
| Mercier | 1923.0 | 299,782 | 30 | 299,700 |
| Michelson | 1926.5 | 299,798 | 15 | 299,796 |
| Mittelstaedt | 1928.0 | 299,786 | 10 | 299,778 |
| Michelson, Pease, and Pearson | 1932.5 | 299,774 | 4 | 299,774 |
| Anderson | 1936.8 | 299,771 | 10 | 299,764 |
| Hüttel | 1937.0 | 299,771 | 10 | 299,768 |
| Anderson | 1940.0 | 299,776 | 6 | 299,776 |

***Table 6.*** *BIRGE'S (1941) TABLE 1, WITHOUT COLUMNS GIVING METHOD AND ADOPTED WEIGHT.*

* *'the mean epoch at which the work was performed'*

** *with regard to the five earlier values: 'Merely for the sake of argument I [Birge] adopt also the stated probable error of the investigator himself, as given by de Bray'*

(iii) In his reply to Cadusch and Tapp and in the second edition of his paper, Setterfield says, with regard to Mittelstaedt, Anderson, and Hüttel,

> *'These experimenters all had one thing in common — they used a Kerr-cell to determine the value of c . . . The early Kerr-cell measurements were all subject to a systematic error due to "the different transit times of the electrons in the detecting tube", an error which Anderson was the first to recognise.'*[11]

While it may be true that the early Kerr-cell measurements were all subject to a systematic error,[12] this was not due to the error to which Setterfield refers. The latter error did not arise in the Kerr-cell which modulated the light, but in the receiving light shutter which consisted of a photoelectric cell and an eleven stage electron multiplier tube. In particular, Anderson attributed certain variations in his results to the different transit times of the electrons in this electron multiplier tube. These different transit times Anderson attributed to

> *'thermal expansion of the mirror holders in the long path, and to building vibrations. The former causes slowly changing shifts usually in one direction or the other during a run of several hours. The building variations, however, show up as rapid fluctuations*

*and shifts. Even a steady wind can produce observable shifts in the minimum point.'*[13]

However, this electron multiplier tube was only added by Anderson in his 1941 work.[14] That is, it was not present in his 1937 work, nor in Karolus and Mittelstaedt's work, nor in Hüttel's work. Therefore, it is inaccurate to say that all the Kerr-cell experiments were affected by this factor, even if Anderson had correctly identified the cause of the variations with which he was concerned. Froome and Essen summarize and comment on Anderson's 1941 work as follows:

*'Nearly 3000 measurements were made giving a result of 299,776 ±14 km/sec. The total spread of the results is not given but the means of groups of over 100 measurements differ by 60 km/sec indicating the presence of systematic errors. Anderson mentions that one such error may arise from the two beams striking different parts of the cathode surface of the photocell.'*[15]

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## Appendix 2.
## Roemer's determination of c (again)

In Appendix 3 to my first paper, I attempted to describe Roemer's method of determining c. I assumed that Roemer made certain simplifying assumptions which limited the accuracy of his value, and this seemed to be supported by the close agreement between the value for c I obtained by using the method I described and the value for c which Roemer published.[1] However, when I removed the main simplifying assumptions in the method I described and then made further calculations, I again obtained a value for c close to Roemer's published value. Therefore, this new method, rather than the one previously described, may have been Roemer's method.

Furthermore, it became clear that Roemer's value was very much affected by the value he used for the period of Io. When a modern value for the period of Io was used in the calculation, a much higher value for c was obtained, although the range of values obtained from individual calculations was so great as to prevent any conclusion about the limits of error of this value.

The new method which I used has the same basis as the previous method. As I said before,[2] four of Jupiter's satellites are easily observable with a small telescope. The innermost of these is called 'Io'. The immersion of Io in Jupiter's shadow can only be observed as the earth approaches Jupiter and the emersion of Io from Jupiter's shadow can only be observed as the earth moves away from Jupiter.

