

has been ejected, and the velocity of the latter could similarly be inferred from momentum conservation if the proton momentum had been measured too. Thus the nature and the motion of one particle are deducible¹ from measurements made on another particle. There is nothing paradoxical about such proceedings.

When the existence of conservation theorems is inadvertently or mischievously concealed, however, the way toward solipsist aberrations opens, for conservation is⁴ the very root of identity. Then it becomes possible to obfuscate the permanent identity of the neutron in the above example, and to state that when the proton is detected "a spooky interaction at a distance" takes place that somehow creates the neutron at that very instant. To some, it will come as no surprise that military planners have seriously considered such delusions as being usable for the purpose of safe and superluminal communication between the Boss of Naval Enterprises and his minions under the ocean (see *PHYSICS TODAY*, April 1985, page 46). How a paradox industry can continue to flourish among seasoned physicists appears less hilarious to contemplate.

As a boon for true aficionados, another kind of fodder is available to keep the beast alive if solipsism should fail, namely the EPR fallacy. This is purely logical and has nothing to do with quantum physics; it consists² in the substitution of an "and" for an "or." By sleight of hand it can result in self-contradictory statements about non-commuting observables, but those are easily avoided by the adoption of a commonsense postulate (see D3 of reference 2).

Once the logic is straightened out in this (or an equivalent) manner, it remains² entirely legitimate to join Einstein, Podolsky and Rosen in their original quest for a specific meaning of the term "reality," or to discuss the topic from alternative points of view. Still, whenever in such discussions EPR inferences are envisaged for handy armchair experimenting, relentless attention must be paid to conserved quantities. They are the alpha and the omega of identification. In particular, and to return to the starting point of this letter, awareness of conservation allows one easily to see through the mystifications surrounding Bell's inequality, which in its essence states only that in a system with mutating identities more randomness will be observed than in a system subject to strict conservation.

Shall we see further, costly experiments done whose outcomes were nev-

er in doubt? Perhaps some sprightly nuclear and particle experimentalists can be found to rally in healthy protest and tell the profession in no uncertain terms that a fair lot of modern physics would be abject nonsense if there were anything paradoxical about the many, many counter techniques in common use for the identification of particles by means of conserved quantities.

References

1. E. Breitenberger, *Nuovo Cimento* **38**, 356 (1965). (Owing to a proofreading oversight, which I deeply regret, this reference attributes a near-solipsism to Bohr; the intended target of the remark was reference 6 of this paper.)
2. E. Breitenberger, *Nucl. Phys. A* **211**, 623 (1973).
3. J. F. Clauser, A. Shimony, *Rep. Prog. Phys.* **41**, 1881 (1978). A. Aspect, J. Dalibard, G. Roger, *Phys. Rev. Lett.* **49**, 1804 (1982), and references therein.
4. See also E. Breitenberger, "Identity and indistinguishability," submitted to *Found. of Phys.*

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5/85

Heisenberg and nuclear physics

In his article "Werner Heisenberg and the beginning of nuclear physics" (November, page 60), Arthur I. Miller points out quite correctly that "the roots of Heisenberg's nuclear charge-exchange force are found in his June 1926 discovery of the exchange energy in atomic processes." He discusses these roots, the exchange force itself and "two early ramifications" of the latter: Enrico Fermi's beta-decay theory and Hideki Yukawa's meson theory. The discussions in Miller's article, as in his recent book *Imagery in Scientific Thought*, on which the article is based, are meant to support the thesis that many physicists (such as Heisenberg) were more sensitive to conceptual problems than to empirical data.

My own view on some of the matters discussed by Miller differs sharply from his. For example, I believe that the empirical discovery of the neutron was as critical to the nuclear theories of Heisenberg, Fermi and Yukawa as anything purely conceptual, just as the nonobservation of the neutrino was the main reason that Heisenberg in 1932 preferred an energy-nonconserving picture of beta decay. Similarly, it is hard to see Fermi's totally different approach to beta decay as a ramification of Heisenberg's exchange force or to imagine its acceptance on grounds other than its agreement with a large amount of empirical data. The real connections of the three theories are a little different: Yukawa's theory stemmed directly out of Fermi's and

Heisenberg's, while Heisenberg tried to modify his exchange force in 1934 on the basis of Fermi's theory.

