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Towards a Critical Examination of the Historical Basis of the Idea that Light has Slowed Down — A Reply

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BOUNDS' APPENDICES 3, 4 AND 5

In a quick reading of "Towards a critical examination. . ." by Vivian E. Bounds one obtains the impression that it is a scholarly work. Certainly the criticism given in Appendix 5 of the Riemannian space argument cannot be refuted and so that suggestion by Moon and Spencer must be rejected. Again, the treatment given to the Roemer method in Appendix 3 generally deserves praise and may fruitfully be included in some standard astronomical tests. Appendix **4** on measuring devices in somewhat more elementary, however. It is also rather unfortunate that Bounds did not have access to the International Edition of **Ex Nihilo,** vol. **1,** no. **1, 1982** in order to make a considered critique. In that edition it was possible to correct, enlarge and check previous statements and omissions in the rough draft that appeared in **Ex Nihilo,** vol. **4,** no. **1, 1981** as a progress report. For that reason much of the criticism that Bounds gives throughout his work is invalidated.

BOUNDS' APPENDIX 2 - OTHER REFERENCES TO DECAYING C

However, from Appendix 2 something else becomes apparent. When talking about the substantial research based on the original documents by M.E.J. Gheury de Bray[1] and the results of that research[2], Bounds makes the surprising statement that de Bray's "opinion that c is decreasing was no more than an uncritical guess". This is rather interesting since Prof. R.T. Birge admitted that the measured values of c were dropping with time, but he rejected the possibility on theoretical grounds.[3] These grounds have since been shown to be invalid by an application of conservation laws.[4]

Bounds further questions the quote in the **Scientific American** article by C.L. Stong in October 1975, p. 120 by stating that "An unsubstantiated question of an amateur scientist is surely not worth noticing in a serious discussion". Indeed! Under these circumstances it might be profitable to recall that last year in a serious scientific article "Time to remeasure the metre", which discussed the fixing of the metre to the proposed constant speed of light,[5] the following statement was made. "But many scientists have speculated that the speed of light might be changing over the lifetime of the Universe. . ." and ". . .it is still possible that the speed of light might vary. . ." The kind of discrepancies that would be noted by physicists under this new scheme of things if the variation in c occurred were then listed. Why go to this trouble if there is not some doubt in the collective scientific mind on this issue? It would appear that Bounds' condemnation of de Bray and the 'amateur scientist' may not be shared by all in the scientific community after all. It further raises the possibility that a number of scientists might be prepared to consider a change in the speed of light.

BOUNDS' ATTITUDE TO THE DATA

But if Bounds' statements in Appendix 2 treat with disdain those who make statements about a possible change in c, the situation is more precarious for his scholarship in the closing paragraphs of the main article. There Bounds states that if "a reliable list of all published determinations of c had been obtained" and that "statistical analysis. . .favoured the idea that c has decreased. . .IT WOULD NOT BE CONCLUSIVE" (emphasis added) as further reworking and re-analysis would be needed. He shows a strange reluctance to accept the evidence of data (on which science is based) even in a watertight case! On this score Dr Peter Cadusch admits that[6] these "determinations all appear to be higher than the currently accepted value. . .despite extensive re-working and re-analysis". It is at this point that one notices a certain undercurrent that runs through the whole paper. Whereas Cadusch was prepared to admit that even the re-worked values were higher, Bounds remains absolutely silent about the comparison with today's figures that his re-examination of the data produces.

THE MICHELSON VALUES, CORNU AND NEWCOMB

If we take the values that Bounds supplies for the accurate Michelson experiments, we have an excellent illustration of his silence on this important matter. They are tabulated in Table 1.

Table 1. Accurate Michelson Values

DATE	VALUE OF C (km/sec)
1879	299,910 ± 50
1882	299,853 ± 60
1924	299,802 ± 30
1926	299,796 ± 4

Table 2. All Michelson Values

DATE	VALUE OF C (km/sec)
1878	300,140 ± 300
1879	299,910 ± 50
1882	299,853 ± 60
1924	299,802 ± 30
1926	299,796 ± 4

Two things are worthy of note here. First, there is a systematic drop in the measured value of c with time from the same experimenter: Bounds never commented on this trend from his own figures. Secondly, without exception each value measured was ABOVE the present value for c. Indeed, the 1926 value was corrected[7] by Birge to read 299,798 km/sec, a correction upwards emphasising that the lowest value obtained was still above the currently accepted one. Again Bounds never mentioned these observational facts. If the inaccurate preliminary value that Michelson discarded (as Bounds admits) is included in the discussion we have the Table 2 result.