Now, having observed an immersion or emersion of Io at time $t_a$, the time of the $n^{th}$ immersion or emersion after the one at $t_a$ may be calculated by adding n times the period of revolution of Io ($T_i$) to $t_a$. This calculation is only approximate because it does not take into account the inclination of Io's orbit to Jupiter's orbit nor the perturbations in Io's orbit due to the gravitational attraction of Jupiter's other satellites. Ignoring these factors here, the observed time $t_b$ of the $n^{th}$ immersion will be earlier, and the $n^{th}$ emersion will be later, than the predicted time $t_p$ by an amount equal to the difference in the distances between Earth and Jupiter at the times of the two observations divided by the speed of light. That is:

$$t_b - t_p = \frac{\Delta d}{c} \qquad \text{and} \qquad t_p = t_a + n.T_i$$

$$\therefore t_b - (t_a + n.T_i) = \frac{\Delta d}{c}$$

$$\therefore \quad c = \frac{\Delta d}{(t_b - t_a) - n.T_i} = \frac{\Delta d}{\Delta t - n.T_i}$$

Table 7 gives, first, the date and time of observation according to Roemer and second, the Julian day (minus 2,330,000) calculated by Goldstein *et al.* by applying the equation of time obtained from Cassini's tables for the day and year of the observation.[4] These observations are numbered according to Goldstein *et al.*'s Table 1 and excludes the observations numbered 1, 2, 26 and 29 because there were no other observations with which these could be paired to give a comparison between observed and predicted times. My Table 7 also gives the distance between Earth and Jupiter in astronomical units at the time of each observation. This was calculated from the heliocentric co-ordinates for Earth and Jupiter (given in Goldstein *et al.*'s Table II) according to the formula:

$$d = \sqrt{(X_J - X_E)^2 + (Y_J - Y_E)^2 + (Z_J - Z_E)^2}$$

This was converted to kilometres by multiplying by 1.496 x $10^8$.

| No. | Type of eclipse | Year | Date | Apparent solar time<br>h m s | Julian day (-2330000)<br>t | Distance between Earth and Jupiter,d (a. u.) | d (km x $10^8$) |
|---|---|---|---|---|---|---|---|
| 3 | Emersion | 1671 | Mar. 19 | 9 1 44 | 1458.38178 | 4.6673 | 6.982 |
