Edited by
ANTHONY M. GRAZIANO

behavior therapy with children

Volume 2

behavior therapy with children

behavior therapy with children

volume 2

Edited by
ANTHONY M. GRAZIANO

Routledge
Taylor & Francis Group
LONDON AND NEW YORK

First published 1975 by Transaction Publishers

Published 2017 by Routledge
2 Park Square, Milton Park, Abingdon, Oxon OX14 4RN
711 Third Avenue, New York, NY 10017, USA

Routledge is an imprint of the Taylor & Francis Group, an informa business

Library of Congress Catalog Number: 2006051661

Library of Congress Cataloging-in-Publication Data

Behavior therapy with children / Anthony M. Graziano, editor.
p. cm.
Originally published: Chicago : Aldine-Atherton, 1971.
Includes bibliographical references and index.
ISBN 0-202-30862-6 (pbk : alk. paper)
1. Behavior therapy for children—Textbooks. I. Graziano, Anthony M.
[DNLM: 1. Behavior Therapy. 2. Child. WS 350.6B41755 1971a]

RJ505.B4G7 2006
618.92'89142—dc22 2006051661

ISBN 13: 978-0-202-36283-0 (pbk)

Contents

VIII. BEHAVIORAL INTERVENTIONS IN SCHOOLS

IX. FAMILY SYSTEMS

X. SELF-CONTROL

Preface

Like the first volume of *Behavior Therapy with Children,* Volume II is intended for students, therapists and researchers. Its focus is on behavior *therapy*—the behavioral treatment of children's clinical problems—a subset of the larger area, behavior modification.

The available issues of each of thirty-one journals published between 1968 and early 1975 were searched, yielding more than 900 articles appropriate to behavior therapy with children. The articles were reviewed, categorized, and final selections made. Those selections, the overall organization of the book and the editor's introductions reflect this writer's judgments of the current state of the field, tempered, of course, by the space limitations of a single volume.

The present book is intended to cover the developments since the publication of volume I in 1971. Although there is some overlap, it is hoped that the two volumes together will provide some useful representation of the field through early 1975. Future volumes are planned so as to periodically update the material and to provide a continuing chronicle of the field's developments, including the extra-scientific developments.

Before turning to the main introduction, I wish to acknowledge several persons who helped in the preparation of this book. Maureen O'Mara assisted in the initial library search; Amy Graziano helped to organize the extensive bibliography; and Virginia Tate typed the introductory material and bibliography. I wish also to thank the authors and their publishers who granted permission to reprint their articles, and the Aldine staff for their work and support. It must be mentioned, too, that in our literature search we relied heavily on the library facilities of the State University of New York at Buffalo.

A.M.G.
Time Hill Farm
Chautauqua County New York

Behavior Therapy with Children

Introduction: Behavior Therapy with Children

I. BEHAVIOR MODIFICATION AND BEHAVIOR THERAPY

The term *behavior modification* as used here refers to the systematic analysis and change of human behavior. The principal focus of behavior modification is on overt behavior and its relationships to environmental variables. Learning and conditioning paradigms have been and still are extremely important in the historical development of behavior modification. However, modern behavior modification has not only advanced far beyond Watson's early behaviorism, but is no longer limited to "learning" conceptions. The field now encompasses many theoretical variations—a point that Cyril Franks has made repeatedly for a number of years. While some writers, such as Wolpe, maintain a conditioning definition of behavior modification, many other writers, among them Bandura (1969), Kanfer and Phillips (1970), Franks and Brady (1970), and Krasner and Ullmann (1973), have adopted a more eclectic position admitting biological,

perceptual, motivational, developmental and social data. Behavior modification is a large field comprising various approaches to human functioning, all of which, however, emphasize experimental methods, including the careful specification of variables and the functional relations among them.

Behavior modification can be applied in various settings, the nature of which helps to define its subsets. Thus, behavior modification applied in clinical settings, toward clinical goals, encompasses the subset behavior therapy. (Similarly, programmed instruction is the subset which applies behavior modification to academic and many other nonclinical teaching settings.) Behavior therapy, then, involves experimental approaches to clinical problems, viewed in behavioral terms, in which functional relationships are specified. Traditionally, behavior therapy has relied heavily on conditioning and learning paradigms, but modern behavior therapy is not limited to them. *Behavior modification* is the more general term, while the more specific subsets, such as behavior therapy, are defined by the types of behavior on which they focus and the settings in which they are carried out.

II. THE CURRENT GROWTH OF BEHAVIOR MODIFICATION ACTIVITIES AND COUNTERREACTIONS

The field of behavior modification encompasses an astonishingly wide and varied spectrum of concepts about and approaches to education, clinical problems, social programming, and rehabilitation efforts. A conceptually and technologically rich medium, it has developed for more than half a century, nourished by the psychology laboratory, the school, and the psychiatric clinic. It is an area with diffuse boundaries surrounding a highly active center, within which apparently solid landmarks of just three or four years ago have already been worn away by the dissolving action of corrective self-criticism—immeasurably aided by the catalysts stirred in by the field's many critics. The activity continues, the dynamic field boils, and the medium enriches itself.

One measure of the field's current vitality is the dramatic increase of publication activity in behavior therapy (i.e., behavior modification as applied to clinical problems). This may now be the single most prolific area of therapy research and writing. Insalaco and Shea (1970) listed 1,239 articles and 68 books in behavior modification published between 1965 and 1969; since 1969, behavior therapy outcome studies have increased sharply (Bergin, 1971; Mahoney, Kazdin, and Lesswing, 1974), as have the books devoted to behavior modification (Ernst, 1971). Overall, according to Hoon and Lindsley (1974), behavior therapy publications have shown a dramatic, sustained, and exponential growth compared with that of other therapy models. Those authors noted that "the

growth rate of publication activity in behavior therapy since 1960 has been so dramatic that in 1972 [behavior therapy publications] surpassed the number . . . indexed under psychoanalysis" (p.696). Benassi and Lanson (1972) reported the striking growth of college courses in behavior modification, and Yen (1971) noted that 84 percent of psychology doctoral programs offer at least one course in behavior modification. Behaviorally oriented abnormal psychology texts, college courses, and films have introduced increasing numbers of undergraduates to behavior modification. Some of those students have gone on to graduate school and the various mental health professions. Those students who took courses in behavior modification in graduate school are bearing with them alternatives to the standard psychodynamic clinical models that still represent the applied field. Many professionals are attending the increasing number of behavior modification workshops around the country, and are making use of the new behavior modification journals. The first of these journals, *Behaviour Research and Therapy,* was founded in England in 1963 by Hans Eysenck. In 1968 Montrose Wolf founded the *Journal of Applied Behavior Analysis.* In 1970 Cyril Franks founded *Behavior Therapy* and Joseph Wolpe founded *Behavior Therapy and Experimental Psychiatry.*

These are academic, primarily conceptual, developments, consisting of research and theoretical writing, classroom education, and practicum training. But what of the applied fields? Have the behavior modification model, its experimental findings, and its recently trained students effectively influenced contemporary applied fields, such as education and mental health?

It seems that classroom behavior modification in the form of programmed texts, films, hardware, and contingency management strategies, has indeed influenced education, particularly at the elementary level. However, careful documentation of the degree of influence is not yet available.

Our major interest, however, is in the question, To what degree has behavior modification influenced the applied, clinical field as it is practiced day-to-day in hospitals, clinics, and private offices? In a recent study, Pailler and Graziano (1972) attempted a national survey of established and presumably traditional clinical settings, those that seem to account for most of the practice of mental health professionals. Despite a low return of only 27% of the questionnaires, the data did suggest that behavior therapy was still limited primarily to university settings, accounting for only a small portion of clinical activities. Further, it was being carried out by younger professionals, most of whom were psychologists. A more complete and up-to-date survey of the extent of behavior therapy in clinical practice is needed.

There is some indirect evidence of the influence of behavior modification on clinical practice, but this evidence is quite mixed. First, it appears

that neither medical schools nor psychiatric residency programs are currently offering much education in behavior modification (Brady, 1972a, b). Recently trained psychiatrists, then, will apparently not be agents of change toward behavior modification in applied mental health settings. A recent psychiatric task force has recommended that behavior therapy be taught in psychiatric training programs (Birk et al, 1973). However, even if medical schools and residency programs were to immediately incorporate that recommendation, the influence of their graduates in the applied clinical field would not be significant for a number of years.

Another indirect measure of the influence of behavior modification on clinical practice is the manner in which behavior modification is represented by nonbehavioral journals. Franks and Wilson (1974) discussed special issues on behavior therapy published by some journals in social work and psychiatry, and pointed out that much of what those journals presented as behavior modification was trivial and outdated. They commented, "If this is what the non-behavioral mental health specialist thinks that modern behavior therapy is all about, then there is indeed a vast task in professional education ahead of us" (p. 1).

These indirect measures strongly suggest that behavior modification has as yet had little effect on clinical practice in traditional mental health settings, and is largely misunderstood by many mental health professionals, particularly those outside psychology.

In contrast, other evidence suggests that behavior modification has influenced applied practice considerably, but outside traditional mental health settings. We are referring to behavioral programming in governmental correctional institutions, and in a variety of new clinical and rehabilitation settings that have been specifically created around behavioral concepts. Examples of the latter are the Achievement Place programs for delinquent and predelinquent children, such as those reported in this volume by Phillips, Phillips, Wolf, and Fixsen (1973), and special behavioral programs for psychotic children (e.g., Graziano, 1974).

Some national organizations, such as the National Society for Autistic Children, actively support behavioral programming for children, and the education of parents and professionals in behavior modification. Thus, while traditional clinical settings may not have been substantially affected by behavior modification, behavior modification in clinical practice may nevertheless be increasing through the development of new clinical settings. Indeed, such development outside traditional mental health agencies has been recommended as perhaps the most effective means to develop behavioral-clinical services (Graziano, 1969). No data now exist on the number, capacity, or influence of such nontraditional settings.

Public reaction, still another indirect measure of the influence of behavior modification on clinical practice, suggests that the influence of behavior modification on applied practice in such settings as penal insti-

tutions has been sufficient to cause public concern. A recent book by Jessica Mitford (1973) pointedly warned against behavior modification, particularly as it might be used with inmates of correctional institutions. Tom Wicker editorialized against behavior modification in the *New York Times* (Feb. 8, 1974, p. 31). Our former vice president, Spiro T. Agnew, was one of the first and most visible politicians to publicly castigate behavior modifiers. In an address to 2,000 farmers at a Chicago meeting of the Illinois Agricultural Association in November 1971, he attacked "behavioral psychology" as a "threat to traditional American values," identifying B. F. Skinner as the originator of the threat. Agnew's stand was consistent with the conservative nature of the Nixon administration (Graziano 1970), and was considerably less surprising than was his choice of audience for that particular speech.

A series of actions taken by and directed at federal agencies over the past two years is evidence that critical concern has reached high levels of the federal government and has affected policy decisions. In February 1974, two federal agencies, the Federal Bureau of Prisons and the Law Enforcement Assistance Administration, announced thay they would stop funding behavior modification projects. Although "economic factors" and "organizational changes" were stated as reasons, the actions appeared to be due to continued critical pressures from congressional investigators, civil rights groups, and the press. A particularly important source of pressure was the three-year congressional investigation chaired by Senator Sam Ervin. In late 1974 the Ervin Committee issued its 651-page report, *Individual Rights and the Role of the Federal Government in Behavior Modification.* Highly critical of the government's increasing involvement in behavior control projects, the report clearly warned that Congress might be forced to initiate legislation to prevent possible abuses.

In January 1975, the National Institute of Mental Health issued the report *Behavior Modification: Perspectives on a Current Issue.* Prepared by Brown, Stolz, Wienckowski, and Parloff, the report attempted to clarify and distinguish between "warranted and unwarranted concerns" over behavior modification. The authors supported continued behavioral research, particularly research designed to compare the effectiveness of behavior modification with alternative therapies.

The most recent development, the finalization of the Presidential Biomedical Review Panel, occurred in March 1975. The panel was empowered to review behavioral and biomedical research programs conducted by government agencies and to make recommendations by mid-1977 concerning the organization, operation, and content of such programs.

In large measure, these public criticisms of applied behavior modification were emotional reactions to distortions of the facts and to an overly inclusive definition of behavior modification. For example, as Martin (1974), an attorney and a former Senate legislative aide, pointed out, the

public has identified behavior modification with many "dangerous, unwarranted" techniques, these including psychosurgery, chemotherapy, neuropharmacology, electrical stimulation of the brain, and electroconvulsive therapy. As Martin also noted, all of those techniques assume the validity of the medical model—quite contrary to the position assumed by virtually all psychological behavior modifiers. The techniques in question are, therefore, not fairly ascribed to behavior modification, and the profession has a public educative task to attend to. However unfair the public's misunderstanding of behavior modification may be, it presents a real problem, and we are well advised to attend to the public's view. However benign behavior modification may be in the eyes of behavior therapists, the public sees it as a set of powerful and threatening techniques.