Again, Bounds' contention of an unchanging value for c seems to be unsupported from his own figures. If we tabulate the accurate results from Cornu, Newcomb and Michelson as given by Bounds we get Table 3.

Again the downward trend is in evidence despite re-workings of Cornu's values by Helmert and Dorsey. This is not mentioned by Bounds, nor is the fact that even Dorsey's recent re-working still leaves c higher than at present by over 200 km/sec.

THE PERROTIN RESULTS

If we look at Perrotin's results as given by Bounds we notice that there were three spot values and a final value (299,901 km/sec). Taking two of the spot values in the order in which they were obtained and excluding the discarded value we have Table 4.

Note that the highest value was obtained at the earlier date. This is significant as the limits of error in each case are the same. The discarded spot value of 300,032 ± 215 km/sec was (as de Bray points out[8]) the first value obtained in the series and thus does not go against the trend. Table 5 includes all Perrotin values given by Bounds in the order in which they were obtained.

Table 4. Accurate Perrotin Spot Values

DATE	VALUE OF C (km/sec)
1900	299,900 ± 80
1902	299,860 ± 80

TOOTHED WHEEL VALUES: FIZEAU, CORNU AND PERROTIN

A comparison with other experimental methods shows that the toothed wheel gives systematically

Table 3. Accurate Cornu, Newcomb and Michelson Values

EXPERIMENTER	DATE	VALUE OF C (km/sec)
Cornu	1874	300,400 ± 300
Cornu — Helmert	1874	299,990 ± 200
Cornu (Dorsey)	1874	299,900
Michelson	1879	299,910 ± 50
Newcomb	1882.7	299,860 ± 30
Michelson	1882.8	299,853 ± 60

Table 5. ALL PERROTIN VALUES

DATE	VALUE OF C (km/sec)
1900	300,032 ± 215 (discarded)
1900	299,900 ± 80
1902	299,860 ± 80
1902	299,901 ± 84 (Final declared value of series)

Table 6. Toothed Wheel Experiments

EXPERIMENTER	DATE	VALUE OF C (km/sec)
Fizeau	1849	315,300
Cornu	1874	300,400 ± 300
Perrotin	1900	299,900 ± 80
Perrotin	1902	299,860 ± 80

Table 7. Toothed Wheel — All Values

EXPERIMENTER	DATE	VALUE OF C (km/sec)	
Fizeau	1849	315,300	
Fizeau	1849	313,300	Omitted in most discussions
Fizeau	1855	305,650	Omitted in most discussions
Fizeau	1855	298,000	Omitted in most discussions
Cornu	1874	300,400 ± 300	
Cornu (Helmert)	1874	299,990 ± 200	Value re-worked by Helmert
Cornu (Dorsey)	1874	299,900 ± 200	Value re-worked by Dorsey
Perrotin	1900	300,032 ± 215	(discarded)
Perrotin	1900	299,900 ± 80	
Perrotin	1902	299,860 ± 80	
Perrotin	1902	299,901 ± 84	Final value of series)

high results when compared to the rotating mirror. Nevertheless, as Bounds notes that Fizeau, Cornu and Perrotin used this method we tabulate their values from Bounds' own document in Table 6.

Even here a decay in the measured value of c is quite apparent, a trend that Bounds failed to mention. If all the discarded and re-worked values are included we have Table 7 for the toothed wheel as listed by Bounds.

The trend from Tables 6 and 7 is still apparent — the measured value of c is decreasing. However, the toothed wheel with its tendency to give higher values than the rotating mirror has shifted the decay trend into a slightly higher range of values. In like manner, the Kerr Cell gave results that were systematically low, but when their results are tabulated, the decay is again in evidence, but simply shifted into a lower range (see in Table 8). Bounds ignores this evidence from observation, yet it is plainly there in the data that he presents as a result of his own scholarship.

