MARTIN T. ORNE

PSYCHOTHERAPY IN CONTEMPORARY AMERICA

ITS DEVELOPMENT AND CONTEXT

American Handbook of Psychiatry

PSYCHOTHERAPY IN CONTEMPORARY AMERICA

ITS DEVELOPMENT AND CONTEXT

Martin T. Orne

e-Book 2015 International Psychotherapy Institute

From *American Handbook of Psychiatry: Volume 5* edited by Silvano Arieti, Daniel X. Freedman, Jarl E. Dyrud

Table of Contents

PSYCHOTHERAPY IN CONTEMPORARY AMERICA: ITS DEVELOPMENT AND CONTEXT[1]

The practice of medicine has sometimes been described as the art of making the right decision without sufficient knowledge. While great clinicians often intuitively transcend what is definitely known, the effectiveness of a field as a whole nonetheless depends upon the state of scientific knowledge. In order to illustrate the relationship of science to art in the context of psychotherapy, I ask my residents to imagine that they need their gall bladder removed and could somehow choose their surgeon from among Hippocrates, Paré, Billroth, or a second-rate surgical resident from a second-rate medical school in a second-rate hospital today. I then ask them to contrast *this* choice with the choice they would make if selecting their psychotherapist, given the option of Hippocrates, Rush, Freud, or Adler, or a second-rate resident in psychiatry from a second-rate medical school in a second-rate hospital today. Needless to say, their choice for the gall bladder operation is invariably in favor of today's medicine. Their choice of a psychotherapist, however, rarely favors contemporary practice.

In all healing professions the great clinician is preferred to the mediocre practitioner. Nonetheless, the measure of what is really known in a field is whether the midget of today can stand on the shoulders of the giant of yesterday, thereby becoming more effective than the great practitioner of the

past. Though the role of art is likely to remain important in all healing professions, it is fair to ask why psychotherapy (which after all is oft-times described as old as man himself) has failed to develop a readily transmitted body of cumulated knowledge that can ensure the competence of its average practitioner. Perhaps some of the reasons will become clear as we reexamine the nature of psychotherapy, its historical development and underlying assumptions, its role both as a form of treatment and as a social force in contemporary American society. We will examine some of the forces in the development of psychotherapy that have ultimately led to inherent contradictions and to a number of crises, and, finally, we will consider the dilemma faced by the individual psychotherapist as he studies the therapeutic process, tries to evaluate its effectiveness, and seeks to integrate systematic observations into the practice of the clinical art.

Background

The Context of Treatment

Since time immemorial, man has sought to develop means of coping with maladies that afflict him. To the extent that the treatment of priest, shaman, or other primitive physician equivalents does not depend upon specific physiological effects acting upon the true cause of the illness but nonetheless leads to improvement, the process is often considered a primitive

form of psychotherapy (belonging to the general category of faith healing). How a condition is treated in any society depends upon the group's relevant concepts of causality. Primitive cultures recognize simple injuries and can identify some fractures; most have some concept of immobilization and even the setting of bones. On the other hand, primitive cultures vary widely in the degree to which they accept naturally occurring disease as an explanatory mechanism to account for the symptoms of illness. The Hopi, for example, explains symptoms primarily as due to witchcraft (Simmons, 1942). To him, diagnosis is thus a matter of identifying the kind of witchcraft, and therapy is the practice of some appropriate and counteractive magic. Not surprisingly, the culturally defined categories of affliction also determine whether the individual perceives himself as suffering from a malady. Thus, a large number of individuals in many primitive cultures suffer low-grade parasitic infections without being aware that they have a problem. The possibility of *healing* arises only when there is a realization that there is something out of the ordinary to which some cause must be ascribed. While the explanatory principles evoked by folk medicine often seem quaint or naive, if not ludicrous, some primitive treatments contain potent drugs as active ingredients (e.g., quinine, rauwolfia).

The same principles that determine how a physical malady is to be treated, or whether for that matter it is even categorized as such, apply to functional disorders. The same set of behavioral symptoms that one culture

ascribes to spirit possession and treats with toleration arc explained by another culture as witchcraft that requires the immediate attention of a shaman. Yet another culture might consider the behavior as deviant and unacceptable and punish it severely. What matters is how a culture interprets behaviors that we regard as manifestations of functional disorders. Depending upon how the culture explains their occurrence, they will be dealt with by either some therapeutic intervention, by social sanctions, or perhaps by benign neglect. If treatment is called for, the healing process (whether it "works" or not) invariably depends on a generally agreed upon diagnosis, verified and legitimized by an individual whose skill in such matters is generally recognized and is usually based on extensive and arduous training. Frank (1961) has emphasized a number of important similarities among the broad range of techniques employed to treat functional disorders in a wide variety of social settings. These include the importance of a socially defined treatment, usually of a dramatic nature, often accompanied by a heightened state of arousal, and a shared belief system that provides a rationale for both the disorder and the treatment, making possible renewed hope for relief.

