letters

Time variability of G

Because of the interest generated by the article by Paul Wesson, as indicated by the letters that appeared in March, and because of my interest in the problem, I would like to contribute the point of view shared by me and my collaborators on the problem of the possible time variability of G.

While it is clear that the constant versus variable G dichotomy will only be decided by observations, it is also clear that before investing time and effort in determining $\dot{G}/G$, experimentalists have the right to know whether there is anything wrong with the idea of a time-variable G. For that reason, cosmologists, astrophysicists and geophysicists have tried to test what is the bare minimum requirement for a new idea to be taken seriously and further investigated, namely that of being compatible with well established facts. The "consistency test" process that began in 1949 has continued until very recently, seemingly adding every time further doubts about the possibility that G might indeed vary with time.

For the sake of completeness, let me cite the tests most frequently used:

- Luminosity of the sun: $L_\odot \sim G^7 M^5$
- Radius of a planet $r \sim G^{-1/3\gamma-4} \times M^{-2/3\gamma-4}$
- Luminosity of white dwarfs
- Nucleosynthesis and the expansion of the universe: $\dot{R}/R \sim (\rho_\gamma G)^{1/2}$
- 3 K blackbody radiation $\rho_\gamma \sim T^4$

As an example of what a variable G is believed to imply, let us consider the fourth test above: A gravitational constant G that decreases in time as t^{-1} implies such a fast expansion rate as to allow no time for nuclear reactions to occur, so that no helium and deuterium could have been produced in the big bang. Analogously, from the first test one can conclude that the sun by now should probably be a red giant. Equally serious troubles ensue from the third and fifth tests. The implications of all these tests *together* with a $G = G(t)$ seem to be so serious as to discourage even the most ardent supporter of a variable G.

However, a careful analysis indicates that there is a fatal flaw in the use of these tests to detect the variability of G. Consider Newton's law $F = -Gm_1m_2/r^2 = -\nabla V(r)$, where $V(r)$ is the potential energy. If we replace G with $G(t)$, then the potential energy becomes time dependent and energy is no longer conserved. (This point was first stressed by W. T. Bishop and P. T. Landsberg, Nature **264**, 346, 1976). Time variation of G and energy conservation do not mix. However, the test relations use mixed expressions such as $\rho_\gamma \sim T^4$ and $p \sim \rho^\gamma$ (which are consequences of the first law of thermodynamics, that is, conservation of energy) together with a time-variable G.

More technically, one can say that all the relations used so far in the study of a time variable G are derivable from the conservation of energy and momentum, expressed, for example, by $T^{\mu\nu}{}_{;\nu} = 0$, a relation that is compatible with the Einstein equations only if G is constant. Therefore the standard framework is intrinsically inadequate to deal with a time varying G. Unfortunately, this important point was overlooked in most of the attempts aimed at quantifying the consequences of a variable G, thereby leading to incorrect conclusions.

Another important point refers to the observations themselves. It should be kept in mind that one does not actually measure $\dot{G}$, so sentences like "the measured value for $\dot{G}/G$ is such and such . . . " may be very misleading; they are operationally incorrect. What experimentalists measure is the time dependence of planetary periods ($P = 2\pi/n$) and distances, which can then be translated into a value for $\dot{G}/G$, provided one has a theory: The relation $n = n(G)$ is in fact theory-dependent. For example, the often quoted relation

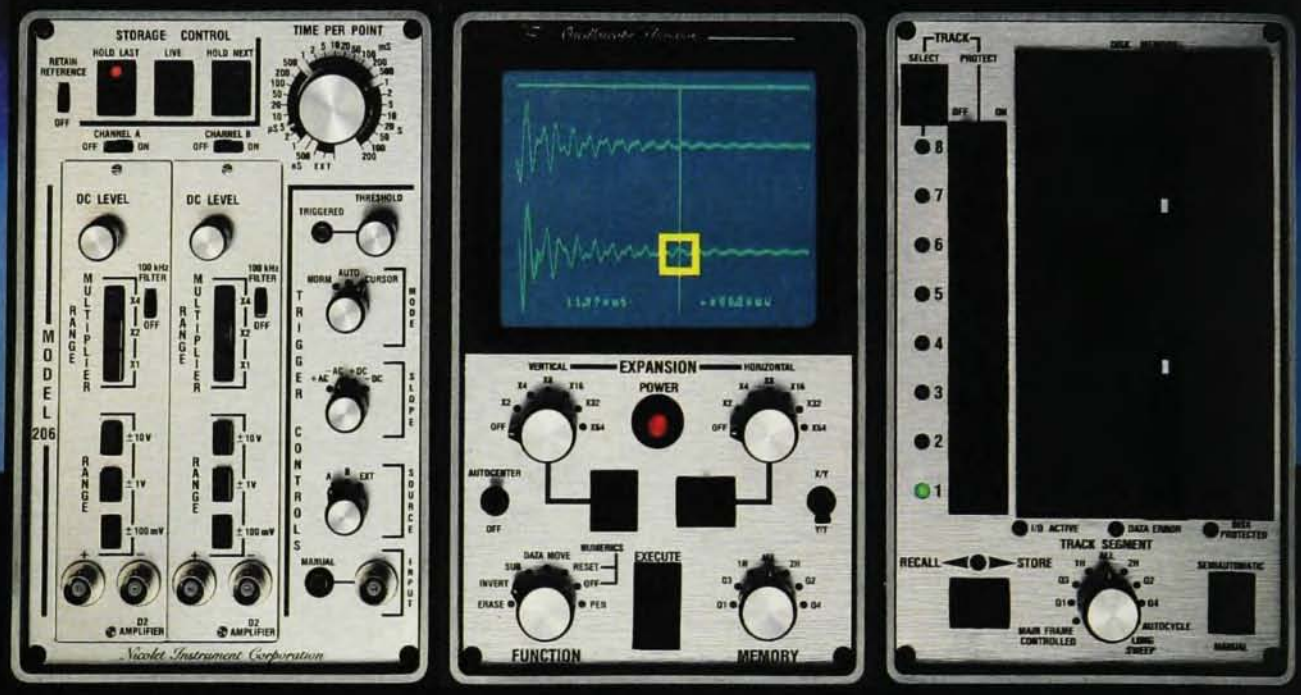
$$\dot{n}/n = (2/GM)d(GM)/dt = 2\dot{G}/G$$

