Science Fashions and Scientific Fact

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As an MIT graduate student and postdoc during the 1970s, I took part in a series of experiments that ended up discovering quarks. The leaders of the MIT–SLAC inelastic electron-scattering experiments—Jerome Friedman, Henry Kendall, and Richard Taylor—shared the 1990 Nobel Prize in Physics for this breakthrough, while collaboration members basked in reflected glory at an unforgettable Stockholm reunion.

In the late 1960s when those experiments began, the quark hypothesis stood far down the list of particle theories. Even Murray Gell-Mann, who conceived the idea along with George Zweig, did not think such fractionally charged entities could ever exist. For Gell-Mann, quarks had to be "mathematical," a convenient rubric for organizing the burgeoning zoo of baryons and mesons. As he wrote in 1964, "A search for stable quarks of charge −⅓ or +⅔ . . . at the highest-energy accelerators would help to reassure us of the non-existence of real quarks."[1]

Undeterred, experimenters still went hunting for these oddities. Some sought quarks at accelerators, where they would have shown up as faint tracks in bubble chambers; others searched in cosmic rays and Millikan-style experiments, hoping to observe fractional charges. By the late 1960s, after none of these experimenters had found anything, it appeared that Gell-Mann had been right. Quarks did not seem to exist. If they had any essence at all, it had to be mathematical. They could not be "real," red-blooded elementary particles.

Do quarks really exist?

Thus we did *not* go seeking quarks in the early MIT–SLAC experiments. Quarks had been largely dismissed by particle physicists, who were far more interested in the then-fashionable bootstrap models, Regge theories, and vector dominance to explain what happens within nuclei. Except for a few stalwarts, theorists were abandoning field theories and constituent models of the strong force.

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We went to Stanford instead to measure electromagnetic structure functions of the proton and neutron, which James Bjorken and Sidney Drell had suggested might show how the stuff inside is distributed. Much to our surprise, a fraction of the electrons fired into protons in the first experiment ricocheted off. Such deep-inelastic scattering was occurring far more often than expected. Bjorken and Richard Feynman proposed that the electrons might have bounced off tiny pits inside the protons, which Feynman dubbed "partons."

But those were only hints, not results. Nobody was booking a flight to Stockholm—or even drafting a press release. Instead, we went back to SLAC repeatedly during the next five years, to make much more detailed measurements. To check parton ideas against other explanations, we observed electrons rebounding at a wide range of angles from both protons and neutrons.

By 1973, when results of these second-generation experiments were in, everything seemed to be coming up quarks. All the fashionable "soft-scattering" theories had fallen by the wayside, despite desperate attempts to patch them up. But Feynman's partons remained in excellent condition; they indeed seemed to behave like fermions with fractional charges. Neutrino-scattering experiments at CERN, as well as proton–proton collisions in its new Intersecting Storage Rings, gave supporting evidence.

Yet one major problem persisted. The putative quarks never seemed to appear outside hadrons, no matter how hard one hit them! The resolution of that seeming paradox eventually emerged from the theory of the interquark force, quantum chromodynamics, which stipulates that the force *increases* as two quarks part company. So you can never pry one out of a baryon or meson. But it took the rest of the 1970s for acceptance to settle in.

Well before then, amazing results from an MIT experiment at Brookhaven National Laboratory and the SLAC–LBL experiment on the SPEAR electron–positron collider forced us to regard quarks as real. The 1974 discovery of the J/ψ particle in those experiments could be explained only by postulating a fourth quark, dubbed the charm quark. This surprising discovery was Nature's slap in the face, which finally made physicists sit up and admit that quarks truly existed. By 1976, when Burton Richter and Samuel Ting shared the Nobel Prize for the discovery, opposition to quarks had collapsed.

Count on experiments

This brief history of the quark discovery illustrates the crucial role that experiments play in making modern physics. It was not theory but experiment that plucked the quark idea from near oblivion. Aided and abetted by theory, experiments made quarks real, transforming them from a wayward hypothesis into concrete objects of experience. Experiments are what ultimately discarded the science fashions of the sixties and turned quarks into hard scientific fact.

As psychologist William James observed in his book *Pragmatism*, "Truth *happens* to an idea. It *becomes* true, is *made* true, by events" [italics in original].[2] He was popularizing the views of his idiosyncratic colleague Charles Sanders Peirce, one of the few philosophers of science with experience doing experiments. For Peirce, the true hallmark of the "real" is the observable consequences that a community of experienced practitioners agrees occur in actual practice.

That hallmark has indeed proved true for quarks, which form the bedrock of the standard model, the dominant paradigm of particle physics. Today we work with quarks almost unthinkingly, taking them for granted in high-energy experimentation. At Fermilab, physicists bash together bags of quarks and antiquarks, hunting for Higgs bosons and other exotica. Quarks have indeed become *things*.

I find it difficult, however, to imagine how such a rigorous criterion of reality could ever hold true for some of the fanciful ideas and constructs that have emerged in recent years from the minds of many theorists. How can we ever hope to work in everyday practice with such entities as superstrings, parallel universes, wormholes, and phenomena that occurred before the Big Bang?

Some of these ideas may have great mathematical beauty and significant explanatory power, but so did many discarded physics fashions of the 1960s. Superstrings are in fact an outgrowth of one of those earlier ideas, the dual resonance model, which John Schwarz resurrected in the 1980s and applied at the Planck scale. But how can we ever hope to make meaningful measurements at this scale when we have such difficulty building particle colliders to work at the comparatively lowly Higgs scale?

