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Who Ordered Theorists?

The science wars that continue in *Physics Today* seem to be only between theorists of various kinds discussing *gedanken* physics. They don't see the real physics as an experimental science that progresses from one experimental breakthrough to another. Theorists are often irrelevant and sometimes actually hinder progress by sitting on committees and opposing the experiments that lead to new breakthroughs. As we begin this new century, "Who needs theorists?" would be an interesting question to ask. How would physics have progressed in the second half of the 20th century--that is, since I received my PhD in 1950--if theorists had been ignored?

The main breakthroughs in physics since 1950 can be characterized as "who-ordered-that?" effects, named after I. I. Rabi's famous remark about the discovery and existence of the particle we now call the muon. The physics of the 1950s was one experimental discovery of a new who-ordered-that particle after another, with no theoretical prediction beforehand and no theoretical understanding afterward. Finally, by the 1960s, enough who-ordered-that particles had been discovered so that Murray Gell-Mann and Yuval Ne'eman could put them into a kind of Mendeleev table without any understanding of who ordered what. The Gell-Mann-Zweig quark model gave an answer that was strongly rejected by members of the theoretical establishment who were still fooling around with bootstraps, and moving and fixed poles, and other irrelevancies.

The next dramatic who-ordered-that experiment was *CP* violation, which is still being debated by theorists after 35 years. Meanwhile the theoretical establishment was again confounded by the discovery of scaling in electron scattering at the Stanford Linear Accelerator Center (SLAC). The nice after-the-fact explanation by a few young theorists, and evidence that the partons were quarks, were resisted by the theoretical establishment, as dramatically reported by David Gross at the 1992 SLAC History Conference.

The 1974 November Revolution began with the who-ordered-that discovery of the J/psi by experiments that theorists had insisted were completely useless and a waste of valuable beam time and budget. Of course, as soon as the discovery was confirmed, a chorus of theorists claimed that they had predicted the existence of this hidden charm particle. But none of them had suggested to Burt Richter and Sam Ting to do the experiments that actually found the J/psi. I was one of the theorists who told experimenters how to look for charm in ways that turned out to be useless. My suggestions that charm would most likely be found in electron-positron collisions and that the charm threshold would be seen as an enormous increase in strange particle production were correct, and were recorded in the review by Ben Lee, Mary K. Gaillard, and Jonathan L. Rosner of how to look for charm. But the effect was masked by the next who-ordered-that effect, the discovery of the tau lepton by Martin L. Perl, which cancelled my strange particle excess by a roughly equal number of nonstrange particles. Theorists had, of course, continually denounced Perl's search for a heavy lepton as nonsense. And Lee and all other theorists missed the narrow width of the J/psi that provided the striking signals observed at SLAC and Brookhaven. Their estimates of the width were off by one to two orders of magnitude.

The November Revolution would have occurred without theorists. It might even have occurred earlier if theorists had not been around at accelerator program committees.

The discovery of two kinds of neutrinos was also motivated not by theorists but by experimenters who noticed the possibility of creating a neutrino beam at an accelerator. Then they actually did it--without help from theorists--and found that there were two neutrinos.

By this time enough experimental data had been accumulated so that the theorists could begin to make sense of them. Now we have the Standard Model. But the Standard Model did not result from great theoretical or philosophical visions. It arose from a succession of who-ordered-that and other pioneering experiments that defied the theorists until there were enough data to enable an after-the-fact analysis that would lead the theorists in the right direction.

I have no patience with social scientists, historians, and philosophers who insist that the "scientific method" is doing experiments to check somebody's theory. The best physics I have known was done by experimenters who ignored theorists completely and used their own intuitions to explore new domains where no one had looked before. No theorists had told them where and how to look.

What guides their explorations? How do they choose where to look? How do they know when to persevere despite continuous failure to find anything new? How do they know when to drop an unproductive line and move on, rather than obstinately pursuing a dead end? These are the questions I should like to see social scientists, historians, and philosophers investigate.

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Theory, Phenomenology, and 'Who Ordered That?'