| 4 | | | Apr. 27 | 7 42 30 | 1497.31938 | 5.2433 | 7.844 |
| 5 | Immersion | 1672 | Jan. 3 | 12 42 36 | 1748.53347 | 4.9208 | 7.361 |
| 6 | | | 10 | 14 32 14 | 1755.61176 | 4.8217 | 7.213 |
| 7 | | | 12 | 8 59 22 | 1757.38109 | 4.7982 | 7.178 |
| 8 | Emersion | | Mar. 7 | 7 58 25 | 1812.34008 | 4.4352 | 6.635 |
| 9 | | | 14 | 9 52 30 | 1819.41786 | 4.4527 | 6.661 |
| 10 | | | 23 | 6 18 14 | 1828.26719 | 4.4956 | 6.725 |
| 11 | | | 28 | 13 45 30 | 1833.57660 | 4.5320 | 6.780 |
| 12 | | | 30 | 8 14 46 | 1835.34653 | 4.5458 | 6.801 |
| 13 | | | Apr. 13 | 12 8 8 | 1849.50575 | 4.6832 | 7.006 |
| 14 | | | 22 | 8 34 28 | 1858.35596 | 4.7902 | 7.166 |
| 15 | Immersion | 1673 | Feb. 4 | 17 31 10 | 2146.74023 | 4.9049 | 7.338 |
| 16 | | | 6 | 12 0 0 | 2148.51034 | 4.8801 | 7.301 |
| 17 | | | 13 | 13 53 20 | 2155.58909 | 4.7852 | 7.159 |
| 18 | | | 27 | 17 40 10 | 2169.74539 | 4.6241 | 6.918 |
| 19 | | | Mar. 1 | 12 9 1 | 2171.51520 | 4.6071 | 6.892 |
| 20 | | | 17 | 10 28 16 | 2187.44228 | 4.4912 | 6.719 |
| 21 | | | 24 | 12 24 30 | 2194.52148 | 4.4628 | 6.676 |
| 22 | Emersion | | Apr. 18 | 9 22 0 | 2219.38970 | 4.4841 | 6.708 |
| 23 | | | 25 | 11 18 5 | 2226.46931 | 4.5234 | 6.767 |
| 24 | | | May 18 | 11 32 44 | 2249.47821 | 4.7378 | 7.088 |
| 25 | | | Aug. 4 | 8 30 41 | 2327.35839 | 5.8674 | 8.778 |
| 27 | Emersion | 1675 | Jul. 20 | 8 22 42 | 3042.35298 | 4.5422 | 6.795 |
| 28 | | | Oct. 29 | 6 7 22 | 3143.24395 | 5.5928 | 8.868 |
| 30 | Emersion | 1676 | Aug. 7 | 9 49 50 | 3426.41306 | 4.2535 | 6.363 |
| 31 | | | 14 | 11 45 55 | 3433.49288 | 4.3157 | 6.456 |
| 32 | | | 23 | 8 11 13 | 3442.34236 | 4.4089 | 6.596 |
| 33 | Immersion | 1677 | Jun. 9 | 12 23 24 | 3732.51523 | 4.5731 | 6.841 |
| 34 | | | 16 | 14 16 14 | 3739.59456 | 4.4729 | 6.691 |
| 35 | | | Jul. 9 | 14 21 54 | 3762.60164 | 4.1982 | 6.280 |
| 36 | | | 25 | 12 37 10 | 3778.52983 | 4.0730 | 6.093 |
| 37 | Emersion | | Aug. 26 | 11 31 50 | 3810.48133 | 4.0341 | 6.035 |
| 38 | | | Sep. 11 | 9 54 30 | 3826.41029 | 4.2691 | 6.387 |
| 39 | | | Nov. 5 | 6 59 0 | 3881.27980 | 4.8271 | 7.221 |
| 40 | | 1678 | Jan. 6 | 5 25 47 | 3943.23100 | 5.6893 | 8.511 |

