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A CAREER IN THEORETICAL PHYSICS

P W Anderson

*Department of Physics
Princeton University*

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INTRODUCTORY ESSAY

It was in the winter of 1966–7 that I first tried to express my philosophy of what was important in science — or at least, of what was important that was possible for me. I had been invited to spend a month in the pleasant climate of La Jolla as the Regents' lecturer at the UCSD, visiting a department staffed by many of my old friends from Bell Labs, such as Bernd Matthias, Harry Suhl, and George Feher, who had been recruited by Walter Kohn, who was also by courtesy a Bell alumnus. One of the lectures I gave was called "More is Different". The original version, now lost, went over with the audience like a lead balloon, at least according to that portion of the audience most closely related to me. But a cleaned up version, which was published in *Science* in 1971, has been surprisingly influential. I have chosen to reprint it, out of chronological order, as an additional introduction to this collection.

I already had this overall vision, if only very vaguely, as I completed my thesis and other early work on the breadths of spectral lines (see paper 2 in the book). This work was among the first to approach what was normally thought of as a dissipative process and dealt with by Boltzmann equations for the microscopic variables, in terms of the macroscopic moments of the system as a whole in my own mind called it the "one big molecule" method. I was beginning to see that somehow, starting from the fundamental atomic-level laws of quantum theory, and without any more or less mystical appeals to the difference between the atomic domain and the macroscopic domain where deterministic Newtonian physics held way, one could really understand many of the properties of matter in that macroscopic domain. I also experienced two other insights: that these properties were often extraordinary; and that this understanding was often intellectually challenging and deep, at a level which was for me at least more exciting and worthwhile than any other purely intellectual exercise.

Thus I was launched into the search for the "Aufbauprinzip" of the everyday world. I admired Fröhlich's book on dielectric theory because it took the same point of view; I encountered it in my early work on ferroelectrics (papers 1 and 3). Much more important, I came to see the crucial importance of the concept of broken symmetry, which has been a lifelong interest (papers 6, 7, 9, 10, 11, 15, 22, 26, 32, 39). Broken symmetry is the clearest instance of the process of emergence which lies behind "More is different". My interest began with the antiferromagnetic ground state work in 1952–3, from which I developed the concept of the Goldstone boson. In applying similar thinking to superconductivity, the Higgs phenomenon and the concept of Generalized Rigidity arose, the latter referring to the similarities between superfluidity, superconductivity, and elastic rigidity of all sorts. The topological theory of defects and dissipation, of which papers (7), (11) and

(27) are examples, is another facet of the conceptual structure of broken symmetry. Other examples were omitted for reasons of space.

The first natural direction in which to seek for understanding of the real world is the explicit quantitative calculation of properties of real materials. This enterprise is particularly rewarding when the basic principles which govern these properties are new, or when the effects are subtle. This has been the mainspring of much of my work, from the early work on magnetic resonance and other spectra, to superexchange (paper 8), the microscopic theory of superconductivity (papers 4, 5, 9, 14, 18, 30, 33), and even work on the electronic structure of solids such as paper 21, foreshadowing the corrections to local density theory, and paper 29 where the idea is to develop a method which explains and exploits the strong locality of the chemical bond, perhaps the most fundamental observation in the field of chemistry, which nonetheless seems to interest quantum chemists not at all.

In 1956, stimulated by a pioneering paper on percolation by Hammersley and by some puzzling experiments, I started to look at the next level of complexity, those phenomena which are intrinsic to disorder. The first result was the theory of localisation of waves (papers 25, 28, 16). This was slow of acceptance, and, perhaps discouraged by this fact, it was not until 1969 that I returned to it and also began to look at other disorder phenomena such as glass and spin glass (papers 23, 24, 34). Spin glass stimulated a very fertile period of worldwide activity, first in the development of totally novel methodology, such as a new statistical mechanism of nonergodic systems, and then in applications of the new methodology to a "cornucopia" of new ideas and concepts and problems: neural networks, computer algorithms, rugged evolutionary landscapes, even immunology. This has left me involved in such fields as economics and the origin of life. The interest in complex, emergent phenomena made it natural for me to participate at an early stage in the Santa Fe Institute, founded on the twin principles of interdisciplinarity and of the importance and tractability of genuinely complex systems. There has not been much space for these ideas; only papers 24, 31, 34 and 39 touch on them.

Left behind, in my old field of quantum condensed matter physics, was a worry about the validity and completeness of the theories of many-body quantum systems which had occupied that world in the 1950's and 60's. My work on the Anderson model of magnetic impurities (paper 12) had led on to the "infrared catastrophe" in Fermi systems which was the key to my work on the Kondo phenomenon (papers 13, 20) which solved that isolated many-body system but only at the cost of going beyond the conventional perturbation theory. I was also disturbed by a class of strongly interacting superconducting materials (30), as well as our notable lack of success in dealing with the field of quantum valence fluctuations, i.e. "heavy electrons" (35, 36, 37). With the appearance of the high T_c cuprates in 1987, my apprehensions became manifestly justified, and I began to formulate theories of metals based on new principles (papers 40, 41, 42, 43). This is the area which now most absorbs me, and where my efforts have been rewarded with the discovery of

several new and fascinating phenomena. I reach my 70th year still involved in two major, very different types of problem. Although there is no easy way to round this one off, I have tried to give a flavor of the ideas involved.

My original vision seems to me to have been sound. The problem of understanding the “here and now” world seems to show no sign of being exhausted. The contribution of physics is the method of dealing correctly both with the substrate from which emergence takes place, and with the emergent phenomenon itself. Examples are legion: for one, superconductivity cannot be properly understood simply as a phenomenology without understanding electrons and their interactions, nor on the other hand as a property of a small number of electrons without taking into account the macroscopic system. Ever newer insights into the nature of the world around us will continually arise from this style of doing science. There will be physics even in a world without the SSC.

Regarding the choice of articles in this book: the sheer volume of either a complete selection of research articles or, even more, a complete set of reviews would have been much too great. I have given preference to materials which are for one reason or another not easy of access: lecture notes and out of print sources, for example. I have tried but failed to provide a selection of all major topics; the book is long enough. I have included a few items on history as I saw it, and on general aspects of science.

Oxford, December 2, 1993

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