There are at least three important issues here that behavior modifiers must recognize: (1) in the popular wisdom, behavior modification has, to some extent, taken on the color of Clockwork Orange; (2) in the judgment of many concerned and thoughtful people, behavior-change technology suggests the possibility of new abusive power, threatening to civil liberties, perhaps requiring restraints on and accountability for its future use; (3) as behavior modification is increasingly applied in different social settings and as, concurrently, the general level of social criticism continues to rise, we will witness an increase in legal challenges to behavior modification (Graziano 1973), and the field will be compelled to meet legal as well as scientific tests. This last point is particularly interesting in light of a recent article by Murray Levine (1974), arguing for an advocacy model in science, much like that used in the courts. Perhaps behavior modification will be an early test of Levine's suggestion.

Many difficult issues are involved in this extraordinary escalation of criticism from the level of intraprofessional squabbling to that of federal policy decisions. We can expect the issues to remain active and can foresee a heated-up flurry of concern and publicity about mid-1977, when the recommendations of the Presidential Biomedical Review Panel will be due.

In summary:

1. As measured by publication activity and surveys of university courses, there has been a marked increase in the conceptual and training dimensions of behavior modification.

2. This increase has been almost exclusively limited to the academic discipline of psychology. Medical school and psychiatric residencies offer little behavioral training.

3. The impact of behavior modification on applied fields is more difficult to assess. A good survey is needed. Behavior modification, when translated to the various applied areas, defines subsets, i.e., programmed instruction in schools, behavior therapy in the mental health field, and

various forms of training, socialization, and rehabilitation programs in such special settings as correctional institutions and institutions for the retarded. These behavior modification subsets are defined not so much by differences in basic concepts or approaches as by the types of problem behaviors on which they focus.

a. Programmed instruction appears to have been quite widely incorporated into the curricula of both regular and special schools. Behavior therapy has also been utilized in individual and classroom contingency management, but the extent of this use is less apparent.

b. Behavior therapy and rehabilitation programming has been incorporated into federal and state programs, and large, heavily financed projects have been created in some correctional institutions. The overall correctional field has not been radically altered by behavior modification, but significant parts of that field have adopted behavior programming.

c. In contrast, clinical settings have not been so clearly affected. The available evidence, unsystematic and mainly indirect, suggests that the everyday delivery of mental health services in established clinics, hospitals, and private offices has been little affected by behavior modification.

4. Behavior therapy services are being offered, however, in new, specially created settings that have developed as alternatives to existing mental health service agencies. It may be that continued development of alternative agencies will be the most successful route to the provision of behavior therapy programs. At present, no adequate assessment of the extent of such new programs is available.

5. Finally, a remarkable degree of public concern has developed over behavior programming. The extent of this concern may be unique in the history of psychology, and it will probably continue to escalate around issues of science, morality, and governmental control. Legal tests will become more frequent. The issues are not new, but this appears to be the first time that psychology as a discipline and as a profession has been so pointedly involved. The processes by which and the manner in which these issues are resolved might very well be of extraordinary importance for the continued development of applied psychology.

III. BEHAVIOR THERAPY AND "HELPING"

A major problem in sorting out the development of behavior therapy is that it has not only radical, even unique aspects, but also common roots in some very long academic and applied traditions. Behavior therapy is commonly thought of as a recent development, suddenly appearing in the early 1960s, and it has sometimes been described as a "revolution." There may have been some radical changes in thought, but the label

"*revolution*" may be misleading. As commonly used, this label suggests a complete reversal of an established position, a sharp, even violent, break with the past, and, at least implicitly, it urges the discarding of all "old" notions. It should be recognized, however, that even political revolutions have their roots in a common existence with the "old order" and that postrevolutionary conditions differ from prerevolutionary conditions along only a limited number of dimensions. Behavior modification has not caused sharp, major breaks—the schools remain much as they were; the clinical establishment continues to operate without a change of pace or direction. Clinical training programs in psychology have included behavior modification, but few of these programs have been "revolutionized," and psychiatric training settings largely ignore the whole development.

Behavior modification has emerged over nearly seventy-five years as part of the trend toward objectification in psychology in the early 1900s. It is a rational extension of basic data and concepts that had been largely experimentally developed in comparative psychology and behaviorism by the 1940s. Except for a few attempts in the mid-twenties and the thirties (Yates, 1970, lists twenty-eight articles published from 1920–39), behavior modifiers did not turn to applied clinical problems until the late fifties and the early sixties. At that point, it appeared as if learning theory psychologists had switched tracks and jumped from academia into applied clinical educational psychology. It was the contrast of the old behavioral traditions against the background of even older clinical traditions that gave these applications a "revolutionary" appearance. What was new was the juxtaposition of two previously parallel developments, academic behavioral psychology and clinical mental health, the latter having been primarily psychiatry. The academic psychologists joined the mental health "mainstream" with its long traditions of "helping." As a result, behavior modification ceased to be a separate, totally academic, endeavor, but began to share the long traditions, the ethical concerns, and even the specific and highly traditional forms (e.g., the therapist-patient dyad) of even the most traditional psychoanalyst. The point here is that despite all the revolutionary rhetoric of change, behavior therapy is now rooted commonly with other traditions of "helping," differing along some dimensions, but sharing many others. Thus behavior therapy is not a new, categorically different approach, but one of several variations in our continuing attempts to influence our well-being. It is now a part of the mainstream of helping, and it will continue to follow many of the established traditions in concepts and approaches of that mainstream and to be affected by the same moral, economic, and legal constraints that we believe will increasingly come to bear on psychotherapeutic "helping." Much of the literature seems to have emphasized the differences between behavioral and traditional therapy, elevating to priority those dimensions

on which real differences do exist—but obscuring their sharing of many other, important, dimensions.

In summary, this section has proposed that behavior therapy, despite its earlier rhetoric of categorical difference from traditional therapies, is now rooted in the mainstream of psychotherapeutic development, and subject to shared concerns, goals, traditions, and constraints—though many of the last have yet to be imposed by an increasingly sophisticated public. Behavior therapy constitutes a major variation in helping endeavors, and coexists as one of many such variations. It is not known whether these variations will ever be integrated—nor whether such integration is even desirable. What appears most likely for some time to come is a continued coexistence, and a periodic shifting of "ascendancy," of behavioral, psychodynamic, and organic models.

IV. PSYCHOLOGICAL HELPING AND ISSUES OF THERAPIST CONTROL

Psychological helping, of whatever theoretical model, is still based on the essential elements of the helping situation—i.e., through an at least implicit contract, a "helper" or therapist intervenes in the life of a client who seeks help, and the therapist brings to bear his particular skills to influence the client to change. This helping relationship between therapist and client has traditionally been personal, face-to-face, and highly verbal, and, despite the common disclaimers by therapists, it is primarily a concentrated attempt by one person to influence another. That influence may be grossly obvious and acceded to by the client, as is the case, for example, when the therapist directly coaches a client in assertive behavior. Or the influence may be less overt, as in the therapist's selective response, by interpretations, reflections, or displays of interested listening, to only certain classes of the client's verbalizations. Behavior control by any other name is still behavior control, and, even if influence is denied by the therapist, it remains a central fact in therapy. The major ethical issue here is not *whether* therapists exercise influence over their clients, but what the ethical limits of that influence are. To what degree should a person or a professional system influence the lives of people in the name of helping? This issue of control exerted by therapists is a general issue, common to *all* therapies, and it must be recognized as such. The client is thus in a vulnerable position and depends for protection on therapy's "moral surround," i.e., on the therapist's integrity, and on the legal and ethical constraints on abusing the power to influence. "Helping," of whatever models may be currently dominant, is surrounded by a larger system of moral values, legal constraints, and cultural traditions. One might argue that, as compared with the immensity of this shared reality, the importance of technical differences between the norms of

behavior modification and those of "traditional" therapy no longer appear to be quite so important.

Professionals and laymen have expressed concern about the control which they fear behavior modification can exert over the citizenry. Many of the critics who single out behavior modification have become desensitized—or perhaps never were sensitized—to the powerful controls that have been imposed routinely by the more traditional therapies. Such contemporary writers as Daine (1964), Goffman (1962), Liefer (1969), and Szasz (1961) have clearly pointed out abuses in the application of those controls. Not long ago, Clifford Beers made the same indictment; well before Beers, in the late 1700s, Chiarugi, Joli, and, later, Pinel took action against the abusive control of mental patients. In fact, concern over abuses of the welfare and rights of citizens judged insane was debated by senators and physicians in Rome before 300 A.D. (Graziano, 1969).

The ethical issues of control or influence in therapy are very old and extremely important. Yet they have never been adequately dealt with by the profession. Perhaps now, with the advent of behavior therapy, they will be resolved.

Many important issues of psychotherapy will have to be examined in light of the growing public challenges that can be expected in the next few years. In order to determine whether any therapeutic practice does in fact transgress ethical and, perhaps, constitutional limits, it will be necessary to clearly identify the details of the operation in question. Only then can it be subjected to ethical and legal tests. The first major issues will consist primarily of answers to the question, What is the nature of the controls that operate within "helping" endeavors? Specifically, what are the details of those operations? It appears to us that no therapy models have adequately answered those questions, and that prior to the advent of behavior therapy and of Skinner's analysis of subtle controls in society, there had been no technology for a careful functional analysis.

Do any of the identified control mechanisms that are inherent in the helping settings go beyond ethical limits? Only by answering the empirical question can the issues of ethics be fully explored and resolved, for only when the operational details are fully known will we be able to apply the necessary ethical and, perhaps, legal tests. Ethical and empirical issues in therapy are of *equal* importance, and one is as much our responsibility as the other.

Many critics of behavior therapy seem to have avoided this first, empirical step, preferring to leap directly into philosophical arguments in which they can define the problem in any manner most useful to themselves. Behavior therapy, according to many of its critics (Bruck, 1968; Maslow, 1969; Matson, 1971; Portes, 1971), "dehumanizes" and "stereotypes" human beings with its "too simplistic" view of man, a view which cannot

account for all the data of neuroses and psychoses. "Humanistic" psychotherapists supposedly operate in ways that "free" their clients (Rogers and Skinner, 1956) and implicitly guard against control, while behavior therapy "has nothing at its core that prevents its practitioners from becoming behavior controllers" (Portes, 1971, p. 305). Aside from the obvious rhetoric that labels one approach "free" and the other "controlled," a rhetoric which clearly begs the question by assuming that to be free is "good" and to be controlled is "bad," this position obscures the most important issue: *the clear, honest recognition of the control that all therapies and mental health systems actually have over their clients.* This point is going to become increasingly clear as the debates around behavior therapy grow more intense. More than any other therapy system, behavior therapy specifies its controlling operations, and perhaps largely because of this it has been an easily perceived target for attack. However, a target that has been highly polished by abrasion is also a good reflector, and ethical criticisms of behavior therapy constitute criticisms of the entire therapy field.

Skinner (1948, 1971) has discussed issues of social control on a more general level, and one of the important points he has repeatedly made over the past quarter century is that "freedom" is a philosophical ideal and that people are largely but subtly controlled by complex webs of reinforcing stimuli. Skinner has advocated a forthright appraisal of social control, and if we are ever to disentangle ourselves from personally counterproductive control, our first step must be to clearly recognize that such control does, in fact, occur. As for psychotherapists, Skinner calls for some long overdue reality testing; i.e., for a disavowal of the comforting myth of "freedom" in therapy as an all-or-none event, and for realistically perceiving and admitting the operation of social control in the therapy situation.

The mental health profession is here taken to task for having refused to admit its exercise of control over clients, for having persisted in presenting to the public the idea that such a grossly contradictory situation as a control-free influence system exists in the form of "humanistic" or "traditional" psychotherapy. It is becoming increasingly difficult to maintain that particular distortion, and those professionals who attempt to do so appear, at least to this writer, to exercise a contempt for the public in general and for mental health clients in particular.

The current reemergence of behavior therapy, with its explicit use of stimulus control, is forcing attention back on other therapy models in which control is implicit and subtle, but no less real. To repeat an earlier point, the major ethical issue is not *whether* therapists exercise control over their clients, but what the ethical limits of that control should be. To what degree should one person or one professional system influence the lives of people in the name of helping? Clearly, the first step for the

mental health profession is to recognize and specify its controlling functions, to evaluate them against ethical and legal tests, and then to carefully develop rational ethical guides to prevent the abuse of its power to control. Effective ethical guides will not be developed so long as the profession refuses to admit that there is any danger of control over clients in any system other than behavior therapy.

In summary, the complex ethical and empirical issues of control in *all* therapy need clear recognition, and obtaining that recognition must be high—perhaps first—in the profession's priorities. In response to critics, we argue that they are correct—control in behavior therapy is a crucial issue. However, these critics have not applied their strictures generally enough—and the issue, rather than being limited to behavior therapy, is crucial for the entire mental health field.

V. "HELPING," CHILDREN, AND MENTAL HEALTH POLITICS

Berlin (1975) estimates that each year six million children aged five to nineteen have mental health problems severe enough to require professional treatment. Six million! Of course, they do not receive the treatment.

Facing the reality of such massive numbers is the mental health profession, the major social agent charged with the responsibility of providing "helping" services. This profession—or any single social system for that matter—is totally inadequate to meet the demands and perhaps, in fairness, it should not be faulted if it has failed. However, criticism is in order because, having accepted social and economic status as recognized child experts, the profession seems to have relegated concern for children to just about the lowest priority.

