symmetric gravitational field of the Sun or the Earth (neglecting rotation) and that the metric chosen to obtain equation 16 is isotropic: $g_{\alpha\beta} = (g_{00}, g_{ik})$, where $g_{ik} = -\eta_{ik}f$, so that $ds^2 = g_{00}x^0x^0 - f(x^1x^1 + x^2x^2 + x^3x^3)$. Using this metric, equation 16 is easily derived as a first-order approximation in the gravitational potential GMc^2/r from equation 87.3 of *Field Theory* by Lev D. Landau and Evgenii M. Lifshitz. As the final step of this derivation one has to change to the frame with the usual local rulers and clocks. The choice of an isotropic metric does not permit us to get rid of our force by using equivalence-principle elevators, and therefore one can say that the result for the light bending angle does arise from the global geometry of the central field—the point that is usually stressed in textbooks on gravity.

Engelbert Schucking from New York University has informed me that he has obtained a generalized exact formula for what he calls "the relativistic apple," valid in all approximations with respect to the potential GMc^2/r:

$$\mathbf{F}_g = -\frac{G_N M(E/c^2)}{r^3\left(1+\dfrac{G_N Mc^2}{2r}\right)^3} \times\left[\mathbf{r}\left(1+\beta^2+\frac{G_N Mc^2}{2r-G_N Mc^2}\right)-\boldsymbol{\beta}(\boldsymbol{\beta}\cdot\mathbf{r})\right]$$

(I'm grateful to Schucking for this communication. I am also grateful to him and to Mikhail Voloshin and Alexander Dolgov for very enlightening discussions.) The first-order approximation in gravitational coupling implicit in equation 16 is very good for the cases of the Sun and the Earth.

I have found equation 16 in only one book.[2] Unfortunately the formula is constructed there semiempirically, and the book itself is full of $E = mc^2$ and all that.

The lack of space and time didn't allow me to discuss in my article such important questions as the mass of a system of particles. I consider this and some other problems in more detail in an extended version of the article.[3]

I don't think we should try to banish $E = mc^2$ from T-shirts, badges and stamps. But in the textbooks it should appear only as an example of a historical artifact, with an explanation of its archaic origin.

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Broken Symmetry Can't Compare with Ferromagnets

I was sorry to see, in the otherwise excellent history of the "standard model" for particle theory by Paul Langacker and Alfred K. Mann (December, page 22), a repetition of the false analogy between broken symmetry and ferromagnetism that is very common among the writings of particle physicists.

In ferromagnetism, specifically, the ground state is an eigenstate of the relevant continuous symmetry (that of spin rotation), and as a result the symmetry is unbroken and the low-energy excitations have no new properties. Broken symmetry proper occurs when the ground state is not an eigenstate of the original group, as in antiferromagnetism or superconductivity; only then does one have the concepts of quasidegeneracy and of Goldstone bosons and the "Higgs" phenomenon. I have discussed the origins of the concept of broken symmetry elsewhere.[1]

Reference

1. P. W. Anderson, in *Gauge Theories and Modern Particle Theory*, R. Arnowitt, P. Nath, eds., MIT P., Cambridge, Mass. (1975), p. 311.

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LANGACKER AND MANN REPLY: Our description of a ferromagnet as an example of a broken symmetry followed the language that is common in many books on condensed matter physics,[1] and the ferromagnet is a valid analog of what is called a spontaneously broken global symmetry in elementary-particle physics. It was not our intention to imply that the ferromagnet is an example of the "Higgs" phenomenon, and we apologize if the wording in the article was not sufficiently clear. We thank Philip Anderson for emphasizing the important distinction between ferromagnets (in which the order parameter commutes with the symmetry generators) and antiferromagnets.

Reference

1. See, for example, D. Forster, *Hydrodynamics, Fluctuations, Broken Symmetry, and Correlation Functions*, W. A. Benjamin, Reading, Mass. (1975).

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Where Did Einstein Lament Lambda?

We were very interested in the article "Landau's Attitude Toward Physics and Physicists" by Vitaly L. Ginzburg (May 1989, page 54). In the section headed "General Relativity" the author raises the issue of the introduction and renouncement by Einstein of the cosmological constant Λ and mentions that he tried to find an original paper on that subject. We recently tried to trace where and when Einstein gave up the idea of $\Lambda \neq 0$: The references can be found in the excellent biography by Abraham Pais, *'Subtle Is the Lord . . . ': The Science and the Life of Albert Einstein* (Oxford U. P., New York, 1982, page 288).

