

confluence that constrains their respective values.

In the history of physics, when three or four independent experimental procedures achieve the same result, with none opposing, that result is considered to be a fact. Such confluent relations are then fundamental and permanent; despite nature's being a participatory book, they are the precise points in which objectivity and truth enter into physics.

## References

1. M. Planck, quoted in R. Dunbar, *The Trouble with Science*, Faber & Faber, London (1995), p. 12.
2. N. Bohr, *Atomic Physics and Human Knowledge*, Wiley, New York (1958).
3. For further discussion, see J. A. Wheeler, *At Home in the Universe*, American Institute of Physics, New York (1992).

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**A**rthur Eddington, in his book *The Philosophy of Physical Science* (U. of Michigan Press, 1958), posed the question whether Ernest Rutherford had found or manufactured the atomic nucleus. If he were still alive, I suspect Eddington would be asking a similar question about quarks. The kind of approach to physics that concerns Michael Riordan was alive and well before World War II and was not without its critics then.

Herbert Dingle, philosopher and historian of science, wrote a *Nature* article entitled "Modern Aristotelianism,"<sup>1</sup> in which he attacked the ideas of P. A. M. Dirac, Eddington, and E. Arthur Milne, for many of the same reasons as Riordan attacks what he calls Platonic physics. Dingle's article provoked many responses.<sup>2</sup> Omitting the three from the people criticized, the replies were roughly equally divided for and against Dingle's point of view. Eddington's belief that dimensionless ratios of the constants of nature could be deduced by pure reason was, of course, part of Dingle's target. That belief is sometimes thought of as the preoccupation of Eddington's old age, but in 1937 he was only in his fifties. And the correspondence is evidence that he was not alone in thinking along those lines, even if he did pursue the idea more single-mindedly than others did. Perhaps this alternative kind of science will be ever with us.

## References

1. H. Dingle, *Nature* **139**, 784 (1937).
2. See ref. 1, pp. 997 and 1025.

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**M**ichael Riordan appropriately concludes his Opinion piece by quoting Galileo: "Philosophy is written in this great book, the Universe, which stands continually open to our gaze." But, perhaps to maintain his antitheoretical tone, Riordan withholds from us Galileo's next, and I think crucial, sentences: "But one cannot understand this book if one did not learn how to understand the language, and does not know the characters in which it is written. It is written in the language of mathematics. . . . Without [mathematical concepts] it is impossible for us to understand a single word of it."

Few sane people would quarrel with Riordan's main point that the essential criterion for a theory's acceptability is that it have predictive power. That means, first, that it should be experimentally verifiable or contradictable, and second, that it should encompass phenomena or events that are extensions of ones already encompassed. Riordan overlooks the second point.

Surely, though, predictive power is not the only acceptability criterion. A good theory must also be systematic, comprehensible, attractive—even beautiful. Although it would be disastrous if, as Riordan fears, some people suggested that "mathematical beauty, naturalness, or rigidity . . . should suffice," an equally grave error would be to discard such properties in assessing the acceptability of a theory. We are well advised to listen to Albert Einstein, who said, "A theory is acceptable to us only if it is beautiful." And P. A. M. Dirac added, "Einstein introduced the view that something that is beautiful mathematically is bound to be correct physically. The proof [of a complex theory] comes not really from experiments. The real foundations come from the beauty of the theory. . . . It is the essential beauty of the theory which, I feel, makes us believe in it." Henri Poincaré said, "Science is useful because it is beautiful." Such statements may sound exaggerated, but science is not here only to discover isolated facts. It always was and should remain an inspiration to and enrichment of the human spirit and

a means to discover the overall structure of events, not just facts, as Eugene Wigner emphasized.

An interesting example of how criteria other than experimental verification are also essential for scientific progress is the following: Often it happens that one has a fine theory, but that new observations or experiments reveal phenomena that cannot be encompassed by the relevant established theory. One then tries to accommodate new "facts" by adding extraneous elements to the beautiful theory. But such patching up, although successful, makes the entire edifice ugly. More often than not, that ugliness is a sign that the underlying theory is incorrect. A completely new conceptual beginning becomes necessary, and eventually, a new beautiful theory will emerge—to be tested by utterly new suggested experiments. This example also illustrates well the interplay between experiment and theory, which Riordan seems to see rather one-sidedly. He suggests, perhaps unwittingly, that the main role of experiments is to disprove erroneous theories.

Murray Gell-Mann was certainly right when he insisted that his quarks are just mathematical entities. After all, his SU(3) *flavor* quarks (with only three flavors, *nota bene*) and broken symmetry have very little to do with the physical, unbroken SU(3) *color* quark symmetry. The quark picture of matter became possible only after Y. Nambu and others came to the idea of color as a purely theoretical consideration to reconcile the possible quark picture with the spin-statistic theorem. Even that was not enough to accept quarks as physically real. The entirely theoretical edifice of renormalizable quantum chromodynamical field theory had to be developed first. Riordan disregards those facts in the discovery of quarks and overemphasizes the role of the beautiful deep inelastic scattering experiments. These experimental results indeed led Richard Feynman to the idea of pointlike entities inside nucleons; but partons are not quarks.

Finally, I can't see how the unexpected experimental discovery in 1974 of the  $J/\psi$  meson was, as Riordan put it, "Nature's slap in the face, which finally made physicists sit up and admit that quarks truly existed." That discovery merely showed that there is at least one more flavor than in the Gell-Mann–Zweig scheme, so that instead of the SU(3) flavor group, perhaps an

SU(4) flavor symmetry should be considered. Of course, it eventually turned out that this is not the correct way to deal with the new facts.

