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letters

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(extreme) confusion.

Although to the science-oriented individual the specific (particular) meanings of such words are obvious and may be determined readily from context, people who are not science-oriented may fail to understand what they are reading or hearing simply because they don't realize the words are being used in any sense other than the common ones. Because they are not understanding what appears to be ordinary English, they assume they can't possibly understand science, and we are on our way to reinforcing the alienation from the scientific and technological fields that has developed over the past decade.

As scientists and educators we have an obligation to do what we can to reverse this alienation. One step is to recognize jargon when we use it in the classroom or community, and a second is to be sure our listeners realize that such words are indeed technical terms. Science teachers in college and high school should not assume that discussing or illustrating technical concepts is sufficient. The best explanation of potential difference will not be successful if the student is thinking "possible" or "different ability" rather than "voltage." The whole concept of ideal gas behavior is lost if "ideal gas" is assumed to be the brand of gasoline that gives the best mileage.

Admittedly, this awareness will not in itself make the public more supportive of science education; but if those who are not science-oriented become more comfortable with the language itself, then they might find that science is not quite so strange and intimidating after all. And that would be a beginning.

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Identifying the Higgs boson

In the article entitled "Has the Higgs boson been seen in the Crystal Ball?" (October, page 18), there is a common misconception that your readers should be alerted to in case other small, narrow resonances are discovered in the $c\bar{c}$, $b\bar{b}$ or $t\bar{t}$ resonance regions.

You state that a normal $b\bar{b}$ resonance "would obey $e-\mu-\tau$ 'universality.'" That is to say, because it couples to lepton pairs only by way of an intermediate virtual-photon state, a decaying heavy meson would have roughly equal

branching fractions to e^+e^- , $\mu^+\mu^-$ and $\tau^+\tau^-$. The statement is true for the example you use, the Υ (9.46 GeV), but many other $b\bar{b}$ states decay to lepton pairs in a "universality"-violating way. Consider, for example, the 1S_0 state of a bound $b\bar{b}$ system. The lepton-pair decay modes of this pseudoscalar meson strongly favor the $\tau^+\tau^-$ channel. As a matter of fact, the decay width is proportional to the square of the lepton mass just as it is for the Higgs—but, at first sight, for an entirely different reason: helicity suppression. Vector (and pseudovector) couplings prefer to conserve helicity. Vertices that involve helicity flip pay a penalty depending on the lepton mass. This argument is familiar to many particle physicists as the explanation for the π meson's preference to decay to $\mu\nu$ rather than $e\nu$.

In the case of the Higgs decay, there is no vector coupling. The reason the Higgs prefers to decay to the heaviest possible lepton pair is linked to its role as generator of mass. The cancellation of ultraviolet divergences in the $SU(2)\times U(1)$ gauge-field theory depends on the mass dependence of the Higgs coupling to leptons being the same as the mass dependence of helicity-suppressed decays. So it is no accident that the Higgs and 1S_0 mesons decay to lepton pairs in the same way.

I have attempted to calculate the decay rate for all $b\bar{b}$ states into lepton pairs. Those that violate $e-\mu-\tau$ "universality" also tend to have exceedingly small decay widths, even to $\tau^+\tau^-$; these widths are much smaller than those that a Higgs in the 10-GeV mass region would exhibit. (These calculations have very large uncertainties, however.)

In conclusion, if a narrow resonance is found in the mass region of the $c\bar{c}$, $b\bar{b}$, or $t\bar{t}$ mesons, whose decay rate to a lepton pair is proportional to the square of the mass of the lepton, it is not necessarily the Higgs. It might be one of the many meson resonances that must decay to leptons of equal helicity.

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11/84

The gyroscope experiment

The article by Barbara Levi (May, page 20) on the "Orbiting test of general relativity" is misleading in several respects. A subsequent letter by C. W. F. Everitt (August, page 84) partially clarified the situation in one respect but then confused it in other ways.

Referring to the work of B. M. Barker (University of Alabama) and myself, Everitt quoted a 1974 paper of mine, completely ignoring many relevant subsequent contributions. In addition,

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letters

Everitt said: "R. F. O'Connell (and) B. M. Barker applied the analogy of spin-orbit and spin-spin coupling to investigate higher-order terms affecting the gyroscope experiment as well as a variety of astrophysical phenomena." I would like to point out that

► most of the so-called "higher-order terms" are larger than the accuracy sought by the experimentalists

► the theoretical results transcend one-body situations (as in the case of a gyroscope orbiting the earth) and are applicable to two-body problems (such as the binary pulsar PSR 1913 + 16)

► the orbiting gyro experiment is *not* the only way to test the so-called motional precession

► there is no viable reason for expecting that a future quantum theory of gravitation will have an effect on the predicted result for the motional precession.

Schiff's original proposal in 1960 considered two contributions to the precession of a gyroscope, namely, the spin-orbit (geodetic) and spin-spin (motional) precessions. The main interest is in the latter effect, which experimentalists desire to measure with an accuracy of 1 milliarcsec per year, an even lower figure of 0.3 milliarcsec/yr being quoted in a more recent description of the experiment.¹ For the most part, the discussions of the theory¹ are no different from what has been presented by Schiff and completely ignore the host of other contributions that serve to make the goal of isolating the motional precession more difficult to attain.

