though most of the phases are further subdivided into several "stratigraphic zones," each zone may comprise a number of occupation levels or floors, as the published profiles clearly indicate. Unfortunately in this publication the materials are presented not by successive floors but by phase groupings or by stratigraphic zones which do not necessarily correspond to floors or occupation surfaces. There is thus no way to check the precise vertical distributions or associations of materials. Similarly, it is nearly impossible to find out the horizontal distributions and contexts of most of the artifacts described or illustrated. Indeed, there are times when even the gross descriptions of materials by subdivisions of phases is not consistently followed even when it might have given a certain amount of information. Thus I wanted to know for my own purposes more about the distribution of obsidian in the lowest or Bus Mordeh phase. In the absence of any finer distinctions it would have been helpful to know whether obsidian occurs in greater or lesser frequency in the earlier or $C_2$ stratigraphic zone as compared with the later or $C_1$ zone; but this information, presumably noted during the excavation procedure, is not given in this publication, which shows only the total quantity of obsidian from each phase.

For another, the principles on which the phases or periods are based may lead to some confusion. These phases are defined by breaks in the continuum corresponding to "adaptive changes" in economic and social life, together with "non-adaptive" changes such as the appearance of "an easily recognized new complex or assemblage of styles and/or artifacts." Priority in definition is given to the adaptive changes, but in those cases where the economy remained relatively unchanged for long periods artifact complexes were mainly used. There is not space here to discuss in detail the arguments for and against this approach, but in effect it means that the earlier phases of their sequence are defined primarily on the basis of adaptations and the later ones on the basis of style. One consequence is that it may be difficult to correlate the earlier phases at these two sites with phases or levels in other sites where there may not be the same conditions of preservation of seeds, bones, and other organic materials as at Ali Kosh. It might have been better to use the two sets of criteria separately and to create a column of phases or periods defined by artifact changes and a column of stages defined where possible by adaptive changes. As it is, the definitions of some of the phases (for example, Bus Mordeh, Ali Kosh) are perhaps premature, since only one occurrence for each is so far known, and these from very small exposures in the lower parts of the Ali Kosh site. This in turn raises the problem of adequate sampling and the interpretations based on the sequence outlined here. In view of the relatively small proportions of the sites which were excavated, and especially of the restricted areas of occupation surfaces exposed, there must remain a question as to whether the variations noted from phase to phase, especially in floral and faunal remains, are functions of time to the exclusion of other variables.

A third criticism is that the presentation of the illustrated artifacts by types rather than by phases makes it difficult for the researcher who is interested in comparing any particular phase of the Deh Luran sequence with any one of his own phases or levels. To get a visual impression of the artifacts it is necessary to search through the descriptions in the captions under each figure. Although the contents of each phase are also given in the tabulations of each class of artifact (pottery, stone, and so on), this is not always a convenient way to present the data if the types created are broad. Thus I find the categories for many chipped stone artifacts, based on their alleged functions (sickle blades, reamers, and so on) are too broad to be very useful for detailed comparative purposes; workers using finer categories will find it hard to identify their own types in the tabulated breakdowns.

The book is pleasingly free of typographical and other technical defects, and the illustrations are generally good. The presentation would have been greatly improved, however, if a chronological table had been included to show not only the estimated correlations of the phases excavated in Deh Luran with sites in the rest of Khuzistan (given in very parsimonious form on p. 9) but, even more important, the correlations with sites in other parts of southwestern Asia which are frequently discussed in the text. A more detailed map showing the distribution of sites than that provided by fig. 1 would also have been helpful.

A colleague who has read this book argues that its defects are inherent in research which is oriented to a single approach. I don't agree with him. Much in this book is good, and the weaknesses were avoidable.

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# In the Absence of Stimuli

**Sensory Deprivation.** Fifteen Years of Research. JOHN P. ZUBEK, Ed. Appleton-Century-Crofts, New York, 1969. x + 526 pp., illus. $9.50. Century Psychology Series.

