Covering Condensed Matter Fundamentals

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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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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 do, in the brief section 9.4, describe localization as an interference between time-reversed paths. The absence of a diagram to illustrate the point led me to overlook their written description and to claim that they had not covered that aspect of electron localization.

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A Physicist in Industry: One Reader's Experience

Marc D. Levinson's letter (PHYSICS TODAY, June 2003, page 15) about the fate of physicists in industry is entirely too pessimistic. First, most physicists must expect to be employed in industry; academia and government do not have enough positions for physicists. What industry requires is the ability to solve problems, particularly when the solution requires making an invention. The gadget invented must also be delivered on time and within budget.

Once, I was presented with a problem outside my area of expertise; the device was essentially already designed, but I had to invent the method of construction. Admittedly, the device only lasted a couple of months, but long enough to prove the principle of the entire system. And it was delivered inexpensively and within a couple of weeks, soon enough that the project won the system contract for my company.

Another time, an overly imaginative engineer envisioned a fantastic new device and set a date for a news conference only three weeks hence. Since his configuration would have failed because of the second law of thermodynamics, I invented a modification, oversaw its construction, and delivered it in time for an operational demonstration at the news conference.

Invention—of ideas, gadgets, methods, even engineering computer programs—is the key to success. I must have made at least 60 inventions. Twenty-five made it into US patents; the others were obviously good because, unknown to me, other people had already thought of them.

All of this sounds like engineering, but a physicist brings a broader, cross-disciplinary knowledge for solving the more complicated problems.

Industry also expects physicists to develop their own budgets—by selling their expertise and that of their labs to both internal project managers and external organizations willing to subsidize applied research. Yes, an industrial physicist must be a salesperson, at least for his or her own product, whether it be a gadget, an analysis, or a consultation. Recently, most of my products have been specialized engineering computer programs that gave managers the answers they needed.

Furthermore, keeping up to date is important to maintaining one's value to the company. Twenty-five years after I got my doctorate, I started taking one graduate course each year for 11 years. By avid reading of the literature, I already knew half of the material, so I excelled. But 50% of the material was absolutely new to me! I did need retooling. So, when I was 70 years old, I had not only survived five 10% downsizings, but I received a raise and a promotion.

True, industry does not award tenure. Who needs it? Certainly not a physicist who is capable, inventive, productive, and self-confident. And yes, I enjoy reading "Dilbert," too.

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