***Table 7.*** *TIMES OF ROEMER'S OBSERVATIONS OF IO AND THE DISTANCES BETWEEN EARTH AND JUPITER AT THESE TIMES.*

Table 8 gives, for each specified pair of observations, the difference between the times of the observations, At, the number of revolutions of Io in that time, n (found by dividing Δt by $T_i$ and taking to the nearest whole number), and the difference between the distances between Earth and Jupiter at the times of the observations, Δd. The table then gives the difference between Δt and the predicted difference $n.T_i$, and the calculated value for c, viz. $\Delta d/(\Delta t - n.T_i)$, for two values for $T_i$: 1.76980 days and 1.76986 days.

The first value for T. is the value Roemer used. Cohen writes that on a portion of a Roemer manuscript there are computations giving the mean period of revolution as follows: 1671–2 1 day 18 hours 28 minutes 30 seconds, 1672–3 1 day 18 hours 28 minutes 31 seconds,[5] that is, 1.76979 days and 1.76980 days respectively.

| Pair of obss. | Time btw. obss. At (days) | No. of revs. n | Ad (km x10⁵) | $T_i$ = 1.76986 | | | $T_i$ = 1.76986 | |
|---|---|---|---|---|---|---|---|---|
| | | | | $\Delta t$ - n.$T_i$ | | c(km /sec x $10^5$) | $\Delta t$ - n.$T_i$ | c(km /sec x $10^5$) |
| | | | | (days) | (sec) | | (days) (sec) | |
| 4- 3 | 38.93760 | 22 | 862 | 0.00200 | 173 | 5.0 | 0.00068 59 | 15 |
| 6- 5 | 7.07829 | 4 | -148 | -0.00091 | - 79 | 1.9 | -0.00115 -99 | 1.5 |
| 7- 6 | 1.76933 | 1 | -35 | -0.00047 | -41 | 0.9 | -0.00053 - 46 | 0.8 |
| 9- 8 | 7.07778 | 4 | 26 | -0.00142 | -123 | -0.2 | -0.00166 -143 | -0.2 |
| 10- 9 | 8.84933 | 5 | 64 | 0.00033 | 29 | 2.2 | 0.00003 3 | 21 |
| 11-10 | 5.30941 | 3 | 65 | 0.00001 | 1 | 65 | -0.00017 - 15 | -4.3 |
| 12-11 | 1.76993 | 1 | 21 | 0.00013 | 11 | 1.9 | 0.00007 6 | 3.5 |
| 13-12 | 14.15922 | 8 | 205 | 0.00082 | 71 | 2.9 | 0.00034 29 | 7.1 |
| 14-13 | 8.85021 | 5 | 160 | 0.00121 | 105 | 1.5 | 0.00091 79 | 2.0 |
| 16-15 | 1.77011 | 1 | -37 | 0.00031 | 27 | -1.4 | 0.00025 22 | -1.7 |
| 17-16 | 7.07875 | 4 | -142 | -0.00045 | -39 | 3.6 | -0.00069 - 60 | 2.4 |
| 18-17 | 14.15630 | 8 | -241 | -0.00210 | -181 | 1.3 | -0.00258 -223 | 1.1 |
| 19-18 | 1.76981 | 1 | -26 | 0.00001 | 1 | -26 | -0.00005 - 4 | 6.5 |
| 20-19 | 15.92708 | 9 | -173 | -0.00112 | -97 | 1.8 | -0.00166 -143 | 1.2 |
| 21-20 | 7.07920 | 4 | -43 | 0 | 0 | ±∞ | -0.00024 - 21 | 2.0 |
| 23-22 | 7.07961 | 4 | 59 | 0.00041 | 35 | 1.7 | 0.00017 15 | 3.9 |
| 24-23 | 23.00890 | 13 | 321 | 0.00150 | 130 | 2.5 | 0.00072 62 | 5.2 |
| 25-24 | 77.88018 | 44 | 1690 | 0.00898 | 776 | 2.2 | 0.00634 548 | 3.1 |
| 28-27 | 100.89097 | 57 | 2073 | 0.01237 | 1069 | 1.9 | 0.00895 773 | 2.7 |
| 31-30 | 7.07982 | 4 | 93 | 0.00062 | 54 | 1.7 | 0.00038 33 | 2.8 |
| 32-31 | 8.84948 | 5 | 140 | 0.00048 | 41 | 3.4 | 0.00018 16 | 8.8 |
| 34-33 | 7.07933 | 4 | -150 | 0.00013 | 11 | -14 | -0.00011 -10 | 15 |
| 35-34 | 23.00708 | 13 | -411 | -0.00032 | -28 | 15 | -0.00110 -95 | 4.3 |
| 36-35 | 15.92819 | 9 | -187 | -0.00001 | - 1 | 187 | -0.00055 - 48 | 3.9 |
| 38-37 | 15.92896 | 9 | 352 | 0.00076 | 66 | 5.3 | 0.00022 19 | 19 |
| 39-38 | 54.86951 | 31 | 834 | 0.00571 | 493 | 1.7 | 0.00385 333 | 2.5 |
| 40-39 | 61.95120 | 35 | 1290 | 0.00820 | 708 | 1.8 | 0.00610 527 | 2.4 |

*Table 8. VALUES FOR C CALCULATED FROM TIMES AND DISTANCES IN TABLE 7.*

An approximate value for Io’s mean period may be obtained by considering the times of eclipses of Io about one synodic year apart when Jupiter and Earth are the same distance apart. By dividing the difference between these times by the number of revolutions of Io in that time, an approximate value for the mean period of Io may be obtained. From Table 7, pairs of observations made about one synodic year apart were examined, and the seven which were made when Jupiter and Earth were nearest to the same distance apart were selected. Table 9 gives the mean periods of Io calculated from these pairs of observations. (The significant variations from year to year show once again that differences in distance between Earth and Jupiter is not the only factor in determining observed times of lo’s eclipses.) The mean period calculated for 1671–2 and 1672–3 may be compared with the mean periods which Roemer calculated.