If ever a subject has accumulated an enormous literature in need of a comprehensive and critical review, it is sensory deprivation. There are some 1300 items in the bibliography of this book. Even before the publication of the first findings on the "effects of decreased variation in the sensory environment" (Bexton, Heron, and Scott, 1954), word had spread far and wide about the experiments at McGill in which normal college-student subjects, kept isolated and deprived of perceptual experiences, experienced vivid hallucinations, body-image disturbances, and thought disorders. Rumor exaggerated both the conditions of the experiment and the "psychotic" manifestations, but the first published report—a rather short and modest paper, quite tentative in tone, in the *Canadian Journal of Psychology*—easily captured the imagination of serious investigators, who quickly put together their own experimental setups (sound-deadened room, respirator, or water tank) to see for themselves. These early investigators—John Lilly at the National Institute of Mental Health, Jack Vernon at Princeton, Philip Solomon at Boston City Hospital, and a handful of others—were essentially exploring the range and limits of this new experimental technique. Soon both the specialized journals and the popular press were filled with discussions of per-

ceptual isolation, sensory or stimulus deprivation; reviews of anecdotal data appeared; in 1958 the first symposium was organized—a new field had been born.

Why the widespread fascination with sensory deprivation? In part it was due to the original tie-in with "brainwashing"—a timely subject in the early '50's. In part it was due to its relevance to another popular concern, the Mercury space program, which included sensory deprivation among the stress procedures for screening potential astronauts. But I think that Peter Suedfeld, one of the contributors to the present volume, has put his finger on an underlying reason. As he says, "Here, truly, was a manipulation that made a *difference*—unlike so many pallid experimental situations, a difference you could almost taste." To be able to induce by psychological means major behavioral and physiological changes—that indeed was worthy of attention. To induce a phenomenon experimentally is to get close to understanding its causation. That was the promise held out by research on sensory deprivation.

In reading the present volume one is impressed by the manysidedness of the sensory deprivation field. The range of topics, ideas, and foci in the literature is staggering. Though it is easy to criticize so-called "method-oriented" research, it must be remembered that methods do legitimately open doors to the investigation of a variety of problems other than the ones to which they are first applied. Sensory deprivation is a powerful technique—a life-sized, temporally extended test that calls for a real adaptive response of some kind rather than simply for a test response. It offered an avenue to the study of personality, ego autonomy, stress, response to unstructured situations, sensitivity to internal cues, stimulus-seeking behavior (see Austin Jones's chapter), and certain clinical problems (see Wesley Jackson, Jr.'s chapter on eye-surgery patients). It permitted exploration of the social psychology of the psychological experiment, as Suedfeld points out. It brought within the sphere of the laboratory analogues of a number of real-life problems—space flight, arctic weather operation, small group confinement (see Seward Smith's chapter). And it required behavioral researchers at least to acknowledge such cognitive or phenomenological notions as purpose, meaning, doing, structure, and states of consciousness, and such poorly understood phenomena as boredom, encapsulation, and sense of isolation. (The inclusion in this volume of a few protocols from actual experiments might have helped both to impress the reader with the phenomenological, holistic aspects of the observations and to illustrate the narrowing of perspectives that is the price of exacting quantification and precision of measurement. It would certainly have impressed the reader with the difficulties of doing research with meaningful units of behavior.)

What has been the actual payoff in knowledge? What have we learned through and about sensory deprivation? This book, edited by John Zubek, himself probably the most systematic investigator in that field, attempts answers to these questions. Zubek has recognized the need for filtering the enduring, replicable, unambiguous, and meaningful findings from the inevitable mass of the artifactual, ambiguous, and trivial. He also recognized that the job was too big for one person, and asked a few others currently active in sensory deprivation research to prepare comprehensive reviews of various topics. Michael Rossi contributes an excellent chapter on the many methodological issues that sensory deprivation research, like any new field of inquiry, has had to confront at every turn in its development—problems about the choice of conditions, measurement, baselines, and controls.

