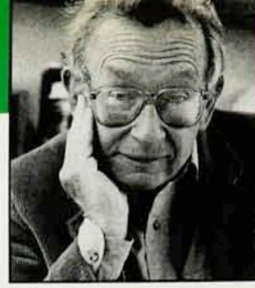

SOLID-STATE EXPERIMENTALISTS: THEORY SHOULD BE ON TAP, NOT ON TOP

Philip W. Anderson

The very distinguished head of the Kamerlingh Onnes Laboratory, Cornelis Gorter, a delightful and strong personality, was editor of *Physica* during the 1950s. As editor he instituted a firm rule against including theory and experiment in the same paper. This rule was old-fashioned even then, and seemed obstructive to those of us working at Bell Labs on the emerging science of solid-state physics, where such great experimental–theoretical teams as John Bardeen and Gerald Pearson, Bardeen and Walter Brattain, William Yager and Charles Kittel, and Conyers Herring and Theodore Geballe developed a sophisticated approach to solids that eventually achieved spectacular success.

A number of years later, I began to appreciate the sound logic behind Cor's apparently anti-intellectual stand. In fact, I have usually been happier with myself and with the outcome when I have published independently of—even if in tandem with—the experimental result. I have come to believe a theorist should appear as an author only if he has participated in the design of the experiment or done substantive, nontrivial analysis of the results. The obvious pitfalls of experimentalists and theorists sharing authorship are not the worst ones, though they are bad enough. The theorist may be riding piggyback, by which I mean that he is simply adding to his publication list on the basis of some relatively minor suggestion or encouragement, better described by an acknowledgment. A second problem is that the paper has two chances to fail if it contains theory, rather than one: The results may be right and the interpretations wrong.

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Both occur frequently—and obviously—enough to need no detailed descriptions. A little of both, for instance, occurred in the paper reporting the discovery of phonon structure in superconducting tunneling, where the trivial, but mistaken, explanation of a second-harmonic origin for a second bump at twice the frequency of the first was the only justification for the inclusion of a theorist in the authorship. (The second bump turned out, actually, to be caused by the longitudinal phonon spectrum, where the first bump was the transverse one, and the two were unrelated.) Later on, the full theory and experiment were published by J. Robert Schrieffer, Douglas Scalapino and John W. Wilkins (theory) and John Rowell, Anderson and D. Thomas (experiment), yours truly breaking his own rule because he had basically organized the collaboration—that is, designed a lot of the experiment.

Much more serious is the distortion of priorities, of communication and of the refereeing process that occurs when excessive weight is given to theoretical interpretation. We don't want to lose sight of the fundamental fact that the most important experimental results are precisely those that do *not* have a theoretical interpretation; the least important are often those that confirm theory to many significant figures. Two examples that have disturbed me, some 20 years apart, both are also drawn from the tunneling literature.

Around 1965 a young visiting experimentalist at Bell Labs observed that there was an interesting logarithmic anomaly of the tunnel conductance at zero voltage in certain junctions between normal metals. He was extremely reluctant to publish results on the effect because there was then no theoretical explanation, even though the experimental data could be fitted phenomenologically very neatly by simple logarithmic expressions. I remember that he withheld publication, at least for some time, until I assured him there was going to be such an explanation. It was an important, if not world-shaking, discovery of some interest when the theory finally appeared: local moment-assisted tunneling with an attached Kondo singularity. If such respect for theory had been the norm when the "Kondo effect" was first discovered (fortunately in a country where Cor Gorter's rule was observed), it might have delayed the publication (by van den Berg) of the original observations for some 30 years. I use the quotation marks because it seems a bit unfair that theoretical inability to understand what came to be called the "resistance minimum" deprived its true discoverer of recognition in the name, and that it is named for a theorist instead.

Even more serious problems seem to be besetting solid-state physics today, and especially the field of high-T_c superconductivity, of which I am an avid, if not impartial, observer. Here again there was no theory to go by for almost any fundamental measurement, not only tunneling. That is why high T_c is such an interesting problem. Yet often the experimental papers on high T_c list theoretical collaborators among their authors. My own instinct is to disregard many such papers, since the data may or may not have been influenced by theoretical prejudices, and in any case the abstract and the interpretation section often do not reflect the experimental observations, especially where these contain—as they often do—new and unusual features.

My second example comes from tunneling studies in both the normal and the superconducting state. One of the few consistent experimental observations about tunneling in high-T_c materials is that the tunnel conductance of the normal state over a wide voltage range has a characteristic "V" shape, roughly $\sigma(V) = A + B|V|$, which persists to low temperatures as a voltage-dependent background. One always sees this

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behavior more or less disguised by other "bumps" or "gaps" or artifacts that are often the ostensible point of the paper as submitted, and which vary radically from specimen to specimen, lab to lab and so on. Often, in fact, the experimentalist seems to go on until he sees some simulacrum of "BCS" behavior, which, he feels, has a "theoretical" justification, and then publishes immediately. On the other hand, it took two years after these tunneling curves began to be reported for there to appear in the published literature a quantitative experimental study of the much more consistent, puzzling, and therefore important "V" data on the normal state. I am told that unless such studies were backed by enormous prestige in the form of the right author and institution, most referees were in the habit of rejecting them. This example appears to me to reveal a major weakness in our approach as scientists: a collective unwillingness to welcome new or anomalous results.

Of course, in urging independence of theory, one must not relieve the experimentalist of the responsibility of understanding the theoretical constraints—such things as the essential symmetries the data should satisfy or obviously invariant branching ratios such as neutrons per fusion. Also, I am happy to see the relevant theoretical parameters presented. For example, nmr experimentalists *should* relate their data to Korringa theory, thermal conductivity *should* be compared to the Wiedemann–Franz law, and (what is surprisingly rare) the relevant Mott minimum metallic conductivity should serve as a standard of comparison for electrical conductivity: The reader should not have to calculate the Mott number. Sometimes it is even, regrettably, necessary to ask about minor experimental details such as whether contacts were interchanged à la H. H. Montgomery.

A number of factors, such as the funding crunch and the publication explosion, are working against originality and innovation in solid-state physics, and in science generally. The undue influence of theory is an unnecessary addition to this burden, since we are doing it to ourselves. The prejudice in favor of a pat "interpretation," no matter how anomalous the observed phenomena, is particularly stifling when, as in journal refereeing and grant reviewing, it is essential to get consensus: Originality and independence of mind are least to be found in a committee. Perhaps it is even time to return to Cor's rule and free experimentalists totally from their theoretical friends and colleagues. ■