Einstein wrote that there is no need for a Λ term in his paper "Zum kosmologischen Problem der allgemeinen Relativitätstheorie."[1] There we read, "Unter diesen Umständen muss man sich die Frage vorlegen, ob man den Tatsachen ohne die Einführung des theoretisch ohnedies unbefriedigenden Λ-Gliedes gerecht werden kann" ("Under these circumstances, the question should be raised of whether one can satisfy the facts without introducing the Λ term, which anyway is theoretically unsatisfactory"), and, in the conclusion, "Bemerkenswert ist vor allem, dass die allgemeine Relativitätstheorie Hubbels neuen Tatsachen ungezwungener (nämlich ohne Λ-Glied) gerecht werden zu können scheint als dem nun empirisch in die Ferne gerückten Postulat von der quasi-statischen Natur des Raumes" ("It is remarkable that the theory of relativity seems to satisfy Hubble's new results more naturally [Pais translates this as "in an unforced way"], namely, without the Λ term, than the empirical postulate of a quasistatic space, now set aside"). One year later, in a paper with Willem de Sitter,[2] Einstein wrote (in English), "It now appears that in the dynamical case this end [the existence of a finite mean density in a static universe] can be reached without the introduction of Λ."

As for the oft-quoted sentence about Einstein that "the introduction of the cosmological term was the biggest blunder he ever made in his life," it is to be found only in George Gamow's autobiography *My World Line* (Viand flavors, and can hardly wait for particle physicists to dig down to how the world works at subquark distances. But confinement doesn't befuddle him the way quantum mechanics does. "*Conceptually* it's no stranger than Hooke's law," he says, "except for the absence of a cutoff."

▷ Georgi should not hold Professor Mozart responsible for my own exuberant merging of the goals of cosmology and particle physics. Mozart's point was more subtle: that the possibility of an endless hierarchy of shorter and shorter time scales in the early universe, each with its own characteristic features, suggests the analogous possibility of a hierarchy of shorter and shorter length scales, each with a newer and more "fundamental" particle phenomenology than the one above it. Thus he finds in cosmology some serious warnings about the path particle physicists are pursuing. While it's all a bit speculative for my taste, I can't see the slightest hint of a comparable moral for condensed matter physicists in the behavior of TV sets.

▷ Who said anything about the SSC? Can no opportunity be lost to praise its scientific, intellectual and morally uplifting qualities? As it happens Professor Mozart is a big supporter of the machine. He's filled with curiosity about what will turn up in the next layer and delighted that the public is willing to invest billions in a purely intellectual exercise, with major spinoffs for cosmology. "Certainly it would be tragic to stop digging now," he insists, "comparable to the loss of the Great Library at Alexandria. Whatever the layer at which we finally have to stop, it will be tragic." Nor is he worried about the drain the project might impose on the rest of science. He says his productivity has actually increased since his funding was cut. "Fewer reports to file, fewer graduate students to worry about and more time to follow my nose, wherever it takes me." He is, of course, a theorist, but as for the experimentalists, "A temporary return to string and sealing wax on the kitchen table would refresh them all. Science has become entirely too dependent on high technology. Nor am I among those who would blot the landscape of intellectual history, disgracing all of basic science, for the sake of better TV sets. I'm proud that the American people have decided to put up with low-resolution screens for a few extra years, in their excitement and eagerness to get inside those massive intermediate vector bosons." (I can't agree with Mozart about the Great Library. After all, if we have to wait a century or two the bosons will wait with us, but those plays of Euripides and Sophocles are gone forever.)

Unlike Georgi, who suggests that Professor Mozart may have "lost most of his marbles," Drasko Jovanovic merely thinks him diseased. I am puzzled by the violence of both responses. Mozart seems to think that the last word has not yet been said on the meaning of the quantum theory—that experiment may still have something more to teach us; that we will find endless hierarchies of new structure as we probe to shorter and shorter length scales; and that naive reductionism is too innocent a basis for a deep understanding of the physical world. I can understand disagreeing with any or all of these opinions, but is holding them evidence of dementia? As for the argument Jovanovic offers in support of his diagnosis, I can only say that while I myself sometimes orchestrate a case for a prize for good physics, I am entirely unimpressed by the view that, conversely, prizes or even Prizes provide a definitive measure of scientific merit. (See my Reference Frame column of January 1989, page 9.)

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1/91

Reflections on Broken Symmetry

Philip W. Anderson (May 1990, page 117) states that ferromagnetism is not a case of broken symmetry, but that antiferromagnetism is, because in the latter "the" ground state is not an eigenstate of the symmetry. I must disagree. If there is a unique ground state, it must of course respect the symmetry. The real question is whether the eigenstate of the order parameter belongs to a representation of the symmetry group. In ferromagnetism one may assume the order parameter to be the magnetization, including both magnitude and direction, in which case it does not respect the spin rotation symmetry. In the macroscopic limit an alternative is to choose the magnitude of the magnetization and the direction in which the component is maximum. This does define a representation of the group, and the fluctuations in direction are negligible in the macroscopic limit.

