statements in Freud's writings, however, are empirical generalizations for which the evidence can be clearly defined; but other generalizations are hard to evaluate. Sherwood gives as example of the latter the statement that the longer an obsession lasts, the more the obsessional acts approximate to "infantile sexual acts of a masturbatory character." The problem is, when is an act other than masturbation to be considered of "masturbatory character"? This blurring of the line between observation and interpretation is a pervasive flaw in psychoanalytic writing today. The import of Sherwood's discussion would appear to be that this defect is unnecessary.

A book sufficiently similar in its topic to invite comparison is Leon Levy's *Psychological Interpretation* (1963). Levy takes his own variant of the hypothetico-deductive method as the model for all scientific reasoning. Levy refers to psychoanalytic theory vaguely and at times grossly inaccurately; Sherwood refers to Freud's writings with meticulous exactness. Anyone who reads both books will agree that Sherwood has written a more scholarly and more closely reasoned book. On one point, however—the difficulty of applying the criterion of consistency to a psychoanalytic explanation—Levy has a stronger case.

In evaluating the adequacy of explanations, Sherwood suggests as criteria self-consistency, coherence, and comprehensiveness. He recognizes and discusses the difficulties in applying the criterion of self-consistency. The existence of opposite motives or trends in a person is not evidence for inconsistency, since this is a patent feature of human nature; here he might have stressed more strongly that psychoanalysis postulates inner conflict as the core of every neurosis. The example he gives as evidence of inconsistency, something along the line of the existence of both a positive and a negative Oedipus complex in a single case, is the sort of thing that a psychoanalyst would say is the general rule rather than an exception. There are few diseases, neurotic or otherwise, that protect one against other diseases. Sherwood has not helped us to apply the criterion of consistency to psychoanalytic explanations; indeed, there may be no help. Perhaps the other criteria, coherence and comprehensiveness, suffice.

If psychoanalysis bypasses the distinction between causes and reasons, ego psychology does not, and therein lies the riddle of a psychoanalytic ego psychology. Again Sherwood offers no help. One can see how he missed the difficulty, since he took as point of departure psychoanalysis as of 1909, when the problem still lay more than a decade ahead for Freud. Paul Ricoeur's *De l'Interpretation: Essai sur Freud* (1965), a book hardly known among American psychoanalysts, contributes profoundly to this topic. Ricoeur develops his argument beginning from the word "decoding," a term that Sherwood tosses aside in one sentence as a mere synonym for interpretation. Ricoeur concludes that to understand the person one needs both an archeology and a teleology, that is, in Sherwood's terms, one must understand both causes and reasons, and that this dialectic can be found in Freud's later writings.

In the United States psychoanalysis has, as Freud feared, become a medical specialty, bloodless surgery, rather than a psychological science. It has sequestered itself in its own institutes apart from other academic disciplines and other therapeutic ideologies. The ecumenical spirit does not prevail there. If contemporary analysts admired Mill as much as Freud did, they would understand that the quickest way to kill an idea is to isolate it from all challenge and all competition. The competence, vitality, and interest of such books as those of Sherwood and Ricoeur point to a potential rejuvenation of psychoanalysis as theory if some way can be found to open the door to philosophers, psychiatrists, psychologists, and others on the basis of competence rather than of membership in the guild and certification of orthodoxy.

> Now if thou wouldst, when all
> have given him over,
> From death to life thou might'st
> him yet recover.

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An Unparalleled Success

Think. A Biography of the Watsons and IBM. WILLIAM RODGERS. Stein and Day, New York, 1969. 320 pp. + plates. $7.95.

Rodgers has written an unauthorized and officially disapproved account of the Watsons and IBM, and it bears the marks of its independence: reasonably malicious and poorly informed. On the whole one prefers this tolerably conscientious version to the usual authorized biography, which presents a nauseatingly bland description of a shiny, lifeless knight suitable for immediate presentation at Madame Tussaud's waxworks. Whatever the merits of either type of biography as light reading, and I hold them to be negligible, they both succeed in avoiding the central question of business success.

Thomas J. Watson, the First, was a superb salesman who served a demanding apprenticeship at National Cash Register under another remarkable entrepreneur, John H. Patterson. Discharged by this irascible man shortly after both were sentenced to jail (a sentence later dismissed) for antitrust law violations, Watson joined the Computer-Tabulating-Recording Company in 1914. One of its products was the Hollerith tabulating machine. Three years later the company's name was changed to International Business Machines. Sales were about $4 million Watson's first year, a figure now equaled five times each day of the year.

The utterly remarkable thing about Watson's next 40 years and IBM's next 55 years was that a position of dominance was achieved and maintained in an area of unceasing, and at times wildly revolutionary, changes in technology and product. Surely no comparable achievement can be found in industrial history. Henry Ford's economic triumph was immensely larger in the first 20 years of his company's life, but thereafter his enterprise faltered to a dismal halt—in an industry in which basic technology was and continues to be remarkably smooth in its evolution, and hence much easier to cope with. The success of IBM, to repeat, is without parallel.

How did Watson, and later his sons, maintain the IBM leadership? Decisions of critical importance had to be made frequently, with very incomplete information on costs, performance, and customer acceptance of new products. A number of powerful firms, such as Honeywell, National Cash Register, General Electric, and RCA, entered the computer industry. Sperry Rand was for a time the technological leader. Confronted with an erratic flow of opportunities, opportunities to make ruinous error as well as ever-rising profits, how did the Watsons mostly guess right? Rodgers does not help us to understand this unprecedented performance. We are told of the accidental meeting of Watson with Benjamin D.

