

APPENDIX 5

CRITIQUE OF ALLEGED EVIDENCE FOR DECREASING G

by

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ABSTRACT

The claim by Van Flandern that empirical data indicates $\dot{G}/G = (-8 \pm 5) \times 10^{-11}/\text{yr} < 0$ is reviewed. Large but tacit ad hoc assumptions are pointed out, including complete neglect of several profound and fundamental yet unsolved problems of gravitational many-body celestial mechanics (including truncation effects in [literal] 'general perturbations' and resonance problems in [numerical] 'special perturbations'). The data are equally compatible with $\dot{G}/G = (-8 \pm 8) \times 10^{-11}/\text{yr}$; positional astronomy cannot confirm or refute the value of $\dot{G}/G \approx 3 \times 10^{-13}/\text{yr} > 0$ which cosmological evidence supplies.

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May 2, 1975

Mrs. Deb Bensen
Gravity Research Foundation
58 Middle Street
Gloucester, MA 01930

Dear Mrs. Bensen:

Enclosed are three copies of an Errata sheet for Appendices 1-4 of my Essay submitted on March 29, 1975. (I have no corrections for the Essay itself.)

Subsequent to the April 1 deadline for the contest, Dr. Van Flandern (APS Bulletin, April) and Drs. Fomalont and Sramek (Physics Today, April) have received considerable national publicity (newspaper articles, etc.) regarding statements which, if taken at face value by the Foundation's judges, would destroy the validity of my Essay. Since the judges can be expected to have read their daily newspapers, it seems only fair to allow me to submit post-deadline rebuttals to their many fallacious statements, which I have done in the new Addendum and Appendix 5, which I trust you will forward to the judges as expeditiously as possible.

Sincerely,

Robert W. Bass

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Professor of Physics and Astronomy

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It has been shown elsewhere that abundant geophysical, astrophysical ($\gamma > 1$), and cosmological ($q < \frac{1}{2}$) evidence requires, with 95% probability, that standard General Relativity (GR) must be modified in a manner similar to but in essence exactly opposite to the Jordan, Brans, Dicke (JBD) theory, to include a variable Cavendish parameter G satisfying the two properties

$$(1a) \quad (\delta G/G) > 0 \quad , \quad (b) \quad (\dot{G}/G) > 0 \quad .$$

Here (1a) means that the value of G measured by two test-particles increases as the test-particles approach a ponderable mass, e.g., the Sun; a critical test which could be performed by a gravitational clock onboard the Helios satellite (perihelion $1/3$ AU, aphelion 1 AU) would be

$$(2) \quad \frac{G_{\text{per}}}{G_{\text{aph}}} = \begin{cases} 1 + 0.9 \times 10^{-9} & , \text{ present theory;} \\ 1 & , \text{ GR;} \\ 1 - 1.3 \times 10^{-9} & , \text{ JBD .} \end{cases}$$

Similarly, (1b) means that G increases as cosmic time t increases, e.g., there is a Milne-type relation

$$(3) \quad G \propto c^3 t / M \quad , \quad (0 < t < + \infty)$$

where M denotes essentially the rest-mass of the visible universe.

A summary of the evidence in favor of the present theory is as follows:

1. GEOPHYSICAL EVIDENCE

1.1. The interior temperature of the Sun has been measured by neutrino astronomy. According to accepted nuclear transmutation theory, the corresponding thermonuclear energy production is too small to account for the Sun's presently observed luminosity L . Experts have stated that the lack of a non-ad hoc explanation for this anomaly is a scandal which has reached crisis

proportions. However, if $(\dot{G}/G) > 0$, then the resulting contraction of the Sun's core would provide the desperately sought for additional energy source.

1.2. There is an unexplained discrepant residual terrestrial core heat efflux of $35 \text{ ergs/cm}^2\text{sec}$. The most straightforward assumption which would account for this anomalous energy production would be the hypothesis that $(\dot{G}/G) > 0$, which would entail compression, friction, heating and loss of heat to the crust by convection and conduction.

