the positions of the particles, or maybe three deltas in momentum space. In any case, the angular momentum of every particle should be highly undefined, its position or momentum being very well defined. The question is, How can you prepare a system such that the spin part of the angular momentum is well defined, but the orbital part is not defined at all, if the partition between spin and orbital is not Lorentz invariant?

From another point of view, we may state the problem as follows: Any test of locality should involve measuring positions as well as spins, polarizations or other properties. In the GHZ experiment we must measure, besides the operator $S = \sigma_x^1\sigma_x^2\sigma_x^3$, considered by Mermin, another operator such as $R = \rho_a^1\rho_b^2\rho_c^3$, where ρ_a^1 takes the value $+1$ (-1) if particle 1 is (is not) inside the small region a, and similarly for the other particles. For any actual preparation procedure of the three-particle system (by a spin-conserving *gedanken* decay, in Mermin's words) the probability that the measurement of S gives -1, conditional on a result of $+1$ for the observable R, will likely not be unity. It is far from obvious that this probability cannot be reproduced by a local hidden-variables model where spin and linear momentum are conveniently entangled. Consequently, I remain unconvinced that, parodying EPR, "quantum mechanical *destruction* of physical reality can be considered complete."

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MERMIN REPLIES: The nine successive spin measurements that Mikolaj Sawicki contemplates have nothing to do with the EPR argument. The crucial property of the GHZ state for making the EPR argument is simply that regardless of which particle (1, 2 or 3) or spin component (x or y) you are interested in, as a matter of perfectly orthodox quantum mechanics the result of measuring that particular spin component of that particular particle can be determined in advance by measuring an appropriately chosen spin component of each of the other two faraway particles. No other measurements are made, except, if you want, for a final measurement of the spin of the original particle to check that the prediction was indeed correct. There are only three spin measurements on three distinct particles associated with three commuting observables.

As Sawicki points out, however, you cannot do all the measurements necessary to learn the values of both spin components of all three of the particles. This is also correct, and has something of the flavor of Bohr's reply to EPR, who actually anticipated this objection in their original paper. Their compelling rejoinder, in the terms of the GHZ setup, is that if you agree (as quantum mechanics does) that a given spin component has a definite value only if you have actually determined that value by two faraway measurements, but insist that it does not have a definite value if you have not, then whether or not a spin component of a given particle has a value depends upon your choice of what to measure far away from that particle. This struck them as entirely unreasonable, whence their conclusion that both components must have had values in advance of the measurements, even though only one of those values can actually be determined. [Those who say (as many do) that there is nothing spooky about EPR, since the actual measurements merely give us additional information about the faraway particle, are implicitly embracing the EPR position, but simply refusing to take the next step.] GHZ refute EPR not by the irrelevant (from the EPR point of view) fact that you cannot do all the measurements needed to reveal all the values, but by the elementary observation that there is no possible way to assign all those values that can produce the right data for each of four different choices of what to measure in each of the three far-apart wings of the experiment.

A clarification of this issue might be found in *American Journal of Physics* **58**, 731 (1990), where I describe how to extract the EPR argument and its subsequent refutation directly from the data produced by spin measurements in the GHZ state, avoiding any reference to quantum mechanics, except for an unproblematic calculation of those data.

Emilio Santos is too quick to dismiss the ability of detector inefficiencies to cover a variety of conspiracies. If in each run of the three-particle GHZ experiment one of the particles (1, 2 or 3, randomly selected) is designed so as to evade detection if its spin is measured along a particular one of the two directions (either x or y, randomly selected), then it is easy to specify the other five "elements of reality" m_μ^i so that the data I described are always observed in those runs in which all three detectors do fire. Whether or not one finds loopholes like this attractive depends on whether one is more appalled by quantum nonlocality than by particles that, in order deceptively to imitate quantum nonlocality, conspiratorially exploit our failure to realize that our detectors are more efficient than we thought they were.

