LETTERS *(continued from page 15)*

WILCZEK REPLIES: Friedwardt Winterberg's shrewd question goes directly to the heart of the matter. To answer it, I will have to recall the most profound concepts discussed in the two articles he cites—my essay in PHYSICS TODAY and my 1999 paper in *Nature*.[1]

If the strong coupling g were simply a numerical quantity, as it is in the classical limit of quantum chromodynamics, Winterberg's argument would be conclusive. When we treat QCD as a quantum theory, however, we learn that things are not so simple. We learn that the state we call "vacuum" teems with evanescent virtual particles and acts as a dynamic medium. As a result the vacuum reacts to charge, and the effective strength of the coupling to a test charge depends on the distance at which it is measured—namely, $g = g(r)$. This phenomenon, the running of the coupling, was originally derived theoretically by David Gross and me, and independently by David Politzer. On that basis, Gross and I proposed modern QCD as the theory of the strong interaction. The running of the coupling, in the predicted form, has now been verified in many precise experiments.[2]

Because of the running of the coupling, simple dimensional reasoning (as used by Winterberg) is invalid. Specifically, we can define a length l in terms of dimensionless quantities through the equation $g(l) = 1$. Having a length, we can derive a mass scale from $\mu = \hbar/lc$. All the masses of physical particles in QCD (or, more precisely, in QCD Lite) are then expressible as calculable numbers times this μ.

Dimensional analysis is not mocked, since QCD is powerless to compute μ itself. Indeed, the numerical value of that parameter has no significance within QCD! Different choices of μ will describe "different" worlds that behave identically, as far as their strong interactions are concerned, except for an overall change in the scale of length (or inverse mass). Just as parity symmetry asserts the physical equivalence of looking-glass worlds, so the scale symmetry of QCD does the same for magnifying-glass worlds.

We can check our understanding by determining whether calculated mass *ratios* agree with experiment. They do.[3]

Of course, other interactions, notably gravity, do involve the numerical value of μ. That is where the unification of couplings comes in. Given the limited space available here, I must refer readers seeking an adequate discussion to my 1999 *Nature* article[1] and to a PHYSICS TODAY article on unification that I cowrote with Savas Dimopoulos and Stuart Raby (October 1991, page 25). I will remark, however, that the logarithmic running of couplings that is central to this analysis is firmly rooted in basic principles of quantum field theory, and has been observed in many experiments. To be sure, we are extrapolating the observed results many orders of magnitude down in distance, or up in energy, beyond where we have direct evidence. But the depth of the roots, and the impressive success of the extrapolation, seem to me to make this circle of ideas much more consequential than vague hydrodynamic analogies.

Most of Antonio Ruiz de Elvira's questions call, in one way or another, for aesthetic judgments. Such questions go beyond rational argument. If I show you something I think is incredibly beautiful and you say "Nah!" or "So what?" subsequent rational argument may never bring us closer to agreement. I've been involved in discussions just like that, about atonal music. In discussing the beauty of the laws of physics, the situation seems much more hopeful, because the beauty of the concepts emerges more clearly with deeper understanding.

Here I will address only Ruiz de Elvira's third question. Electrons in various parts of the universe have undergone drastically different histories. Furthermore, new ones are created, and old ones are destroyed, in cosmic-ray reactions and nuclear decays. To postulate that all electrons have the same properties is, on the face of it, a complex *ad hoc* hypothesis. How much more satisfactory it is to understand that a single, uniform, and universal field is responsible![4]

References

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2. S. Bethke, hep-ex/9609014 (paper presented at QCD Euroconference 96, held in Montpellier, France).
3. R. Burkhalter, hep-lat/9810043.
4. F. Wilczek, Rev. Mod. Phys. **71**, S85–S95 (1999); reprinted in *More Things in Heaven and Earth—A Celebration of Physics at the Millennium,* B. Bederson, ed., Springer-Verlag, New York (1999), p. 143.

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Science, not Politics, Should Set Science Curriculum *in Toto*

Although I think the outrage of the scientific community about the Kansas decision regarding the teaching of evolution ("Scientists View Kansas Board's Decision as a Wake-Up Call," PHYSICS TODAY, November 1999, page 59) is more than justified, I disagree with the position taken by Fred Spilhaus, Molleen Matsumura, and others cited by reporter Jean Kumagai. They see the remedy in scientists engaging in "active participation in state and local decision making"—that is, in deciding the issue in the political arena. In my opinion, their position is fundamentally wrong because it endorses the idea that the contents of a scientific discipline (be it a professional or highly specialized one) can and should be decided politically—that is, by being voted upon by laymen (note that scientists, in principle, are laymen outside their individual fields of expertise).

It may be legitimate to decide by vote whether a curriculum should contain, say, biology, physics, or the Old Testament, but definitely not whether biology should include the evolution of species, physics the theory of relativity, or the Old Testament the Sixth Commandment. As