SUPPORTING ARGUMENTS: MAJOR OR MINOR?

Throughout his paper then, Bounds shows that he has a strange reluctance to investigate all the possible conclusions which are compatible with the data. He talks in his opening paragraphs about "minor supporting arguments" for a decay in c. However, he again fails to mention the considerable support for c decay offered by the other physical constants.[9] The experimentally obtained values for these constants confirm the theoretically predicted variation that each would have with varying c. A solid state physicist from the University of N.S.W. has commented on this point.[10] "The possibility of these theoretical predictions matching the behaviour of thirteen other physical constants is 0.6 of a chance in a million. This very strongly suggests that the theory is correct". Bounds apparently considers this observational back-up from the other constants as only

Table 8. Accurate Values up to 1945

EXPERIMENTER	YEAR	VALUE OF C (km/sec)	COMMENT
Roemer	1676	327,000 193,120	Range ascribed to Roemer in the literature
Bradley	1728	303,000 ± 6000	
*Delambre & Glasenapp	1790	301,000 ± 2000	Average of 2 values by Roemer's method
Fizeau	1849	315,300	Often quoted as 315,000 km/sec
Fizeau	1855	305,650	Omitted in most discussions
Fizeau	1855	298,000	Omitted in most discussions
Foucault	1862	298,000 ± 500	On the basis of 0.7mm deflection
Cornu	1874	300,400 ± 300	
Cornu (Helmert)	1874	299,990 ± 200	Helmert's revision of Cornu's value
* Cornu (Dorsey)	1874	299,900 ± 200	Dorsey's revision of Cornu's value
Michelson	1879	299,910 ± 50	
Newcomb	1882.7	299,860 ± 30	
Michelson	1882.8	299,853 ± 60	
* Perrotin	1900	300,032 ± 215	Discarded, though not mentioned by Bounds
* Perrotin	1900	299,900 ± 80	
* Perrotin	1902	299,860 ± 80	
Perrotin	1902	299,901 ± 84	Final declared value for series
Michelson	1924	299,802 ± 30	
Michelson	1926	299,796 ± 4	Birge corrected to 299,798 km/sec
Karolus & Mittelstat	1928	299,778 ± 20	Kerr Cell — registers systematically low
Pease & Pearson	1935	299,774 ± 11	Unstable base line — systematically low
Anderson	1941	299,776 ± 14	Kerr Cell — systematic error — low result

'minor support' however. On the other hand he notes in the paragraph under his final table that the data presented is "deficient" and "on this basis alone the conclusions may be rejected". This may indicate that Bounds' cautious conservatism could be a little one-sided in its application.

DEFICIENT DATA CLAIM REFUTED

Now if the data is supposed to be deficient, what are the values of the supposedly more accurate data from the original sources that Bounds has elucidated and what extra values has his scholarship turned up that have otherwise been missed? Remembering that this is his basis for rejecting the conclusion that c has decayed, and remembering that he acknowledges that the listing is incomplete in his paper, let us tabulate the accurate data revealed by Bounds' analysis up to 1945 in Table 8.

From Bounds' statement one might have expected that some substantive data had been omitted. Instead, the only values omitted from the International Edition of **Ex Nihilo,** vol. 1, no. 1, 1982 were the 5 starred values in Table 8. Of these three were from Perrotin (about whom there was some confusion in the literature[11]), one was Dorsey's re-working of Cornu's value and the other was the Delambre/Glasenapp average, and this value had been placed against Roemer on a mistaken reading of Froome and Essen. I thank Vivian Bounds for clarifying that position. The addition of these five extra data points does nothing to invalidate the basic proposition. A glance down the figures of Table 8 leaves the impression unchanged that the experimental results favour a decay in c with time.