which is based on Newtonian mechanics (considered as a limit of Einstein's theory) should actually be written as

$$\dot{n}/n = 0$$

because the conservation law $(GT^{\mu\nu})_{;\nu} = 0$ implies GM is a constant. The use of a variable G while keeping M constant is invalid.

To quantify the effects of a variable G



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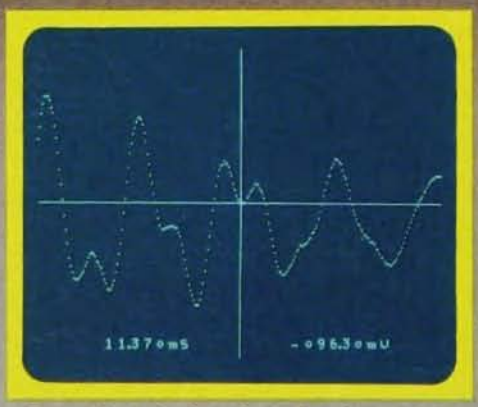

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one needs a framework that allows G to vary. Carl Brans and Robert Dicke proposed a scheme in which G can vary with time while energy is required to be conserved. The price they had to pay is that the Einstein equations got modified, leading to well-known difficulties.

In 1977 S. -H. Hsieh and I (together with P. J. Adams and E. Tsiang) proposed the scale-covariant approach,[1] a formalism (not a full theory as yet!) that allows one to study the consequences of a time-variable G in a consistent manner, free of internal contradictions. We have so far analyzed the five tests above[2] as well as other cosmological data,[3] with the exception of nucleosynthesis, which is presently under study. The result of our work is that not a single contradiction has emerged. Furthermore, the quality of our fit to the data is either similar to fits with a constant G or often better, certainly never worse.

If nucleosynthesis will also yield positive results (as preliminary results indicate), the conclusion will be that a variable G is fully compatible with all present available data from astrophysics, cosmology and geophysics. Even in that case, however, we shall not be lured to conclude that G must vary, but only that if it does, nothing wrong follows.

Considering the widespread opinion generated over the last thirty years that a variable G can easily be disproved on several independent grounds, I believe that the positive verdict that is emerging from our work will motivate the experimentalists to increase their efforts to provide the conclusive answer to this problem.

References

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3. V. M. Canuto, S. -H. Hsieh, J. Owen: Astrophys. J. Suppl. **41**, 263 (1979); V. M. Canuto, J. Owen, Astrophys. J. Suppl. **41**, 30 (1979).

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Threat to free publication

During the past year an incident has occurred in the US which, I think, should interest all scientists, and indeed any person who believes it is important to preserve the great tradition of scientific research.

The Schlumberger-Doll Research Corporation of Ridgefield, Connecticut, made a contract with David Bergman of Tel Aviv University, a colleague of mine and a member of the American Physical Society, to obtain consultation services from him during 1979 and 1980. Bergman consequently spent some time at the Schlumberger Laboratories in Connecticut where he did research for the company in collaboration with regular company employees. Some of the results of that research, which were not of a confidential or proprietary nature, were written up and submitted for publication in the *Physical Review* by Bergman and Lacour-Gayet, a company employee, with full approval by the Schlumberger Corporation. Other similarly non-confidential results were in the process of being written up for submittal. At that point, the company notified Bergman that it would not allow any of these articles to be published since publication would associate the company's name in print with the name of a scientist holding a permanent position at an Israeli institution (Tel Aviv University). It was explained that this ban had been decided upon because the company, which carries on oil-well-logging activities in many countries, is afraid that terrorists might view this association unfavorably and seek to take revenge upon company employees and equipment. I believe this is the first time that such a position has ever been taken openly by any R&D company in the US.

The very idea of trying to anticipate the possible demands of blackmailers and accede to them in advance is so new that it does not even have a name yet! (Pre-appeasement?)

Bergman has complained about this incident to the American Physical Society, and they have taken up the issue with the Schlumberger Corporation.

The present position of the scientific articles under discussion is that those that were in preparation have been suspended, since the company has prevailed upon its employees to refrain from any further contact with Bergman. The article that had already been submitted to the *Physical Review* has been accepted for publication and is about to be published. However, the company has prevailed upon Lacour-Gayet to unilaterally withdraw his name and the company's name from the article, so that it is going to appear under Bergman's name only. This is, of course, a gross injustice to Lacour-Gayet, and a humiliation for Bergman to be treated like a pariah. Above all it is, I think, a serious blow to the principle of the freedom of scientists to do original research and publish it without any interference from political pressures or discriminatory practices.