We now turn to a brief discussion of the other effects that contribute more than 1 milliarcsec/yr. An altitude of 500 (the figure most often quoted) miles is assumed, but the numbers will, of course, be even larger if one uses the recently quoted¹ altitude of 550 km.

► The earth's quadrupole moment contributes² 4 milliarcsec/yr for a gyro in the desired polar orbit, but it also makes an indirect contribution³ of 1.33 milliarcsec/yr, due to the resulting distortion of the satellite orbit from a pure elliptic orbit.

► The Sun makes a relatively large contribution⁴ of 19.2 milliarcsec/yr; it also deflects the light from the reference star, thereby causing an apparent drift of the gyroscope.⁵ In the case of Rigel (the present choice of the experimentalists), we calculated that the effect can be as large as 14.4 milliarcsec.

► Even if a gyroscope could be made perfectly spherical, it would distort due to the rotation and acquire a quadrupole moment⁶—which, in turn, will make a contribution to the drift rate that is greater than the experimental accuracy desired (0.3 msec)—if the

alignment of the gyro is off as much as just 20 minutes of arc from being either in the orbit plane or perpendicular to the orbit plane.

► There are contributions⁷ due to the aberration of the starlight (20.5" due to the earth's orbital motion and 5" due to the satellite's motion).

► Finally, we note that the accuracy with which the proper motion and parallax of the reference star itself can be measured could be another weak link in a long chain since 3 milliarcsec appears to be the present limit of attainable accuracy⁸ and the use of the space-telescope observatory⁹ will probably not reduce this number below 1 milliarcsec.

We conclude that there are a plethora of contributions to the gyro precession that are larger than the desired accuracy. As a consequence, the desired goal of measuring the motional precession is more difficult than originally envisaged.

Turning now to the general problem of the gravitational interaction of two rotating bodies of arbitrary mass to post-Newtonian accuracy, which was derived¹⁰ for the first time by Barker and me: We pointed out that each term in the Lagrangian (spin-spin, spin-orbit, and so on) was analogous to corresponding terms appearing in the quantum electromagnetic Lagrangian for the interaction of two charged, spinning particles. Corresponding to what is regarded as the most rigorous derivation of the latter result (using one-photon exchange), our derivation is based on one-graviton exchange.¹¹ One of the most interesting applications of these results has been the demonstration that large precessions (compared to the earth-gyro precession) may be obtained for astrophysical bodies. In particular, we calculate that the pulsar spin axis of PSR 1913 + 16 may be precessing by about 1°23/yr.¹⁰

Finally, while agreeing that a measurement of the spin-spin precession would be very interesting, it should be emphasized that—contrary to the implication given in the recent *PHYSICS TODAY* article—while most theorists agree that "Einstein's general theory of relativity... may need to be amended...", such amendment is not expected to change the predicted results for the earth-gyro precession. The spin-spin interaction in gravitation should be viewed as no more mysterious than the corresponding term in quantum electrodynamics that gives rise to hyperfine structure. In addition, assuming only the conservation of total angular momentum, it follows¹⁰ that any contribution to spin precession is accompanied by a corresponding contribution to the periastron precession, which might provide another possible test of the spin-spin interaction. Still

another possibility for such a test, one which we feel has potential, is the proposal to use¹² a ring-laser interferometer to measure the frequency difference of the counterpropagating beams in a Sagnac-type experiment, with the possibility of testing both the geodetic and motional precessions.

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9/84

Pair theory in QED

This past year (October 1983 to April 1984) I engaged in a losing battle to communicate with physicists at large through a theoretical paper in *Physical Review Letters*. Rejection of manuscripts for that journal is certainly commonplace, but the nature of my paper and the correspondence associated with its rejection was certainly not. The first rejection was a form letter accompanied by no comment whatsoever from the referee or editor. The second rejection was almost as brief—the suggestion that perhaps if it were expanded it might be publishable somewhere, but certainly not *Physical Review Letters*. Both the divisional (particles and fields) and staff editors concurred. Although my two resubmittal letters repeatedly pointed out the significance of the very simple findings contained in the manuscript, I met a stone wall of silence. There was not a single comment offered throughout the correspondence on the accuracy of the mathematical results in the manu-

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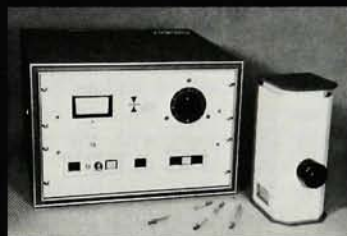
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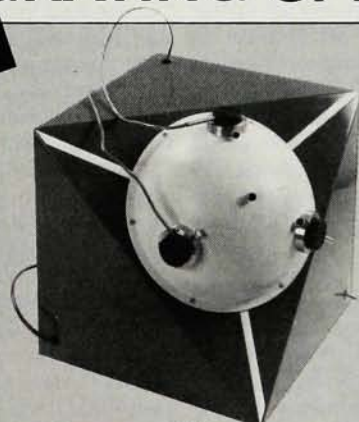
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