Harry J. Lipkin (Physics Today, July 2000, page 15) writes that, on the basis of the past 50 years, scientific progress did not primarily result from experiments designed to check theory. Looking back at the same period, I strongly disagree.

The most exciting results immediately following World War II were the precision atomic experiments verifying the renormalized quantum electrodynamics of Richard Feynman and Julian S. Schwinger. Enrico Fermi's theory of the weak interaction incorporating Wolfgang Pauli's neutrino hypothesis predicted the interactions of neutrinos. The famous experiment of Clyde Cowen and Frederick Reines in 1956 was designed exactly to verify this prediction.

Hints from kaon decays led Tsung-Dao Lee and Chen Ning Yang to propose that parity was violated in the weak interaction. This idea led directly to the experiment of C. S. Wu, which showed the asymmetry of the emitted electrons from the decay of a polarized nucleus. Immediately thereafter, the V - A theory was formulated by Feynman and Murray Gell-Mann, and Robert E. Marshak and E. C. G. Sudarshan; a whole series of experiments that followed verified this theory, particularly precision experiments on muon decay.

Although the V - A theory was successful, except for the mystery of charge conjugation-parity (*CP*) violation, it was theoretically unsatisfactory because of its divergence problem. Steven Weinberg and Abdus Salam were then led to propose the spontaneously broken gauge theory. To check this, an experimental search for the predicted neutral currents in neutrino reactions was carried out, which led to the provisional acceptance of the theory.

Proving that theory required detection of the W and Z bosons, which in turn required construction of the proton collider at CERN. The theory was precision-tested by electron-positron colliders built specifically for this purpose: the Large Electron Positron Collider (LEP) at CERN and the Stanford Linear Collider at SLAC.

As a result of many experiments, we now have a Standard Model that describes nearly all observed elementary particle phenomena in terms of a Hamiltonian that can be written on one line. Current experiments at B-meson factories are designed to test whether this theory also explains *CP* violation.

We do not have a theory of everything, although some of my colleagues dream of one. When new domains of energy are explored, we will not be surprised to discover that there are things in the heavens and on Earth that are not described by our present theory. Our goal, then, must be to find a more encompassing theory and design experiments to fully test it. That, I believe, is the scientific method.

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In his letter, Harry J. Lipkin says, essentially, that no fundamentally new theory in physics has emerged for the past 50 years. Many physicists will disagree.

A specific exception to Lipkin's premise can be found in the Yang-Mills theory of 1954 as an extension of Maxwell's equations. The theoretical Standard Model that Lipkin describes as hindsight is based on Yang-Mills particles (gluons; see Frank Wilczek, Physics Today, August 2000, page 22), in conjunction with symmetry breaking mechanisms. The past 50 years of particle physics might then be seen as an experimental search into the validity of Yang-Mills theory and its renormalization. Furthermore, Lipkin's examples of great accomplishments in experimental physics were all taken from particle physics. The debate hardly stops with particles.

Having described such experiments, Lipkin then confuses theory with serendipity. Everyone knows that serendipity ("who-ordered-that") is an unstated part of any exploration initiative that searches where no one has looked before. NASA addresses it, sometimes explicitly. However, it is rarely stated because taxpayers don't like to fund it.

Physics is a model or paradigm where theory and experiment must work together. It is a search for understanding. A prominent goal is completeness and consistency, which is where theory plays its role. Theory is also important because it defines what is "observable" and what is "unobservable." The observable is where experimentalists find fame and fortune. The unobservable includes such things as axioms, boundary conditions, postulates in relativity, and Hilbert space. Take the most important concept in wave mechanics, the wavefunction ψ. It is unobservable. Is Lipkin looking for that experimentally?

The unobservable part of physics, the part that experimentalists can never measure, is fundamental to completeness. In a sense, it is metaphysics. Without it and the theorists who define it, Lipkin's world would be incomplete and inconsistent.

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Harry J. Lipkin forgets or disparages the important role of theorists in some crucial experiments in modern particle physics. Unfortunately, such an ahistorical view by a well-known particle physicist feeds into the present misunderstanding of science in some segments of academia, and should not be left uncorrected.