Wood, and how this far-visioned man helped to expand the horizons of Watson's thought and plans. Yet Watson must also have met innumerable fools, and his rivals have also met far-visioned men. Rodgers probably lacked the information, as an outsider, to distill the talents and policies which created this empire, but there is no evidence that he recognized the magnitude or nature of the puzzle to be solved. Not elementary, Watson.

Instead we get a potpourri of anecdotal biography, portraying a trigger-tempered, vain, paternalistic man in some of his business, philanthropic, and political activities. This is not rich fare: businessmen lead lives almost as placid as professors', devoid (for professors until recently!) of danger, immensely repetitive from year to year, remarkably empty of amorous exploits or titillating fraud. We might have profited if Rodgers had also looked more closely at Watson, the Chairman of the Board of Trustees of Columbia University. My impression is that Columbia would have been better served by someone interested in higher education even if he needed to be advanced his subway fare to attend trustee meetings. We might have profited too if Rodgers had sought to measure the impact of the government's policies (including antitrust policies) on IBM; on the whole I conjecture that they were highly beneficial. In short, we would have profited if Rodgers could have obeyed that absurd Watsonian admonition that forms the title of this book.

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Nuclear Physics

Third Symposium on the Structure of Low-Medium Mass Nuclei. Lawrence, Kans., 1968. J. P. DAVIDSON, Ed. University Press of Kansas, Lawrence, 1968. viii + 296 pp., illus. $12.50.

This symposium was the third in a series that began in 1964. The smallness of these meetings (about 50 people) allows an informality and a depth of discussion not possible in a larger gathering. The success of the venture can be judged by the call, for the first time, for formal publication of the proceedings. One can only hope that the publicity will not expand future gatherings to a size that makes them ineffective.

The volume consists of the text of all the papers and the discussions. Most of the manuscripts appear as presented by their authors, and no attempt has been made toward uniformity in style. Such a decision by the editor does not detract from the usefulness of the book and is justified in view of the rapid publication.

Over half of the 13 papers presented at the symposium are deep surveys of experimental data on selected nuclei in the ($2s$, $1d$)-shell and the problem involved in getting "simple" interpretations. Two of the papers present subject matter new to the series in that they concentrate on hardware—on the dynamitron accelerators (M. R. Cleland) and heavy ion accelerators (P. H. Rose and W. E. Stark).

Three experimental papers are of note. J. A. Becker gives a review of the use of triton beams on nuclei in the ($2s$, $1d$)-shell with a host of new spin assignments and mixing ratios. A. E. Litherland makes a detailed comparison of properties of the mirror nuclei ^{25}Mg and ^{25}Al—a pair which are unique in being both well studied and exhibiting rotational bands. It is pointed out that one can not only infer from information derived from one nucleus information concerning its mirror, when the information may be difficult to extract experimentally in the mirror, but also, in principle, test to a greater accuracy the nuclear wave functions. P. M. Endt gives what is essentially a continuation of his talk two years previously on the gamma decay of analogue states. It now seems that the earlier interpretation involving the antianalogue states was too naive, and, in the details of fitting, it has been found necessary to retreat to the unsatisfactory solution of invoking different criteria for each nucleus studied.

Those who study low- and medium-mass nuclei are at present making one of their periodic critical reviews of their subject. The physical interpretation of the shell model is now being probed more deeply and the consistent derivation of every operator (interaction, electromagnetic, and others) that should be used is being questioned. Thus it is no longer thought satisfactory simply to parameterize the nuclear residual interaction, and efforts are being made to derive this from the free nucleon-nucleon interaction. In the symposium the direct derivation of the "bare" interaction from the known phase shifts is discussed in talks by J. P. Elliott and D. S. Koltun; it is of interest to see their different approaches side by side. Perhaps it is unfortunate that no talk was scheduled on the G-matrix evaluation from the Brueckner theory—the conflicting approaches would have made interesting reading. The use of the interaction as derived from the G-matrix in the shell model calculations, however, is discussed in a comprehensive paper by Edith Halbert. The problem of further renormalization of the residual interaction arises here again after the need is seen for further truncation of the shell model basis when the full calculation within a shell becomes intractable. The solution so far has been to return to the phenomenological parameterization of the interaction; although the question of the meaning of the phenomenological calculations was raised, no answers were given, in the presentation of the results, for such a hypothesis. One can look forward (possibly at the fourth symposium?) to the setting up of criteria by which one can judge the physical meaning of simple models in highly truncated spaces with phenomenological operators.

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Solution Chemistry

Ion Exchange and Solvent Extraction of Metal Complexes. Y. MARCUS and A. S. KERTES. Interscience (Wiley), New York, 1969. xii + 1044 pp., illus. $44.95.

The authors write that their aim was to prepare a monograph that would be useful to workers in solution chemistry, coordination chemistry, and the analytical and industrial aspects of separation chemistry. Their book, the product of much painstaking labor, fulfills the aim.

The authors treat the theory of electrolytes first, then ion exchange, and finally solvent extraction. They have appended an up-to-date and useful summary of distribution data for the extraction of almost all known metal complexes. A detailed treatment of the general theory of ion-exchange and solvent extraction is included in the chapters on these subjects. Many of the distribution data summarized are critically analyzed. Similar care has been taken in the pre-