1.3. There is an unexplained secular acceleration of the Earth's rate of rotation, determined originally by Dicke and Curott as $(7 \pm 3) \times 10^{-11}$ parts per year, and in 1974, after a more extensive analysis, by Muller and Stephenson as $\dot{\omega}/\omega = (15 \pm 6) \times 10^{-11}$ parts per year. [The R. R. Newton, Hoyle, and Van Flandern attempts to explain away this anomaly will be dealt with below.] Accepting the Hess-Murphy estimate that $\dot{\omega}/\omega = -\dot{r}/r \approx (0.1)(\dot{G}/G)$, this yields $\dot{G}/G \approx (15 \pm 6) \times 10^{-10} > 0$.

2. ASTROPHYSICAL EVIDENCE

1.1. Highly persuasive arguments have been presented (e.g., by Bondi and by Sciama) that classical GR is fatally incomplete unless it is modified by inclusion of the Berkeley-Mach principle. The interpretation of this principle in the manner of Einstein, Milne, and Sciama leads immediately to (1 a,b), which is the exact opposite of the basic hypothesis of the JBD theory.

1.2. The hypothesis (1 a,b) is, in the class of scalar-tensor field theories, mathematically identical to the requirement that the Eddington-Robertson (PPN) parameter γ satisfy

$$(4) \quad \gamma > 1 ;$$

whereas, in GR, $\gamma = 1$; and, in JBD, $\gamma < 1$. A Maximum Likelihood estimate of

γ from all 24 known solar system experiments yields

$$(5) \quad \gamma = 1.02 \pm 0.01 ,$$

i.e., (4) holds at the 2σ confidence level.

3. COSMOLOGICAL EVIDENCE

3.1. It can be shown that the observed hyperbolicity of the universe ($q < \frac{1}{2}$: Sandage, 1974; Gunn, 1974) is physically incompatible with GR and JBD and, in fact, necessitates (3), i.e., (1b), which via (4) is equivalent to (1a).

3.2. The hypothesis (1a,b) implies that complete gravitational collapse is impossible: black holes do not exist; and the scatter-diagram nature of the quasar red-shifts can be readily explained by the hypothesis that quasars are merely compact pulsating stars (red-shifted by Mach's principle).

3.3. The experimentally observed result (5) implies that the dimensionless coupling constant between the (1/G) field and its mass-density source, ω , satisfies

$$(6) \quad \omega \cong -30 .$$

Insertion of Sandage's 1974 values $H^{-1} \cong 2 \times 10^{10}$ years, $q = 0.05$, in the newly discovered relation $\dot{G}/G \cong (3/|\omega|)qH$ then yields $\dot{G}/G \cong 3 \times 10^{-13}/\text{yr} > 0$.

4. CRITIQUE OF ALLEGED COUNTER-EVIDENCE

The preceding eight separate reasons for postulating $\dot{G}/G > 0$ make a prima facie case that G is increasing. However, except in the neglected 1935 work of E. A. Milne, the hypothesis of increasing G has been so little considered that nearly all of the scores of authors who have discussed the possibility of a varying G have assumed (with little more justification than the 1938 speculation of Dirac) that the only possible case to be investigated was the case of $\dot{G}/G < 0$.

Fortunately, however, virtually all of these investigators have recognized that their attempts to make an empirical determination of \dot{G}/G , however, exhaustively pursued and diligently argued, in the final analysis rested on very shaky grounds and of necessity remained imponderable pending definitive resolution of certain profound unsolved problems in dynamical astronomy.

Of the many quotations to this effect which could be cited, we select only the following four:

- (a) R. Dicke: ". . . the foregoing discussion cannot be marshalled as evidence for a gradual decrease in the gravitational constant."
- (b) Sir Fred Hoyle: "Classical astronomers have an attitude to probable errors which I have always found strange. . . . A definitive answer to the question of changing G can be obtained by . . . within a few years this will undoubtedly be done . . . Whatever the ultimate outcome may be . . ."
- (c) L. V. Morrison: "It seems to me that [after reviewing essentially the same data which Hoyle and Van Flandern find to support $\dot{G}/G < 0$] the close agreement of . . . suggests that $\dot{G} = 0$, or allowing for an uncertainty of $\pm 3T^2$, $\dot{G}/G \lesssim 2 \times 10^{-11} \text{yr}^{-1}$."
- (d) R. R. Newton: "The changes in C_e [proportional to Earth's radius] and G cannot be inferred with confidence until an important question in lunar theory is resolved . . . the reader should demand considerably greater assurance than this paper can provide, in view of all the current uncertainties, before accepting the conclusion that G is decreasing and that C_e is increasing." [Italics added.]