The second value for $T_i$ used in Table 8 is a modern value which was calculated from the modern text-book value of 1.769138 days for the sidereal period[6,7,8] as fol-

| Pair of observations | Years | $\Delta d$ (km x $10^5$) | Time between observations $\Delta t$ (days) | No. of revolutions n | Period $\Delta t/n$ (days) |
|---|---|---|---|---|---|
| 13- 3 | 1671–2 | 24 | 391.12397 | 221 | 1.769792 |
| 15- 5 | 1672–3 | 23 | 398.20676 | 225 | 1.769808 |
| 17- 7 | 1672–3 | 19 | 398.20800 | 225 | 1.769813 |
| 22 - 10 | 1672–3 | 17 | 391.12251 | 221 | 1.769785 |
| 23 - 11 | 1672–3 | 13 | 392.89271 | 222 | 1.769787 |
| 27 - 23 | 1673–5 | 28 | 815.88367 | 461 | 1.769813 |
| 38 - 30 | 1676–7 | -24 | 399.99723 | 226 | 1.769899 |

*Table 9. PERIOD OF IO CALCULATED FROM TIMES IN TABLE 7.*

lows. In the time between oppositions of Earth and Jupiter (that is, one synodic period = 1.09211 tropical years, and 1 tropical year = 365.2422 days)[9] Io revolves

$$\frac{1.09211 \times 365.2422}{1.769138} = 225.4684 \text{ times}$$

However, it is another 1.769138 x 0.09211 = 0.1629553 days before Io passes through Jupiter's shadow again (see Figure 1). Distributing this difference throughout the synodic period, the period of Io relevant to these calculations (viz. the period from one passage through Jupiter's shadow to the next) is

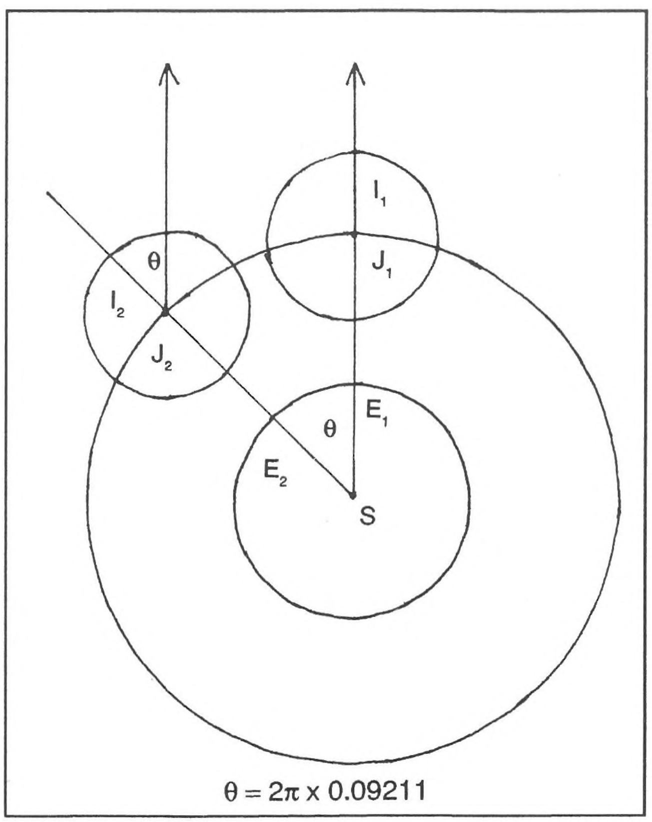


***Figure 1.*** *The configurations of the Sun (S), Earth (E), Jupiter (J) and Io (I) during two of Io's eclipses.*

$$1.769138 + \frac{0.1629553}{225.4684} = 1.769861 \text{ days}$$

With regard to the values for c obtained in Table 8, the large spread of values and presence of negative values means that there is no point in calculating their mean. However, a median value may be obtained by arranging the values from 0 to +∞ and then from -∞ to 0. They are arranged in this way rather than from -∞ to 0 and then from 0 to +∞ because errors in observation resulting in a value for $\Delta t$ - n.$T_i$ either further away from or closer to zero result in a corresponding value for c which is closer to zero or closer to ±∞.

Now Roemer said in a letter to Huygens that it was the 1672–3 observations which he used to obtain his figure of 22 minutes.[10] Arranging the values for c obtained for 1672–3 from 0 to +∞ and then from -∞ to 0, the following arrangement is obtained:

0.9, 1.3, 1.5, 1.7, 1.8, 1.9, 1.9, 2.2, 2.2,

2.5, 2.9, 3.6, 65, ±∞, -26, -1.4, -0.2.