VALUES REJECTED BY EXPERIMENTERS AND BOUNDS APPENDIX 1

It should be pointed out that Bounds considers values rejected by the experimenters themselves. This seems rather incongruous: the experimenters of the day were in a far better position than us to be able to assess which experiments were successful and which had problems associated with them. In any case there are only four, and Bounds admits that

Table 9. Rejected Values

EXPERIMENTER	DATE	VALUE OF C (km/sec)	COMMENT
Cornu	1872	298,500 ± 300	Rejected by Cornu
Michelson	1878	300,140 ± 300	Rejected by Michelson
* Newcomb	1880	299,627	Rejected by Newcomb
* Newcomb	1881	299,694	Rejected by Newcomb

* N.B. These Newcomb values were for AIR NOT VACUUM and correction is thus somewhat uncertain as the atmospheric conditions were unknown. A nominal correction can be applied but problems still exist: see Bounds Reference 38.

the experimenters themselves rejected them. They are listed in Table 9 for the sake of completeness, thus giving a full listing of all values that Bounds considers in the period to 1945.

Bounds' inconsistency in considering these values becomes apparent when his statements in the concluding paragraphs are noted. In talking about the procedure he felt should be adopted to make the data acceptable, he admits that some data should be "omitted altogether according to their reliability". If any data qualifies for omission, surely it is that rejected by those who did the experiments themselves. It was on this basis, and on the basis of critical comment that data, though included in the relevant Tables in the International **Ex Nihilo,** vol. 1, no. 1, 1982 was not considered for the final refined curve of decay. Bounds criticises this procedure in Appendix 1. The reply is simple. The values ARE included in the Tables in the full article. Furthermore, when ALL these values are INCLUDED in the data analysis they give A HIGHER RATE OF DECAY ON ALL REGRESSIONS USED.[12] In other words, Bounds' claim that the proposition of c decay should be rejected on the basis of data omission cannot be supported.

THE POST-1945 VALUES AND BOUNDS APPENDIX 1

As far as the post-1945 results are concerned, it can be stated that they are generally far less critical to the c decay proposition. In Appendix 1 Bounds admits that omission of the results of the 1950 experiments by quartz modulators was "probably justified" given the large spread of results by McKinley and Houstoun (500 km/sec and 180 km/sec respectively). Indeed the frustration experienced by those doing this experiment is apparent and they admit that an accurate value for c was not easy to obtain. In the words of Houstoun[13] "to say that its determination gives no feeling of aesthetic satisfaction is an understatement". As far as the radar measurements were concerned Bounds himself admits that there are problems not only due to the limits of error, but also with the conversion of the values of c in air to that of c in a vacuum. It is stated that "the refractive index correction of air at radio frequencies is so dependent upon the moisture content in the air that there is little point in relating this (air value of c) with c_0 (vacuum) without information concerning atmospheric conditions". Again, "the refractive index correction for a path of varying height is difficult to compute". For this reason, these values in air were omitted. Aslakson's value was retained because his experiment measured the atmospheric variables as required and issued a reliable vacuum reduction.[14] As far as the spectral lines omission was concerned, the reason may be found in the statement that[15] "the accuracy in measuring. . .is thus reduced to 1 part in 10^4, though some systematic errors may cancel." This means that the final result may be doubtful to the order of tens of km/sec. Under these circumstances, Bounds' caution in criticising the omissions post-1945 seems justified and his rejection of the c decay proposition on this basis is nullified.

THE ROEMER VALUES

There are left only two disputed points to consider. These are the Roemer and Bradley values. Bounds is to be commended for his generally satisfactory presentation of the experiment that Roemer conducted to obtain his figures. There are several aspects which could have been mentioned but are not vital at this stage. The main thrust of the criticism of the adopted value in **Ex Nihilo** concerns the supposed misreading of the Goldstein et al paper. As has been explained elsewhere, the value quoted by Froome and Essen was taken to its error limit and harmonised with the maximum possible error margin from the Goldstein et al result. As Bounds has pointed out, the value given by Froome and Essen is in error and so the exercise is thus rendered invalid.