The only investigator who feels (private communication) that he has established $\dot{G}/G < 0$ at a 3σ confidence level is Van Flandern, who has recently (Mon Not. Roy. Astr. Soc., vol. 170 [1975], pp. 333-342) published a very

thorough discussion of the subject. Accordingly we shall here restrict discussion principally to a critique of Van Flandern's analysis.

Briefly, Van Flandern uses the Hoyle method

$$(7) \quad \dot{\frac{G}{G}} = \frac{\dot{n}_M^G}{n_M}$$

where n_M denotes lunar mean motion $n_M = 17''.33 \times 10^8/\text{cy}$, and where, by definition,

$$(8) \quad \dot{n}_M^G = \dot{n}_M^{\text{AT}} - \dot{n}_M^{\text{ET}},$$

where the superscripts AT and ET refer, respectively, to Atomic Time and Ephemeris Time. Van Flandern's analysis leads him to choose the values

$$(9a) \quad \dot{n}_M^{\text{AT}} \equiv \dot{n}_1 \pm \sigma_1 = (-65'' \pm 18'')/\text{cy}^2,$$

$$(9b) \quad \dot{n}_M^{\text{ET}} \equiv \dot{n}_2 \pm \sigma_2 = (-38'' \pm 4'')/\text{cy}^2.$$

Then, by the theory of probability,

$$(10a) \quad \dot{n}_M^G \equiv \dot{n}_3 \pm \sigma_3 = (-27'' \pm 19'')/\text{cy}^2,$$

because

$$(10b) \quad \dot{n}_3 = \dot{n}_1 - \dot{n}_2,$$

$$(10c) \quad \sigma_3 = [(\sigma_1)^2 + (\sigma_2)^2]^{\frac{1}{2}}.$$

Hence by (7) he has

$$(11) \quad \dot{G}/G = (-8 \pm 5) \times 10^{-11}/\text{yr} < 0.$$

But his claim to have proved that $\dot{G}/G < 0$ would be completely destroyed if it could be shown that systematic error sources not considered by him rendered

more prudent, instead of (9a,b), the choices

$$(12a) \quad \dot{n}_M^{AT} \equiv \dot{n}_1 \pm \sigma_1 = (-65'' \pm 25'')/\text{cy}^2 ,$$

$$(12b) \quad \dot{n}_M^{ET} \equiv \dot{n}_2 \pm \sigma_2 = (-38'' \pm 13'')/\text{cy}^2 ,$$

for then by (10b,c) we should have to replace (11) by

$$(13) \quad \dot{G}/G = (-8 \pm 8) \times 10^{-11}/\text{yr} \begin{matrix} \geq \\ < \end{matrix} 0 ,$$

which is quite compatible with the value

$$(14) \quad \dot{G}/G \cong 3 \times 10^{-13}/\text{yr} > 0$$

estimated in the present theory.

Thus we have reduced everything to the question of whether or not the confidence levels of (12a,b) are more appropriate than those of (9a,b).

That the issue just raised is not a mere debating point, but a very fundamental and profound unsolved problem in dynamical astronomy can be seen by reference to any of the more mathematically rigorous modern monographs on celestial mechanics (e.g., those of Arnol'd, or Siegel, or Moser), or by consideration of Sir Fred Hoyle's comment on this point from his 1972 Presidential Address to the Royal Astronomical Society: "Classical astronomers have an attitude to probable errors which I have always found strange. It is frequently the practice to quote small 'internal probable errors' without mentioning the possibility of gross systematic errors. In the present case, the 'internal' error is probably about 10 percent of the quoted value. What the systematic error might be I do not know. Allowance has to be made for planetary perturbations involving the fine details of the lunar gravitational theory."