The median value is 220,000 km/sec, which is the same as the mean of the sample results obtained in my first paper.[11]

The median value for $T_i$ = 1.76980 days for all pairs of observations in Table 8 is 250,000 km/sec, whereas the median value for $T_i$ = 1.76986 days is 390,000 km/sec. This shows that the value taken for T. was much more significant than the observations selected for analysis. This was confirmed when calculations were made for $T_i$ = 1.76980 days for all possible pairs of observations (e.g. not just 9-8, 10-9, 11-10, etc. but also 10-8, 11-8, 11-9, etc.). A median value of 230,000 km/sec was obtained. Because of the small difference in the result obtained, calculations were not made for all possible pairs of observations for T. = 1.76986 days.

Apart from Roemer's conclusion that light has a finite velocity, three conclusions may be drawn. First, Roemer may have obtained his published value by calculations more like those described here than like those described in my first paper. He may not have made the simplifying assumptions involved in the method described in the first paper for it may have been practically possible for him to carry out the calculations involved in the method described in this paper. Indeed, Goldstein *et al.* report Cohen's and Mayer's conclusion that Roemer used only a few pairs of the observations in his calculations.[12]

The second conclusion which may be drawn is that Roemer's published value was largely affected by his value for Io's period of revolution.

The third conclusion is that, even when a modern value for Io's period is used, the range of values obtained is such that the median value does not give significantly greater support to Setterfield's hypothesis than to the hypothesis that c has been constant. Boyer reports that '[Roemer] *himself conceded, in correspondence with Huygens and in a note to the Academie in 1677, that uncertainties made impossible an exact determination of the time for light to traverse the orbit of earth.*'[13]

Finally, other works on Roemer which may be mentioned are:

Christiaan Huygens, **Oeuvres completes** (Société Hollandaise des Sciences, La Haye, 1888–1937), **XIX:** 463–469. (This gives Huygens' account of Roemer's demonstration.)[14]

**Roemer et la Vitesse de la Lumière:** Table ronde du Centre national de la recherche scientifique, Paris, 16 et 17 juin, 1976. Avant-propos de René Taton (C. N. R. S. Collection d'histoire des sciences, 3) (Vrin, Paris, 1978)

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## POSTSCRIPT

Trevor Norman and Barry Setterfield have referred to and quoted from the unpublished manuscript for the above article in their monograph, The Atomic Constants, Light and Time. In the course of their discussion of Roemer-type determinations of c they write:

> *'Roemer in 1675 gave [the light travel time] as 11 minutes from selected observations . . . An examination of the best 50 Roemer values was undertaken by Goldstein in 1975 after initial work in 1973. The correction of a procedural error, only recently noted, "gave a light travel time 2.6% lower than the presently accepted value. The formal uncertainty is ±1.8%" Roemer's value thus becomes 307,600 ± 5400 km/s.'*[1]

This passage has recently been referred to by D. Russell Humphreys in 'Has the Speed of Light Decayed Recently?', which is a discussion of Norman and Setterfield's monograph. Humphreys writes,

> *'About 1973, an astronomer from the University of Virginia, Samuel Goldstein, began doing some careful analyses of* [Roemer's] *data ... At first he made a logical error in the analysis but when the mistake was discovered a few years ago, he corrected his work ... On page 11 of* [Norman and Setterfield's] *monograph, Setterfield quotes Goldstein as giving a speed of light 2.6% faster in Roemer's day than now, citing as his reference 21: 'Goldstein, S. J., private communication, Feb. 25, 1986.'* [The superscript "21" comes after the word "correction" in the passage quoted above from Norman and Setterfield's monograph.] *I asked Setterfield for a copy of the Goldstein letter. Setterfield wrote in reply that the letter had not been sent to him and he did not have a copy of it. Instead, he had copied the quotation from a preprint of a new paper for the Ex Nihilo Technical Journal by Vivian Bounds.'*[2]

Humphreys then continues,

> *'When I wrote Goldstein for a copy of his letter to Bounds, he sent it. Also he added the information that he had stated his result ambiguously, apparently misleading both Bounds and Setterfield. What Goldstein had meant to say was the speed of light according to Roemer's data was 2.6% slower than it is now. Professor Goldstein has given me permission to quote the following from his 2 November 1987 letter to me: "The new result is that the velocity of light was slower in 1668 to 1678 by 2.6% than it is today. I do not think that the difference is significant, however."* '[3]