Reporting on children's services, the Joint Commission on Mental Illness and Health (1961) stated that "the present operation of clinics leaves much to be desired if we continue . . . to set adequate or sufficient therapeutic service as our goal." Graziano (1971) noted that the current delivery of children's mental health services

> involves complex problems of finance, public education, identification, diagnoses, treatment, personnel and physical facilities. Each of these and numerous other problems pose complex and difficult tasks, cutting across conceptual, professional, political and logistical areas. Together their collective challenge is staggering; millions of dollars and millions of professional and lay man-hours are being poured into the support of complex and costly clinical structures which are presumably justified on the assumption that psychotherapy works. If this assumption is questioned and found to be untenable, then so must the entire present clinical structure be questioned (p. 6).

Since 1903, White House Conferences on children have been held

regularly, with much fanfare and nearly complete futility—for virtually nothing of substance has been achieved in the *three quarters of a century* since that first conference. No major legislative action has resulted (Berlin, 1975), and there has been no rush by the mental health profession to develop effective treatment for children (Levitt, 1957; Silver, 1963; Colby, 1964; Hersch, 1968) or even to *plan* comprehensive children's programs (Smith and Hobbs, 1966).

Writing in the mid-1960s about planning for comprehensive mental health centers, Smith and Hobbs noted that mental health programs traditionally tended to neglect children, and that plans submitted by states for their mental health centers had been "conspicuous in their failure to provide a range of services to children." Traditional psychotherapy with children is adult-oriented, proceeding through interpretive and highly verbal psychotherapy which does not appear to be relevant to the problems of childhood. Smith and Hobbs termed the movement for child guidance clinics a "less than encouraging experience," and pointed out that, by and large, mental health professionals had not yet come to grips in any relevant and significant manner with the problems of the "disturbed" child.

It appears that children's welfare is given low priority in social service programming. With specific regard to the mental health field, children's services are few and, where they do exist, are too often of high cost and doubtful quality. Despite the past seventy-five years of massive evidence of children's needs, our mental health profession has accomplished little of social significance in reducing the psychological suffering of children.

In a number of previous papers, we have developed the thesis that the inadequate delivery of services is due primarily to political power distortions of original humanitarian and scientific goals; that the mental health field operates largely for its own benefit, at the expense of the public, which must tolerate scarce, overpriced, and questionably effective services. A report by Larry Brown and Judith Weitz (1972) is instructive in its discussion of the effects of mental health politics on children. These authors and many other individuals, including clinicians and administrators, have found their professional lives largely exhausted in the frustrations of their struggles to change that powerful self-interest system.

Innovations, ideals, and good intentions, we have maintained, are routinely corrupted by the political power functions of the mental health field. Traditional psychodynamic therapy has failed to provide appreciable help to significant numbers of children. However, as the above summary should make clear, not the therapy model, but the self-serving politics of the mental health profession, is the greatest barrier to effective service delivery.

Traditional psychotherapy for children has not provided very good answers; but now we have an alternative model, behavior modification.

Behavior therapists are proposing technological changes within the mental health field, new approaches to old and still unsolved problems. But before we happily laud the new developments, let us remind ourselves of the deadly political entrapment that is endemic in our mental health field and recognize that the social value of behavior therapy will ultimately depend not only on its future technological, empirically validated effectiveness but, to an equally important degree, on *its success in avoiding the political power distortions that have already perverted the mental health field.* Given that reminder, we must ask ourselves this question: Even assuming the technical superiority of behavior therapy, *what leads us to believe that our therapy will be any more successful than the therapies of our predecessors in avoiding that political distortion?* At present, we do not seem to be sufficiently aware of the functional importance of professional politics, and we are not doing anything to insure that we will be any more successful than our predecessors (Graziano, 1973).

In summary, behavior therapy accounts for only a very small portion of contemporary mental health systems, the major reality being professional-political, self-serving control over resources. It has been this writer's thesis for a number of years that such control has not only dominated but has thoroughly distorted all previous models of psychological "helping," resulting in virtually ineffective delivery systems. Behavior therapy is now part of the therapeutic "mainstream" and is therefore now equally vulnerable to the massive, extrascientific political controls that have been imposed upon all other helping models. Avoiding that eventuality is at least as important as developing the technology, but behavior therapists do not seem to be aware of the importance of doing so.

VI. SOME HISTORICAL CONSIDERATIONS

The current practice of behavior therapy came about as academic psychologists with a behaviorist orientation joined the much older and primarily psychiatric field of applied mental health. Psychiatry had always been an "applied" area. Even at the very beginnings of primitive medicine, the healer's starting point was a "sick" person and the intent was clearly to correct a perceived malfunction.

Explanations and treatment of observed deviant behavior have varied throughout history, but they all seem to have revolved around a similar observation: the individual is not "in control of himself" but is under the influence of powerful, disruptive, internal forces. To primitive peoples, these internal forces were independent and self-directed, and they impelled the unfortunate possessed person to disordered behavior. The major control was magic, and magical rites were used in attempts to force the demons and spirits out of the afflicted person's body. Later in cultural development, religion began to displace magic, and man prayed to and

beseeched supposedly higher powers than himself to intervene and drive out the demons.

Against this background of religious and magical animism, the classical Greeks and Romans developed naive but rather naturalistic and humanitarian views of disordered behavior. They sought organic, physical causes, and a few, such as the Roman physician Soranus, insisted upon humanitarian treatment for mental patients.

During the Middle Ages, however, the earlier beginnings of naturalistic and humane treatment were submerged as religious animism was revived in the treatment of persons thought to be "possessed." The major approaches were at times drastically inhumane, as both secular and church people tried to drive out the devils through severe exorcisms.

In the sixteenth century, the earlier mental illness concept was revived by, among others, St. Vincent de Paul, who asserted that "mental disease is no different than bodily disease and Christianity demands of the humane and powerful to protect and the skillful to relieve the one as well as the other" (Coleman, 1972). This revived "illness"concept, while objecting on humanitarian grounds to the vicious treatment of disordered persons, retained the basic idea that the individual's behavior is caused by internal forces beyond his control. That people were then seen as possessed by mysterious internal forces of illness rather than by demons and devils does not alter the basically animistic nature of the concept. The major focus of treatment, as in demonology and exorcism, was on driving out, reducing, redirecting, or otherwise weakening the pathological forces within the individual. This animistic focus, in some form of face-to-face contact between the "possessed" and the "healer," has remained the cornerstone of contemporary psychodynamic theory and practice.

In the late eighteenth and early nineteenth century, an alternative concept, that of "moral treatment," developed. Its impetus came not from science, but from a broad, worldwide revolution which introduced humanitarian social ideals and reforms. Moral treatment, the application of humanitarian ideals to the organized group living of mental patients (Bockoven, 1963), did not utilize the mental "illness" concept, but, as Bockoven has noted; "The moral therapist acted toward his patients as though they were mentally well. He believed that kindness and forebearance were essential in dealing with them. He also believed in firmness and persistence in impressing on patients the idea that a change to more acceptable behavior was expected" (p. 76).

The late eighteenth and early nineteenth century was an extraordinarily turbulent period affecting all aspects of life, including psychiatry. One of the many developments that deserve mention is the emergence of what we have termed "Romantic Psychiatry" (Graziano, 1969), a set of ideas that was to have considerable significance a hundred years later.

The historical significance of Romantic Psychiatry has not been sufficiently recognized or explored, and at the risk of an apparent digression we are going to devote the next few paragraphs to it. The points to be made are only suggestive. Much more research and documentation are needed before a definitive treatment of the subject can be made.

Briefly, Romantic Psychiatry emerged in the early 1800s as part of the Romantic Protest, the nineteenth century's reaction against the revolution, rationality, and science of the previous century. The Romantic Protest emphasized man as an irrational organism; preached individuality, diversity, dynamic growth; objected vehemently to revolution, and to the attempts of science to marshal humanity into ordered columns of predictable patterns and quantities.

Perhaps the most influential idea of the romantic period is that of natural, organic growth unfolding toward an ideal—the universe is the result of long growth and development and, in order to be understood, must be viewed through knowledge of its past history. The natural growth process, it was thought, was not random, but evolved toward refinement and improvement. In Hegel, Leibniz, Pope, and Goethe's *Faust,* existence is growth, life is viewed as continuous striving, and the very meaning of life is seen as lying not in its goals but in the life process itself, in *being* and in *becoming.*

Growth, evolution, dynamic changes, and constant refinement to higher levels; historical-genetic science; feelings, emotions, the "inner man"; constant striving in the process of *becoming;* distrust of rationality as it might interrupt the natural unfolding of mankind and its institutions; deep introspection and internal experience—these were the hallmarks of the romantic era, and when these views influenced the treatment of behavior disorders, the emphasis was not on rationality, as it had been in the previous century, or on physical structures, as it was to be later, but on feelings and natural, internal, evolutionary forces which unfold as the person matures. Romantic Psychiatry emerged as a sometimes florid mixture of Calvinistic, even evangelical, morality, as in Heinroth's ideas or in Reil's "noninjurious torture" (See Zilboorg and Henry, 1941, p 287), along with enlightened humanitarian views and prophetic psychodynamic models.

As early as 1803, Johann Reil (1759–1813) asserted that "mental disease" must be treated by "psychic methods." Moreau de Tours (1804–84) emphasized introspection and distinguished between irrational, internal, subjective life and the objective life that results from communication with the external world. Dreams and insanity were believed to have the same basis; i.e., in both, a person's irrational, internal activity, unrestricted by reality, was in full control. The insane person was thus alienated from the world and perceived his own subjective forces as real.

Johann Heinroth (1773–1843) described three levels of psychological

functioning: (1) the basic level of instinctual forces, which are directed toward pleasure; (2) the self-centered and reality-oriented level, which functions through the intellect and aims to obtain security and enjoyment in the real, external world; (3) the "conscience," the highest level, which develops through differentiation out of the lower levels. The mentally healthy person, in Heinroth's view, is one who has fully assimilated the moral principles of his conscience within his otherwise self-centered, reality-oriented level. Likewise, mental disturbance is caused by the continuing dynamic conflict between conscience and unacceptable thoughts. Heinroth not only anticipated Freud's basic personality trichotomy, but also defined mental illness in terms of inner conflict and anticipated the entire psychodynamic view.

Other early Romantic psychiatrists, such as Alexander Haindorf (1782–1862), Frederick Groos (1768–1852), and Karl Ideler (1795–1860), emphasized internal drives and emotional conflicts which upset the total psychobiological functioning of the individual. Groos asserted that man is unaware of the physiological forces which determine his actions and which, if blocked, result in mental illness. Karl Gustav Carus (1789–1869) centered his thinking on the concept of the "unconscious," the depths of which must be known if the "psyche" is to be understood. Frederick Beneke (1798–1854) and Ernst von Feuchtersleben (1806–49) anticipated the modern psychosomatic approaches, asserting that ideas can be symbolically expressed in physical reactions and that mental illness is a disturbance of the whole psychobiological entity, rather than of "mind" or "body" alone.

By the early 1800s, some one hundred years before Freud's influence was felt, the early Romantic psychiatrists had emphasized man's internal, unconscious states, the totality of interactive psychobiological function, the reality of subjective experience, a psychodynamics of internal conflict, the natural development of higher levels of functioning out of an instinctual, physiologically based level. There were attempts, particularly by Heinroth, at an integrative model to both explain and provide treatment rationales for psychiatric disorders, and treatment by psychotherapy was emphasized. Beginning with the assumption that humans are basically irrational and driven by complex inner forces, Romantic Psychiatry in the period from about 1790–1850 developed models that largely anticipated those developed by psychoanalysis a hundred years later.

By the end of the nineteenth century, however, both Romantic Psychiatry and moral treatment were eclipsed by the subsequent strengthening of the "illness"model, through the assumption of dominance by the medical profession. As humanitarian reforms continued, the imposing fortresslike asylums of the nineteenth century evolved into mental hospitals which provided continuous medical care, strengthening the concept of mental "illness" and clearly defining its treatment as a medical respon-

sibility. The reassertion and subsequent full development of the medical model were a major factor in the demise of moral treatment. On this point, Ullmann and Krasner (1965) wrote: "There was a complete shift from the procedure of treating the patient as well and expecting more acceptable behavior to the concept that the patient was, by definition, a danger to himself and others" (p. 5). As a result, little other than passive cooperation was expected of the patient while he was custodially contained in order to receive treatment. The earlier focus on meaningful group living, which included social stimulation and pleasant, comfortable surroundings, was soon considered an unessential luxury that might even interfere with the "real" task of treatment.

In the last half of the 1800s, largely influenced by Dorothea Dix's earlier crusade to provide care for mental patients, big mental hospitals were constructed and American psychiatry became identified as organic psychiatry focused exclusively on *pathology* and applied in hospital settings. It was not long before Europeans were coming to the United States to learn about hospital administration. American psychiatry in the last half of the nineteenth century became a strictly organic approach to large numbers of adult "psychotics" who were detained en masse in huge mental institutions.

