The Brilliant Hallucination of Untested and Unmeasured Theory

In his reply to Michael Riordan's Opinion piece, Pantazis Mouroulis (PHYSICS TODAY, December 2003, page 13) made statements that reflect the common viewpoint that theories can sometimes be shown to be true or factual. This is something I don't think can happen with the most fundamental and general of our theories.

Mouroulis wrote, "But the aim of every scientific theory should be . . . to have its truth proven beyond doubt." However, by the basic logic of science, it is impossible for any widely applicable theory—any of the theories that we most value—to be proven beyond doubt.

A theory is a set of statements, T, that don't logically contradict one another and that, taken together, imply an infinite set of testable consequences $\{C_1, C_2, \ldots\}$. In other words, T implies $\{C_1, C_2, \ldots\}$. To test a scientific theory, then, is to test a finite subset of that infinite set of implied consequences. The theory's statements are not directly tested. If a large number of the consequences have been tested by well-verified experiments and found to correspond to measurements within experimental errors, we can conclude only that the theory being indirectly tested hasn't failed. In fact, while "T implies C_n" is not logically equivalent to "C_n implies T," it is logically equivalent to "not-C_n implies not-T." Consequently, although establishing the truth of a theory is not possible, we can, as Riordan suggests with his "evolutionary metaphor," use experiments to discard theories that imply consequences inconsistent with measurements.

The issue of whether a theory is or can become a fact is different from the question of whether a specific deduction from a theory can be found to be factual. When Riordan wrote about the existence of quarks, he was referring to a deduction from a set of theories; the first few of those theories, because they lacked the concept of the color interaction, were not identical to today's quantum chromodynamics. The original theories were not fully correct. Yet they implied the existence of quarks, which seem to be as real as any other objects that we take seriously. Were the original theories that implied quarks simply subsets of the "factual" theory of QCD? That seems unlikely. The addition of the color interaction was a boldly creative step that produced a new

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theory; it wasn't just a case of simple addition. However, what matters to this argument is that the new theory of QCD can never be called a fact because it implies an infinite number of new consequences based on different initial conditions. Yet we still seem to have quarks.

Of course, the quarks of QCD are not precisely those of Murray Gell-Mann's and George Zweig's original theories. Nor are they precisely Richard Feynman's partons. The most complete understanding of quarks comes from the strongest theory we have (that is, the one that has best survived tests of its implied consequences) that includes quarks among its deductions. In that sense, the meaning of our concepts in science comes only from our theories, and as theories change, so do the meanings of concepts. For example, the meaning of mass in Newtonian physics is not identical to the meaning of mass in Einstein's special theory of relativity.

In the case of a theory that hasn't been tested or that has failed tests, the meanings given to its implied concepts are empty. That's the difference between science and mathematics or philosophy. In science as in no other field, physical experiments allow us to objectively weed out theories. Without tests against measurements, the most wonderful-looking theory is, at best, brilliant hallucination. In our mind's eye, we seem to see something, and the something even has meaning to us, but unless we have objective means of checking, we don't know if the something exists.

An odd and poorly understood feature of modern science is that its most central objects—general theories—cannot be known by scientific procedure to be true or factual. That feature is a strength rather than a weakness. Without general theories, we would be limited to advancing knowledge by making lists of facts. Such a linear approach is extremely slow compared to the methods of science in which a large but finite number of theories are discarded in order to keep a few that imply an infinite number of consequences. Many people have remarked on how strange it is that this works so well, but there is little doubt that it does.

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Mouroulis replies: Albert Einstein wrote, "The justification (truth content) of the system [physics] rests in the proof of usefulness of the resulting theorems."[1] Robert Reiland argues that only specific deductions from a theory, not the theory itself, can be tested to be true. I confess I fail to see the usefulness of that distinction. Newtonian mechanics or electromagnetic theory can be seen as false, as outside the categories of true and false, or as true within their sphere of applicability. I believe the third view has the most value.

Reiland says a theory that makes an infinite number of predictions can never be shown to be true through any necessarily finite number of tests. Does it therefore have a truth content of zero? Einstein's view implies that when a large number of positive tests have accumulated (and obviously in the absence of negative tests for a core prediction), the theory has achieved high truth content. My statement that the aim of every scientific theory should be to have its truth proven was only meant to imply that experimental verification should not be abandoned, especially not on any a priori philosophical grounds about the nature or quantity of attainable truth.

According to Reiland's other argument, a scientific theory connects concepts that derive meaning from it; hence the theory cannot be known as factual. Yet a look at sciences other than physics would reveal that this does not hold for all of "our concepts in science." To give one example, should geological theories be factual or should they merely connect concepts that derive their meaning from the theory? Do earthquakes derive their meaning from geological theories?

