ume is an account of a whole life, and there are other parts of the story. Earlier chapters tell of Weaver's boyhood; of his student days at the University of Wisconsin and his conversion from engineering to mathematics; of his appointment to teach mathematics at Throop College (then soon to become the California Institute of Technology—an institution he was allowed to leave but from which he was not permitted to resign; Robert Millikan's reply to his letter of resignation concluded, "You will continue to be a professor of the California Institute of Technology, on leave until you return"); and of his return to Wisconsin and his collaboration with Max Mason on *The Electromagnetic Field.* Later chapters tell of his work at the Sloan Foundation and other "retirement" activities.

It is all written by a man who has for long successfully practiced the art of writing. Any autobiography ought to let the reader know what kind of man the author is. This one does. It is neither overly introspective nor does it hide the personality, the values, or the manner of thinking that lie behind the actions. The three final chapters are not personal chronology at all, but essays that express some of Weaver's ideas and values: "Science then and now," "Some limitations of science," and "Science, contradiction, and religion."

Weaver is one of a number of men who changed, in midcareer, from the scholarly life for which they had prepared to an administrative role which they had not earlier anticipated. There are not yet autobiographies of many such scientists, and Weaver's is therefore of value not solely as the story of an unusually effective man but also because of what he can tell us of such midcareer changes and of the life of a science administrator. Why should a successful professor of mathematics turn into an impresario of experimental biology? He explains:

> I think . . . that I was both realistic and accurate about my abilities and my limitations. I loved to teach, and knew that I had been successful at it. I had a good capacity for assimilating information, something of a knack for organizing, an ability to work with people, a zest for exposition, an enthusiasm that helped to advance my ideas. But I lacked that strange and wonderful creative spark that makes a good researcher.

The positive part of this statement has often been verified. Toward the end of the Rockefeller period and later he was a trustee of the Alfred P. Sloan Foundation and a member of the Sloan Foundation staff, a member of the National Science Board, a director and president of AAAS, a member of research advisory groups for a variety of private and governmental agencies—all positions that capitalized on the abilities he recognized in himself in deciding to forsake his Wisconsin professorship to become a philanthropoid.

As for the negative part of his explanation ("I lacked that strange and wonderful creative spark that makes a good researcher"), it takes a mathematician to appraise mathematical creativity, and I am no mathematician; but I do not think he should be accused of false modesty. Some eminently successful research scientists have gone on to become highly successful administrators, but research originality of the highest caliber is not essential for the effective administration of scientific affairs. The ability to formulate research questions whose answers will open new vistas is not necessarily the same kind of imagination and originality that allowed Weaver in 1931 to bet on experimental biology and in 1941 to bet on experimental agriculture.

Men of high research competence receive research grants and professorships; they are elected to the National Academy of Sciences; if they are good enough they receive Nobel Prizes. But how should we reward the men who make it possible for other men to win Nobel Prizes? Weaver has received a goodly number of honorary degrees and similar recognitions. France, Great Britain, and the United States have decorated him. In 1957 he was awarded the National Academy of Sciences' Public Welfare Medal. In 1969 he was elected to the National Academy of Sciences. This election illustrates one of the problems of giving due recognition to nonresearch contributions. In terms of quality, and in terms of the significance of the research Weaver's assistance helped other scientists to perform, his election to the most prestigious national group of scientists was merited decades ago. But his own work was not of the kind that is normally honored by election to the Academy; electing him after he had twice retired was both an honor and an anomaly.

Perhaps the esteem of one's fellows is the best recognition of all. That he has had in abundance. When he was given the first Arches of Science Award for his outstanding contributions to the improvement of the public understanding of science (an aspect of his work scarcely mentioned in his autobiography), Rockefeller-assisted Nobel laureates from both hemispheres and from both sides of the equator cabled their congratulations.

There is another kind of reward—the satisfaction of enjoying one's work and knowing it has been good. This reward Weaver has had:

> When one spends years on a job involving a mass of almost daily detail and a multitude of projects, he is fortunate if, looking back, he has the satisfaction of having been associated with one or two activities that have had sizable, successful, and permanent impact. . . . I have that sort of satisfaction.

Scene of Change is an illuminating and graceful introduction to a man who has earned that satisfaction through four decades of remarkably effective work in advancing science and its useful applications and in promoting better general understanding of both the technical and the humane aspects of the scientific enterprise. As scientific activities become larger, more highly organized, and more intimately related to the other affairs of mankind, we need more men of his kind.

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Very Early Influences

Prenatal Determinants of Behaviour. J. M. JOFFE. Pergamon, New York, 1969. xii + 368 pp., illus. $13. International Series of Monographs in Experimental Psychology, vol. 7.

This volume is a thesis expanded to the proportions of a book. As a thesis, it is a first-rate job. For a book, however, the thesis format leaves much to be desired. The first half of the volume consists of a review of the literature on prenatal influences (stress) on the subsequent behavior of animals. This leads to a detailed account of the author's thesis experiment and is followed by a three-chapter addendum about human studies, which is interesting but of limited relevance to the preceding sections. The author argues that substantive findings must be predicated on reliable methodologies, and his focus is on methodological points rather than substantive results, issues, or concepts.