In the antiferromagnet the same is true as regards the spin rotation group. There is, however, a further symmetry here: the displacement that interchanges the even and odd sites. The antiferromagnetic order clearly breaks that symmetry. The exact ground state is still an eigenstate of the symmetry: It would be an even superposition of the two states, with even and odd sites interchanged. The energy difference between this and the odd superposition is exceedingly small, as it involves the matrix element for the whole substance changing from one antiferromagnetic configuration to the one with odd and even sites interchanged. In the infinite-volume limit the energy difference is zero, the ground state is degenerate, and there is freedom of choice of a ground-state wavefunction. As long as we are dealing with the antiferromagnet in isolation, there is no argument for any particular choice, but the broken-symmetry state (characterized by an order parameter) is favored because it is the zero-order approximation for dealing with an interaction, however small, with other magnets or fields.

This situation of a degenerate ground state and the appearance of a symmetry-breaking order parameter is characteristic of many cases of broken symmetry.

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I was saddened to see the beautiful, far-reaching and actually quite simple concept of broken symmetry, or, more precisely, spontaneously broken symmetry, confused by Philip W. Anderson's letters-column exchange with Paul Langacker and Alfred K. Mann. Anderson makes two statements that directly contradict earlier statements of his; also the venerable term "eigenstate" was implicitly given a different meaning.

The self-contradictions as well as the new usage are contained in the statement "In ferromagnetism, specifically, the ground state is an eigenstate of the relevant continuous symmetry (that of spin rotation), and as a result the symmetry is unbroken.... Broken symmetry proper occurs when the ground state is not an eigenstate of the original group, as in antiferromagnetism or superconductivity; *only* then does one have the concept ... of Goldstone bosons ..." (italics mine). Table 1 of Anderson's book *Basic Notions of Condensed Matter Physics* (Benjamin–Cummings, Menlo Park, Calif., 1984) is a list of broken-symmetry phenomena, and *ferromagnetism is one of the entries.* Furthermore, *ferromagnetic spin waves are listed as Goldstone bosons.* Concerning the new usage, the ferromagnetic ground state, with, for example, all spins in the z direction, is obviously not an eigenstate (in the usual sense) of, say, a spin rotation about the x axis; Anderson has informed me that what he meant by "eigenstate" in his letter to PHYSICS TODAY was "belonging to a single representation."

The simplicity of the concept of symmetry breaking, in my opinion, can be grasped only if a symmetry-breaking field $\hat{z}B$, the thermodynamic limit and thermal equilibrium are considered. To discuss this briefly, let Q be the order parameter operator. (For ferromagnetism Q is the z component of the total spin, and B is the uniform magnetic field; for simple antiferromagnetism Q is the z component of staggered or sublattice spin, and B is the staggered field.) Also assume the Hamiltonian is invariant under reversal of all N spins. Thermal equilibrium then implies that the expectation value $\langle Q\rangle$ vanishes by symmetry if $B = 0$. If on the other hand the order parameter[1]

$$m = \lim_{B\to 0}\lim_{N\to\infty}\langle Q\rangle$$

does not vanish, then the symmetry is said to be spontaneously broken: It's broken because $\langle Q\rangle$ is not zero (as it would be if $B = 0$, as demanded by symmetry), and it's broken *spontaneously* because B is taken to zero. While the two limits might seem abstract and formidable, the definition is really very physical and simple. It merely recognizes that for macroscopic N even the smallest possible value of B in the laboratory would already saturate $\langle Q\rangle$ (the limiting curve $\langle Q\rangle$ vs B has a jump at $B = 0$).[1] In ferromagnetism m is proportional to the saturation magnetization; certainly by this definition ferromagnetism is an example of spontaneously broken symmetry. For the interesting antiferromagnetism whose Hamiltonian has full spin rotation symmetry, this definition removes the mystery of how the symmetry can be broken spontaneously even though the $B = 0$ ground state is a nondegenerate singlet (and therefore does *not* break the symmetry) for any finite system. Related matters have been discussed recently for this (quantum) antiferromagnetism.[2]

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ANDERSON REPLIES: Neither of the two letters commenting on my rather

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casual remarks addresses the actual question that was under discussion in the May 1990 letters column. Paul Langacker and Alfred K. Mann might or might not agree with me on the dictionary definition of "broken symmetry" for the purposes of solid-state physicists, but what was in question was the meaning and use of the phenomenon in particle theory. Neither Sir Rudolf Peierls nor Thomas A. Kaplan refers to the original work by Yoichiro Nambu and G. Jona-Lasinio or by Steven Weinberg and his colleagues, which are in fact the only relevant references on this question.