Although Reiland makes some fine distinctions about the role of theories, it is not clear that those distinctions extend beyond a specific set of physics theories. The difference between that set and theories in other areas of science probably has to do with the degree to which they are remote from everyday experience and deserves further exploration in a different forum. However, I still think it is useful to assign truth content to scientific theories; otherwise, one risks lapsing into metaphysics.

Reference

1. A. Einstein, *Out of My Later Years*, Carol Publishing Group, New York (1995), chap. 13.

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The Opinion piece by Michael Riordan (PHYSICS TODAY, August 2003, page 50) and its follow-up discussion in the December 2003 issue contained some strong statements about the nature of scientific knowledge. Many of those statements touch on the cornerstones of philosophy of science, and therefore merit a lengthy debate. However, the one I find the most interesting is that theorists who generate empirically indemonstrable models are doing metaphysics rather than regular physics. I think this view and its underlying assumptions are dangerous in that they may communicate an inaccurate image of science.

The overall view conveyed by Riordan's opinion and some of the letters is that scientific knowledge is somehow superior to other kinds of knowledge because it holds a deep connection to reality. The comments also convey the idea that only empirically corroborated theories are purely scientific because only through experiments can we discover scientific truths. The disregard of the importance of metaphysics in scientific discovery is disconcerting.

Every scientific enterprise is deeply rooted in a metaphysical context. That we subscribe to a particular "ism"—pragmatism, relativism, positivism, reductionism, and so forth—is in itself a clear choice of a philosophical framework. We cannot say, a priori, that a particular framework is better than the others, but we may, through pure rhetoric, persuade some of our peers that one is more appealing than the rest. In some way, choosing a specific philosophic approach is like choosing a religious belief: a personal and subjective decision.

Embracing the philosophical content of theories is not equivalent to relaxing our criteria of what is and what is not scientific. It is simply acknowledging that science has a great structural complexity shaped by many theoretical and discursive levels that go well beyond Galileo's "Book of Nature."

In my opinion, the interplay between science and metaphysics is crucial in the evolution of scientific theories. Often, the metaphysical nature of a theory is what dictates the path a research program will follow.[1] In Galileo's day, the choice between the Tychonic (anthropocentric) and Copernican (heliocentric) models of the universe was made on philosophical grounds. Mathematically, both models were identical, so no experiment could have distinguished one from the other.[2] Similarly, the choice between the deterministic and indeterministic versions of quantum mechanics was made not because of experiments but because the second is consistent with the philosophical environment that permeated the early 20th century.

Denying the role of the metaphysical backgrounds of many theories and models is not only a rough and inadequate approach to the study of the history of science; it is also a dangerous premise for the future, in the same way that Riordan's "Platonic physics" is considered a dangerous relaxation of scientific criteria. If science is independent of philosophy in that it unveils truths, then scientific facts are unquestionable. That view of science puts researchers in the position of priests of the oracle's temple and confers on them a special connection to the mysteries of nature. History has shown that such strict discourses are threatening because they entail the rejection of many alternative research programs. With the complex challenges facing our societies in the future, it is our obligation as scientists to explore the viability of alternatives. Adopting a reductionist posture on scientific discovery will not allow us to explore with calm the options that are on the table.

Riordan makes a compelling point about the nature of science and its connection with reality. I agree that too much metaphysics results in the untimely death of physics. However, too little produces a rigid body of tautologies and universal facts, which can in turn evolve into a dangerous discursive weapon for those with a lax scientific background. A clear example of this occurred in the history of economics. When Leon Walras, Carl Menger, and William Jevons developed the foundations of what is now called neoclassical economics, they used some ideas from physics—specifically mechanics—to justify the scientific nature of their approach.[3] Almost 150 years later, many economists (Pierre Bourdieu, to mention one) question both the validity of that connection and the argument that underlies it.[4] Balance is an essential matter. The question is whether we can reach such a balance; as I see it, we can. In my opinion, the problem is solved by distinguishing what is empirically proved from what is pure metaphysical speculation. One cannot say with total certainty whether today's metaphysics will become tomorrow's everyday physics. By clarifying the distinction and avoiding quantifiers like "more" or "less" scientific, we leave the doors open for future revolutionary progress.

References

1. T. Kuhn, *The Structure of Scientific Revolutions*, 3rd ed., U. of Chicago Press, Chicago (1996).
2. T. Kuhn, *La Revolucion Copernicana*, Ariel, Barcelona, Spain (1996).
3. P. Mirowski, *More Heat Than Light: Economics as Social Physics, Physics as Nature's Economics*, Cambridge U. Press, New York (1999).
4. P. Bourdieu, *Le Monde Diplomatique* (December 1998); available at http://mondediplo.com/1998/12/08bourdieu. Similar papers are available at http://www.paecon.net.