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**H**ighlighted in Michael Riordan's Opinion piece is the danger of relaxing the criteria for what constitutes scientific fact. He is, however, in danger of blunting a valuable new tool of science when he identifies computer experiments as part of the problem rather than part of the solution.

Fifty years ago, Enrico Fermi, John Pasta, and Stanislaw Ulam invented the computer experiment and predicted recurrence in nonlinear systems. They programmed the early MANIAC computer at Los Alamos Laboratory to simulate an array of 64 weakly coupled nonlinear oscillators. The researchers expected the array to relax into a random equipartition of energies. Instead, it periodically returned to the starting condition. Fermi affectionately referred to that phenomenon as a "little discovery."<sup>1</sup> Since then, Fermi-Pasta-Ulam recurrence has been experimentally confirmed and has become a key concept in understanding the behavior of complex nonlinear systems.

A few years ago, NSF Director Rita Colwell gave a talk in which she referred to simulation as "the third branch of science."<sup>2</sup> She based that statement on the use of computer simulation in fields such as astrophysics and Earth sciences, where system complexity prevents evaluation of theoretical predictions by any means other than computer simulation. In those fields, computer simulations bridge the gap between theory and experiment for complex nonlinear systems so that the theoretical predictions can be compared far more precisely to experimental results. Without simulations, approximations must be used, which limit accuracy and introduce unknown errors into predictions.

Thus, computer modeling and simulation are primarily theoretical tools. A powerful adjunct for the theorist, they provide additional predictions but never replace experiment. For example, the numerical predictions of gravity-wave emission from merging black holes are beyond analytical check and will only be confirmed with data from the Laser Interferometer Gravitational-Wave Observatory (LIGO) and other experiments. Modeling and simulation can

point to new directions for both experimental and theoretical investigation, so they truly merit being called the third branch.

## References

1. S. Strogatz, *New York Times*, 4 March 2003, p. A25.
2. R. R. Colwell, "Complexity and Connectivity: A New Cartography for Science and Engineering," remarks from the American Geophysical Union's fall meeting, San Francisco (1999). Available online at <http://www.nsf.gov/od/lpa/forum/colwell/rc991213agu.htm>.

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**I** greatly enjoyed Michael Riordan's Opinion piece criticizing the Platonic aspects of contemporary theoretical physics. He is right that several of today's research areas—superstrings, wormholes, and extra dimensions, for example—have cut loose almost completely from experimental reality.

Unfortunately, though, Riordan's arguments were undercut by his appeals to Charles Sanders Peirce and the pragmatist definition of truth. Although I believe Riordan is right to be proud of physicists' discovery of quarks, that feeling would not be justified if the reality of quarks meant merely that experienced practitioners agree that quarks are a "convenient rubric," as Riordan called it, for mocking up the observable consequences of certain experiments. That was also true of Ptolemy's epicycles, phlogiston, and Lamarckian evolution—not to mention young-Earth creationism. No, the discovery of quarks is impressive because they are more than a useful fiction; as proved by experiment, they really do exist outside our imaginations.

The pragmatist notion of truth is based on radical philosophical skepticism and leads logically to outright subjectivism—the claim that all scientific theories are mere "fanciful ideas and constructs." And like the Platonism that Riordan criticizes, that kind of error has done real damage to physics.

Consider, for example, Andreas Osiander's plea that Copernicus didn't really mean it, Ernst Mach's bizarre and influential refusal to believe that atoms represented more than a useful rubric for organizing experience, and the ongoing refusal to face and fix what John Bell called

the "unprofessionally vague and ambiguous" foundations of quantum theory.<sup>1</sup> This refusal is usually based on the claim that the wavefunction is merely a mental construct, and does not refer to physical reality (see the Opinion piece by Christopher Fuchs and Asher Peres, *PHYSICS TODAY*, March 2000, page 70). Consider also the contemporary attacks on science from the social-construction crowd; as those attackers point out, the pragmatist conception of truth gives scientists the same claim to know reality as any other group: none.

Pitting science against Platonism tells only half the story. What makes the scientific method unique is that it rejects both Platonism and skeptical subjectivism. Unlike Platonism, science demands that its conclusions be based on hard, empirical evidence. But science also rejects the idea that we are cut off from true reality, forever confined to superficial appearances, subjective constructs, and useful fictions.

At its best, science neither rejects empirical evidence in favor of rationalist flights of fancy nor dismisses as impossible the task of uncovering deep truths about the external world. Instead, it demonstrates, in the face of both traditional philosophical approaches, that hidden realities can be reliably grasped by means of empirical evidence. And that is an achievement all physicists can be proud of.

## Reference

1. J. S. Bell, *Speakable and Unspeakable in Quantum Mechanics: Collected Papers on Quantum Philosophy*, Cambridge U. Press, New York (1987), p. 173.

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**T**he Opinion column by Michael Riordan was thoroughly enjoyable. I agree with his overall view, with only one or two exceptions. He says that "good experimenters are irredeemable skeptics who thoroughly enjoy refuting the more speculative ideas of their theoretical colleagues." True, but the history of physics abounds with stories of bad experimenters who got self-duped while trying to confirm their own pet theories. Also, I have another model for the term "Platonic physics." Rather than Riordan's philosophical view, I go by the much more pedestrian idea of Platonic love. In both Platonic