Marvin Zuckerman writes about the many variables that have been found to affect and confound the interpretation of experimental results: types of confinement, instruction, duration, restriction of movement, expectation, and the ever-present variable of "personality," to name but a few. Although, as he says, sensory deprivation presents a "nightmare to the experimentalist who craves clean-cut, well-controlled" experimental data, Zuckerman succeeds in introducing a surprising degree of order into the hodgepodge of research output, and comes up with cogent (though far from universally accepted) conclusions. Similarly, one finds a much-needed classification of the results concerning hallucinations and imagery (Zuckerman), cognitive performance (Suedfeld), physiological and biochemical measurements (Zubek), and tolerance of deprivation (Myers). The confusion that each author initially confronts is gradually cleared away, and in the process the reader is afforded a detailed view of the accumulation of past research and is given many valuable leads for future work.

The final chapters, by Zuckerman and Suedfeld, evaluate the various theoretical perspectives that have, over the years, been brought to bear on sensory deprivation. Theories of various degrees of generality, involving most prominently the constructs of arousal, primary and secondary processes, expectation, information processing, and field dependence, have been invoked to account for this or that finding. The evidence for each conceptualization is carefully reviewed and a final synthesis, involving the concept of an optimum level of stimulation, is attempted by Zuckerman in a series of interrelated hypotheses.

The original aim of sensory deprivation research was, as the term implies, to determine the effects of a drastic curtailment of sensory stimulation on human functioning. The approach paralleled that used in animal experiments: human beings were placed in laboratory settings in which the salient "experimental variable" was presumed to be a reduction of environmental stimulation, whether in the variety or the amount of sensory input (the oft-made conceptual distinction between perceptual isolation and sensory deprivation proper, which, as it turns out, has little empirical significance). As the present volume makes abundantly clear, the original aim met with obstacles from a variety of methodological sources, so that interpretation of findings was difficult. It was nearly impossible to control for each of the many potentially relevant parameters inherent in the complex of conditions. And because each lab used its own basic experimental setup and its own procedures for assessing effects, comparability of data suffered. Effects originally reported as due to sensory reduction might in fact have been caused by any number of other variables, singly or in combination.

Moreover, with increasing experience it became evident to most investigators that another very troublesome obstacle to unambiguous findings lay in the fact that a subject undergoing sensory deprivation is in a totally altered life situation that affects his self-systems, defenses, fantasies, motivations, and cognitive and interpersonal stratagems. Individual differences in effects loomed large and were not reducible to their determinants; at least most personality test batteries employing traditional trait or factor conceptions failed to identify

them. In his synthesizing chapter Zuckerman appropriately emphasizes the importance of individual differences, and even proposes as a central proposition that "every individual has characteristic optimal levels of stimulation and arousal for cognitive activity, motoric activity, and positive affective tone," involving constitution, age, learning, recent levels of stimulation, task demands, and diurnal cycling as factors in the particular optimum level at a given moment.

Despite the obstacles, however, it would be fair to say that the bulk of evidence gathered over the last 15 years in the main supports the original claims. On specifics, there is still little agreement across studies. The developments over the years have been toward greater specificity both in the delineation of fundamental parameters and in the attribution of effects. For example, one would no longer claim "hallucinations" (or its current operational variant, "reported visual sensations") as effects due directly to sensory reduction; the current view would require more qualification and would speak of them as being facilitated by or occurring with greater frequency in sensory reduction as compared with a number of other conditions, depending on arousal level, set, personality, and so on. Certainly, today's researcher would be most cautious about linking these phenomena to psychosis. Similarly, the current position on cognitive impairment would specify that it is more likely to occur on tasks requiring complex, self-directed efforts; and so on down the line with each behavioral index. Nevertheless, the overall conclusion of the early McGill studies remains: namely, that sensory stimulation is an important ingredient in the maintenance of effective functioning. In fact Zuckerman, summarizing his chapter, offers the reader a stronger version of the old aphorism when he suggests that variety (in stimulation) is the *bread*, not simply the spice, of life.