The two most important dynamical astronomers of the past century were Simon Newcomb and Ernest W. Brown. Much as their achievements deserve admiration, the fact has to be faced that in the final analysis their work [which is the basis upon which \dot{n}_M^{ET} of (9b) has been determined] rests upon quicksand. The series which they employed do not, in general converge, and a physical result computed from a divergent series is quite literally meaningless nonsense. Space precludes a full rehearsal of this matter here, but I have (Bass, 1974) already published a very extensive discussion of this matter which includes citation of more than 70 technical works on celestial mechanics and dynamical astronomy. Newcomb thought that his 'general perturbations' would be valid for 10^9 years. In 1931, in his retiring Presidential Address to the American Astronomical Society, E. W. Brown reduced the claim to $10^8 - 10^7$ years, but shortly thereafter with no further explanation, retreated to $10^7 - 10^6$ years. The plain truth (which has been sketched by dynamical astronomer, W. M. Smart, and demonstrated in explicit detail by myself) is that such calculations cannot possibly be relied upon for more than 10^5 days \cong 300 years [Smart's exact words are "one or two centuries"], although these series were employed over a 2000-year interval in the determination of \dot{n}_M^{ET} .

The discrepancy between the asserted but not proved Newcomb-Brown results and the rigorously demonstrated Smart-Bass results just quoted can be understood more readily if one is familiar with some little-appreciated confessions that Brown made toward the end of his career:

The mathematical processes which are used in developing the theories of the planets . . . are largely formal. While mathematical rigor is desirable when it can be attained, nearly all progress . . . would be stopped if complete justification of every step in the formal processes is justified whenever experience shows that the results . . . are useful for the prediction of physical phenomena. Thus when calculating with an infinite series whose convergence properties are not known, one has to be guided by the results obtained; if the series appears to be

converging with sufficient rapidity to yield the needed degree of accuracy, there is no choice save that of using the numerical values which it gives. We have not attempted to deal with convergence questions, but have retained throughout the practical point of view. . .

Those of us who, like myself, are mainly concerned with the applications of the laws of motion and gravitation to the actual problems presented by nature are often quite justly accused of a lack of logic in the processes we follow. Sometimes this apparent lack of logic has been explained, as in the case of divergent series, which Poincaré showed could give a limited degree of numerical accuracy. In other cases no explanation is forthcoming and we fall back on the unproved thesis that since these processes coordinate the phenomena with high accuracy, the probability that they are valid is great. . . . All progress would be stopped if the rigid standards of the pure mathematician had to be applied at every stage of the work. The point of view I take is similar to that of the physicist who generalizes from a limited number of experiments.

A considerable portion of what I have to say is . . . quite frankly critical in the destructive sense. Owing, perhaps, to the fact that the number of those who work in my field is very small, a certain amount of dust has gathered in the corners. In any subject in which there is considerable activity, correction of errors, fundamental or otherwise, is often rightly regarded as unnecessary: the results are soon forgotten. In my subject it has not been so. Statements which are wholly or partly erroneous have obtained currency and I have found no solution except that of giving the reasons why such statements cannot be accepted. . . .

The validity of the proofs of many of the earlier theorems which have been quoted has disappeared. . . . [Italics added.]

In order to see how basic this time-interval of validity is to the matter under consideration, return to the 1968 investigations of R. R. Newton cited above, wherein he is engaged in work very similar to that of Van Flandern represented in (9) - (11). Newton needs to determine a parameter μ which according to present observations has the value $\mu = -0.6$. But, using this value he computes that the radius of the earth is shrinking and that G is increasing! However, he finds these results so "implausible" that he arbitrarily replaces μ by 0.001 of its present value, in order to find G decreasing. His only justification for this completely ad hoc procedure is a quotation from Brouwer and Clemence based upon the Newcomb-Brown idea of "millions of years" [which, as we have seen, was mistaken by a factor of 10^4]. He says:

IF the variations in e_s over the past several million centuries have remained within the bounds predicted for the next million years and IF the expression above represents the lunar perturbation adequately over several million centuries, we may expect the average lunar acceleration to decrease as the time span used in averaging increases. Thus it is PLAUSIBLE that the correct value of μ to use here is less in magnitude than, say, 0.001 of its present value.