Lipkin asks, "How would physics have progressed in the second half of the 20th century . . . if theorists had been ignored?" and gives as one of his "who-ordered-that" experiments the discovery of charge conjugation-parity (*CP*) violation in neutral K decays. But in his Nobel Prize acceptance speech, Val Fitch said, "It is difficult to give a better example of the mutually complementary roles of theory and experiment than in telling the story of the neutral K meson," which culminated in the discovery of *CP* violation.[1] In fact, one of the main aims of this experiment was to test the theoretical proposal of Lev Landau and others that *CP* is conserved in the weak interactions.

"The theoretical establishment was again confounded by the discovery of scaling," Lipkin continues. This is correct, but he fails to point out that this discovery was made possible because a theorist, James D. Bjorken, suggested plotting the inelastic electron scattering data using a scaling variable that he had introduced earlier on.[2]

Lipkin also claims that "the discovery of two kinds of neutrinos was also motivated not by theorists," but this is incorrect. The search for a second neutrino was motivated by a theoretical puzzle that was first pointed out by theorist Gerald Feinberg: The muon does not decay into an electron and a gamma ray as expected from a single neutrino hypothesis.[3] Subsequently, theorists predicted the existence of a third neutrino, the tau neutrino, which apparently has now been observed at Fermilab. Indeed, the only example given by Lipkin in which theoretical guidance did not play a direct role was the unexpected discovery of the J/psi. There are other such examples, notably Martin Perl's discovery of the tau lepton,[4] but their existence does not support Lipkin's broad generalization that "theorists are often irrelevant."

Seeking the answer to Lipkin's question, "What guides their [experimenters'] explorations?" one needs to look no further than the accounts given by the discoverers themselves, who invariably acknowledged the important contribution of theorists.[1-4]

References

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2. H. W. Kendall, in *Nobel Lectures*, World Scientific, Singapore (1992), p. 694.
3. M. Schwartz, in *Nobel Lectures*, World Scientific, Singapore (1992), p. 469.
4. M. Perl, in *The Rise of the Standard Model*, L. Hoddeson, L. Brown, M. Riordan, M. Dresden, eds., Cambridge U. Press, New York (1997) p. 79.

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The issue of Physics Today with Harry Lipkin's provocative letter, "Who

Ordered Theorists?" arrived by chance at the time I was reading Brian Greene's *The Elegant Universe: Superstrings, Hidden Dimensions, and the Quest for the Ultimate Theory* (Vintage Books, 2000) in which the author tells of the promise and excitement of string theory. Reading such dissimilar views about theory and experiment induces me to comment on an older theorist-experimentalist matter that Greene brings up in his survey of pre-string physics. He states: ". . . Maxwell's theory showed, quite unexpectedly, that electromagnetic disturbances travel at [the speed of light]" (p 24).

This statement about electromagnetic waves falling out of theory is exactly as it was presented to me nearly 50 years ago, and seems to me to be nearly universal, so there is no reason to criticize Greene for it. Yet the facts are just the opposite. Maxwell knew that his equations had to produce wave-like solutions because in 1856, W. Weber and F. Kohlrausch[1] had measured the ratio of electrostatic to electromagnetic units, a quantity known from dimensional analysis to be a velocity, and had found it equal to the velocity of light. In the experiment, a Leyden jar of known charge capacity had had its potential determined by an electrometer, thereby establishing its charge in electrostatic units; it was then discharged through a ballistic galvanometer calibrated in magnetic units.

Michael Faraday had shown a bit earlier that polarized light was affected by magnetism, furnishing a hint that light and magnetism were related, but this new result went far beyond a hint. Its significance was hardly lost on Maxwell, who wrote, "We can scarcely avoid the inference that light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena."[2] His manipulation of the equations that described the laws of Gauss, Faraday, and Ampere had a definite goal, one that forced the bold assumption he made.

Books on electricity for the last decades of the 19th century referred frequently to the Weber and Kohlrausch experiment, which was often reproduced as experimental techniques improved, but when electricity and magnetism began to be taught as derivative from Maxwell's equations, the significance of the experiment was lost and the implication grew that it was all the consequence of a desire for symmetry. The replacement of gaussian by SI units removed c from its rightful place, and the trip to the memory hole was complete.