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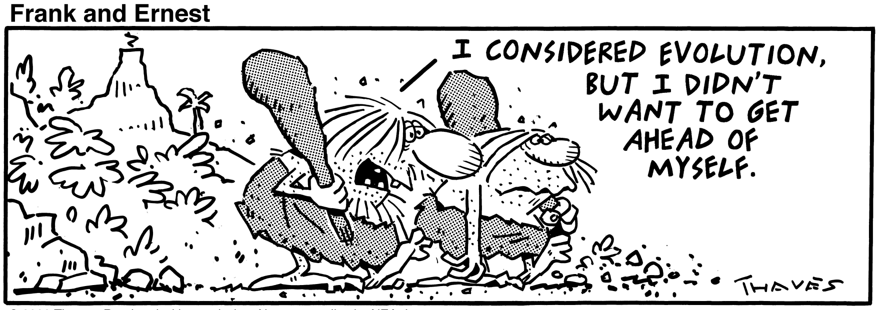

far as the theory of evolution is concerned, it does not even matter in the given context whether it is right or wrong. The only thing that matters is that it belongs in a science curriculum by virtue of being a *scientific theory*, which, by definition, is one concerned with presenting explanations of nature *without taking recourse to God and the supernatural.* On the other hand, comprehensive coverage of any scientific theory should include a discussion of its limitations—phenomena that it does not satisfactorily explain—because that is the best stimulus for advancing science. As Richard Feynman put it in his famous television interview on 25 January 1983 in the Public Broadcasting Service's series *Nova*: "The thing that doesn't fit is the thing that's the most interesting, the part that doesn't go according to what you expected."

Indeed, the evolution issue is not about science at all; it is about subordination of science to an ideology. What is important is not whether a Kansas-type decision could have taken place in one school board, one state, or even "a couple dozen states," as Tim Miller observes in Kumagai's story, but that it did take place at all in a country like the US at the end of the 20th century! The crucial question, frightening as it is, is the following: Could the Kansas decision be just a symptom of a deeper rot, as was the banning of genetics and cybernetics in the USSR in the 1950s, or the political defamation of relativity in Germany in the 1930s?

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PT Fails to Measure Up to SI Standards (but Only by Inches)

I was surprised by Stephen Benka's use of what he calls "the dreaded English system of units" in his January editorial (page 10). If it is dreaded, then we should definitely avoid it. Life is too short for us to live in dread.

Most of PHYSICS TODAY's readers have a scientific background, so we can assume that we all know what a meter is and approximately what thickness is represented by, say, 4 mm. Therefore, using the "dreaded system" is certainly not necessary for the sake of our scientific community.

Given that some of the magazine's readers do not have a scientific background, surely it is the community's responsibility—and, I think, PHYSICS TODAY's duty—to demonstrate correct practice and use of the units of measure that constitute the International System of Units, or Système International d'Unités (SI). According to the Gaithersburg, Maryland–based National Institute of Standards and Technology, SI has long been "the language universally used in science [and] the dominant language of international commerce and trade" and is "the preferred system of weights for United States trade and commerce." [1] That view is also taken by the Sèvres, France–based Bureau International des Poids et Mesures, which declares that its task is "to ensure world-wide uniformity of measurements and their traceability to the International System of Units." [2] And both views, in turn, are reflected in the three metrology-related articles included in PHYSICS TODAY's August 1999 "Buyers' Guide" supplement.

My perspective here is that of an individual who works at Canada's Institute for National Measurement Standards (see http://www.nrc.ca/inms/ for an overview of the institute's activities). However, the above comments are just my personal opinion.

References

1. Quoted on NIST's Web site, http://physics.nist.gov/cuu/Reference/contents.html.
2. Quoted on BIPM's Web site, http://www.bipm.fr/.

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Info Recurs, Going Back to Zermelo, Loschmidt Paradoxes

As a geophysicist interested in stochastic processes, I found George M. Zaslavsky's "Chaotic Dynamics and the Origin of Statistical Laws" (PHYSICS TODAY, August 1999, page 39) very stimulating. It surprised me, however, that in his discussion of Ernst Zermelo's paradox of recurrence and Josef Loschmidt's paradox of reversibility, Zaslavsky does not include any references to either Marian von Smoluchowski's or Subrahmanyan Chandrasekhar's contributions to resolving these paradoxes.[1,2] It seems to me that Chandrasekhar's conclusion—an almost literal translation of von Smoluchowski's earlier one—that "a process appears irreversible (or reversible) according as whether the initial state is characterized by a long (or short) average time of recurrence compared to the times during which the system is under observation" [2] would only add to Zaslavsky's argument. Incidentally, Chandrasekhar, clearly perturbed by the protracted lack of recognition accorded to von Smoluchowski, also commented that "the absence of references, particularly to Smoluchowski, is to be deplored since no one has contributed so much as Smoluchowski to a real clarification of the fundamental issues involved [in the laws of thermodynamics]." [2]

References

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2. S. Chandrasekhar, Rev. Mod. Phys. **15**, 1 (1943); reprinted in N. Wax, ed., *Selected Papers on Noise and Stochastic Processes*, Dover, New York (1954), p. 3.

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ZASLAVSKY REPLIES: To my knowledge, the first detailed explanation of the Zermelo and Loschmidt paradoxes should be attributed to Paul and Tatiana Ehrenfest[1] (an English-language translation of their paper is given in reference 3 of my article[2]). Unfortunately, space limitations prevented me from including in the article all of the other interesting and important references. So I think that Remko Uijlenhoet's comment is likely to be useful to your readers. Also, more discussions of the paradoxes, especially in relation to systems with chaotic dynamics, can be found in my 1984 book (reference 7 in my article).[3]

References

1. P. Ehrenfest, T. Ehrenfest, in *Enzyklopädie der Mathematischen Wissenschaften*, B. G. Teubner, Leipzig (1912), vol. 14 (6), p. 213.
2. P. Ehrenfest, T. Ehrenfest, *The Conceptual Foundations of the Statistical Approach to Mechanics*, Cornell U. P., Ithaca, N. Y. (1959).
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