

physics and Platonic love, the two parties never really meet the way that they should!

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Riordan replies: I am pleased that my article has elicited such a range of thoughtful replies. Some of the writers make important points that offer me good opportunities to elaborate further on issues I raised there and to enter into a more textured dialog.

I wrote from concerns about the nature of scientific fact and about claims made by scholars of the relativist or postmodernist persuasion that it is no different from other kinds of truth. Pantazis Mouroulis echoes that last thought in his letter. Taking the discovery of quarks as an example, one can ask how the statement "there are six kinds of quarks" is any truer than the statement "Picasso was a great artist." In both cases, a community of experienced practitioners—art critics and historians in the latter—made a decision, a *choice*, based on the evidence at hand. I believe, however, that it is the repeated testing of scientific theories by measurements, combined with what I call the withering skepticism characteristic of experimenters, that makes scientific facts harder, more durable, more stable than the statements of our humanistic colleagues.

Yet scientists admit that scientific theories are tentative, subject to further testing and elaboration—perhaps just steppingstones on the path to better theories. How do we reconcile that characteristic with the durability and stability of scientific facts? By recognizing that the truth of an earlier theory is not necessarily invalidated by this process of elaboration. Einstein did not reject Newtonian physics, he extended it to domains of ultrahigh velocities and masses. And I expect that, centuries from now, physicists will continue to hold that $E = mc^2$ (plus perhaps small correction terms), while I am not as certain that art historians will still consider Picasso a great artist.

Travis Norsen questions my obvious reliance on the pragmatist philosophy of Charles Sanders Peirce—especially if carried to the extreme of radical empiricism, which I do not propose to do. But I still find Peirce's ideas the best definition of what we physicists mean when we say something like a quark is "real." We not

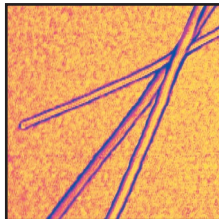
only have a variety of independent measurements that can be reconciled by that hypothesis, a point that Miguel Ferrero stressed and that Peirce took to be a key hallmark of the real. But particle physicists also use quarks—or at least some fractionally charged entities that they identify by that term—in their daily practice. At Fermilab, for example, the protons and antiprotons colliding by the millions in the Tevatron are thought of as bags of quarks and antiquarks. Or, as philosopher of science Ian Hacking (also a Peirce admirer) wrote about electrons, quarks

are real because we can "spray" them at one another in this manner. I remain an unrepentant pragmatist.

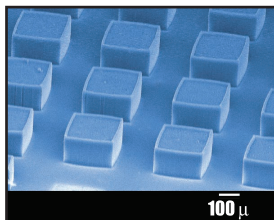
Unfortunately, I may also have come across as an unrepentant empiricist, for which Paul Roman correctly admonishes me. I also thank him for including the full Galileo quote that the Book of Nature has been written in the language of mathematics. I can only plead that the requirements that an Opinion piece be opinionated and strictly limited in length have skewed my argument toward the empirical side, perhaps in the process diminishing

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what I agree is the importance of mathematical theory in understanding nature.

Historians and philosophers of science increasingly recognize that there is no such thing as “raw data,” that theory molds measurements and our understanding of them almost every step of the way. I am, however, uncomfortable with Roman’s insistence on the “beauty” of theories as a valid truth criterion, because beauty is a bit too subjective for me. One person’s beauty may be another one’s ugliness. I prefer simplicity to beauty as a criterion. The discovery of a fourth, charm quark was indeed “Nature’s slap in the face” that practically forced physicists to accept quarks as real particles. Not only was its existence the only way to account for the J and ψ particles that suddenly proliferated in the mid-1970s; the charm quark was also needed for a host of other reasons—for example, to account for the absence of strangeness-changing neutral currents. Not just another ad hoc addition to the company of quarks, the charm quark did so many things in a simple, economical package.

The close interaction between theory and experiment is part of what makes the history of physics so interesting. That interplay is also what makes physics such a vital activity and allows it to extend human understanding. For a good example, just consider what has been happening in cosmology during the past two decades. That discipline has finally become a true experimental science, and it is advancing by leaps because cosmological theories now confront observations and experiments—and vice versa—almost daily.

Alas, this kind of close interaction has most definitely not been happening in the case of superstrings, the subject of much theoretical activity but absolutely no experiments during the same period. Despite recent encouraging possibilities that superstrings might have observable effects at the TeV scale, string theory remains an almost exclusively mathematical activity isolated from any serious threat of experimental test. Within the tight community of string theorists, ideas are judged not by their ability to account for observations but by such criteria as elegance, rigidity, and mathematical consistency. This kind of activity is the modern—or maybe I should say postmodern—equivalent of medieval Scholastic arguments about how

many angels can dance on the head of a pin.

I just do not see how such ideas, unchallenged by experiment, are any more valid than those proffered by humanistic scholars. In my article, I called such mathematically intensive theoretical activity “Platonic physics,” but it can also be characterized as “postmodern physics.” So far, the acceptance of ideas such as superstrings, wormholes, and parallel universes within their respective subdisciplines is based almost completely on subjective criteria held dear by these communities—much as happens in the humanities—and not on any wider, more objective standards.

Most of the respondents share my concern that physics may be in danger of relaxing its acceptable standards of truth. If that happens, physics will lose its claim to special knowledge, and the postmodern humanist scholars will have won the debate.

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Covering Condensed Matter Fundamentals

It is an honor to have one’s book reviewed in PHYSICS TODAY, which has a wide and well-informed physics readership. The standard of reviews is generally very high.

We thus were disappointed to see, in the May 2003 issue (page 64), Piers Coleman’s inaccurate and cursory review of our book, *A Quantum Approach to Condensed Matter Physics* (Cambridge U. Press, 2002). Coleman states that the section on mesoscopic physics “fails to explain localization as a constructive interference between time-reversed paths,” yet section 9.4 is devoted to doing precisely that.

Furthermore, Coleman writes that our chapter on the Kondo model and heavy fermions “does not explain the concept of a localized

moment.” In fact, the sections on the Kondo problem are centered about the role of local spin and end by showing how the Kondo effect is well described by a density-of-states expression that adds a resonant state at the Fermi energy for each impurity with a local moment. Similarly, the chapter on superconductivity has a section on the Ginzburg–Landau theory of type II superconductivity. In that section, we explain how to construct the free-energy density in terms of a spatially varying complex gap parameter. Yet Coleman says instead that we “never allude . . . to the order parameter.”

Authors must always be prepared for adverse reviews based on a reviewer’s dislike of an author’s choice of subject matter. However, it is painful indeed to have seeming omissions criticized by someone who does not seem to have read further than the table of contents.

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Coleman replies: The writer of a book review is caught between the conflicting requirements of encouraging the authors and assessing the book honestly for the community. In my review of the book by Philip Taylor and Olle Heinonen, I commended the authors on their effort, but expressed my concern that the book did not provide many of the basic principles that underpin modern correlated matter physics. I cited as possible shortcomings the authors’ failure to discuss the concept of broken symmetry and the notion of an order parameter with a phase stiffness; I also noted the absence of any discussion of the origin of local moments and the renormalization group description of the Kondo effect.

However, I apologize for a serious oversight in my review: The authors

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