Early in the twentieth century, against the dominant background of organic psychiatry, there were many important developments in and around the mental health field. These have been well discussed by Boring (1950), Woodworth (1948), Zilboorg and Henry (1941), Reisman (1966), Levine and Levine (1970), and many others. Our intention here is to note three of those developments: (1) E. L. Witmer's psychoeducational approach to children and his ignored proposals for the development of a new clinical psychology. Beginning around 1894, as an outgrowth of his education in psychology, Witmer developed an amazingly modern clinical approach to children. Witmer's work was in complete contrast to the dominant psychiatry, which focused on the organic pathology of hospitalized adults. Witmer emphasized the "normal" adaptive behavior of the child, in the child's own natural environment. He utilized systematic, detailed, and programmatic teaching techniques, and enlisted the aid of significant adults in the child's environment to carry out a variety of creative remediation programs. Witmer's psychoeducation developed to sophisticated levels between the mid-1890s and the 1930s. This bare listing of some of the elements of Witmer's approaches seems much like a brief outline of the most up-to-date approaches to childhood functional problems. Unfortunately, Witmer's work has been largely ignored. (2) Behavior modification as a clinical approach emerged about the mid-1920s, but virtually died out by the early thirties. There had been, for example, the clinically relevant work of Haberman (1917), Jacobsen (1929), Watson and Raynor (1920), Burnham (1924), Kantorovich

(1929), and Holmes (1936). Further, the term *behavior modification,* as used with reference to applied issues, may have been coined by J. Stanley Gray, in a 1932 article entitled "A biological view of behavior modification." As discussed by Graziano (1970), Gray urged that educators disavow, in good Watsonian tradition, the focus on subjective, internal, teleological explanations and that they focus, instead, on behavior itself. He wrote,

> Such terms as insight, experience, feelings, willpower, consciousness, intuition, etc., unless they are interpreted to be forms of behavior, must not be used in the description of educational techniques and certainly not considered as educational goals. Educators must use scientific terminology if they are to be understood even by each other.

Gray asserted that teachers should avoid subjective concepts and, instead, should focus on the pupil's behavior, determining, for each pupil, *what* behavior is to be modified and then, with careful attention to details, controlling and manipulating all relevant learning variables in that environment. Gray maintained that any failure in learning is due to the failure of the educator to effectively manipulate the relevant learning variables.

More than forty years ago, behavior modification concepts had already been directed to the problems of maladaptive behavior, behavior therapy had been initiated, although not labeled as such, and behavior modification had been proposed as an appropriate model for the education of children.

Thus, by the late 1920s two clinical alternatives to organic psychiatry had emerged, each with extraordinary implications that are only now, some forty years later, being recognized. Almost immediately, they were overshadowed by psychoanalysis, Freud's revival and popularization of the Romantic Psychiatry of a century earlier. Both psychoeducation and behavior modification had originated in academic psychology, and that fact might have been a major reason for their lack of influence on the mental health field. After all, psychology was a newcomer with little established validity in psychotherapeutics and virtually no political power.

Freudian Romantic Psychiatry, popularized in this country around 1930, has been, in one form or another, the dominant conceptual model in mental health. Psychodynamic therapies have pursued complex systems of diagnosis and techniques of going "beneath" symptoms in order to ameliorate the underlying pathology. In the twenty years between the two world wars, anthropology, psychology, and sociology increasingly influenced psychiatry to define the underlying psychopathology of mental illness as a combination of orgainic, psychological, and sociological factors. Freud's hypotheses were modified and supplemented by a long line of theorists from several disciplines. Whatever the extent of these modifications, however, psychodynamic therapies continue to be based

on the "disease entity" model, emphasizing the primary importance of the underlying basic pathology and de-emphasizing overt behavior, which is considered to be only symptomatic. Whatever the variation, Freudian to Rogerian, all psychodynamic therapies maintain the basic evolutionary model of Romantic Psychiatry, and preserve the even older presumed relationship between the skill of the healer and the animus of the patient.

VII. THE REEMERGENCE OF BEHAVIOR MODIFICATION IN THE 1960s

However new behavior modification approaches may appear to be, they are, of course, descendants of a great deal of earlier work, such as the animal psychology of Thorndike, Hunter, Hamilton, Yerkes, and Small, and the Russian objective psychologists Sechenov, Behkterev, and Pavlov; Watson's strongly stated behaviorism; the mid-1920s forays into behavior therapy; and the later development of behavioral psychology by such researchers as Tolman, Hull, Guthrie, Lashley, Spence, and Skinner. Skinner's work has, of course, become increasingly important since about 1950, and has been considered by some to be "radical behaviorism." Generally supportive of this whole development has been the continuation of American objective psychology, since the late 1800s.

According to some writers, behavior modification emerged in the 1960s because of a favorable zeitgeist—i.e., "the time was right," "society was ready." Portes (1971), for example, argues in his critique of behavior modification that its success is not due to any greater validity than that of other models. Rather, he claims, its "rational simplicity . . . and greater claim to scientific status" make a "behavioristic conception" of man a "better fit" in today's socioeconomic structure. We will argue, however, that a good deal more than "buying"the concepts occurred.

As we have noted, behavior modification emerged briefly in the mid-1920s, was immediately eclipsed, and reemerged in the 1960s. It appears to us that this reemergence was the result of many developments that can be summarized as two main trends, both of which were given considerable impetus by the demands of World War II. Essentially, (1) the main conceptual models, Romantic Psychiatry and Organic Psychiatry, and the near-complete domination by the medical profession were weakened; and (2) the profession of psychology had grown both conceptually and in size, and was beginning to offer alternative behavioral concepts to explain human functioning.

Behavioral psychology, as an academic, laboratory-based study of animal behavior, continued to develop through the 1930s and 1940s. Investigators produced a great deal of research on increasingly complex issues of learning. Elaborate learning theories were formulated, and many con-

cepts of operant and respondent conditioning were applicable to human behavior.

There was not only important conceptual development but also increasing enrollment in the psychology departments of graduate schools, and a growing number of psychologists were trained in or at least becoming aware of the development of systematic approaches to learning. As the experimental field grew more sophisticated, tentative moves were made to apply learning principles to human behavior.

By the 1940s, however, although there were psychological concepts and manpower and tentative applications of learning principles to human behavior, psychologists, on the whole, had little impact on the mental health field. Their opportunity to enter the field in significant numbers was provided by the manpower demands of World War II.

Clinical psychologists, used at first to supplement military medical staffs as part of the essentially medical specialty in recruitment and service, accepted psychiatry's dominance, trained for and worked within the psychiatric periphery, utilizing the basic medical model. In the early postwar years, many of the most demanding problems were interprofessional political issues.

Clinical psychology, however much it became a "peripheral psychiatry" in the fifties, was and still is an applied professional discipline that is based on the empirical study of behavior. Ullmann and Krasner (1965) note: "Psychologists who were identified as research workers, such as Azrin, Bachrach, Bijou, Ferster, Goldiamond, King, Lindsley, Patterson, and Staats, turned their attention to clinical populations and problems so that the distinction between experimentalist and clinician became blurred." The influential work of Eysenck and Wolpe was already having an effect. By the early sixties, clinical problems were being looked at from the combined clinical-experimental vantage point, to become the basis for more systematic empirical and operational approaches to malfunctional behavior.

Perhaps the basic difficulty in contemporary clinical approaches based on the medical model and the major difference from the behavioral model, is the focus on internal conditions, all of which are abstractions and inferences, and none of which are well defined empirically. Clinical services, then, may constitute attempts to use nonvalidated methods to measure ill-defined constructs which have only incidental, haphazard, and casual relationships to facts. The reemergence of the more objective learning concepts as applied to clinical problems occurred as psychologists sought to clarify the vagueness of traditional psychodynamic thinking and to develop operationally defined and objective concepts, goals, and techniques. Work by French (1933) and by Sears (1936) had already begun to bring together Freudian and Pavlovian concepts, and perhaps

also helped to stimulate the now-classic work, *Personality and Psychotherapy* (1950), in which Dollard and Miller translated psychodynamic theory into contemporary Hullian learning theory. In this work, Dollard and Miller accepted much of psychoanalysis, including the basic interpersonal psychotherapy situation. They presented analyses which not only clarified existing psychodynamic theory, but also served heuristically as a point of departure, influencing several generations of students in psychology and stimulating thought and research increasingly toward learning theory concepts and, perhaps ironically, away from the psychodynamic theory which they had intended to support.

Dollard and Miller clearly emphasized the necessity for more rigorous scientific approaches in psychotherapy and personality theory and the importance of systematic theory construction for organizing empirical data and making predictions for future research. They made the laboratory and the clinic mutually accessible. This increased the probability that research psychologists would look more closely at clinical problems and thus helped make possible the more recent developments in behavior therapy.

As has been more fully discussed in our previous volume (Graziano, 1971), there is some controversy over how much Dollard and Miller may have helped or impeded the development of behavior therapy. In our view, however, they are among the major influences in the shift toward a more objective and detailed focus on the role of learning.

In the fifties, the growing emphasis on objective evaluation and manipulation of learned behavior was carried through by many investigators who focused on verbal behavior in psychotherapy. Through the systematic control of learning variables, such as verbal reinforcement, therapists attempted to modify the client's verbal behavior and thus to bring about changes in, for example, "self-concept." The focus on behavior provided a means of intervening in central cognitive or psychodynamic processes. Grossberg (1964) pointed out that a basic assumption in such verbal-learning approaches is that systematic changes in verbal behavior are necessary to bring about improvements in other maladaptive behaviors, and that such verbal changes in therapy do generalize to "real life" situations. This assumption, as he noted, had been challenged by Greenspoon (1962), Kanfer (1961), Bandura (1961), and Hobbs (1962). Grossberg concluded that there seemed to be little evidence that verbal and insight therapies, even when they used reinforcement theory, achieved much else than changes in verbal behavior during the therapy sessions.

The next major shift was to focus on the overt behavior itself, rather than on internal conditions, as the therapeutic target. In more recent work, the modification of behavior is the overall goal of therapy, whereas in Dollard and Miller and in psychotherapeutic verbal learning the emphasis on behavior seems to have been primarily the means to attain the

goal of changing a psychodynamically conceived core condition. Thus, in the past two decades psychotherapy appears to have moved away from the idea that central internal conditions give rise to overt behavior which is merely symptomatic and is important only indirectly. It has moved, first, toward a focus on learning theory and on overt behavior, including verbal behavior, as more objective, scientific means for achieving changes in the central condition; and thence to the view that learned behaviors themselves constitute the condition to be modified, and that there is no "underlying"central psychodynamic condition to be dealt with.

Salter (1949), Eysenck (1952, 1960, 1961), Wolpe (1954, 1958, 1961), and Wolpe and Lazarus (1966) all proposed similar views of neuroses as maladaptive habit patterns formed through conditioning which can be permanently and significantly modified through the systematic application of learning principles. Like Dollard and Miller (1950), these writers accepted a reinforcement, drive-reduction theory, but they differed in the importance they place on overt behavior. For Dollard and Miller, neurotic behavior is symptomatic of the internal neurotic condition; for Eysenck, Wolpe, Lazarus, and Salter, neurotic behavior is itself the neurotic condition.

Skinner (1953, 1956) regards the label "neurosis"as an explanatory fiction. For Skinner, people behave as they do, not because of internal and perhaps "distorted" motivations, but because of environmental contingencies which are subject to observation, control, and manipulation. Thus, human behavior can be significantly and permanently modified. Skinner went further than other writers in rejecting the existence of central conditions, making none of the assumptions about drive, motivation, anxiety, and inhibition which, consistent with both Pavlovian and Hullian formulations, were utilized by Eysenck, Wolpe, and Salter.

This growing shift from clinic to laboratory concepts and from psychodynamics to learning factors in the modification of behavior is also seen in statements by Pascal (1959) in his volume *Behavioral Change in the Clinic.* Pascal wrote, "If one is to extrapolate and speculate, the least one can do is to begin with some fairly well-known facts. The human organism behaves, and his behavior changes. These are data" (p. 1). And, "In any case, the findings of the psychologic laboratory are the only source of systematic knowledge bearing on behavioral change in the intact organism" (p. 4).

By the early 1960s, the application of learning theory principles to psychological intervention was being actively investigated and a good deal of work had already been completed. One of the most important reviews was that by Bandura (1961), who wrote:

> Psychotherapy rests on a very simple but fundamental assumption, i.e., human behavior is modificable through psychological procedures. When skeptics raise the question, "Does psychotherapy work?" they may be responding in part to

the mysticism tnat has come to surround the term. Perhaps the more meaningful question, and one which avoids the surplus meanings associated with the term "psychotherapy," is as follows: Can human behavior be modified through psychological means and if so, what are the learning mechanisms that mediate behavior change?

By the late 1950s, then, most of the elements had been gathered for the remarkable surge of behavior modification activity that seemed to appear so suddenly in the early 1960s. One more element must be described—a "spark," perhaps, that seemed to have set off what some have considered an "explosion" of activity. In the late 1950s, a most dramatic event occurred, the launching of Russia's first Sputnik, the first man-made earth satellite. One result of this event, a result unanticipated by Sputnik's makers, was the establishment of a precedent for the use of behavioral approaches to solve human problems. It seems to us that Sputnik dramatically sensitized the United States public to the potentials of behavioral models and quickly gave these early behavior modifiers socially legitimated roles and entry into important social settings, the schools. As with so many of the significant events in behavior modification, Skinner was right in the thick of this development. To explain, it is necessary to go back to the period in which behavior modification first emerged, the 1920s.