In this work the property that is used is the actual change in symmetry of the excitation spectrum, which is consequent on the order parameter's not being a conserved quantity, that is, not commuting with the original Hamiltonian. Therefore excitations—read "particles" in the electroweak or chiral symmetry-breaking theories—are no longer classified by representations of the original group. In the work of Nambu and Jona-Lasinio, for instance, the original group includes chiral symmetry, while the resulting particles—pions and nucleons—do not have a chirality quantum number. This is analogous to BCS theory, where the Hamiltonian is charge conserving but the quasiparticles do not create charge eigenstates. In the ferromagnetic case the excitations—spin waves—can be chosen to create states with a definite spin quantum number, so the analogy to ferromagnetism is flawed. There are no particle theories with spontaneously broken symmetries of the conserved type. It was this point I wished to make, and it is this definition of "broken symmetry" which is natural in the context of particle theory. (It is also useful in understanding the sometimes mysterious properties of excitations in solid-state systems, such as that phonons do not have a true momentum quantum number, nor antiferromagnetic spin waves a fixed spin.)

Let me discuss the two letters individually. Kaplan's use of my own words against me is a tactic not worthy of a reply. The rest of his letter is a dictionary definition for solid-state physicists, combined with a discussion essentially equivalent to part of that given in my original 1952 paper in which, I believe, this kind of question was first correctly treated; Nambu and Geoffrey Goldstone's original work is also useful, in that they first made explicit the idea of quasidegeneracy and coined the phrase "broken symmetry"—for which service, I should imagine, they earned the right to define the words.

Peierls's discussion seems to be seriously incomplete, in that he misses the large quasidegenerate manifold of states with spin quantum numbers from 0 to N: In the idealized system, these states are rigid rotor eigenstates, with energies $J(J+1)/N_{\rm t}$. He seems to have read neither my original paper on this question nor the relevant references by Nambu, Goldstone, Abdus Salam and Weinberg, and I strongly recommend he do so.

Since the entire question is one of particle, not solid-state, theory, I hope that some particle theorist will weigh in with an opinion.

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Teaching Physics to Poets, and Vice Versa

How does one learn to appreciate fine cuisine: by going into the kitchen and apprenticing under a great chef, or by visiting many restaurants and sampling a variety of dishes?

If I understand Leon Lederman's Reference Frame column "Physics for Poets" (July 1990, page 9), a nonphysicist's appreciation of the beauty and excitement of physics must be acquired in the kitchen, that is, through problem solving and laboratory experimentation. This time-honored viewpoint ignores the difficulty that ordinary folks have in casting elementary problems in terms of the simplest mathematics. It also ignores the fact that so many of the problems and experiments examined in low-level courses are not particularly interesting to poets and philosophers.

Now, as in the past, the guiding principle seems to be that somehow the pain a nonphysicist experiences in even attempting to solve physical problems will be transformed into insight and appreciation. To the contrary, my personal experience was one of acute indigestion.

Perhaps it's time for the physics community to try the opposite extreme: Invite the poet and philosopher to sit down at the best table in the house and sample the rich fare of contemporary physics, the same stuff that is served in issue after issue of PHYSICS TODAY. The classical basis for these concepts might be introduced qualitatively by imaginative use of interactive computer graphics. The poet could, for example, play with the Navier–Stokes equations and observe the beautiful patterns that emerge as boundary conditions and other parameters are changed. And if he can't derive the equations, so what?

I don't see how students taking such a course could help but be captivated by the world of physics. Many, out of interest, would probably continue to keep up with what's happening in the field, and those who end up in the political arena would better understand the importance of funding this project or that. Certainly all would emerge with a much deeper understanding of the role physics has to play in approaching the global problems with which mankind is faced today.

Of course there is always the danger that if the course was too successful, the starry-eyed physics major might also wish to enroll, thereby earning an easy credit.

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I enjoyed Leon Lederman's "Physics for Poets" very much and agree with him wholeheartedly: We must do better at educating everyone on the importance and relevance of science in today's world. But the other side of the educational coin also needs addressing. I would like to propose a course called "Poetry for Physicists," with a parallel goal to "Physics for Poets"—namely, teaching what physicists should remember about poetry (or history or music or whatever) in 10 or 15 years, when we are working on global warming or creating the next Stealth bomber. In the past few years I have noticed that ethics courses in business colleges are becoming more popular. It seems to me that ethics for scientists is at least as important—perhaps even several orders of magnitude more important.

In my academic utopia, we physicists would first sharpen our intellectual scalpels on the problems of what it means to be a human being before going at what it means to be a hydrogen atom. And if physics departments let in a little more liberal arts, perhaps the liberal arts departments would return the favor. Then we would all be able to remember, in 10 or 15 years, why it was that we bothered to study at an institute of higher learning, and not simply a trade school.

CRAIG R. HAAS
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Apropos Leon Lederman's important plea, the following should be of help in the economics of teaching the methods of physics to large numbers of citizens: Include among lecture demonstrations several in which the student makes all the observations from