It is hardly surprising to find that two historians differ in their interpretations. However, my main point here is to draw attention to what I regard as flaws in Miller's *method*. Because of limitations of space, I will consider only the interpretation of one of Heisenberg's paragraphs, that with which Miller claims "modern nuclear physics begins." (In his book, he adds elementary-particle physics as well.)

The quotation in question is the third paragraph (except for its noncontroversial introductory sentence) of part I of Heisenberg's important three-part article "On the structure of atomic nuclei," received by the *Zeitschrift für Physik* on 7 June 1932, shortly after James Chadwick announced the discovery of the neutron. Since the matter of translation is crucial to my argument, I will quote the German version and then Miller's translation as it appears in the *PHYSICS TODAY* article. (The translation in Miller's book is different and it contains additional problems.) The paragraph reads:

Bringt man Neutron und Proton in einen mit Kerndimensionen vergleichbaren Abstand, so wird—in Analogie zum H_2^+ Ion—ein Platzwechsel der negativen Ladung eintreten, dessen Frequenz durch eine Funktion $J(r)/h$ des Abstandes r der beiden Teilchen gegeben ist. Die Grösse $J(r)$ entspricht dem Austausch- oder richtiger Platzwechselintegral der Molekültheorie. Diesen Platzwechsel kann man wieder durch das Bild der Elektronen, die keinen Spin haben und den Regeln der Bosestatistik folgen, anschaulich machen. Es ist aber wohl richtiger, das Platzwechselintegral $J(r)$ als eine fundamentale Eigenschaft des Paares Neutron und Proton anzusehen, ohne es auf Elektronenbewegung reduzieren zu wollen.

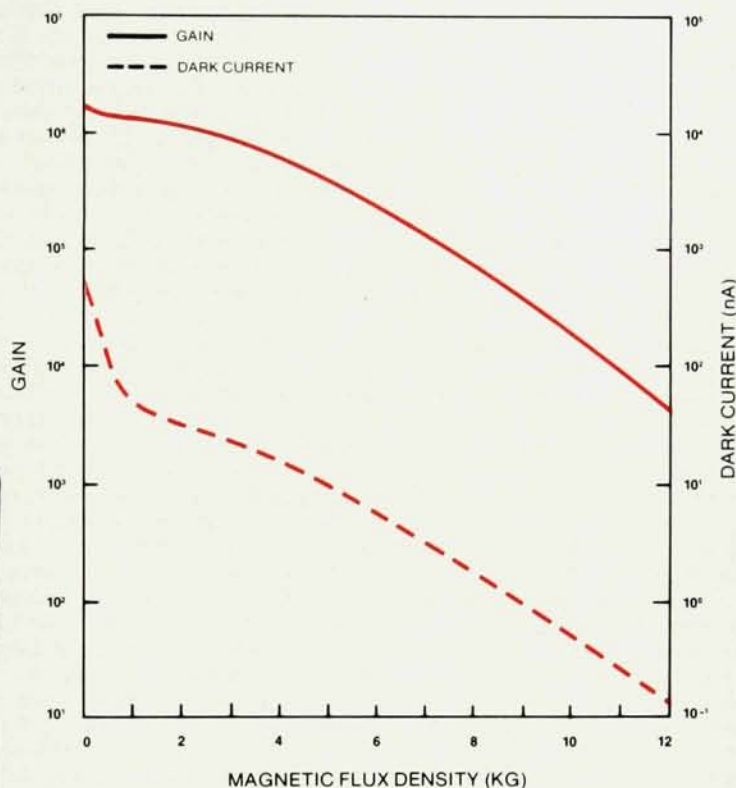
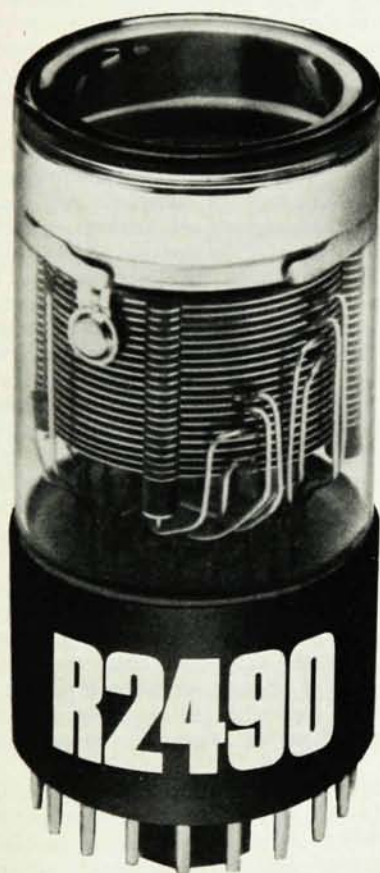
This is Miller's translation:

