

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?

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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

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3/85

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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

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letters

when action is taken to restore balance *does not* contribute to regaining balance, but that some other mechanism is responsible, such as a change in body configuration or a subtle movement of the point of support at the floor.

Physicists will agree with Newton that if there is no unbalanced force acting on a body at rest, no manipulations of the body can change the location of the center of mass. Further, if there is an unbalanced force on the body, the center of mass must be accelerated. If a horizontal force on the body from the floor is the only horizontal force acting on the body, then the center of mass *must* be moved. Suppose the center of mass is initially displaced to the right of the vertical axis through the point of support at the floor, creating an unbalanced situation. Appropriate manipulations of the body will create a net horizontal force on the feet toward the left, which, being the only horizontal force acting on the body, will move the center of mass back toward the balanced condition.

Lam claims that because the force from the floor acts through the pivot point, it cannot create a torque around that pivot point and therefore the position of the center of mass is unaffected. Further analysis shows that the center of mass may be displaced even though the torque around the pivot point is zero. Such is the subtlety of dealing with rotations of nonrigid bodies!

Consider an example in which the center of mass of a dancer almost in balance on one foot lies to the right of a vertical axis through the support at the floor. When the upper body bends rapidly toward the right, the feet tend to move toward the right but are prevented from slipping by the friction force from the floor acting to the left on the feet. The clockwise motion of the upper body represents a clockwise contribution to the total angular momentum around the pivot point on the floor. Other parts of the body will be accelerated in a counterclockwise direction around the pivot, so that the total angular momentum of the body around the pivot remains zero. Because the rotating upper body is farther from the pivot axis than is the center of mass, the net result will be that the center of mass actually moves to the left (a counterclockwise rotation around the pivot) without ever creating a nonzero net angular momentum around the pivot point. Of course there is a net angular momentum around the center of mass, but we know that the angular momentum depends on the choice of rotation axis.

Dancers and others who have experimented with their own body motions

will readily report that it is possible to regain or to lose balance while the point of support at the floor does not move. Many observed dance movements, such as a *tombé* or a multiple-turn *pirouette* on balance, would otherwise be quite impossible!

KENNETH LAWS

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7/85

Identifying 'the first'

I wish to echo Manuel Cardona's complaint about the practice of stating that a calculation or experiment is "the first" (December, page 9). Physics is a symphony, not a brass band with everyone blowing his own horn.

This is the second time that a complaint on this matter has been reported, but it is the first time anyone has reported that it was the second time that a complaint on this matter has been reported.

The author has additional reports in progress.

JAY KANGEL

Minneapolis, Minnesota

12/85

Educating teachers

Your editorial of January 1985 (page 160) focused upon the need to improve science education but did not discuss the quality of ideals and instruction within American graduate schools.

If we see smoke, we trace it to find the fire. Why then don't we trace poor science teaching back to inappropriate ideals and training in our graduate schools? It is because professors in our graduate schools profess impersonal ideals that misdirect our attention to shield themselves from being exposed as not facilitating learning!

In a letter in *PHYSICS TODAY* (January 1983, page 116) I asked readers for information pointing to any graduate physics department that was genuinely dedicated to preparing physics graduate students to fulfill their future instructional responsibilities as professors of physics in colleges and universities. I have not yet heard of any. There apparently are none! A recent article makes¹ a related comment about our American training of high-school physics teachers.

It is no wonder that personable, bright science students of all ages are "turned off" by impersonal science teachers. Sensitive students rarely survive impersonal science teachers later to become science professors with integrity.

Research scientists' impersonal ideals trickle down through our whole

science-education system. Teaching professors guided by these impersonal research ideals systematically block the personal information feedback loops that are essential for educating professors in the personal dynamics of learning and failure. We don't need better technologies or larger commitments of resources to impersonal ideals. We need instead more personable ideals that can unite us into a global community with both personal and communal integrity.

Almost all of the writings about the crisis in science education miss the central issue: Unless there is a revolution in the ideals of graduate science professors, we will not resolve our crisis in science education, nor will we survive.

Reference

1. D. Bower, P. Kenealy, *et al.*, *Phys. Teach.* **30**, 6 (1985). See the last paragraphs of the article.

PAUL A. SMITH

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10/85

Gravitational-constant data

During the last few years, I have prepared¹ two versions of a research bibliography on measurements of the Newtonian gravitational constant, G . The measurements indexed in these documents are of the absolute value of G and its variation with respect to time, intermass spacing, test-mass temperature, charge content, state of quantization and so forth. I am preparing to publish updated versions of these bibliographies in *Metrologia* presently, but before doing so I want to be certain that they are as complete as possible. Therefore if anyone reading this has made a measurement of G of one kind or another and has it written up but not published, or published obscurely (as in a technical or industrial report, uncataloged thesis or dissertation, or low-circulation conference report), please send me the appropriate reprints or preprints so that I can reference them.

Reference

1. G. T. Gillies, *The Newtonian Gravitational Constant: An Index of Measurements*, Int. Bureau of Weights and Measures Technical Report, Rapport BIPM-82/9 (July 1982). G. T. Gillies, *The Newtonian Gravitational Constant: An Index of Measurements—1983 Edition*, Int. Bureau of Weights and Measures Technical Report, Rapport BIPM-83/1 (February 1983).

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