1. Goldstein's Error Margin Faulty

However, much is made of the Goldstein et al statement that the value of c obtained by a reworking of Roemer's data did not differ by 0.5% above or below the current value.[16] Proof positive that c has **not** decayed. . .? On the contrary, one does not have to go beyond the introductory statement in their paper to notice that there is an immediate problem. What Goldstein et al did was to adopt a model for the Jupiter — Io system and calculate the eclipse times for any given position of the earth. Roemer's 40 reliable eclipse times were then compared with the times from this model. As Goldstein and his co-workers point out at the beginning, their method results in a root mean square deviation of observed times compared with the model of 118 seconds. In other words, the model predicted times that were not matched by Roemer's observations, the difference averaging almost 2 minutes! Out of a total of 40 observations there were only two shining examples of anything like agreement, one being only 3.1 seconds out and the other being 5.9 seconds out, the rest being out by about 10 seconds or more. This renders their conclusions of somewhat lower significance than has been placed on them hitherto. An error estimate of 0.5% is inconsistent with 118 seconds rms deviation.

2. Goldstein's Incorrect Procedure

However, there is a further problem. It takes the form of an unfortunate conceptual blunder and a mislabelled Table (Table IV). To understand the problem, let us define the following quantities. T_0 = times of observed events of Io; T_a = calculated times of events; T_f = calculated times of observations = (T_a + D) where D = calculated delay time due to light travel. The authors adjusted the delay times to minimise the sum of $(T_0 - T_f)^2$. Their error arose as follows. The calculated times of events T_a were adjusted to an "empirical" initial point. The ACTUAL PHASE of Io was NOT projected back over 300 years in absolute time contrary to Bounds' assertion. That would involve knowing the orbital period of this satellite of Jupiter to an accuracy better than 1 part per billion. Instead, the adjustment was accomplished by setting the sum of $(T_0 - T_f)$ to zero. THEY FAILED TO READ JUST THE PHASE AS THEY VARIED D. Hence, instead of adjusting c to account for the VARIATION in the period of Io, they were adjusting the AVERAGE predicted time of observation to agree with the average predicted time of observation. This procedure gave them right back again as the best value the **same value of c** that they used to adjust the initial phase of Io. In addition, the data given in their Table is really the predicted minus the observed times. Lew Mammel Jr. of AT and T Bell Laboratories has pointed out with surprise (he was criticising c decay[17]) that when the correct procedure is adopted (which involved subtracting the average delay time D_0 from each D and following through the maths) he had to SUBTRACT 6% of the nominal delay time FOR EACH DATUM to get the best fit. The delay times were therefore being REDUCED by 6%, meaning that the value of c was 6% higher than now or of the order of 317,700 km/sec. The expected error is 8.6%. He points out that adjustment to the longitude of the ascending node of Io with respect to the plane of Jupiter's orbit will reduce the value and error somewhat and the final calculation should be possible once this quantity is known for the period around 1670. Nevertheless the value of c will be significantly higher than the present. Conclusion: Roemer's data when re-worked correctly by the Goldstein method shows c was somewhat ABOVE the present value and is completely consistent with a decay in c with time.

3. Other Results

By way of confirmation of this result it should be noted that Delambre, from an immense number of observations of eclipses of Jupiter's satellite in the 150 years to 1809 fixed the delay across the radius of the earth's orbit as 493.2 seconds.[18] Using the standard value of 1.496×10^8 km for this radius we obtain c = 303,300 km/sec. Again in 1875 Glasenapp[19] of Pulkova reviewed all available eclipses of Io between 1848 to 1870 and obtained results between 496 and 501 seconds delay across the earth orbit radius, with an average of 498.5 seconds. This latter result gives a value of c of 300,100 km/sec. More recently, Sampson[20] in 1909 derived a value of 498.64 seconds from his own reading of the Harvard Observations while the Harvard readings themselves gave a value of 498.79 ± 0.02 seconds, the difficulty being the inequalities of Jupiter's surface preventing a more exact determination. Thus Sampson's value for c becomes 300,016 while the official Harvard records give 299,925 for the same epoch. These values by the Roemer method are thus in accord with the other experimental values that indicate a decay in c. The Roemer method values are given in Table 10.