Suppose we bring the neutron and proton to a separation comparable to nuclear dimensions; then in analogy to the H_2^+ ion, the negative charge will undergo a migration [Platzwechsel], whose frequency is given by a function $J(r)/h$ of the separation r between the two particles. The quantity $J(r)$ corresponds to the exchange [Austausch], or more correctly, migration integral [Platzwechselintegral], of molecular theory. The migration can again be made more intuitive by the picture of electrons that have no spin and follow the rules of Bose statistics. But it is surely more correct to regard the migration integral $J(r)$ as a funda-

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mental property of the neutron-proton pair, without intending to reduce it to electron motions.

The bracketed insertions in the English text by Miller emphasize his new translation of the word *Platzwechsel* as migration, not exchange. Other translators fail to do so; for example, D. M. Brink does not include¹ the phrase *oder richtiger Platzwechselintegral* in his translation, and renders the second *Platzwechselintegral* in the quotation as "exchange-integral," thus ignoring Heisenberg's verbal distinction.

However, Heisenberg was a careful writer, and we should try to fathom his meaning. Fortunately, high-school German and elementary quantum mechanics will suffice. Heisenberg refers to an integral of molecular theory, there being two types of such integrals in the elementary theory, called "Coulomb" and "exchange." The Coulomb integrals involve the charge densities in the form of squares of the spatial wavefunctions; the exchange integrals involve the products of wavefunctions with different space variables. The spin structure is factored out, and Heisenberg was reminding the reader that he was referring to what might be called "more correctly the space-exchange integral."

The translation as "migration integral" is quite misleading because, as all textbooks point out, the frequency $\mathcal{J}(r)/h$ is much too high to be observable, so that the electron is more like a commuter than an immigrant. In the nuclear case the frequency is much higher still and can be pictured only as the exchange of an imaginary (and I don't mean virtual) electron, one obeying Bose statistics and having no spin, not an actual electron at all. Thus Heisenberg ends by cautioning us against "wishing to reduce [the exchange force] to electron motions." (In his book, Miller translates this last phrase as "unintentionally reducing to electron motions," thus rendering, however unintentionally, a negative sentiment as a positive one, and this "unintentionally" continues to echo throughout the book and the article as well.)

Miller argues that Heisenberg was trying to initiate a conceptual revolution that would have made the nuclear forces "visualizable." Thus Miller writes, "In 1932 Heisenberg introduced the depictive component of visualizability in another virtuoso performance." Indeed, the 1932 nuclear theory of Heisenberg was a landmark, but for a different reason. His virtuosity consisted in making a *phenomenological* theory of nuclear forces, eschewing any fundamental theory of particle exchange, and thus setting limits on the

extent to which one would attempt to apply quantum mechanics in the nucleus. It is not fair to say of this outstandingly pragmatic physicist that he "played all ends against the middle in this theoretical free-for-all," as Miller does in the book and the article, not fair to say it of someone who maintained a correspondence with Niels Bohr, Paul A. M. Dirac and Wolfgang Pauli and pleaded for their (not gentle) criticisms.

Why then did Heisenberg insist on having electrons in the nucleus, if not that they might be exchanged? There are many empirical reasons, including² the observation of cosmic-ray showers before the pair-production process had been established. Throughout his three-part paper, Heisenberg agonized about the neutron as an electron-proton compound—this characterization of his feelings is appropriate, because it was a conceptual nightmare. His brilliant nuclear model was burdened with a totally unacceptable model of the composite neutron. He was quick to abandon it when Ettore Majorana pointed out that another type of exchange force gave the correct deuteron spin (as Heisenberg's did not).

The person who tried to construct a genuine quantum field theory of Heisenberg's exchange force was Yukawa, as we have discovered in a number of unpublished notes among Yukawa's papers in the Yukawa Hall Archival Library in Kyoto. Although he did not, of course, succeed in doing so, his attempt prepared him to seize the opportunity to create the meson theory after he learned of Fermi's beta-decay theory and the failure of Igor Tamm and Dmitri Iwanenko to fit the range and the strength of nuclear forces simultaneously by exchange of the so-called Fermi field, that is, the electron plus the neutrino.