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Riordan replies: I appreciate Robert Reiland's critique of my Opinion piece and the opportunity it gives me to elaborate. I agree that what we call a general theory, such as quantum chromodynamics (QCD), can never be proved absolutely true in all possible cases. But after a long string of successful tests, a theory is generally considered true, and it will have to be incorporated in any theory that may supersede it. For example, the somewhat more limited statement that quarks exist will always be true in some context. It has become a "scientific fact."

Yes, the quarks we recognize today are substantially more than what Murray Gell-Mann predicted in 1964. Our physical idea of quarks has been extensively elaborated since then—especially by the emergence of QCD, as Reiland notes. Theories indeed play the central role in establishing the physical meaning of our concepts, and thus their meaning evolves. But I do not agree that they play the *only* role in this normal historical process.

Even when today's dominant standard model of particle physics is replaced by a wider, better theory in the near future, as many physicists expect, I am certain that quarks will continue to exist in some sense. Protons and neutrons did not cease to exist just because we discovered quarks in the 1960s and 1970s. But our physical picture of them changed dramatically.

Despite Juan Pablo Pardo-Guerra's objections, I will continue to assert that theorists who generate physical models that cannot be reasonably tested are engaging in metaphysics rather than physics. But I am not, as he claims, denigrating the philosophical underpinnings of science, for which I have a high regard. Where he uses "metaphysics," I prefer the word "philosophy," which for me has a slightly different, broader meaning.

As Gerald Holton and other historians of science have noted, our deep philosophical predispositions have long guided theory choice—even the selection of what physical problems are worth addressing. My own favorite scientific philosophy happens to be pragmatism. Although it may not be an a priori better choice than other philosophies, it is certainly a more effective philosophy in that worthy scientific theories are required to have observable consequences. They have to *do* things, not just be. Most practicing scientists share this philosophical prejudice, consciously or not.

In my Opinion piece, I protested what I see as the emerging divorce of theory from experiment: Mathematically adept theorists are increasingly publishing ideas that may have no observable consequences. And like any divorce, this one can originate on either side. Raw Baconian empiricism, devoid of theoretical interpretation, is also not science. Perhaps I was a bit too strident in championing experiments in my article; I seem to have been misinterpreted as being against theory. Modern physics, I admit, cannot exist without it.

What natural philosophers developed in the 17th century, the marriage of ideas and observations, is an immensely powerful intellectual process that has radically altered human activity and, with it, our physical landscape. It has sometimes been a difficult marriage, but it has been a very rewarding one, too—and one that physicists must continue to live with, for better or for worse.

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Carefully Chosen Words on Antievolutionism

The November 2003 decision of the Texas State Board of Education to reject demands made by antievolutionists is good news for science. However, the report of this decision in the December issue of PHYSICS TODAY (page 36) uses language that will offend some supporters of evolution and could be useful to the enemies of science.

The article refers to the textbook critics as "antievolutionist," "creationist," and "social conservative." The first two terms are pertinent, since those critics are challenging the correctness of evolution and promoting creationism or intelligent design as alternatives. The term "social conservative" is irrelevant and misleading.

A social conservative may be a supporter of evolution, an opponent of evolution, or a person who is unconcerned about evolution. The number of physicists and readers of PHYSICS TODAY who think of themselves as social conservatives is not negligible. Use of this characterization gives the appearance of criticizing supporters and potential supporters based on unrelated considerations. Thoughtless language is poor public relations.

The introduction of social and political terminology into the December story aids dangerous opponents of science. The following words, written by science historian Michael Riordan, appear on page 51 of the August 2003 issue of PHYSICS TODAY: "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." Darwinian evolution is included in the biology curriculum because it is the accepted scientific interpretation of biological facts. Injecting social and political considerations supports the claim of postmodernists that the conclusions of science are socially determined.

Science cannot avoid interaction and conflict with various forces in society. Riordan's statement calls attention to threats that confront science from two different sources—postmodernism and religious fundamentalism. In addressing these threats, I urge consideration of the following facts. First, postmodernism is a threat to both science and religion. Second, within the sphere of religion, only fundamentalism is a consistent opponent of science. Most people who believe in God accept science as true and regard as allegorical those elements of the Bible that conflict with science. Third, belief systems are not always based on religion—social conservatism is an example—and do not necessarily determine a person's view of science.

Science might have more support if its institutions and centers of power were more diplomatic. I offer two recommendations. First, avoid linking science concerns with unrelated social and political disagreements. Second, when communicating about public issues of concern to both science and religion, avoid extending the conflict with religious fundamentalism to include religion in general.

Stephen Hawking's immensely successful book *A Brief History of Time* (Bantam Books, 1988) provides an outstanding example of how to achieve that second objective. His text presents physics to a general audience in a manner that avoids showing disrespect for religion and demonstrates sensitivity to its concerns. Perhaps some eminent biologist will write *A Brief History of Life* in the same spirit.

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