letters

continued from page 15

tions that the Earth has, they might be more reliable than the apparently positive value of G obtained for Earth, treated as problematical in Wesson's article.

► *White dwarfs.* A more recent result, probably too recent to qualify for inclusion in Wesson's article, comes from the study of white-dwarf stars. If G varies, there will be a contribution to the luminosities of these stars in some of the variable-G theories, and recent analyses of existing observations illustrate that these theories encounter difficulties.[7]

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Wesson has given a very thorough update regarding the question of a varying gravitational constant. One method of testing this variation, but not mentioned by Wesson, is with the use of radioactive dating.

The uranium/lead method is based on the α-decay of uranium and the rubidium/strontium method on the β-decay of rubidium. According to R. H. Dicke, based on Dirac's suggestion, the α-decay constant should be essentially independent of the age of the universe, whereas the weaker β-decay constant should be inversely proportional to the age of the universe. In an attempt to discern some discrepancy, Dicke compared the ages, by the two dating methods, of the oldest dated material—meteorities—but came to no meaningful conclusion. A few years later, in 1963, Kanasevich and Savage attempted a comparison by using the ages of terrestrial rocks dated by both methods, and concluded that their data were more consistent with a β-decay rate independent of the age of the universe.

The applicability of the idea of radioactive dating to testing variable gravity still holds, and with the development of newer radioactive dating methods, and the accumulation of more Rb–Sr and U–Pb age comparisons, this approach may yet make a decisive contribution to this nagging question.

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THE AUTHOR COMMENTS: R. D. Davies is correct in pointing out that radioactive dating can be used to test some variable-G theories, in particular the Brans/Dicke theory. I did not have space to discuss this in my article, but a full discussion is given elsewhere[1].

I would certainly agree with Harry Shipman that one should be cautious about all aspects of variable-G cosmology. Prof. T. K. Menon once said to me: "Cosmology is all right—as a hobby." I endorse this statement. However, of the things which Shipman mentions, his point and the reference quoted there is the only outright negative result as far as variable-G is concerned. And myself and others are of the opinion (see pages 51 and 55 in reference 2) that this negative result is open to dispute. Thus, I think the variable-G issue is still undecided.

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I read with great interest Paul Wesson's article. I had long ago heard of the concept of a shrinking value of G, but had always though that it was based on something so esoteric that a simple experimentalist like me could never understand it. I had a very pleasant surprise.

However, there are still some points that I don't understand. May I bring them up here in the hope that an explanation may be forthcoming?

Wesson states that the reason leading to equation 1 of his article is still the most compelling theoretical reason for believing that G varies with time. Equation 1, shorn of the atomic parameters that are assumed not to vary with time (why not?) states that $G \approx (\epsilon_0^2 c^3 t)^{-1}$. Why then is it not ϵ_0 that varies with time? After all, it bears the same relation to the electrostatic field that G bears to the gravitational field. It would be expected to vary more slowly than G, going as the square root of t rather than as the first power.

Perhaps more interestingly, we can assume that it is the speed of light that varies. This has the advantage that the value of c varies even more slowly, as the cube root, and solves the problem of precedence caused by the need of choosing between the gravitational and the electrostatic fields. And the choice of c would probably do interesting things to the theories of relativity (both special and general theories, I believe).

The above may sound facetious, but I assure you that I ask the questions in all seriousness. Why G?

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In my opinion the evidence for extragalactic non-Doppler shifts is stronger than ever (see Arp, Proc NY Acad. Sci., Vol. 336, page 94 and ESO Preprint No. 114). Paul Wesson does not point out that the theories he discusses, for example, atomic time differing from gravitational time, require non-Doppler shifts as a function of age. It would, perhaps, be quite usual for the acceptance of a theory to lead to the acceptance of the observations which motivated it.

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THE AUTHOR COMMENTS: I agree with Herbert Malamud that no convincing answer can be given as to why, a priori, G should vary whereas the atomic parameter should not (e—or e and ϵ_0, depending on one's choice of units; and c). However, variations of e (or e and ϵ_0) and c have been discussed from the practical side, and ruled out. In particular, Gamow suggested that e and not G should vary, but the consequences of this for solar physics were found to be unacceptable. A discussion and references are given in reference 6 of the PHYSICS TODAY article.

I disagree with Halton Arp that the case for non-Doppler redshifts is strong. But on the other hand I would defend his right to present evidence for them. (Some astrophysicists are as intolerant of any mention of non-Doppler redshifts as they are of any mention of changing G.) I did actually point out on page 35 of my article that the Hoyle-Narliker theory predicts non-Doppler redshifts. There is a chapter in reference 6 of the article which is devoted to a discussion of the evidence for and against non-Doppler redshifts in astrophysics. I have discussed Arp's case at length there.

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Concert-hall acoustics

Manfred Schroeder has been doing interesting work in room acoustics for many years, and his techniques, as reported in October (page 24 and also in reference 1) presage further developments. There are, however, areas of controversy that ought be amplified.