What should we conclude? Miller quotes from Heisenberg's 1926 paper on the many-body problem that there should be a limitation "on the discussion of the intuition problem." That may be a good idea.

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1. D. M. Brink, *Nuclear Forces*, Pergamon, Elmsford, N.Y. (1965), p. 143.
2. L. M. Brown, D. F. Moyer, *Am. J. Phys.* **52**, 130 (1984).

Laurie M. Brown
Northwestern University
Evanston, Illinois

12/85

MILLER REPLIES: Laurie Brown's principal disagreement with my article is that, as Brown writes, "many physicists (such as Heisenberg) were more sensitive to conceptual problems than to empirical data."

He writes, "I believe that the empirical discovery of the neutron was as critical to the nuclear theories of Hei-

senberg, Fermi and Yukawa as anything purely conceptual." Let me state again my position: "Investigating the roots of these papers [on nuclear physics] brings into bold relief a fascinating aspect of the development of atomic physics, namely, that for Bohr, Heisenberg and Erwin Schrödinger, among others, conceptual problems were often more critical than considerations of empirical data." By writing this I was certainly not trying to set up a struggle between the extremes of a sort of Platonic rationalism and empiricism. My goal in the article was to exhibit the importance of conceptual (that is, intuition) problems to Heisenberg's contribution to nuclear theory. I would like it to be clear that in my opinion empirical data played a decisive role in this contribution, as a catalyst for deep conceptual changes. When quoting from Heisenberg's nuclear-physics paper that Chadwick's discovery of the neutron offered an "extraordinary simplification for the theory of the atomic nucleus," I meant clearly to show that without the neutron Heisenberg would not have formulated his nuclear theory. Nevertheless, to understand the very process of Heisenberg's work, I believe it necessary to comment on conceptual problems. Did I initiate some new historical "method" in stressing the key role of such problems in the history of science? Of course not. Historians of science who have explored the genesis of quantum theory agree on the importance of intuition problems for Bohr, Max Born, Heisenberg and Schrödinger, because that conclusion emerges with overwhelming evidence from careful study of both primary and archival sources.

More precisely, I wanted to point out in my article the role that interpretation of language played in the genesis of quantum mechanics, and to do so I discussed Heisenberg's November 1926 paper "Quantum mechanics" in *Die Naturwissenschaften*. Two sentences from that paper serve to place in bold perspective Heisenberg's concern at that important juncture in the history of physics with the intuition problem and with the language that one must use to describe the submicroscopic world:

Investigation of the species of physical reality that is proper to electrons and atoms is precisely the subject of atomic physics and thus also of "quantum mechanics."... Here the point we wish to make is to emphasize that the investigation of that typically discontinuous element and of that "species of reality [*Realität*]" is the essential problem of atomic physics and consequently also the content of all quantum-mechanical considerations.