In the early twenties, Sidney L. Pressey, an experimental psychologist at Ohio State University, developed an automated *testing* device which, on subsequent investigation, he found to have considerable value as an automated *teaching* machine. Pressey argued that further development of such devices could lead to an advanced technology of education and to significant advances in instruction. However, despite the obvious promise of his early teaching machine, and despite the continued interest of Pressey and other investigators, such as Peterson (1930) and Little (1934), Pressey was largely ignored. He abandoned his project in 1932, noting, "The writer has found from bitter experience that one person alone can accomplish relatively little, and he is regretfully dropping further work on these problems. But he hopes that enough may have been done to stimulate other workers, that this fascinating field may be developed" (Pressey, 1932). That same year, J. Stanley Gray wrote "A Biological View of Behavior Modification," an article urging schools to utilize the concepts of learning being developed by psychologists. Little attention was given to these developments in the 1930s, and they submerged for about twenty years, reappearing in the mid 1950s largely with the work of Skinner. Lysaught and Williams (1963) suggest two reasons why Pressey's work was ignored. First, they note, Pressey had made no provision "for systematic programming of materials to be used in [his] machines, and, second, the onset of the depression and its impact on social conditions and education offered an unfavorable environment for an 'industrial

revolution' in the nation's schools" (1963, p. 6).

Laboratory work on learning continued and, inevitably, it was again suggested that learning technology be applied to the schools. In 1954, more than thirty years after Pressey, B. F. Skinner published the important paper "The Science of Learning and the Art of Teaching." Reminiscent of Gray's 1932 article, Skinner's paper described the direct application of laboratory-based behavior modification techniques to classroom learning and teaching. Subsequently, Skinner produced sophisticated apparatus, which were later named "teaching machines." A major difference between Skinner's teaching machines and Pressey's earlier apparatus is Skinner's addition of the concept of successive approximations, which leads directly to the systematic, step-by-step programming of material to be taught.

Almost immediately, researchers and educators became interested, and by 1960 a sizable experimental literature and a variety of applied hardware and programs had been developed. A number of books on programmed learning appeared (Lumsdaine and Glaser, 1960; Holland and Skinner, 1961; Foltz, 1961; Smith and Moore, 1962; Green, 1962; Lysaught and Williams, 1963). Publishers produced more than 350 new programs in 1963 alone (Hanson, 1963), and variations of teaching machine hardware seemed to abound. Work in this area continued amid a good deal of controversy that seemed much the same as that involving behavior therapy today. However, controversy notwithstanding, programmed instruction became a regular part of many school systems across the country.

Skinner's proposals (in 1954, 1958, and 1961) were made to a vastly different world than were Pressey's in 1924. Americans were on an economic upswing and in an "expansionist" mood after the shortages of World War II. More enamored of "technology" than ever before, we were quite willing to spend our money for it. This generally expansive socioeconomic situation and mood was quite different from that faced by Pressey in the depression years of the late twenties and early thirties. Further, Russia's success with the first Sputnik caused a rush in the United States to "search for an explanation of how the Soviet peasant under the Bolsheviks had stolen the march on the free American in a democracy... many were ready to assume that Soviet education had done the trick, that the solution for the United States lay in a 'crash' program for education" (Foltz, 1961, p. 4). The technology, hardware, and programs for automated instruction were there, and Americans seized them gladly, hoping for easy, technical solutions to the educational problems which had caused such loss of honor to what was, then, the world's most powerful and successful nation. By the late 1960s, when behavior modification in the clinical area had climbed the height of controversy and debate, behavior modification in the schools (i.e., automated learning)

had become an accepted part of curricula and had lost most of its controversial quality.

Behavior modification in the form of programmed learning in the schools was an important precedent for the slightly later surge of behavior modification in the form of behavior therapy. The later developments would probably have occurred in any event, but they seem to have been speeded up considerably by the prior emergence of programmed instruction in the schools.

In summary, behavior modification reemerged in the 1960s as a function of a number of developments. First, the increased number of psychologists and the increasing sophistication of psychological learning research provided personnel and conceptual strength. Second, the pressures of World War II on an inadequate mental health system allowed the rapid entry of psychologists into the mental health field. This new psychological—rather than psychiatric—climate was highly receptive of new psychological work, such as that of Eysenck and Wolpe, who were largely outside of this American development. Finally, the successful advent of programmed instruction in the schools constituted an important precedent for the slightly later development of behavior therapy.

VIII. CURRENT STATUS

In the less than fifteen years since the early 1960s, behavior modification has grown rapidly, as discussed in our opening paragraphs. The subset behavior therapy seems to have proceeded through a number of approximate stages: first, in the early sixties, the work was largely a matter of repeated demonstrations that clinically important behaviors of adults and children can be systematically modified. Despite their many methodological weaknesses, those demonstrations were quite convincing, and behavior therapists began to extend their new approaches to greater varieties of problem behaviors and to groups as well as individuals.

During the sixties, many early weaknesses were highlighted. One of the major problems that became apparent was that there was little evidence that modifications in maladaptive behavior were permanent. In fact, it became clear that, all too often, when the contingencies that had changed maladaptive behavior were withdrawn (e.g., when the therapy ended), the maladaptive behavior was reinstated. It was then generally realized that behavior change was only part of the therapy process, that behavior maintenance was at least as important, and that maintenance systems needed to be designed into behavior therapy. At the present time, the technology of maintaining new, adaptive behavior does not seem to be as well developed as that of behavior change, and a good deal of continued work is needed. But the field has at least recognized the issue and has been devoting attention to it.

By 1970, it had been amply demonstrated that clinically problematic behavior can be changed and that behavioral maintenance systems must be designed as carefully as behavior change systems. Early maintenance systems were essentially the continued application of therapeutic contingencies by persons other than the therapist. For example, in the late sixties and early seventies, parents were trained to continue the therapist's contingency management in the home, and teachers were trained to maintain environmental supports for desirable behaviors in the classroom. However, some therapists expressed concern over teaching parents, teachers, and others sophisticated control over children, pointing out that the goals of such control were not necessarily in the child's best interest. Further, a client, particularly an adult, would usually neither accept nor particularly desire the necessary degree of control by persons other than the therapist. Why not, then, teach clients to be in charge of their own behavioral change and maintenance programs? Self-control—learning to recognize and effectively manipulate the variables controlling one's own behavior—was not a new idea, but for the first time it was being looked at by many psychologists as a potentially valuable treatment paradigm. Thus far in the seventies, self-control has been a major and growing area of investigation by behavior modifiers.

Finally, in the past two years or so there has been a marked heightening of the old ethical issues, all grouped around the basic question, What are the conditions under which various behavior modification subsets can be operative without severe conflict with other value systems in our society? As we stated earlier, these ethical issues are not new. However they have acquired a new urgency as our technology has developed to more sophisticated levels.

In summary, after its reemergence in the 1960s, behavior modification has developed through several stages: (1) demonstration that clinically significant behavior can be systematically changed; (2) recognition that behavior maintenance systems must be carefully programmed; (3) development of self-control paradigms for children and adults; (4) increased concern over ethical issues that arise as behavioral technology grows in power and scope and is more likely to clash with society's other value systems. These developments have been approximately sequential, largely overlapping, and at present the field is simultaneously active in all areas.

In its general outline, the field of behavior modification is now nearly complete; i.e., conceptual, technical-empirical, and ethical dimensions are active—indeed, interactive. Still missing, of course, is our coming-to-grips with the political realities of mental health, and this, we believe, might be a significant development of the last half of the seventies.

In fifteen years, the field has matured greatly. In this writer's opinion, it now easily rivals the levels of sophistication attained by other, far older

therapy systems, such as Romantic Psychiatry in general and Freudian psychoanalysis in particular. By any measure, the growth of behavior modification and of behavior therapy has been remarkable. As we have indicated elsewhere (Graziano, 1973), the field gives every indication that it will continue to grow and to assume increasing importance in mental health, provided that political entrapment is avoided.

IX. CURRENT ISSUES

At present, two major models of psychological helping, psychodynamic (i.e., Romantic Psychiatry) and behavioral, each with several variations, coexist within a common mental health field, sharing public space and many common issues. Despite their commonalities, the two approaches are very different both conceptually and in application, and any overall integration into a single "psychodynamic behavior therapy" is highly unlikely at present. What appears likely, as we noted earlier, is their continued coexistence as two divergent, alternative models.

The strong reemergence of behavior therapy threatened disruption of the conceptual monopoly held by Romantic Psychiatry, and stimulated a growing literature critical of the "new" therapies. Forerunners were the dialogues between Rogers and Skinner (1956), which largely reflected the philosophical conflicts of the eighteenth and nineteenth century Romantic Protest. Ullmann and Krasner (1965), in what has become a classic reference for the field, discussed many of the major critical issues that were argued in the early 1960s. Breger and McGaugh (1965) wrote an important critical review that has since been followed by many others. As Mahoney, Kazdin, and Lesswing (1974) point out, the criticisms have changed as the field has matured. It is clear that by 1975 many of the earlier critics had modified their stand to a much more accepting position (e.g., Birk et al., 1973).

The major critical issues in the field range across (1) ethical, (2) social-political, (3) conceptual, and (4) empirical problems. The first two have already been discussed and will be only briefly summarized here.

Ethical Issues. These are not only modern forms of the recurrent concerns over "scientific" and "humanistic" conceptions of humanity, but, more important, have arisen as our expanded behavior modification subsets have come into increasing contact and, often, conflict with other value systems in our society. The conflict can be resolved and accommodations made (Graziano, 1975), and efforts to achieve this will probably increase in the next few years. One of the major ethical issues is that of therapeutic control—i.e., to what degree of power should one person or one professional system influence the lives of others in the name of helping? This issue of control applies to all therapy and is one of the

highest-priority problems that must be solved by the mental health profession.

Social-Political Issues. We maintain that political issues, above all else, will be the critical determiners of the social usefulness of behavior modification. Viewed against its voracious consumption of social and economic resources, the mental health profession's social contribution is questionable. The major reason for this, we believe, is the profession's entrapment in basically self-serving political modes of operation in which "safe" "acceptable," but nonproductive behavior by mental health personnel is reinforced. Behavior modification is providing alternative models and techniques for therapy, but nothing guarantees that it will avoid the "political power distortions" of the earlier approaches.

Our question, raised earlier, bears repeating: On what basis do we expect to be any more successful than our predecessors in avoiding the political power distortions that have already perverted the mental health field?

Conceptual and Empirical Issues. Several critics (Breger and McGaugh, 1965; Portes, 1971; Locke, 1971) argue that behaviorism, by virtue of its "simplicity" and its historical-philosophical position as a revolt against introspectionism, is inadequate to account for complex human functioning. Its "simplistic" concepts supposedly omit too much of human reality and are inadequate in application to any problems other than "simple behaviors and to noncomplex patients such as children, retardates, psychotic and autistic individuals" (Portes, 1971, p. 304). Because of its conceptual inadequacies, the critics argue, behavior therapy can be used "only analogically" (Breger and McGaugh, 1965), and in actual practice the behavior therapist's techniques are not at all behavioristic. Locke (1971), for example, argues that while Wolpe's formal theoretical system is behavioristic, in practice Wolpe and his colleagues "violate" behavioristic tenets by instructing their clients to "introspect" and "understand," to assess their "feelings" and cognitions, and by attempting to develop a therapy environment of "reassurance, support, encouragement . . . a trustful relationship", and to "make the patient feel fully accepted as a fellow human being." According to Locke, these mentalistic and emotional sensitivities have no place in behaviorism— and if they do occur, then, by definition, the therapy cannot be behavioral.

A related criticism involves the interpretation of the processes by which behavior therapy supposedly operates. Critics argue that a wide gap separates behavior theory from behavior practice and that if the techniques do work, it is not for the reasons given by behavior therapists (conditioning, etc.). Instead, they argue, the "action orientation," the verbal manipulations, and the implicit emotional support which occur in behavior therapy as well as the client's perception and understanding of

the information feedback, create cognitive and emotional changes within the client, and those are the events that account for whatever success the therapy might have. Behavior therapists, they maintain, have ignored those important cognitive and emotional processes.

It has also been argued that the empirical base for claiming effectiveness for behavior therapy is faulty—that the evidence cited has been drawn largely from poorly controlled case studies that are seriously confounded by sampling and observer bias. Where evidence does support behavior therapy's effectiveness, such effectiveness is said to be limited to relatively uncomplicated, discrete problems, such as simple phobias. However, even the obvious successes of behavior therapy have not been left unqualified, for a common and, surprisingly, still-expressed criticism is that even if behavior change is brought about, if the gain has been accomplished without sufficient attention to underlying motivation, then it will be achieved at the cost of symptom substitution.

All of the conceptual and empirical issues have their practical-applied counterparts; i.e., issues at the practical level of clinical application are essentially the operationalized forms of the ethical and conceptual issues discussed briefly above. At the applied level, behavior therapy is criticized as controlling and coercive; as not clearly behavioral, but implicitly utilizing cognitive and emotional inferences and techniques common to other therapy systems (e.g., emotional support); as depending for its practical success on cognitive and emotional changes; and as limited in usefulness to "simple," "noncomplex" cases. Such criticisms suggest that behavior therapy provides a set of possibly useful techniques, best used in some ancillary fashion with other, more complete therapy systems, and aimed at relatively simple clinical problems.