The coverage and organization of the literature are excellent, and this

collation of studies will be of considerable help to researchers in this field as well as to investigators in other areas of biological research who may be unfamiliar with the potential influence of prenatal and early experiential variables. According to the editor of the international series of monographs in which the book is included, the technical nature of the subject does not make for easy reading. It is, in fact, a difficult book to read—unnecessarily so, because many of the methodological details provided are irrelevant to the author's critical evaluation.

Although complete and critical, the review of the literature is not an especially constructive one. It will be left to the reader, for example, to distinguish between empirically founded criticisms and the author's unsubstantiated conjectures about sources of variability which tend to cast suspicion on what may be perfectly valid data. The author frequently adopts a negative rather than a positive phraseology, and he uses his own substantive interests in formulating criteria against which to evaluate the methodologies of other investigators. A more serious fallacy is his use of data from a study in which variables have been confounded (that is, one from which no definitive conclusions can be drawn) in order to criticize the procedures used by some other investigator.

Joffe does make a number of cogent remarks concerning the procedural conditions necessary in order to attribute behavioral effects to events occurring during fetal life. The uncritical acceptance of all his arguments, however, would foster a uniformity in design and orientation which, in this writer's view, would stifle creative research in this field. By being more selective and less exhaustive the author could have accomplished his aims in a more succinct, highly organized, and tightly reasoned review article. This would have been far more difficult for the author but far more rewarding for the reader. Although the serious student will find it helpful and convenient to begin with a careful reading of this book, he will find it essential to evaluate the literature in terms of the original authors' intentions and his own research interests.

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Interpersonal Processes

Interaction Concepts of Personality. ROBERT C. CARSON. Aldine, Chicago, 1969. xiv + 306 pp., illus. $7.50. Perspectives in Personality.

Among the little-understood aspects of human behavior perhaps none is so fascinating and complex as the most common of all: the way we relate to, influence, and are influenced by our next-door neighbor, our boss, our wife. The deceptiveness of our feeling of familiarity with these everyday situations is revealed when communication fails and we are at a loss for ways to reestablish it. And where pathological behavior is involved, our ignorance of interaction effects is dramatically exposed. This book constitutes a good stride forward in the study of this formidable problem.

Too often normal and pathological behavior are studied separately, as if it were possible to describe deviance without specifying what normality is. Carson, however, first analyzes normal interpersonal relations and then proceeds to apply the results to abnormal behavior. The picture of social interaction he obtains stems from the successful integration of two lines of research: structural analysis of interpersonal behavior and exchange theory. The first deals with the essential elements of our behavior toward another person. Exchange theory relates what A does toward B to what B does toward A.

In classifying interpersonal behavior, language is not particularly helpful. In the English language there are thousands of interpersonal verbs; and even so, as Carson notes, certain kinds of behavior are underrepresented. Yet an impressive array of evidence converges on the conclusion that, behind the bewildering variety of interpersonal behavior, there is a simple order. Through a progression of findings from factor analysis to order and facet analysis, we are shown that a good deal of interpersonal behavior consists of giving and taking away two basic resources: love (affect, warmth) and status (prestige, esteem). Each type of interpersonal behavior is not only a communication regarding a particular resource, but also a bid for a specific class of response: dominance invites submission; hostility invites hostility. Interaction balance is achieved when proaction and reaction are alike in love but different in status. This formulation of interpersonal balance provides a natural bridge to exchange theory, and from that to the relation between unbalanced states and psychopathology.

The interaction outcome can be represented by a payoff matrix showing the gains and losses of each participant for every possible pair of behavior types in proaction and reaction. Ideally such a matrix would tend toward symmetry. Some conditions modifying this tendency are discussed by the author. One is the generalized interpersonal style of each actor, that is, his preference for certain types of behavior regardless of the particular dyadic situation, a preference probably rooted in early childhood experiences. A second modifying factor is the relative power of one actor over the other. A third consists of the social norms regulating role relationships. These factors constitute constraints on the possible outcomes while affording considerable opportunity for maneuvers to increase one's payoff.

Mutual adjustment of payoffs rests on the assumption that each partner is likely to modify his goals in the direction demanded by the other. It may, however, happen that one tricks the other to move in a certain direction and then reveals a sudden change in his aims. A classical case is constituted by the lady who entices a suitor to propose intimate relations and then expresses indignation at the proposal. These games are entertaining to read about, as the popularity of Berne's book describing several of them attests. They are, of course, not nearly so amusing for the victim. Carson analyzes several of these situations showing how they deviate from less interesting but more satisfying interpersonal relations. As the author points out, the "victim" of the maladjusted behavior may have his own difficulties in coping with a change toward normality: the husband of a frigid wife, for example, became impotent when she was cured. The analysis of these borderline cases leads to a discussion of personality disorders. For Carson they are basically problems in social behavior, often due to the inability of the individual to differentiate among situations and roles and to choose the behavior appropriate to each one of them. By reducing the range of his behavioral responses the disturbed individual is able to avoid the ones which are incongruent with his self-image. Anxiety, in turn, arises from the perception of incongruence between