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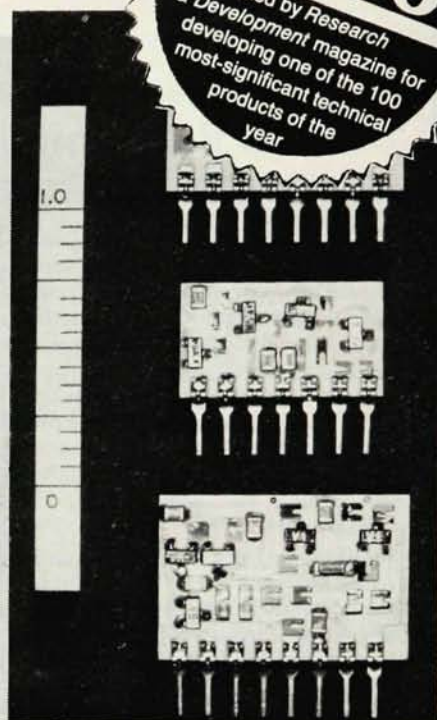
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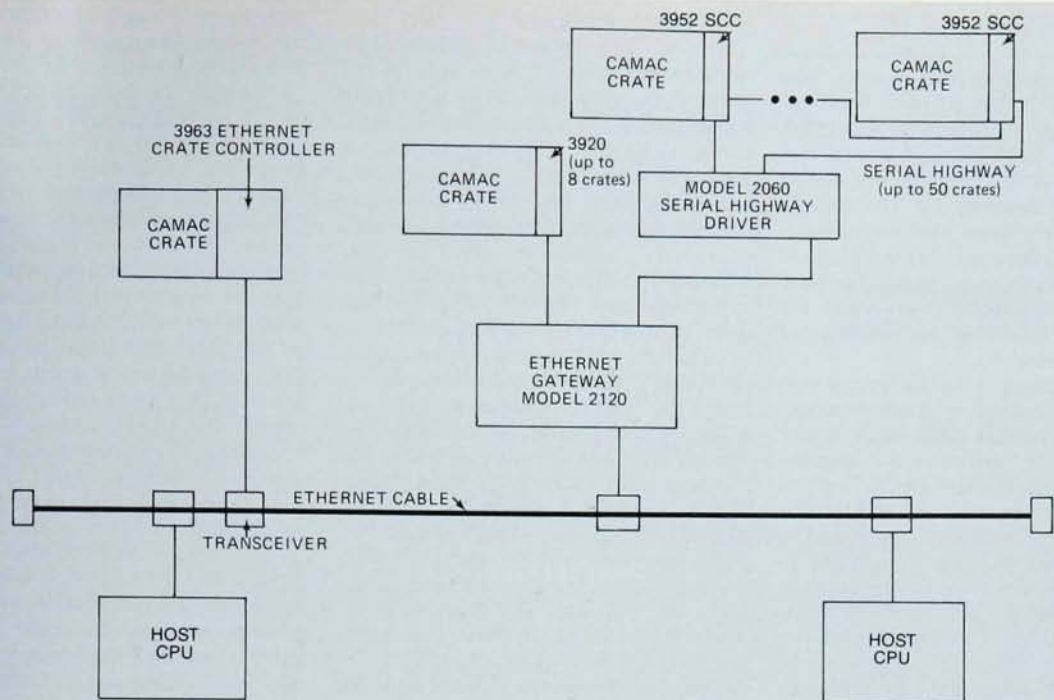
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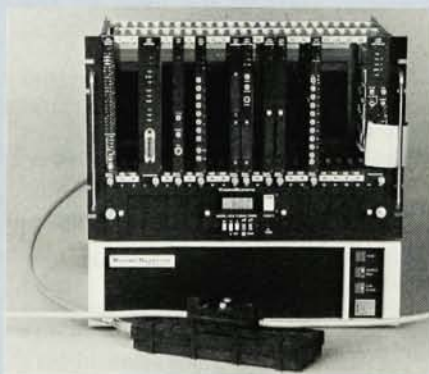
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letters

Heisenberg intentionally used the word *Realität* with its philosophical meaning, suggesting the reality that pertains to entities, such as atoms and electrons, that are beyond our sense perceptions. Because for Heisenberg atomic entities were also beyond description with the everyday language of tables and chairs, it is reasonable that the "intuition problem" emerged for him as the fundamental problem of physical theory.

After complimenting me on my correct translation of *Platzwechselintegral*, Brown writes that my correct translation is "misleading" because because "the frequency $J(r)/h$ is much too high to be observable." While this is correct for molecular physics, and so too for nuclear physics, Brown's comment is beside the point for the purpose that Heisenberg, and later Yukawa, intended for $J(r)$. Yukawa was unconcerned about the unobservability of $J(r)/h$ in molecular-physics problems when he replaced $J(r)$ with a potential that he interpreted as the exchange of a virtual particle.

As Heisenberg writes in the passage that Brown cites *in extenso*, " $J(r)$ corresponds to the . . . migration integral of molecular theory"—that is, $J(r)$ is only analogous to the quantity in molecular theory. Heisenberg makes the distinction because he is writing about electrons that "migrate." (Nor, for good reason, would Heisenberg have used terminology like "actual electron.") The reason is that when Heisenberg discussed atomic entities he always carefully distinguished between degrees of reality, that is, between *Realität*, or the possible states of an electron before measurement, and *Wirklichkeit*, or the measured properties of electrons. In 1932 Heisenberg and Bohr, among others involved in nuclear physics, were unclear as to what sort of electron could change its statistics on leaving the nucleus. Heisenberg included electrons in the nucleus chiefly to explain beta decay. Moreover, as I discussed in my article, dealing with a compound neutron for beta decay and a compound or a fundamental neutron for other nuclear processes, along with a fundamental proton, meant that Heisenberg did not consider the *Platzwechselintegral* to designate what Brown translates as "more correctly the space-exchange integral." [In the paperback edition of my book *Imagery in Scientific Thought: Creating 20th-Century Physics* (MIT Press, Cambridge, Mass., 1986) the clause *ohne es auf Elektronenbewegung reduzieren zu wollen* is rendered as in my PHYSICS TODAY article. I thank Brown for noting this change in nuance concerning Heisenberg's restrictive statement, which Brown prefers to call

a "negative sentiment."]