Santos's other suggestion, that there should be something like superselection rules prohibiting states like those of EPR and GHZ that demonstrate "spooky actions at a distance," has been argued with great eloquence and fervor by Oreste Piccioni. Santos's specific suggestion that constraints between spin and orbital angular momentum might do the trick is ruled out by a new version of the GHZ experiment proposed by Greenberger, Horne, Abner Shimony and Zeilinger (to appear in the *American Journal of Physics*), in which spin (or polarization) plays no role.

Finally, I must emphasize that although "the unnumbered equation between (1) and (2)" was undisplayed and therefore not in violation of Fisher's rule (see my Reference Frame column, October 1989, page 9), I hereby enunciate and plead guilty to a violation of Santos's rule (display—and, of course, number—all equations you think readers might want to refer to, whether or not you think they should) and promise to try not to do it again.

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Enumerating α's Calculators

The interesting Reference Frame column by David Gross on the calculation of the fine-structure constant α (December 1989, page 9) mentions Paul Dirac's matrices but not Dirac's work on the problem. Dirac[1,2] sought to explain why the smallest electric charge e was given approximately by $1/\alpha = \hbar c/e^2 \approx 137$. By considering the wavefunction of an electron in the field of a magnetic charge g he obtained the (charge quantization) relation $eg/c = n\hbar/2$ (where n is an integer) connecting the two charges, but not a relation in e alone. This was a great disappointment to him. Still, he made the best of it and published in 1931 a paper[2] that has stimulated much of the work on magnetic monopoles. (The introduction to this paper reads like an epic poem and does not betray Dirac's disappointment.) Although he thought in 1931 that magnetic monopoles might exist, he was "inclined to believe that monopoles do not exist" on the eve of a conference in Trieste celebrating the 50th anniversary of his 1931 paper.[3] However, he remained fascinated with the fine-structure problem and regarded it as one of two fundamental problems lying ahead in physics. He was impressed with the work of H. Euler and B. Kockel[4] and of Leopold Infeld,[5] which gave $1/\alpha \sim 82$ and 130, respectively. (Their work used Max Born's linear electrodynamics.)

Although both Arthur Eddington and Dirac failed at deriving a value for α, perhaps Eddington's work (which Dirac studied intensively, but found himself unable to understand) sharpened Dirac's interest in the problem and led to his charge quantization paper. Thus the failures of the great in one area of physics might sometimes lead to great physics in another area.

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The views of the late cosmologist Arthur Eddington on the fine-structure constant α were commented on with something less than courtesy by David Gross. I think his comments leave a somewhat slanted impression of Eddington that needs to be rectified.

Eddington was a man of immense originality who did work on various aspects of astrophysics, cosmology and philosophy in the first half of this century, when he was a professor at Cambridge. Some of the things he did were foundational and are still in use today. Examples are the Eddington luminosity relation for stars and the Eddington–Lemaître model of cosmology. He also introduced the idea that a physicist's view of the universe is partly molded by the biological and other conditions necessary for his own existence (see his popular book *New Pathways in Science*). This idea is nowadays called the anthropic principle and is associated primarily with Brandon Carter, Stephen Hawking and John D. Barrow, but its importance was first perceived by Eddington. He believed in the existence of an external, objective universe. But he argued in several books on the philosophy of physics that much of the topic is not really objective but subjective, in the sense it is constrained by the anthropocentric models we employ. Eddington used the analogy that a physicist is like a fisherman who uses a net with 1-inch mesh and concludes that the sea only contains fish larger than 1 inch. This view was once considered blasphemous, but advances in subjects like relativity and particle physics might make it more palatable now.

Toward the end of his life Eddington worked on the connection between general relativity and quantum theory and produced the theory of α that Gross calls "absurd." With the wealth of physics we now have, it does look absurd. However, in the 1930s both areas of physics were in rudimentary shape, and α was not precisely determined. To within the accuracy available to him, Eddington was justified in taking $1/\alpha$ to be an integer (see his posthumously published 1949 book *Fundamental Theory*). OK, so he was wrong. But with our 20/20 hindsight, surely we can be a bit more gracious to Eddington and pass over in silence those of his ideas that were off target.