One or more of the extra dimensions required in superstring theories may soon become observable at the energies accessible at Fermilab or CERN's future Large Hadron Collider. Such a phenomenal discovery, if it occurs, would be tantamount to bringing superstrings down to Earth. But for such large extra dimensions ever to become truly real, experiments would have to exclude *all other* possible explanations of what occurs. That will not be an easy task.

Cultivate skepticism

One of the great strengths of scientific practice is what can be called the "withering skepticism" that is usually applied to theoretical ideas, especially in physics. We subject hypotheses to observational tests and reject those that fail. It is a complicated process, with many ambiguities that arise because theory is almost always used to interpret measurements. Philosophers of science say that measurements are "theory laden," and they are. But good experimenters are irredeemable skeptics who thoroughly enjoy refuting the more speculative ideas of their theoretical colleagues. Through experience, they know how to exclude bias and make valid judgments that withstand the tests of time.[3] Hypotheses that run this harrowing gauntlet and survive acquire a certain hardness—or reality—that mere fashions never achieve. This quality is what distinguishes science from the arts.

But many of today's practicing theorists seem to be unconcerned that their hypotheses should eventually confront objective, real-world observations. In a recent colloquium I attended, one young theorist presented a talk on his ideas about what had transpired before the Big Bang. When asked what observable consequences might obtain, he answered that there weren't any, for inflation washes away almost all preexisting features. Young theorists are encouraged in such reasoning by their senior colleagues, some of whom have recently become enamored of the possibility of operating time machines near cosmic strings or wormholes. Even granting the existence of cosmic strings, which is dubious, I have a difficult time imagining how anyone could ever mount an expedition to test those ideas.

I like to call this way of theorizing "Platonic physics," because implicit within it is Plato's famous admonition that the mathematical forms of experience are somehow more real than the fuzzy shadows they cast on the walls of our dingy material caves. And, in reaction to the seemingly insuperable problems of making measurements to test the increasingly abstract theories of today, some people have even begun to suggest that we relax our criteria for establishing scientific fact. Perhaps mathematical beauty, naturalness, or rigidity—that Nature couldn't possibly choose any other alternative—should suffice. Or maybe "computer experiments," as Stephen Wolfram intimated last year in *A New Kind of Science*, can replace measurements. According to a leading science historian, such a subtle but ultimately sweeping philosophical shift in theory justification may already be underway.

If so, I think it would be a terrible mistake. There would then be little to distinguish the practice of physics from, say, that of painting or printmaking—in which the criteria that distinguish the good from the bad are based largely on opinions of art critics and historians. There is something unique about scientific fact, and that uniqueness has much to do with the often tedious practice of making telling empirical observations. The primary criterion of good science must remain that it has been repeatedly tested by measurements—no matter how difficult they may prove to be—and found to be in excellent accord with them.

Without such a rigorous standard of truth, science will have little defense against the onslaughts of the creationists and postmodernists, for whom it is just one of many ways to grasp the world. How could we ever hope to defend science against such attacks if it were based only on the opinions of its leading practitioners? Mathematics is not enough, no matter how beautiful. Even Einstein, who helped foster this theoretical style, insisted his ideas had to have observable consequences.

The essence of scientific truth rests in the requirement that it should have strong accordance with the natural world that exists outside our minds and beyond human artifice—what Peirce called "the vagaries of me and you." Experimenters must continue ripping away at new ideas to make sure this accordance indeed holds true. Their skepticism plays a role like death in natural selection—only the strongest survive to take their place among what actually lives on.

In this evolutionary metaphor, speculative theorizing plays a crucial role, too, by helping to ensure that science investigates the many philosophical niches where truth might lurk. My one caveat is that hypotheses resulting from such wide-ranging explorations of possible theory space must ultimately lead to testable consequences—a process that may take years, even decades—if science is to advance. Otherwise, theorists are doing metaphysics, not physics.

The Book of Nature

Early in the 17th century, Bacon and Galileo enunciated a new approach to knowledge, based not on the words of Aristotle or the Medieval Scholastics but on reading what they called the "Book of Nature." According to Galileo, "Philosophy is written in this great book, the Universe, which stands continually open to our gaze."[4]

For nearly four centuries, reading the Book of Nature has been the foundation of an extremely powerful practice that has proved remarkably successful in extending cognition into the diverse corners of experience. It was by reading that book, in fact, that we stumbled upon quarks in the late 1960s. To abandon the practice now would be to risk a return to the chaos of opinion that preceded Bacon and Galileo. As physicists concerned about the future of our discipline, we must do everything we can to continue reading this rich and fascinating book.

References

1. M. Gell-Mann, *Phys. Lett.* **8**, 214 (1964).
2. W. James, *Pragmatism—A New Name for Some Old Ways of Thinking* (1907), in J. J. McDermott, ed., *The Writings of William James*, Random House, New York (1967), p. 430.
3. See, for example, A. Franklin, *Selectivity and Discord: Two Problems of Experiment*, U. of Pittsburgh Press, Pittsburgh, Pa. (2002).
4. Quoted in S. Shapin, *The Scientific Revolution*, U. of Chicago Press, Chicago (1998), p. 69. ■