Brown's description of Heisenberg's nuclear theory as a "phenomenological theory" is consistent with an overly empiricist view of Heisenberg's work. Circa 1950, by a phenomenological theory one meant a theory whose basic equations have essentially no fundamental derivation and whose purpose is primarily to fit some restricted class of data. But Heisenberg's nuclear theory concerned the structure of the nucleus, and the theory's equations for comparison with empirical data were derivable from a Hamiltonian. Moreover, the theory covered several nuclear processes, such as beta decay and the binding energies of nuclei.

I turn next to Brown's term "pragmatic physicist," which he uses to describe Heisenberg. Philosophically a pragmatic scientist is someone who deals exclusively with puzzles concerning empirical data. There are no pragmatic scientists at the level of a Bohr, an Einstein or a Heisenberg.

I must say I miss the point of Brown's chiding me for being unfair to Heisenberg when I described Heisenberg's incredible virtuosity in the nuclear-physics papers as "play[ing] all ends against the middle in this theoretical free-for-all." Is it not the case that in those papers Heisenberg dealt with compound and fundamental neutrons, Bose electrons, and conservation and nonconservation of energy, and was not even completely convinced that the neutron was a spin- $1/2$ particle? Heisenberg indeed agonized over including Bose electrons. But that was the best that he could do in that turbulent period. Nevertheless, from Heisenberg's 1932 nuclear theory—flawed in its detailed execution but grand in its concepts—emerged the basis for the improved theories of Majorana, Fermi and Yukawa.

Brown's conclusion is woven around a quotation taken out of context from Heisenberg's 1926 paper on the helium atom. The "intuition" with which Heisenberg was concerned in Brown's quotation was that of classical physics. At any rate, to discern Heisenberg's move toward going beyond *Anschaung*, or classical intuition, requires careful study. Shall we be forgiven for paraphrasing Kant and stating that concepts without history are blind, history without concepts is empty?

ARTHUR I. MILLER

University of Lowell

Lowell, Massachusetts, and

Harvard University

Cambridge, Massachusetts

2/86

The balancing act

In the interesting article by Kenneth Laws (February 1985, page 24) on "The

physics of dance," it seems that the mechanism proposed by Laws to explain the regaining of balance is incorrect. Once the dancer starts to topple to the right, say, a force (such as the reaction due to the rail on the body suggested by Laws) that acts on the dancer's feet will not retard or stop the toppling. This may be seen most easily by taking the contact point between the foot and the ground (or whatever surface the dancer stands on) as the pivot point in calculating the torques acting on the body. The force that acts on the foot contributes a zero torque and hence cannot act to reduce the toppling torque due to the dancer's weight.

In my opinion, to regain his balance, the dancer must change the configuration of his body—for example, by extending the left arm or the left leg, or by bending the torso so that more of it falls to the left than to the right—to shift the position of his center of gravity so that the new center of gravity is closer to the vertical (reducing the toppling torque), and he eventually regains his balance when—and only when—the new center of gravity is on the vertical. At this point, the toppling torque vanishes.

In the example of someone walking on the rail of a railroad track used by Laws in rejecting the above explanation, our observations and interpretations are different. In this case, I agree that when one starts falling to one's right, the upper body will suddenly bend toward the right, as suggested by Laws. However, when one does this, some part of the body—usually the lower part, or the left arm or leg—is displaced to the left. The net effect is to shift the center of gravity toward the vertical, as explained above. A small movement accompanying the bending of the upper body to the right may happen at the foot. That is, by slightly moving one's foot, one can shift the pivot point to the right foot—if one stands initially with the two feet apart—or to the right edge of the right foot. This action will shorten the moment arm of the toppling and reduce the toppling torque itself. It may even enable one to regain one's balance if the center of gravity now falls to the left of this new pivot point—a real possibility during the initial phase of the toppling.

In short, to regain balance, generally speaking, one needs a flexible body. It pays to stay young.

LUI LAM

City College and Queensborough

Community College

3/85

City University of New York

LAWS REPLIES: Physicists and dancers both may contribute insights into the objection raised by Lui Lam in his letter. He claims that the horizontal force exerted on the body by the floor