Eddington is not the only cosmologist to have worked on the origin and nature of the fundamental constants and been the subject of discourteous comments. Others who have worked on this basic subject and followed in the same school include Paul Dirac, George Gamow, Arthur Milne, William H. McCrea and Fred Hoyle. I recall listening to Hoyle lecture about α and the gravitational parameter G while I was a gradute student at Cambridge. He made a persuasive case for believing that G must depend on the expansion of the universe and therefore must be changing slowly with time. I remember thinking this idea was reasonable, and later tinkered with it myself (see my article in PHYSICS TODAY, July 1980, page 32). However, several of the audience members were less receptive, and this showed up in the tone of some of the questions that followed the lecture. The retiring director of the university observatories asked petulantly, "Yes, but is there any reason I can't still take G to be constant?" The answer to this is easier to give now than then. Certain astrophysicists have made observations and found that G and other constants do not vary by more than about 1 part in 10^{11} per year. (One way of arriving at such a limit is by observing the evolution of binary pulsars; see the article by Donald C. Backer and Shrinivas Kulkarni in PHYSICS TODAY, March 1990, page 26.) However, these limits exist only because some people had the right scientific attitude and tested the idea of variability instead of just discarding it.

Eddington and other cosmologists may not always be right. But they have a place in physics: It is to ask fundamental questions.

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David Gross's column "On the Calculation of the Fine-Structure Constant" ends with the remark that an answer to the more fundamental question "Why does the cosmological constant vanish?" may lead to the numerical value of the fine-structure constant α as an incidental byproduct. It is interesting that my work with Francisco Mejía-Lira on the nature of the early universe[1] did indeed lead to an unexpected expression and value for α in terms of the masses of the electron, pion and proton.[2]

Our model of the early universe was based on a model of particle structure[3] that confines the constituents—distinguishable quasiparticles. (I later used this model to describe the internal microstates of a Schwarzschild black hole.[4]) The statistical model for particle structure[3] gives rise to a vanishing Helmholtz free energy for the constituents. The close-packed configuration of particles in our model of the early universe indicates that inside a particle one has a miniature replica of the universe. Accordingly, the free energy of the universe also vanishes; that is, the cosmological constant vanishes.

Although I would be the first to disclaim any finality to our work, it does relate the vanishing of the cosmological constant to the value of α. And in so doing, it interconnects important fundamental concepts—confinement, phase transitions, black holes and so on—and may form the phenomenological basis of quantum gravity and the unification of forces.

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The primary reason for the rejection of Armand Wyler's formula for the

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fine-structure constant α may not have been the existence of many other, more or less equally simple ratios of group volumes close to α and involving integers and π, as David Gross claims in his Reference Frame column. Rather, the rejection may have stemmed from Wyler's failure to demonstrate any clear relationship between the group volumes and any physical theory dealing with broader physical questions.

In fact, after Wyler's initial publications of group-volume ratio formulas for α, he was invited to the Institute for Advanced Study at Princeton to continue his work. The hope, understood to be a long shot, was that work at the institute might lead to some indication of a relationship between the group volumes and physical theory. At the end of Wyler's term at the institute he was no closer to physical theory than he had been at the beginning, so his formula for α was rejected as unphysical mathematics.

The point is that the institute gave Wyler a chance to develop his theory. Unfortunately, his failure to do so seems to have given a "black eye" to other attempts to relate group volumes to physical theory.

Nowadays, institutes comparable to the Institute for Advanced Study circa 1971 are reluctant to take a long-shot chance that such unconventional approaches might be useful. The result is that conventional approaches (such as, currently, superstrings) are not merely dominant, but in practice the only way to go. If fashion happens to be wrong, and a long shot happens to be right, then physics is the loser.