A little further on, Humphreys writes,

> *'Setterfield misunderstood* [Goldstein's revision] *because he seems to have violated several commonly-accepted practices among scholars:*
> *(1) Trace important data either back to its original sources or at least as far back as practically possible,*
> *(2) Never rely on an indirect quotation, and*
> *(3) Ask for permission to publish private communi-*

*cations.*'[4]

With regard to all this, I would like to make a number of points. To begin with, given the care with which I have explained the context in which Goldstein wrote to me and given the extensiveness of my quotation from Goldstein's letter, I do not believe that it was unreasonable for Setterfield to quote from my unpublished manuscript without having seen the original letter. In doing so he did not need to ask for permission to publish this private communication since he might have assumed that I had already asked for this permission. In fact, I had written back to Goldstein to say that I was submitting the above article for publication and, having enclosed a copy of the section of the article in which the quotation from his letter appears, I asked him to let me know if this was not all right. However, there is no question that Setterfield has violated another commonly-accepted practice among scholars by not showing that he was quoting from a quotation. Furthermore, by giving his citation as he did, he gave the impression that he had received the letter from Goldstein. Setterfield now seems to have given Humphreys the impression that he copied the quotation from a 'preprint' of my article, rather than from the unpublished manuscript. As far as I know, there has been no 'preprint'.

Having said that, I admit that I did not quote paragraph 4 or the first two sentences of paragraph 5 of Goldstein's letter. These are the only other parts of the letter which are relevant to the present discussion of Setterfield's theory and they are as follows:

> *'Since I cannot think of any reason for the velocity of light or the earth's orbital radius to change so much, I think that the real uncertainty is enough to encompass zero. Then the light travel time in 1668 to 1678 was -2.6 ± 2.6% compared to the modern value.*
> *An increase in the light travel time (if it were real) means that the velocity of light was lower. Thus, I have not found any support for Setterfield's theory,'*[5]

I explained why I did not quote these parts of Goldstein's letter when l wrote back to Goldstein. I wrote:

> 'I *was puzzled by your first sentence in paragraph 5: "An increase in the light travel time (if it were real) means that the velocity of light was lower." If the light travel time was 2.6% lower than the presently accepted value, as you say in paragraph 3* [quoted in the above article], *or -2.6% compared to the modern value, as you say in paragraph 4, does this not mean that the velocity of light was 2.6% higher? This is the conclusion I have drawn in the enclosed section of my paper, taking your sentence in paragraph 5 to be a slip. If I have been wrong to take it like this, I would be grateful if you would let me know.'*

In the absence of any reply concerning this, I had assumed that the conclusion I had drawn was the right one. However, according to Humphreys' correspondence with Goldstein I had not drawn the right conclusion, and I have, consequently, misled Setterfield. Goldstein apparently meant to say, in paragraph 3, that the light travel time was 2.6% higher than the presently accepted value, in paragraph 4, that it was +2.6% compared to the modern value, and in paragraph 5, that a decrease in the light travel time (if it were real) means that the velocity of light was lower.

Given that Goldstein's result is that *'the velocity of light was slower in 1668 to 1678 by 2.6% than it is today*', that is, that the velocity of light in 1668 to 1678 was 292,000 km/sec, it may still not be claimed, as I did in my first article,[6] that Goldstein's result confirms that c has been constant through time. However, whereas I have concluded in the above article that Goldstein's result is consistent with a past decrease in c, it must now be concluded that Goldstein's result is not consistent with a past decrease in c. Similarly, whereas I have concluded in the above article that Goldstein's result does not give significantly greater support to Setterfield's hypothesis than to the hypothesis that c has been constant, it must now be concluded that Goldstein's result supports the hypothesis (hat c has been constant but does not support Setterfield's hypothesis. As Humphreys concludes,

> *'The error bounds about the Roemer point, according to Goldstein, are a few percent. That is enough to include the possibility of **no** change since the 17th century but it casts doubt on the possibility of significant **decay** since then.'*[7]

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