To summarize, major criticisms of behavior therapy have been:

1. The therapist's control over the client is excessive.
2. The "incompleteness" of the theoretical base for behavior therapy
3. "limits" behavior therapy to a number of clinically derived, possibly effective techniques which have "only analogic" relationship to behavioral theory, and
4. limits the application of behavior therapy to "simple" problems.
5. Cognitive and emotional factors are critical mediators in therapy but have been ignored or denied by behavior therapists.
6. Empirical evidence for the effectiveness of behavior therapy is based primarily on experimentally inadequate case studies and poorly controlled or too-simplistic laboratory analogues. The apparent successes of behavior therapy involve limited problems and
7. may be achieved at a great cost to the client because of the high probability that symptom substitution will occur.

The first criticism, the therapist's control over the client, has already been discussed, but the discussion bears repeating. We submit that this

is a crucial issue which applies to all psychological therapy systems. It is a gross disservice to limit this criticism to behavior therapy, thereby erroneously suggesting that other, presumably psychodynamic, therapies are control-free.

That response also applies to the second and third issues, the "incompleteness" of the theoretical base of behavior therapy and the "only analogic" relationship of behavior therapy to behavior theories. This writer knows of no "complete" or directly applicable theoretical base for any psychological therapy. To suggest that such completeness now exists, but outside of behavior therapy, is, again, a major distortion. Even the Freudian, Romantic Psychiatry model, generally held up as a more "complete" model, can be seen on close examination to be an extremely limited view of human functioning.

Consider the complexity of human functioning—physiology, sensation, perception, cognition, memory, motivation and incentive, varieties of learning processes, overt behavior, including highly complex verbal behavior, change through growth and development, the submergence of these complex and always-changing human systems within at least equally complex physical-social-cultural-historical environments and the astronomical order of all their interrelationships. Each of those subsystems is a complex area of study, and psychology at present can only begin to sketch out the interrelated nature of the whole functioning system. Recognizing such complexity, we must conclude that any current psychological model of personality or human behavior must be grossly incomplete, providing only sketchy suggestions of interrelationships and approximations of nature. When applied in any individual case, such models can at best be used only analogically—*as if* they faithfully represented reality—only as guides to help the clinician organize inferred and observed events about the client.

Out of this complexity, the Freudian model emphasizes one subsystem —motivation—and, even more limiting, only one level of that subsystem —unconscious motivation. The theory then proceeds to explain virtually all complex human function as resultants of unconscious psychic variables and all treatment as necessarily having to deal with those unconscious variables. When viewed against the complexity of human reality, the Freudian model stands out as a grossly narrow, limited, and, in every sense, simplistic notion that ignores the bulk of human complexity. To continue in this somewhat rhetorical vein, we suggest that for the grossly simplistic Freudian model man is much like an electronic robot—a stereotyped creature of compulsive rigidities moving blindly at the command of reified internal forces of which the robot is not even aware. Humans are far too complex to be explained by simplistic Freudian notions.

Any attempts by science to understand complexities of nature are essentially analogical and are limited to incomplete theoretical state-

ments and empirical observations. The concepts of behavior therapy are no less complete than those of any other currently available therapy or personality system.

The next three criticisms, that behavior therapy is limited to "simple" clinical problems, that it has ignored cognitive and emotional variables, and that it offers limited empirical evidence for success, were perhaps valid in the early 1960s, but do not apply to modern behavior therapy.

To categorize the schizophrenic behaviors of children and retardates as "simple" problems (Portes, 1971) is to admit having had little or no direct experience with those clinical populations. Assertion of the non-complexity of these problems is, simply, wrong. While there is agreement that a variety of behavior therapy approaches are effective with fairly circumscribed problems, such as phobic behaviors, no clear evidence exists that any other form of therapy is more successful in more "complex" cases. To what kinds of clinical problems can behavior therapy best be applied? This is an empirical question which continued clinical research will answer.

In the past ten years, behavior therapy has been applied to an increasing variety of clinical problems which are far more complex than the relatively simple and often unidimensional phobias. Behavior therapy now includes socialization and abstraction skills taught to hospitalized psychiatric patients, complex work skills taught to retardates, and the treatment of marital conflict, depression, a variety of thought disorders, and even "existential" neuroses (as reviewed by Mahoney, Kazdin, and Lesswing, 1974). Our present volume discusses a variety of childhood clinical problems as complex as those seen in most clinics. Critics might still argue that the application of behavior therapy to more complex problems may be possible, but that markedly diminished success can be expected as the clinical problems became more diffuse. That, of course, is an empirical issue, and thus far there seems to be no evidence to support the prediction. In fact, as pointed out by Mahoney, Kazdin, and Lesswing (1974), some recent empirical evidence suggests even "more pervasive improvements with these broader targets and procedures" (p. 22).

Clearly, modern behavior therapy cannot be charged with ignoring cognitive variables. That criticism is appropriate to a nearly "Watsonian" vintage of behavior modification, but it does not apply to modern behavior therapy. In the past decade, a major trend in the field has been the integration of cognitive and behavioral processes in behavior therapy, seen most clearly in the emerging self-control technology. Important here is the work of Homme (1965), Staats (1968, 1972), Bandura (1969), Kanfer (1970), Meichenbaum (1971), Cautela (1972), Goldfried and Merbaum (1973), and Mahoney (1974), as well as many years of clinical practice by Albert Ellis and many others.

Similarly invalid is the criticism that modern research in behavior

therapy is limited to single-subject designs or uncontrolled case studies. Again, that criticism may have been appropriate for the early 1960s, but it is no longer applicable. The flood of behavioral literature over the past decade has included notable progress toward more sophisticated, better-designed studies. Research in behavior therapy is at the very least as sophisticated, valid, and reliable as that in other areas of psychological therapy. As summarized by Mahoney, Kazdin, and Lesswing (1974), extensive evidence exists for the effectiveness of systematic desensitization, operant techniques, aversion techniques, modeling, covert conditioning, and self-control.

A still-frequent criticism of behavior therapy by psychodynamic practitioners involves the symptom-substitution hypothesis, which might be stated as follows: Granted that behavior can be modified, behavior is only symptomatic of the basic, underlying pathology. Altering the behavioral symptoms leaves the basic condition unimproved and leads to the formation of new symptoms. This symptom substitution concept can be stated as an hypothesis and empirically tested (Cahoon, 1968). It is instructive to note that the tenacious belief in symptom substitution has not been based on such empirical tests. Rather, the theoretical statement that symptom substitution must occur under certain conditions has been considered powerful enough to constitute evidence in itself. When one searches for evidence to support the hypothesis, very little is found (Meyer and Crisp, 1966; Balson, 1973). Many writers, including Rachman (1963), Wolpe, Salter, and Reyna (1965), Eysenck (1966), Ullmann and Krasner (1965, 1969), Bandura (1967, 1969), Hersch (1968), Yates (1970), Graziano (1971), and Mahoney, Kazdin, and Lesswing (1974), have discussed symptom substitution as a discredited hypothesis, unfounded in fact. Nevertheless, it remains a tenacious idea.

If symptom substitution does occur in behavior therapy (Meyer and Crisp, 1966; Balson, 1973), it occurs as the *exception* and not as the general rule. Overall, a good deal of evidence indicates that symptom substitution does not occur ordinarily and, to the contrary, that a good deal of generalized improvement can follow behavior therapy.

By and large, the major criticisms of behavior therapy over the past ten years seem to have included a general assumption that theoretical limitations, ethical issues, and empirical inadequacy do not apply to "traditional" therapy models. We have argued that that assumption is untenable and that those difficulties apply to all psychological therapy systems. We cannot accept the implicit assumption that because therapy models based on Romantic Psychiatry have been used more extensively for a far longer time than behavior therapy, they must have somehow already solved the problems that confront any "newcomer." The fact is that most of the serious conceptual, ethical, empirical, and political problems have not been solved by any therapy model, that the solutions are still ahead of us. In this regard, behavior therapy, perhaps more than any other

model, has incorporated empirical means to evaluate its own effectiveness.

In the opening article of the present collection of readings, London advises an end to "ideological" arguments and, instead, a focus on developing a workable, effective, empirically based technology of psychological helping. We accept this empirical focus but would add that there are at least three broad areas of necessary development for all models of psychological therapy. First, of course, continued technological development and empirical research is needed. Second the technology must be tested not only against empirical criteria but against the value systems of the larger society. This would include ethical, legal, and social tests. This point has been made by Reppucci and Saunders in the second paper in this collection. Finally, as we have argued in this introduction, if behavior modification is to be a socially useful technology, we must avoid the political entrapment that is endemic to the mental health field. That demanding goal will not be adequately achieved until we take it seriously and devote our problem-solving efforts to its realization.

X. THERAPEUTIC PARSIMONY

There is no disagreement among psychologists concerning the general principle that human functioning involves multi-level interactions of many complex internal and external systems. Further, no single psychological model claims to have identified and analyzed the totality of these complexities. However, among psychotherapists at least, this dual recognition of human complexity and professional limitations is too often clouded by loyalty to one particular model. As a result explanatory models that may be valid for some limited segment of human reality are in clinical application generalized far beyond their powers and are used to explain the whole of human complexity. Such overinclusiveness insidiously erodes psychotherapy models and is nowhere so presumptuous as in psychoanalysis—where, for example, the psychological life of a long deceased artist is supposedly revealed to the acute analyst by a single novel or a few paintings. If we have learned anything in our enterprise of psychotherapy, it should be that no individual can be reduced in essence to his single statement, whether it be a painting, projective test or response to a fear survey. Thus it is a major responsibility of all of us who presume to intervene in the lives of others to recognize that it is, indeed, a presumption and it must be tempered by an abiding respect for human complexity and a concomitant prudence when intervening in the human ecology of even one individual.

Given those admonitions I would like to outline briefly some general guides for behavior therapy that I have found useful, particularly in training students in behavior therapy.

At any one moment the client presents a complex, functioning whole, involving the interactions of many sub-systems (e.g., physiology, sensation, perception, cognition and memory, levels of motivation, varieties of incentives, current and past learning, chains of overt behavior, the controlling power of a complex external social and physical environment, and, particularly for children, developmental processes). Recognizing this complexity and the limitations of therapeutic skills, the therapist must ask, "For *this client* at *this time,* into which sub-system(s) and with what specific strategies can therapeutic interventions be best employed?" A related question is, "Do I have the requisite skills, or will the client be best served by referral to other services?"

These questions implicitly assume a systems model and recognize that the locus of intervention may be internal or external to the client. Intervention thus is determined by the configuration presented by that particular client at that particular time. Further, these questions suggest the necessity for continual monitoring of the effects of the interventions and, in time, given progress or its absence, the need to change the locus and types of therapeutic strategies.

With reference to the admonition of prudence, we suggest a concept of *therapeutic parsimony.* That is, given alternative hypotheses about the client's functioning and predictions of the effectiveness of various therapy strategies, the "simpler" explanations and strategies should be employed, monitored and evaluated. This assumes that the more inclusive interventions, simultaneously involving several sub-systems, will increase the chances for disruptive negative effects within the client's system.

Finally, there appears to be a tendency, particularly among new behavior therapists, such as graduate students in their first behavior therapy practicum, to limit their focus too narrowly to the client's systems of overt behavior, as if assuming that sensitivity to their client's emotions and requests for "understanding" and emotional support are exclusive to "relationship" models and thus have no place in behavior therapy. Our position is that psychological therapy begins with a personal, interactive social situation in which the generally expected human response of interest, sympathy, support, etc.—humane sensitivity, in general—is the *minimum* condition, the *very least* to be expected of the therapist. As such, we maintain, it must be preserved in behavior therapy. Perhaps the most important contribution of behavior therapy is its addition of systematic, objective strategies designed to have practical effects in the objective environment and hence in the ecology of the client. At its best, behavior therapy should provide the client with such humane sensitivity and should proceed far beyond that and provide practical, clearly-defined coping strategies that can be monitored, evaluated and, eventually, exercised independently by the client, under his or her self-control.

We maintain that the behavior therapist must learn and use not only the usual clinical sensitivity skills but, in essence, must exercise a "sensitivity-plus." These additional dimensions mark the uniqueness of behavior therapy and enhance its important contribution to advancing our therapeutic endeavor.

I

VALUE CONFLICTS: BEHAVIOR MODIFICATION IN NATURAL SETTINGS

Behavior modification applied to human problems emphasizes a strategic model of scientific empiricism, aiming for specificity of variables and operations, systematic measurement and recording of data and continued testing of effectiveness of therapeutic manipulations. Most modern psychotherapy systems share investments in those empirical values but in behavior therapy they seem most highly valued and centrally operative. The emphasis on scientific empiricism reflects the laboratory origins of behavior therapy and is what behavior therapists view as its greatest strength, distinguishing it from most other approaches.

This presumed strength, however, is also one of the major sources of current difficulty for behavior therapy. Simply, ours is a multivalued society and scientific empiricism is only one of many "believable" value systems to which many people, including behavior modifiers, maintain

intense loyalties. Value conflicts were thus inevitable when behavior modification moved from its laboratories into that multivalued "natural" environment and, as discussed in the main introduction, these conflicts have led to legal contests and have reached the national level of legislative action. The first four reprinted articles continue that discussion in greater detail, and their placement at the beginning of this book reflects our judgment that these issues are of critical importance.