A serious difficulty arises in knowing whether the perturbation is adequately represented over a time interval as long as several million centuries. As Brouwer and Clemence say, there is ' . . . some ground for belief that the mathematical expressions have some physical significance for at least several million years, although they may have no validity at times as remote as a thousand million years.' Thus, UNTIL the lunar motion is solved with reasonable accuracy for the past several million centuries, the value of μ to use in (4) must remain an ASSUMPTION. [Emphasis added.]

The preceding quotations from R. Newton make crystal-clear the intimate dependence of \dot{n}_M^{ET} on the time-length of validity of the 'general perturbation' series although the uninformed reader of Van Flandern's paper might receive a different impression since he introduces \dot{n}_M^{ET} under the heading "OBSERVATIONAL DATA UTILIZING EPHEMERIS TIME." (In reality, of course, what he should have said is: "observational data after reduction by an unreliable non-rigorous intuitive but traditional procedure which is most assuredly more of an art than a science.")

Any reader who doubts whether the preceding characterization of dynamical astronomy is fair is invited to consult my exhaustive 1974 treatment of this matter, which it is impossible to summarize adequately in the present space.

However, the errors introduced by the use of divergent series quite simply cannot be estimated accurately by any method known to science. Therefore, I leave it to the reader whether or not the replacement of (9b) by (12b) is or is not justified in view of the giant assumption which Van Flandern has tacitly made in taking for granted the traditional Newcomb-Brown dogma that 'general perturbations' are valid for millenia instead of mere centuries.

To reiterate this point, return to R. Newton, who is rather more explicit about his procedures than Van Flandern: "The reader may well have become concerned about the large number of assumptions that enter. . . . Every combination of assumptions tested leads to the result that $\dot{G} < 0$ and $\dot{C}_e > 0$ EXCEPT the use of implausibly large values of the parameters μ and σ ." But as we have displayed quite explicitly above, the so-called "implausibly large values" are precisely those that correspond accurately to contemporary observations! Furthermore, their arbitrary and ad hoc reduction by a factor of 10^3 was motivated by nothing whatsoever more substantial than the traditional prejudice of astronomers in favor of vast time scales, a prejudice already exposed and quantitatively demolished in 1953 by the Regius Professor of Astronomy in the University of Glasgow, W. M. Smart.

The technical difficulty to which we have alluded above is referred to in this context as "a truncation effect in the planetary perturbation terms of the theory" by Garthwaite, Holdridge and Mulholland (1970), who developed the numerical integration technique used by Van Flandern in his determination of \dot{n}_M^{AT} . They also characterize this problem as ". . . discrepancies [which] arise from numerous very small terms whose arguments are closely related to those given here and whose periods differ only slightly from them." In other words, they refer to the effects of dynamical resonance.

There are four separate sources of error in Van Flandern's value (9a) for \dot{n}_M^{AT} , namely

- (i) omission of the effects of finite mass of the Moon in the force function for the perturbations on the Moon due to the oblateness of the Earth;
- (ii) omission of the effect of precession of the Earth's figure, when calculating the same perturbation;

(iii) a libration theory truncated too soon, so that the mean orientation of the lunar bulge toward the Earth was not exactly in the mean Earth direction; and

(iv) omission of complete planetary perturbations.

When Van Flandern included the errors (i) - (iii) one at a time, he realized that his formal mean error in $\dot{n}_M^{AT} = (-65'' \pm 10'')/\text{cy}^2$ was too small to allow for these effects; but when he replaced this value by that given in (9a), namely $(-65'' \pm 18'')/\text{cy}^2$, the errors were covered when considered singly. In view of the fact that (iv) was ignored, and in view of the fact that resonance effects do not show up unless all four corrections (i) - (iv) are considered simultaneously, I leave it to the reader to decide whether or not prudence requires the replacement of (9a) by $(-65'' \pm 25'')/\text{cy}^2$ as in (12a).

This completes the demonstration that Van Flandern's claims (11) are quite unsubstantiated and that (13) is the best that can be soundly deduced from the observations.

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