Again, these figures would seem to support the basic downward trend that the other experimental determinations of c indicate. Certainly there is nothing inconsistent with such a proposition. Bounds' appeal to the Roemer values to refute the claim is therefore on the basis of misleading information supplied by Goldstein et al. Their stated error margin of 0.5% is inconsistent with the rms error of 118 seconds (118 seconds in about 1000 is an 11.8% error equivalent to ± 35,000 km/sec) and the further

Table 10. Roemer — Type Experiments

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

Table 11. Bradley — Type Experiments

MEAN DATE	TIME OF OBS.	OBSERVER	VALUE OF K"	VALUE OF C (km/sec)
1740	1726-1754	Bradley	20".43	300,980
1783	1750-1816	Lindenau	20".45	300,690
1841	1840-1842	Struve	20".445 ± 0.011	300,760 ± 160
1862	1842-1882	Nyren	20".492 ± 0.006	300,070 ± 90
1909.5	1904-1915	Zemtsov	20".500	299,950
1914	1913-1915	Numerov	20".506	299,870
1916	1915-1917	Tsimmerman	20".514	299,750
1922	1915-1929	Kulikov	20".512 ± 0.003	299,780 ± 45
1926.5	1925-1928	Berg	20".504	299,895
1935	1929-1941	Romanskaya	20".511 ± 0.007	299,793 ± 100

problem of methodology that will always give rise to the assumed value for c invalidates the exercise. It is surprising that Bounds passed over the first of these problems at least, without noticing it.

THE BRADLEY - TYPE EXPERIMENTS

This finally brings us to the Bradley value which Bounds gives as 303,000 ± 6000 km/sec. This is still well above the current value of 299,792 km/sec and consequently this determination is not at variance with the changing c suggestion. This figure has been re-worked by a number of authorities including Auwers, Newcomb, Bessel, Peters and Busch. Auwers criticised Busch's reductions, however. All told, these authorities examined Bradley's observations of gamma Draconis at Kew in 1726-1727, his observations of 23 stars at Wanstead during the period 1727-1747, and his observations at Greenwich of gamma Draconis from 1750-1754. The average of these results omitting Busch's re-workings (which give a higher value for c) gives a final value for the aberration constant, K, of 20".43 for a mean date of 1740 AD. If, for the purposes of this exercise, we adopt an approximation to the standard formula used by the Pulkova Observatory

$$c = 6149050/K \qquad (1)$$

then this gives a value for c from Bradley's observations of 300,980 km/sec. However, Bradley was only the first to use this method to determine the value of c. An article in **Nature** of May **13, 1886,** p. **30** mentioned the results obtained at the Pulkova telescope. A further listing was given by Romanskaya.[21] From these observations at Pulkova, plus the Bradley results, Table 11 summarises the Bradley-type evaluations of c.

From these values even a linear regression results in an approximate decay rate of 6 km/sec per year, the straight-line fit having a value of r^2 = **0.89.** These results thus indicate that a decay in c has been registered experimentally by a method whose errors have remained substantially constant. Accordingly, the recorded decay in c evidenced by these values cannot be attributed to equipment improvement or reduction of errors in measurement. This strongly suggests that the decay is genuine. A complete list of data by this method is being prepared for future publication, but a discussion of it is beyond the range of this reply due to space limitations. A preliminary inspection of this additional data indicates a decay roughly similar to the above sample. Bounds may argue about the accuracy of one value by this method, but when a number of comparably accurate measurements indicate a decay over a period of 200 years by the same method, then the basis for Bounds' argument tends to evaporate.

FINAL CONCLUSIONS

In conclusion, it has been shown from the values produced by Bounds' own scholarship that a decay has been registered by each type of experimental equipment employed. Furthermore, the data deficiency claimed by Bounds comprises five values, three of which come from an experimenter about whom there was some confusion in the literature. When these values are included there is nothing inconsistent with c decay. Again, only two values were disputed, Roemer and Bradley. Roemer's is nullified on the basis of inaccurate error limits and incorrect mathematical procedure adopted in Bounds' prime reference material, and Bradley's on the basis that a listing of all experiments by that method do show a decay, the proof not resting upon a single isolated value. Bounds finalises by saying his criticisms have been "minor". Investigation proves this to be true. The space allotted to this minor criticism tends to make it look major and conveys a misleading impression to the reader, more particularly since much of it is invalidated by published material not seen by Bounds before he wrote. Accordingly, his lengthy criticism generally is seen to be without substantial foundation.

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