References

1. See J. C. Maxwell, *A Treatise on Electricity and Magnetism*, (article 771). Oxford, England, Clarendon Press (1892).
2. C. W. F. Everitt, *James Clerk Maxwell: Physical and Natural Philosopher*, New York, Scribner's (1975) p. 99.

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Lipkin replies:

The Dirac equation marked the end of an era when the trail to new physics was blazed by theorists. It was followed by a new era during which trails were blazed by experimenters, with theorists trying to explain puzzling "who-ordered- that" results: beginning with the puzzling number 137, the anomalous magnetic moments of the proton and neutron, and the discovery that the muon did not behave like Hideki Yukawa's meson. Another era began many years later with the discovery of neutral currents, charm, and the rise of the Standard Model.

My letter referred to the period between the Dirac equation and the rise of the Standard Model. I therefore do not discuss other periods. However, I note that the conclusion that matter is not continuous but consists of atoms and molecules was settled once and for all because of the extraordinary agreement in the values of Avogadro's number obtained by many different experimental methods.[1] Scientific progress did not result from experiments designed to check theory.

P. A. M. Dirac's goal was to find a description of the electron consistent with both relativity and quantum mechanics. The unexpected spin-off was a remarkable combination of "who-ordered-that" theoretical consequences: the spin and magnetic moment of the electron, the existence of the positron, and all the correct descriptions of electron-positron annihilation and pair creation.

No theorist has since found anything comparable to the Dirac equation. Remarkable and even great theoretical achievements cited in this set of letters are simply not in the same league.

At Princeton University in 1946, I saw all the great theorists--you name them, they were there--completely at a loss about the infinities that plagued quantum electrodynamics (QED). Niels Bohr insisted that quantum mechanics applied only at the atomic scale. The new theory needed at the nuclear scale would be as different from QM as QM was different from Newtonian mechanics. David Bohm tried hard to find such a theory. But so far QM still holds far below the nuclear scale.

Then one great theorist, Willis Lamb, decided that new experimental input was needed, and measured the Lamb shift in an incredible tour de force. I remember the colloquium describing his plans and thinking that he was crazy. Nobody could make that complicated experiment work. But he did. The significance of this work was emphasized this year by President Clinton's award of one of 12 national medals to Lamb.

The most exciting result immediately following World War II was that the Lamb shift was indeed finite and measurable. Its completely unpredicted value started Hans Bethe, Richard Feynman, and others on the way to a new predictive formulation of QED.

Despite the great respect many theorists held for this new formulation, Feynman deprecated it as "bookkeeping," not physics. He regarded the conserved vector current as his major discovery in "real physics."

Tsung-Dao Lee and Chen Ning Yang deserve the highest praise for their proposal that parity was violated in the weak interaction and for pushing the experiment of C. S. Wu. But this is not "theory." This is phenomenology, analyzing the latest puzzling "who-ordered-that" data and pointing directions for further experiments. They had no theory. The initial "who-ordered-that" experimental parity violation in kaon decay that started the tau-theta puzzle was not explained by the V - A theory and was not understood by theorists until many years later, when it became clear that kaons and pions were not elementary bosons but were made of quarks.

Unfortunately, the great advances made by phenomenologists in pushing back the frontiers of knowledge have generally been undervalued. Another example of great phenomenology was the 1975 six-quark, six-lepton model of Haim Harari, who introduced and named the top and bottom quarks. The six-six model fit the data, explained all perplexing puzzles while nothing else did, and told experimenters what to look for next.

I began my physics career with an experiment, the first test of whether relativistic positrons obeyed the Dirac equation. But such tests did not yield clues to the new theory that Niels Bohr said would replace quantum mechanics. Now we are back at another level looking for clues to new physics beyond today's Standard Model. Collaboration between theory and experiment is certainly needed. But let us not forget the crucial role of phenomenology.

Reference

1. H. J. Lipkin, *Nature* **406**, 127 (2000).

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