The Nature of Psychotherapy

It is difficult to arrive at an acceptable definition of psychotherapy. *Dorland's Illustrated Medical Dictionary* (Arey, 1957) defines it as "the treatment of emotional and mental problems" without specifying the remedy,

and therefore it is clearly too broad: it includes psychopharmacology, psychosurgery, as well as Christian Science and faith healing. Somewhat more circumspect, Webster (Guralnik, 1970) and Noyes and Kolb (1959) define psychotherapy as "the treatment of emotional and mental problems by psychological means." This definition specifies that *both* the disorder and the techniques of therapy be *psychological* which, while more satisfying in some ways, tends to exclude the treatment of organic illnesses such as ulcerative colitis, peptic ulcers, asthma, and so on, in which psychological factors may play significant roles. In another sense, one might well want to define psychotherapy as the treatment of disorders by psychological means, focusing only on the modality of treatment rather than on the disorder itself. This has generally been the definition implicitly accepted by psychiatrists who treat psychosomatic as well as psychiatric problems.

None of these definitions is truly adequate since they are overinclusive in some regards and fail to make important distinctions in other regards. For example, the application of psychotherapeutic techniques has been extended to helping individuals deal with psychological components of physical illness, habit disorders, deviant but not necessarily ill behavior, and facilitating emotional growth and development.

In actual practice psychotherapy does not define itself solely either by the nature of the disorder it tries to treat or by the specific procedures it

employs in so doing. In contrast to all other medical-treatment procedures that are defined by *what* is done to the patient, psychotherapy is defined more by *who* does it—by the role relationship and the training of the therapist. An appendectomy is an appendectomy regardless of who performs it, whether a surgeon, a general practitioner, or a paraprofessional. On the other hand, interactions that are behaviorally indistinguishable from those which occur during psychotherapy are referred to as "good teaching" if they occur between a student and his teacher, a "heart-to-heart talk" if between a child and its parent, "rapping" if among college students, and "good business" if between a bartender and his customer. It is not the specifics of the interaction but the context, the purpose, and the social infrastructure that in fact define it as *psychotherapy.*

Specific and Nonspecific Factors

One attribute not emphasized in most definitions but nonetheless crucial is the therapist's belief that his method has *specific effects* on the underlying disorder that is treated. Such effects must have their roots in some systematic, causal-belief system about the nature of the disorder and its therapy. This point has been important to physicians since medicine first became a legitimate field of study. Physicians distinguished themselves from quacks on the basis of their rational approach as opposed to the faith-healing aspects of the quacks' endeavors.[2] Though the effectiveness of faith healing

was often recognized by the physician, and the importance of nonspecific factors was already recognized by Hippocrates, these were differentiated from specific treatments. The practicing physician was generally willing to take advantage of the beneficial effects associated with the patient's faith in him and his treatment, but he saw such factors as ancillary to the specific treatment itself.

The distinction between specific and nonspecific treatment factors is particularly crucial to the development of psychotherapy and well illustrated in the history of hypnosis. Franz Mesmer was highly successful in treating a broad range of disorders. He also showed an intuitive awareness that appropriate patient expectancies, and suggestion, were important factors in treatment. He believed, however, that his cures were the consequence of a specific therapeutic agent, which he called "animal magnetism," and he vehemently rejected the possibility that "suggestive mechanisms" (that is. nonspecific factors) played a major role in his treatment. Indeed, it has been documented that Mesmer was aware of somnambulism (as later described in detail by his student, the Marquis de Puysé-gur), but refused to concern himself with this and related phenomena because he wished to study the scientific aspects of animal magnetism—in other words, what he viewed to be the specific therapeutic agent (Engel, 1956; Merton, 1948).

Similarly, the Royal Commission which investigated Mesmer's claims

focused exclusively on the question of whether animal magnetism was real, a claim that it appropriately rejected. In the process of the investigation it was recognized that many patients derived a great deal of relief from the procedure and that their ailments were often greatly ameliorated. Furthermore, it was acknowledged that Mesmer was at times able to induce anesthesia; however, both the cures and the anesthetic effects were explained as the consequence of "mere imagination"; in other words, not different in kind from other forms of faith healing. Since the theory of animal magnetism was thus refuted, the effects of the treatment were rejected as "mere imagination"—the eighteenth-century equivalent of nonspecific factors. It is no doubt relevant that eighteenth-century science ("the Age of Reason") in an effort to free itself from religion and mysticism, considered causality exclusively in physical terms, and could not ascribe causal status to psychological phenomena.