Each of the four articles presents different aspects of value systems clashes; London focuses on philosophic issues and the professional rhetoric that has polarized around them; Reppucci and Saunders describe the conflicts created within the social structure of a residential treatment center; O'Leary, Poulos and Devine point out some of the conflicts that are faced by parents, for example, when they are counseled to adopt behavioral strategies that appear antithetical to their existing value systems (e.g., "reward equals bribery, and bribery is immoral"). In the final article of this section, O'Leary, Turkewitz and Taffel present evidence for the effectiveness of behavior therapy with children in a university outpatient clinic. However, their paper is presented in this section primarily because one of their findings bears directly on a major value systems clash; i.e., on the criticism that behavior therapy, with its emphasis on specificity, systematic procedures and quantification, is out-of-touch with humanistic values. This paper indicates that humanistic values were well maintained by behavior therapists.

This introduction to Section I has been brief because discussions of many of these issues have already been presented in the main introduction.

— 1 —

The End of Ideology in Behavior Modification

Perry London

When little "behavior modifiers" sit at their professional daddys' knees and ask, "Where did I come from?" they are usually told a story about the "principles of learning" that spawned them, the evil clinicians who left them to perish on a hillside, and the kindly shepherd doctors who found them, raised them as their own, and eventually restored them to their rightful position as the benefactors of behavior. When they grow up, confer their own benefits, and spawn their own little behavior modifiers, they become disabused of the myth. But not entirely—there remains, at the very least, a tradition of deferring to the principles of learning as the ultimate source of all good modifications and a parallel ritual of knocking psychoanalysts, Rogerians, existentialists, and general psychiatrists who have not yet mastered or endorsed the jargon of respondents, operants, and reinforcements.

Like most myths, this tale has been based on some true events and has proved useful for the promotion of behavioristic patriotism and for extending the frontiers of experimentation and the clinical and technical growth of the field. And it makes a good story. But enough is enough. The borders are secure now, the settlers are thriving, the dark untrammeled forests are trammeled and cut down, the stumps blown, the fields

Reprinted from *American Psychologist,* October, 1972, 913-919.

plowed. It is time now to build this domain, not defend it.

Building it requires three things: first, abandoning strife with other modalities and developing peaceful commerce to see what they have to offer and exchange; second, examining the constructs that underlie modifying operations and that give clues for the design of new ones; and finally, devoting ourselves to "criterion validity," that is, to building treatment methods that work. The entire field has been moving in these directions during the past few years, but only partly, I think, as a result of the intelligent pressures of people making these recommendations. The shift results also from technological changes in related disciplines and from attitude changes in the entire society that supports them. New equipment brings new possibilities for use and a new social readiness to use it, and the speed with which it gets advertised and talked about pushes advances in practice that no theory is likely to keep up with. This article argues that it is probably just as well.

For public purposes, behavior modifiers of the 1960s usually described their activities as logically inevitable corollaries of theorems or principles of learning and of ongoing discoveries about how they applied to disordered human behavior. Actually, there were only about three principles that they ever referred to, all of which can be reduced to one or one and a half principles—namely, that learning depends on the connections in time, space, and attention between what you do and what happens to you subsequently (Franks, 1969; Lazarus, 1971). In addition, the special application to human behavior was predicated less on ongoing scientific discoveries about learning or about people than on the idea that human neuroses are probably about the same as animal neuroses (Wolpe, 1958). The critical principles of learning involved had all been spelled out rather elaborately between 1898 and 1938, and the business about animal neuroses was posited by Pavlov in the same period. During that period, in fact, Pavlov, Watson, Jones, Mowrer, and Guthrie pretty much spelled out all of the major behavior therapy methods that became popular for treating people since 1958, with the possible exception of desensitization and operant behavior shaping (London, 1964). And they are exceptions only because the former is not derived from the usual learning principles, and cannot be, and because the latter is something everybody knew about anyhow but never applied ingeniously until Skinner laboriously spelled out its monumental implications.

EVOLUTION OF BEHAVIOR MODIFICATION

The early growth of behavior modification as a professional specialty was largely polemical and political, not theoretical, and most of its scientific hoopla evolved to serve the polemical needs of the people who made it up—not all of it, however, and not only polemical needs.

The study of learning for behavior therapists, in fact, was always more for the purpose of metaphor, paradigm, and analogy than for strict guidance about how to operate or about what it all means. Whatever value theory may have for dictating laboratory procedures, therapeutic operations have been essentially seat-of-the-pants affairs, and still are, because they address immediate practical problems that require solutions in fact, not in principle. The disregarding of principles to see what really works, in this connection, reflects the intelligence and scientific good sense of therapists. The search for principles to explain what works, on the other hand, reflects their integrity and their anxiety—integrity because only fools would accept their own results without question, anxiety because therapists seek principles as much to increase their confidence as to reduce their ignorance. People also look for principles to help fight intellectual battles. Conventional therapies could be assaulted for their ineffectiveness, but the basis for offering new ones had to be more than complaints about the old ones. And in the case of behavior modification, it had to be more than a simplistic appeal to "what works." Of all popular therapies, psychoanalysis in particular was based on a pretentious, respectable, and smart *theory*. It could not be challenged with less than a theory.

Learning theory was an obvious choice: first, because there is a history of suggested therapeutic applications of learning principles, from Thorndike and Pavlov through Mowrer and Guthrie; and second, because the heart of any *psycho*therapy is changing people's behavior without changing their body structure or gross functions—which means, in plain language, teaching them and getting them to learn.

Even so, the evolution of behavior modification did not go so neatly. It could not make much use of learning theory, except for the broadest principles of learning, because most of the principles either do not yet have any really systematic application to disorders, or because they are much more in dispute than naive students realize.

The polemics actually used attacked evident flaws in psychoanalytic and other psychiatric formulations, sometimes also getting a little professional jealousy of psychologists for doctors into the act. Their main points were (*a*) an attack on "the medical model"; (*b*) the insistence that the origins of disorder were in *learning* instead of biochemical or genetic events; (*c*) the proposition that effective therapeutics should treat symptoms instead of their causes, that is, that disorders are identical with their symptoms; and finally, (*d*) the demand that even the name of the game should be changed from psychotherapy to behavior therapy, so no one thinks we are mentalistic or unscientific, as other therapies obviously are (sic).

This description is not exaggerated at all. Eysenck's (1959) tabulation of "the more important differences between Freudian Psychotherapy and

Behavior Therapy," for instance, says that Freudian therapy is "based on inconsistent theory never properly formulated in postulate form," while behavior therapy is "based on consistent, properly formulated theory leading to testable deductions." Similarly, Freudian practice is "derived from clinical observations made without necessary control, observation or experiment," while behavior therapy is "derived from experimental studies specifically designed to test basic theory and deductions made therefrom [p. 67]." Franks (1969) is kinder than Eysenck, but even he says a decade later, "for most behavior therapists, the preferred sequence of events is from experimental observation to clinical practice. This may be contrasted with the approach of traditional psychotherapists, in which the sequence is often reversed [p. 3]."

These arguments are not entirely pointless, but they are not entirely apropos of anything either, except who is smart and who will get the last word. What makes them largely irrelevant is that explanatory concepts are only necessary to explain what is going on—and until you know that, you do not need a theory, at least not much of one. Such theory, at this point in this enterprise, has to be stretched to fit facts rather than tailored to them. And it is damaging in the long run because people may start believing it and making up nonsense to plead silly cases that confuse everybody and enlighten no one. Enormous time and space have been wasted in pious debates on irrelevant aspects of most of the popular polemic issues of psychotherapy.

Discussions of "the medical model," for instance, often contrast "learning theory" approaches to "psychodynamic" approaches. There is a real question about the relative utility of *organic* versus *dynamic* perspectives on some behavior disorders, but all learning theory formulations are, in fact, dynamic ones. Also, there are many medical models, and the general attack on "the" medical model is only applicable to the *infectious disease* model, and only usable against psychoanalysis by lamely borrowing against Freud's (1961) unfortunate statement about putting demonology inside peoples' heads. Medical *epidemiological* models should have some appeal to Skinnerians, in fact, with their emphasis on environmental determinants of disorder, just as psychological models of habit formation are good for understanding the course of some developmental or degenerative medical conditions like heart disease.

The controversy about (learned versus structural) origins of disorder is mostly pointless because it is mostly irrelevant to treatment. The behaviorist attack commonly assumes that a genetic or biochemical view of etiology is bad because if disorder is learned, then treatment must be a learning process. This is nonsense. Etiology and treatment have no logical connection in either direction. Anxiety, for example, may be the result of prior experience, but can be alleviated chemically; or it may be aroused by essentially chemical circumstances (like fatigue plus mild

arousal to danger), but can be soothed physically or verbally (Davison, 1968).

The only treatment question of relevance is that of the functional relationship between the problem and its solution. The process is what matters and nothing else.

The biggest polemic, of course, about treating symptoms versus causes has occurred largely because of the uncertain danger of symptom return or symptom substitution. Everywhere much confusion has occurred mostly because of confusion about the "medical model"—in this instance, about the exact meaning of symptom. If symptom means trouble, surely it should be treated. If not, maybe not. The polemic has been around the question of symptoms as "the main trouble"; this is because of the widespread belief, largely from psychoanalysis, in symptom return or substitution. (Buchwald and Young [1969] point out, incidentally, that some analysts, such as Fenichel and, later, Alexander, thought that symptoms should be treated directly.)

The real issue, in any case, is the consequence of the treatment for the person's condition—or his life. Sometimes too, the issue is prophylactic or preventive rather than ameliorative. This becomes clear by comparing the infectious disease and chronic ailment models in medicine. Behavior disorders are more like chronic ailments, where medical treatment generally aims at the symptoms only. The control of diabetes, on the other hand, is not treatment of symptoms, but their prevention. Is the shaping of behavior likewise? For changing the table manners of psychotics' ward behavior, maybe not; for molding children's manners, maybe so. The same question may be addressed to assertive training or to aversion training. Is teaching someone to talk back to his mother-in-law "reducing his anxiety" or preventing it from happening? Is teaching a homosexual man to attend to women when aroused and to feel repugnance for sex with men relieving his symptom or preventing a disorder (which does not exist legally until he "goes for" the man), teaching a habit pattern or unteaching one, changing a life style, altering a phenomenological field, shifting ego boundaries, redirecting id impulses or superego functions, or cathexis? And if he "feels" different or thinks about it differently, is it still behavior therapy?

These are silly questions. The change in the homosexual is all those things, depending on how you want to talk about them. Understanding the process is what matters, not belaboring the different styles of talking about it. The only important question about systematic treatment is that of the simultaneous relevance of the treatment technique to the person's manifest trouble and to the rest of his life.

Sometimes it accounts for both at once—as when he is suicidal. Sometimes it accounts for the rest of his life but not for the symptom—which is the behavior modifier's claim against insight types' preoccupation with

motivation. Sometimes it accounts for only the manifest trouble—which is the insight therapists' accusation against behavior modifiers, based on the specious presumption that everything in a person's life is integral to it.

All of these polemics were not so much meaningless as overdone. The assault on psychoanalysts and existentialists was too extreme; the scientific claims on learning were too grandiose (Buchwald & Young, 1969; Franks,1969). Changing from psychotherapy to behavior therapy may have been useful for people brought up on mind-body dualism, but for those of us who always thought the mind hung around the brain and that behavior meant "what happened," the distinction was graceless and gratuitous.

When you eliminate the polemics and politics and gratuities, however, what remains of theory to define the field and to tell you what it is about? Not a whole lot. The definition of the field either becomes very inclusive (Lazarus, 1971; Marks, 1971; Marston, 1970; Paul, 1969; Skinner, 1971) or very narrow (Eysenck, 1960; Skinner, 1963; Wolpe, 1968). This probably makes no serious difference to anything. As Kuhn (1962) said, "Can a definition tell a man whether he is a scientist or not [p. 160]?"

But what about theory? If behavior modification lacks theory, then is it not reduced to a technology rather than a science? Yes it is, and I believe that is what it should be, just as medicine is technology rather than science.

There is some dispute in the field about this point. The sides are well represented by Lazarus, who agrees with my view, and Franks, who does not. Lazarus (1971) said, in his latest book:

> The emphasis of the volume is upon techniques rather than upon theories. . . . Methods of therapy are often effective for reasons which are at variance with the views of their inventors or discoverers. Technical eclecticism (Lazarus, 1967) does not imply a random melange of techniques taken haphazardly out of the air. It is an approach which urges therapists to experiment with empirically useful methods instead of using their theories as a priori predictors of what will and will not succeed in therapy (Eysenck, 1957, p. 271). The rationale behind the methods described in this book is predicated upon London's (1964) observation that: "However interesting, plausible, and appealing a theory may be, it is techniques, not theories, that are actually used on people. Study of the effect of psychotherapy, therefore, is always the study of the effectiveness of techniques" [p. 33].

Franks (1969) argued, to the contrary:

> It would seem to be highly desirable for the therapist to aspire to be a scientist even if this goal were difficult to realize. To function as a scientist, it is necessary to espouse some theoretical framework. For reasons too obvious to detail here, this is true of the behavioral therapist. . . . How the behavior therapist practices (including his choice of technique, his approach to the problems of general

> strategy, and his specific relationships with his patient) thus depending both upon his explicit theoretical orientation and upon his implicit philosophical and cultural milieu [p. 21].