First, Schroeder's emphasis of diffuse reflection, according to Lambert's law, is important in ways different that he realizes. Heat-transfer engineers and lighting designers long have used an elegant version of integral geometry, form-factor analysis, which is based on the assumption of diffuse reflection, to give exact answers to problems of distributing radiant energy in enclosures.[2] The tables for lighting design in textbooks of architecture are based on such theory. The method is directly applicable to room acoustics. The necessity of diffuse reflection in large rooms follows directly from Denis Gabor's theory of information in holography; the state of a concert hall is remarkably like that of a diffuse volumetric hologram that varies with time.[3]

Second, Schroeder's brief mention of stage design must be taken further. Joseph Henry provided half the solution, arguing for a compact stage, in 1856.[4] The designers of concert halls also have much to learn from loudspeaker engineers, who know very well that bounding surfaces amplify bass tones.[5] Oddly, here as elsewhere, much good physics has been ignored.

Third, the reason for such missteps is not hard to find. Sabine, as Schroeder mentioned, concentrated upon the absorbing power of soft materials, generating a useful theory of noise control in "live" rooms. But the concert hall problem is not a problem in noise control; it is a problem in the detailed physics of the several varieties of reflections from hard surfaces, as mentioned above. Sabine assumed that the state of a room is like the state of a container of gas in the kinetic theory: an ensemble characterized by uniform energy density, which varys slowly with respect to the individual events; the result supposedly is an exponential growth and decay of a field nearly uniform in space. Sabine never quantified this assumption; when one does, no matter what statistical method one uses, the result is that there are only two or three reflections within the supposed time constant. In real rooms the supposition is incorrect: there is no statistical ensemble; characteristic parameters, such as the "reverberation time," cannot be measures of physical reality. This is the basic problem that has plagued room acoustics since the time of Sabine. The detailed physics of the problem has been smothered in a reverberant stew.[6]

There is a small, but growing, number of engineers and physicists who believe that a giant step toward better acoustics for concert halls—a scientific revolution *à la* Thomas Kuhn—is long overdue. Sabine, as a practical designer, did not trust his theory too much, with brilliant results. We should do as he did, not as he said, and, above all, try to discover why what he did works.

Incidentally, Schroeder's enthusiam for the electronic auditorium and "assisted resonance" is misleading. No first class hall has any electronic enhancement of "reverberation." In fact, there are only two or three really good halls, and all were built before the invention of the thermionic amplifying valve! There is no better testimony to the wrongness of the theory used since the time of Sabine. We must get the physics of the architecture right before we concern ourselves with fancy electronics; so much should be clear.

Kuhn emphasizes the critical role of perception and conceptualization in scientific revolutions. Where Priestly saw dephlogisticated air, Lavoisier saw oxygen. Where Sabine and his followers, the "normal" scientists of today, perceive a "reverberant field," a few revolutionaries, following Joseph Henry, hear a short sequence of discrete reflections.

Soon we shall know if the revolution succeeds.

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THE AUTHOR COMMENTS: James Lee's analogy between reverberant sound fields and volume holograms is amusing and not completely misleading: The statistical relationships of randomly interfering waves are rather alike for acoustical and optical fields. In fact, in my course on coherent optics at Göttingen, I demonstrate some aspects of speckle statistics with standing sound waves in the lecture hall—the students being asked to move their heads to "scan" the acoustical interference patterns. However, I fail to see how "The necessity of diffuse reflections in large rooms follows directly from Denis Gabor's theory of information in holography; . . ." Rather, I believe *that* necessity follows from the work described in my October article and the references cited there.

Lee's emphasis of the importance of early discrete reflections is quite properly placed. In modern times, Princeton physicist Joseph Henry was apparently the first to appreciate this and act upon it: His design of a new lecture hall for the Smithonian Institution was considered highly satisfactory.[1]

I also agree with Lee that the potentialities of electronic sound enhancement are no carte-blanche for poor architectural design, although Royal Festival Hall in London has been so saved. Rather, the great advantage of electroacoustics is that it makes *multi-purpose* halls possible: the conversion of a given enclosed space from satisfactory concert hall to intelligible drama theater or lecture hall. A large population center, like New York City, can (perhaps) afford separate buildings for different functions, but the smaller towns of our (or any) country will have, I maintain, much to gain from electroacoustic sound modification.

Reference

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Quark history

Edward Witten has written a nice article on quarks and the $1/N$ expansion (July, page 38). However, the paragraph where he relates the symmetry group SU(3) to the three colors of quarks is, in a certain way, unfair. This group was largely used when we knew only three quarks, namely u, d and s. Even nowadays we often refer to the "early quantum period" or to the "old quantum period." In the same way proper credit should be given to the "early quantum-chromodynamics period" which led to the introduction of quark color.

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THE AUTHOR COMMENTS: There are two SU(3) groups in quantum chromodynamics. There is the approximate (*not* exact) symmetry among the three light flavors of quarks (up, down and strange). This is the group Guy Faucher is referring to. It is *not* a gauge symmetry. There is also the *exact* color *gauge* symmetry of QCD, which is also SU(3). This latter group is the one that is important in the mechanism that binds quarks and gluons into hadrons, and it is the *only* group I referred to in my paper. It is only an "accident," not currently understood, that the two groups are both SU(3). The accident is really that the number of colors (three) is the same as the number of light flavors. If the charmed quark