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## The Straight and Narrow Path in Psychology

**The Scientific Evolution of Psychology.** Vol. 2. J. R. KANTOR. Principia, Granville, Ohio, 1969. xxii + 430 pp. + plates. $12.

It is doubtful that any psychologist has ever managed to believe even one impossible thing, much less six before breakfast. A great many, however, have succeeded in believing things that are, to say the least, very difficult. Take, for example, John B. Watson's (fl. 1920) famous assertion that "so-called thinking" is nothing more than minute, subvocal contractions of the muscles involved in the production of speech. Clearly, this is a belief that does not come to one without a certain measure of effort and practice. Watson, though, proved himself to be more than equal to the task, for in the end he succeeded in believing a whole family of difficult things. Thus, in his view, *all* matters that fall under the heading of "so-called consciousness"—"sensation, perception, attention, will, image and the like"—are in reality nothing more than bits of external behavior that have been variously misconstrued. And it is this insight, he went on to suggest, that "when rightly understood goes far in breaking down the fiction that there is any such thing as mental life." Thus, to take a small example, Watson could have spoken quite comfortably of a toothache stimulus and of the resulting toothache behavior; but the toothache itself (the "so-called toothache," he would have said) would have seemed to him nothing but a fictional construction. It is not known whether Watson's own toothache behavior was in any way altered by this insight.

Watson was aware that the evidence for his view was not entirely overwhelming. Even so, he felt obliged to put it forward, for so far as he could see it was the only account of "so-called consciousness" that was acceptable "in terms of natural science." Of course, many years have passed since Watson's proclamation, and in that time the enthusiasm to which it initially gave rise within psychology has waned considerably. Still and all, the Watsonian doctrine that the paraphernalia of mental life are fictions, and thus of no account "in terms of natural science," survives in many quarters and is in all events a force to be reckoned with. Probably the most widely known of the doctrine's present-day supporters is B. F. Skinner, who speaks of the "fiction of a mental life" with something approaching militancy. Less well known outside of psychology, though equally fervid in his support of the Watsonian doctrine, is the author of the volume here under review, J. R. Kantor. As one may judge from its title, Kantor's book is the second volume of a two-volume study in the history of psychology. The first volume, which was completed and published some years ago, covered the period extending from Greek classical antiquity up through the end of the Middle Ages. The present volume takes the story up again in the 17th century and brings it about as close to the present as space and historical perspective will allow.

The scholarship and historiographic art that have gone into the production of these volumes simply cannot be doubted. They are sound and respectable works, and whoever reads them will surely gain a good knowledge of the historical development of psychology. Nonetheless, the prospective reader must be issued a caveat, especially if he is not yet privy to the fact that psychology is a science in which it is still possible to enjoy the pleasures of polemic. For these are not works of pure, disinterested historiography. Quite the contrary, they take throughout the form of historical stones for the grinding of contemporary axes. This is not necessarily a flaw in a work of this sort. Indeed, the reviewer would ardently agree with Kantor when he observes that the psychologist would do well to use the history of his discipline "as a tool for understanding and promoting psychological work here and now." Still, the work has its biases (honest and forthright though they be), and this is a fact of which the prospective nonpsychologist reader ought not to lose sight.

It is Kantor's view that psychology has evolved into a natural science precisely insofar as it has succeeded in recognizing the fictional character of those "transcendental concepts" with which psychological thought and research have been so long laden. One might imagine that what he has in mind here are such disreputable musings as "incorporeal soul," "entelechy," "vital force," and the like. Of course, he would include notions of this sort under the heading of "transcendental concepts," but he would also want to include all those matters that the pre-Watsonian psychologist spoke of, without compunction, as the "phenomena of mental life." Thus, in Kantor's view, the scientific maturity of psychology at