The argument that behavior modification should view itself as technology rather than as science is not meant to denigrate the importance of theory, either scientifically or heuristically. Scientifically, theory is valuable because it directs the systematic search for new information by interpreting and integrating what is already known. Heuristically, theory is valuable because it lays our biases out in the open and, by explicating them, makes it easier for us to disavow them when they are found to be inadequate.

The question with respect to behavior modification is twofold: First, is current theory very good (scientifically) in this case? Second, is any theory very useful for the current development of this field at this time? I think the answer to both questions is no.

In reality, behavior therapists never did have so much a theory as an "ideology"; in my own paraphrase of Bell (1960) and Tomkins (1964), "Ideology does not mean just ideas, but ideas to be acted on . . . [London, 1969]." What behavior therapists called theory actually served as bases for commitment or a rallying point for talking about disorder and treatment in a certain way and, more important, about action on it within particular sets of limited operations, that is, technical limits. In this case, the commitment is to the *functional analysis of problems.* And it is for this reason that the domain incorporated within the legitimate purview of the field becomes broader and broader and the polemics milder and milder —as it is recognized that even Rogerians and humanists and psychoanalysts may analyze some things functionally and act on them accordingly.

What results from the increasing functional analysis of problems is an increasing number of plausible methods for coping with them—until the proliferation of methods is tested and found to work, no theory is really very necessary to explain *why* they work. For practitioners, none may be needed even then, if they work well. In any case, theory will largely grow from practice (Lazarus & Davison, 1971) and practice from instrumentation, in the broadest sense; this will happen to behavior modification just like it did to psychoanalysis, but with two generations' more experience, the results, hopefully, will be more scientific.

The status of theory comes largely from the belief that technique develops out of theory, that is, that science underlies engineering. But this is only partly true even among very "hard" sciences, less so among "soft" sciences, like the social and behavioral sciences, and not at all true for many endeavors where the existence of the technological capacity and the practical need is what produces the technical application and, indeed,

what "nourishes" much of the theoretical development itself (Oppenheimer, 1956).

This does not mean that the alternative to rude theory is rude empiricism; rather, it suggests a close-to-the-situation functional analysis instead of a premature and precocious search for general principles from which to get overextended, or for a professional ideology to which to be committed. Such a development seems to be proceeding now, with practitioners using the techniques of behavior modification but declining the identity (Paul; 1969b), and scholars recommending heterodoxies like "technical eclecticism" without shame (Lazarus, 1971). The rest of this article spells out what this development means, so we will see clearly what we are doing, with the view to doing it better.

By deducing the intellectual condition of behavior modification from the technology it uses, there seem to be two kinds of conceptual schemes which characterize it. Borrowing from Price's (1971) book, *Abnormal Psychology: Perspectives in Conflict,* in turn derived from Kuhn's (1962) *Structure of Scientific Revolutions,* these are first, *metaphors* or analogies, and second, *paradigms* or models.

In the conventional classification of how much we seem to know what we are talking about in science, these concepts fit the lower end of the scale of certainty whose upper rungs are called "theories" and "laws." A scientific law, in other words, is an idea that seems absolutely to comprehend all of the facts it talks about; a theory is one that hopes to do so, but is not so compellingly known to do so. The activities involved in behavior modification are a good distance below both those points on the same continuum. A few are paradigms or models, that is, ideas that seem to explain a group of facts pretty precisely; most are metaphors or analogies, that is, ideas that look like they might fit a group of facts, where there is no clear evidence that they really do.

In general, the treatment methods derived from speculations about conditioning studies of animals are metaphorical or analogous, especially desensitization and implosion, or flooding. In general, the treatment methods that fall under the heading of education or training or, in jargon, of instrumental learning are paradigmatic or exemplary, including modeling, shaping, and possibly aversion techniques.

The difference between the metaphoric and the paradigmatic treatment is not in how well they work, but only in how well their workings are understood. This, in turn, reduces to how much of the mechanics involved can be predicted or explained with what degree of precision, which reduces still further to how many of the details are visible or can be inferred with different degrees of correlation. For practical purposes, the difference is simply one of the extent to which you can see what is going on enough to figure out how variations in your approach will affect

it. The more you can peer intellectually into the "black box" in other words, and the prediction proves correct, the more you are dealing with a paradigm rather than a metaphor. A paradigm, in other words, is an attempt to construct a direct model or example of how something works. A metaphor is a more oblique comparison, in which the flaws in the analogy are obvious, but the similarities are still big enough to allow us to ask whether the comparison might "work."[1]

Desensitization is only a metaphor or analogy to conditioning: first, because it involves a specific cognitive process (the use of language) that classical conditioning does not; second, because it involves a mechanism of sequential imagination that can only be guessed at from conditioning studies; and third, because it is subject to a variety of successful variations that could not be predicted from the situations from which the metaphor was derived in the first place. Finally, desensitization does the same thing that implosion or flooding does, though it appears to be an opposite method.

Implosion is a better analogy than desensitization because Stampfl's original experiment (London, 1964) seems to parallel both Mowrer's (1950) theoretical statement, Solomon, Kamin, and Wynne's (1953), and Black's (1958) animal experiments more closely than Wolpe fits Masserman's cats or Pavlov's dogs. Also, implosion technique does predict all of its own variants—including Watson, Gaind, and Marks' (1971) finding that practice helps to extinguish phobic reactions and that using real objects instead of imagination as fearful stimuli also is helpful—but it is metaphorical nonetheless because it is between people, and because it is administered in social situations where the pressure *not* to escape is obviously coming from the thoughts that the patient has about the situation, not from physical restraints on his action.

Shaping is more exemplary or paradigmatic than conditioning treatments because it presumes less, in the first place, on the black box—modeling, likewise. Stated differently, both shaping and modeling methods can aim for precision in establishing the conditions that will work the desired effects without having to worry much about what is going on in the person that makes them work. *Aversion* treatment is more paradigmatic than metaphoric because its critical stimulus is not cognitive; that is, it hurts the patient physically. Giving a person an electric shock is much more like shocking a dog than telling scary stories to a person is like blowing air up a rat's behind; ergo, it is more paradigmatic than metaphoric.

Neither metaphors nor paradigms are scientific theories by some distance, but both of them are useful intellectual tools. *Metaphor* and *analogy*

1. Oppenheimer (1956) distinguished analogy from metaphor because the "structural similarity" of analogous events implies more precision in their comparison than is ordinarily true of metaphoric comparisons. I am putting them together here, however, to distinguish them both from paradigm.

are good heuristically—they help to turn images into thoughts and inchoate hunches into articulate propositions. *Paradigms* are more literally useful—they model to scale what you need to do, or give a mathematical formula into which you can put specific quanta.

Notice, however, that none of this has anything to do with whether anything works for any practical problem. Desensitization may be a poor deduction from conditioning, but it is a fine treatment for phobias. Behavior shaping may be an excellent paradigm for getting autistic children to hug people, but it does not teach them syntactical speech.

Whatever scientific concern people have about the intellectual status of the field, at this point in the development of this enterprise, none of this material is yet very important to practitioners, and it is barely important to clinical research. What *is* important, at this juncture, is the development of systematic practice and of a technology to sustain it. My thesis is that, in the long run, scientific understanding will derive from them.

The practitioner tries to figure out what will work, for the most part, and then, if he is scientifically inclined, looks for a way to rationalize it if it does. The technologist devises machinery to support him. This is not a bad idea, even if it doesn't add up to test-tube-pure, white-coat science. Systematic desensitization is a good example of this process because, by "working" in the first place, it has given rise to technological refinements of itself that also work and that still leave the reasons unexplained. In the long run, this must force better and better logical and experimental confrontations with the reasons for the phenomenon—which success alone would not have done.

New equipment, new drugs, new gimmickry and gadgetry should now be the basis for systematically developing new methods of behavior modification and for streamlining the established techniques with controlled experimental testing. Instead of looking for new principles, or justifying worn-out ones, we should look for new applications: What could we do to treat such-and-such if we had such-and-such machinery? What would be required to build it? To test it? And then, finally, to determine what it means?

The critical point is that *good technology always undermines bad theory.* "Bad" is not meant to be pejorative here; it means metaphoric. Precise technology reveals the empirical error in metaphoric reasoning and in the operations that result from it. So Lang's (1969a, 1969b) on-line computer removes the therapist from the desensitization process; Quirk's (1970) machine removes the imagining and the relevance of the phobic subject matter; and Wolpin's (Jacobs & Wolpin, 1971; Wolpin, 1969; Wolpin & Raines, 1966) removes the need for hypnosis, for the fear hierarchy, and, indeed, for relaxation. Since the method still works, the need for explanation becomes more and more apparent (Davison, 1969), as does the inadequacy of the very plausible metaphor that launched Wolpe (1958) on the whole business.

Technology promises, in fact, more and more to turn metaphor to paradigm, if not paradigm to theory, by going directly into the black box and doing funny things to it and, increasingly, by bringing the black box out into the open where we can see more and more what has been happening in it. For the former, I refer to drug research and to the work in progress on electrical stimulation of the brain (Cohen, 1969; Delgado, 1969; London, 1969). These activities undercut specious distinctions between learned and unlearned patterns, either by manipulating the patterns out of hand or by potentiating learning so effectively that the issue of its importance disappears.

Bringing the black box out into the open is one of the main results likely to accrue from *biofeedback* research, and it is probably the best illustration one can find of technology feeding science with one hand as it enhances practice with the other. All of the biofeedback methods work on a single principle, whether they teach people to ring buzzers from a switch in their heads or to alter their blood pressure or heartbeat or skin resistance by monitoring lights and guages. The principle is finding a means of accurately externally recording some internal process, then of projecting that record into the person's consciousness so that he can literally see or hear what his blood vessels or brain waves or heart muscles are doing. Then let him learn to manipulate his conscious sensory experience in whatever way people learn to do anything consciously—something about which we really know next to nothing.

The clinical value of biofeedback methods remains to be seen and will certainly not be what alpha-machine hucksters seem to wish—instant cool for overheated psyches. Its clinical utility is not yet the point, only its technological sophistication and the scientific potential that comes from that.

To turn the point around, I am saying that theory has worn itself out in behavior modification and that technology, essentially of treatment, should now be a primary focus, perhaps, in the long range, even for serving scientific purposes. What began with the Mowrers' quilted pad in 1938 is extending now into therapeutic on-line computers, electric pottie chairs, electronic skin braille, and an almost self-generating brace of valuable hardware for therapeutic purposes. A summary of much of it will appear soon in a book by Robert and Ralph Schwitzgebel (in press) entitled *Psychotechnology.*

But the proper development that I am suggesting is not limited to hardware and is not self-generating, but reaches to the systematic exploration of all kinds of therapeutic things without inhibition or concern as to whether they fit ostensible *principles* of learning, or reinforcement, or whatever, but with a singular focus on whether they fit the *facts* of human experience. No one has been more forthright or resourceful in this connection than Lazarus (1971), whose latest work, *Behavior Therapy and Beyond,* goes into such things as imagery exercises for depression;

thought control, straight out, for whatever it works for; hypnosis for all kinds of things; exaggerated role taking à la the late George Kelly; differential relaxation; covert sensitization for aversive imaging; and Masters and Johnson's penis squeezing for premature ejaculation. It would take a genius or a madman to shelter this whole array of techniques under the intellectual umbrella of conditioning, or learning, or any other single psychological theory. It would take a fool to want to. The first issue, scientifically as well as clinically, is the factual one—Do they work? On whom? When? The how and why come later.

With the era of polemics virtually ended, and Skinner (1971) citing Freud over and over again in his latest work, and the American Psychoanalytic Association at a recent convention casually incorporating behavior modification into its discourses on psychotherapy, the political utility of learning theory, so called for the definition of the field, is ended. It was never really theory anyhow, as we used it, but ideology for professional purposes and mostly metaphor for clinical ones. It is time now, I think, for the remedial branch of this business to stop worrying about its scientific pretensions, in the theoretical sense, as long as it keeps its functional nose clean, and to devise a kind of engineering subsidiary, or more precisely, a systems analysis approach to its own operations. We have gotten about as much mileage as we are going to out of old principles, even correct ones, but we have barely begun to work the new technology.

— 2 —

Social Psychology of Behavior Modification: Problems of Implementation in Natural Settings

N. Dickon Reppucci
J. Terry Saunders

The frontiers of behavior modification are changing. Five or six years ago, the central questions surrounding behavior modification focused on its application with individuals (e.g., Ullmann & Krasner, 1965). The primary questions then were whether behavior modification was as good as, or better than, traditional forms of treatment (Paul, 1966); whether behavior modification was really a placebo effect (Davison, 1968); whether behavior modification merely eliminated symptoms, leaving behind the basic neurosis (Baker, 1969); and similar issues. While it is not out intent to suggest that these issues have been resolved in any ultimate sense or that research in these areas has ceased, the issues have at least received wide exposure and are now part of the established concerns surrounding behavior modification. Although questions about behavior modification on the individual level are still to be resolved, a major new frontier of interest has opened up. This new frontier is centered around the application of behavior modification techniques to populations larger than one in natural settings (e.g., Ayllon & Azrin, 1968).

As with any emerging field of interest, this social application of behavior modification is characterized by certain anomalies. One of the most noticeable of these is the difference between what is real in the field and

Reprinted from *American Psychologist*, September, 1974, 649-660.