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Some Thoughtful Words (not mine) on Research Strategy for Theorists

P.W. ANDERSON

Joseph Henry Laboratories of Physics

Jadwin Hall, Princeton University

Princeton, NJ 08544

I quote from one of the greatest theoretical physicists of the postwar era:

"The principal error I see in most current theoretical work is that of imagining that a theory is really a good model for ... nature rather than being merely a demonstration (of possibility)—a 'don't worry' theory. Theorists-almost always become too fond of their own ideas... It is difficult to believe that one's cherished theory, which really works rather nicely, may be completely false. The basic trouble is that many quite different theories can go some way to explaining the facts. If elegance and simplicity are... dangerous guides, what constraints can be used as a guide through the jungle of possible theories?... The only useful constraints are contained in the experimental evidence. Even this information is not without its hazards, since experiment 'facts' are often misleading or even plain wrong. It is thus not sufficient to have rough acquaintance with the evidence, but rather a deep and critical knowledge of many different types, since one never knows knows what type of fact is likely to give the game away...

Theorists... should realize that it is extremely unlikely that they will produce a useful theory just by having a bright idea distantly related to what they imagine to be the facts. Even more unlikely is that they will produce a good theory at their first attempt... they have to produce theory after theory... The very process of abandoning theories gives them a degree of critical detachment which is almost essential."

The missing words indicated by dots would give the game away, that this is Sir Frances Crick talking about theory in biology, at the conclusion of his autobiography, "What Mad Pursuit". He, in fact, distinguishes biological theory from physical theory on the basis that the mechanisms arise from the complex process of evolution. But in the absence of definitive advice on this matter from such other successful theorists as Crick's contemporaries, Richard Feynman and Murray Gell-Mann, it seems to me that one should, perhaps, take him more seriously as a guide to how theory is actually done than he may himself do. After all, in physical theory, we now know that whether or not the original cosmic egg was as scrambled as some astrophysicists such as Linde seem to think it was, almost all the phenomena we study, both in condensed matter and in particle theory, are the result of emergent processes and broken symmetries nearly as complex and evolutionary as biology.

My own experience has certainly been that most successful theories are the result of successive corrections to errors that may verge on the ludicrous, corrections normally dictated by a careful look at experiment. The long and tortuous tale I have told elsewhere of spin glass is one example; another is localization—who could have guessed, even in 1978 after certain prizes had been given out, that potential scattering, spin-orbit scattering, and magnetic scattering would turn out to give qualitatively different localization phenomena? Localization, in the presence of a magnetic field, seemed simple at first—until the experimentalists showed us that it led to the utterly unlikely phenomenon of Hall resistance quantization, leaving us theorists scurrying to catch up. In another example familiar to me, at least, the right A phase of superfluid helium three was predicted by solving the wrong Hamiltonian in the wrong way. Yet that is, too, a delightful example of Crick's "demonstration" theory: that paper demonstrated that phases of different symmetry were possible, which, in the end, turned out to be the really useful and important conceptual result.

Young theorists in my field, especially, would do well also to take Crick's words about experiment to heart. They often seem to believe that there is some kind of "Miranda rule" about what kind of evidence is admissible. Theorists discuss theory either in an experimental vacuum, or in relation to experiments endorsed by some previous paper or produced by the most fashionable experimental methods, rather than searching out the anomalies which are the real guide to the truth.

As I see it, even the "standard model" of particle theory—like it or not—was arrived at by the same kind of random walk guided at every stage by experiment, and many of its features still seem to have been as unpredictable on the basis of general principles of elegance or simplicity as the convolutions of biological evolution.

In conclusion, it appears that in all its branches physics is still an experimental science. Its basic goal is not mathematical elegance or the achievement of tenure, but learning the truth about the world around us, and Sir Francis Crick's words are as good a guide to that end as I have seen.