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GROSS REPLIES: I agree completely with Ibrahim Adawi that some of the greatest discoveries have been made following false leads. Paul Dirac's analysis of the role of magnetic monopoles in quantum electrodynamics gave the first explanation of the *quantization* of the electric charge, although it provided no clue as to the value of the charge quanta. In current theory, charge quantization emerges when truly unified theories combine the electric charge as part of a non-Abelian group whose representations are labeled by quantized integers. Not surprisingly, this is related to Dirac's analysis of magnetic monopoles, since these theories necessarily contain monopoles with a charge given by Dirac's formula.

Paul S. Wesson accuses me of being ungracious and discourteous to Arthur Eddington. Perhaps, but no more so than his contemporaries, who knew enough in the 1930s to be able to dismiss Eddington's *theory* of the fundamental constants. (See, for example, the harsh attacks on Eddington at the Warsaw conference on New Theories in Physics held in 1938, where Eddington gave a rare presentation of his ideas before an audience of his peers.)

My main criticism is that Eddington's approach to these issues was nonscientific. I totally disagree with the comparison of Eddington's work on α to the speculation of Dirac and others that the gravitational constant may be time dependent. The suggestion that this was a logical possibility was good science that led to new experimental observations and tests. Eddington's theory was numerology, not science, and led nowhere. This, of course, should not detract from our admiration of Eddington's important contributions to astrophysics and cosmology.

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How Supernova Shock Revival Was Revealed

In my article "Supernovae" (September, page 24) I stated that James Wilson discovered the revival of the supernova shock after accidentally leaving his computer on overnight. Actually, Wilson had worked for many months to extend the computation from about 0.05 second after collapse of the star to about 1 second. (This was a difficult problem, and its solution has only been matched about six years later, by one other scientist.) After the shock could be pursued for this long time, it showed revival, which Wilson interpreted as being due to the absorption of the neutrinos emanating from the core.

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Is Chernobyl News Contaminated?

I found William Sweet's news story "Chernobyl Aftermath to be Assessed by International Team" (July, page 62) very interesting and informative, but somewhat alarmist.

The basic question is the following: Should we trust all news about the Chernobyl accident that appears in the Soviet press? I think we have to be very cautious. Thanks to *glasnost*, the Soviet mass (nonprofessional) media abound today with all kinds of information. Not surprisingly, we have a tendency to consider this information accurate, without realizing that much of it is highly suspect, and some grossly incorrect.

What about official government information? The Soviet government, particularly in the early days after the accident, has been very secretive about the accident and its effects. For us in the West, it is reasonable to assume that the Soviets are hiding something, presumably some very bad consequences or mistakes their professionals and politicians have made, both before and after the accident.

Sweet reports: "The Byelorussian government has asked for international help to relocate and medically treat people living in areas affected by the accident. A Byelorussian diplomat is reported to have said in Brussels that two million Byelorussians live in such areas.

". . . the Ukrainian government reported that more than 1600 villages and towns, with more than 1.5 million inhabitants, were located in the contaminated area. . . . The Ukrainian republic has established special accounts for the deposit of foreign donations."

When Soviet officials are admitting bad things, we in the West automatically believe that the particular event is at least as bad as admitted. But in view of recent changes in the Soviet Union, we should ask whether our old stereotypes are still correct. I do not believe so, at least not in the case of Chernobyl.

Many of the "official" reports about Chernobyl (including those quoted by Sweet) contain very little, if any, quantitative information. What do the words "affected" and "contaminated" mean? We should also notice that it is mainly (only?) Byelorussian and Ukrainian government officials making these statements, while Soviet (federal) officials and professionals are not. Could it be that some officials from the Ukraine and Byelorussia, republics that have strong independence movements, are exaggerating Chernobyl's consequences to further their political aims? There are strong indications that this is indeed the case.

Naturally, the Western media have no expertise to separate sense from nonsense; thus they pick up all these reports (some of which are quite "juicy") and spread them around the world. Since nobody challenges them (admittedly, some Soviet professionals do, but they have a very uphill battle to fight), these reports appear