Two hundred years later we know only too well that the empirical distinction between nonspecific faith-healing effects (which in psychopharmacology are conveniently subsumed under the concept of the *placebo effect)* and specific therapeutic effects is difficult to draw. One needs to be as much on guard against the possibility of being deceived by a placebo response as against rejecting a treatment that is in fact specific.

Imagine a nineteenth-century scientific inquiry into the treatment of

dropsy by a witch's brew that includes such choice ingredients as bat wings but also an extract of foxglove (albeit picked at the new moon). Such an inquiry might well have rejected all claims of the potion's effectiveness because the rationale of why it worked could not stand up to close scrutiny. Yet many years were required before the active ingredient was identified, and much has yet to be learned about the mechanisms of action of digitalis. In fact, in this instance the question of therapeutic effectiveness—so carefully skirted in the investigation of Mesmer— provided the basis for continued interest.

In other cases, later analysis showed that despite a very plausible rationale, there was no specific treatment after all. Consider the widespread use of internal mammary artery ligation in the treatment of angina pectoris as recently as 1958. This by no means innocuous surgical intervention was based on an eminently reasonable rationale and initially yielded dramatic results. It was only after a careful, systematic double-blind study (Cobb, 1959) was carried out that it became clear that the therapeutic effects could be totally accounted for as a placebo response.[3]

It is difficult to fully appreciate the ubiquitous nature of the placebo effect. Recently Evans (1974) reanalyzed a large number of studies that had compared active analgesic drugs and placebos. In several double-blind studies where aspirin had been compared with placebos, the effectiveness of a placebo was found to be roughly 54 percent that of aspirin. On the other

hand, when a far more potent drug —morphine—was compared with a placebo, again in double-blind studies, placebos still turned out to have approximately 54 percent of the effectiveness of morphine. Thus it seems that the strength of the placebo response depends in large part upon the physician's belief in the effectiveness of the active drug being administered. Thus, the placebo given in a double-blind study along with morphine proved to be a far more powerful placebo than that given in a double-blind study with aspirin. The effectiveness of the placebo in other studies was determined by the context of physician expectations in which it was administered.

An individual's response to a placebo is incidentally quite independent of his response to hypnotic suggestion. Thus, a hypnotic suggestion of analgesia administered to hypnotizable individuals leads to a considerable increase in the pain threshold, but this increase is uncorrelated with the same individual's placebo response. On the other hand, the hypnotic suggestion of analgesia administered to *un*-hypnotizable individuals will also lead to an increase in the pain threshold that, while smaller, is nonetheless significant: in this group the increase *is* highly correlated with their response to placebos (McGlashan, 1969; Orne, 1974). It may therefore be appropriate to speak of a placebo response that occurs in the context of hypnosis but is independent of and separable from the specific effect of hypnotic suggestion. Thus, hypnotic suggestion, usually conceived of as a nonspecific effect, can be quite specific after all.

The difficulty in distinguishing between placebo effects and specific pharmaceutic action in physical medicine is great enough, but it becomes immense when we try to evaluate the effects of psychotherapy.[4] In part, the problem lies in the prior difficulty of defining both psychotherapy and the disorders to which this form of therapy tries to address itself. Thus, any meaningful test of the distinction between specific and nonspecific factors presupposes some initial decisions on these crucial but unresolved issues. To see these problems in perspective it will be helpful to consider how our modern concept of psychotherapy has evolved, and the extent to which we can meet a minimal requirement: to specify what psychotherapy is *not.* Until we can do at least this, it will be well-nigh impossible to answer questions about specific psychotherapeutic effects.

Though no definition of psychotherapy is currently shared by all those who claim to practice it, it is clear that the term, however, is clearly used both in a broad and in a more restricted sense. In the broadest sense, the term can be used to describe all forms of nonphysical treatment, and in this generic sense it includes procedures ranging from advice, habit training, coercion, and persuasion to interpretation, the analysis of associations and dreams, the ventilation of feelings, abreaction, and all those procedures generally subsumed under behavior therapy as well as suggestive therapeutics and faith healing. In contrast, most psychotherapists use the term psychotherapy in a more narrow and more technical sense—at least some of the time. To

them, psychotherapy refers to a method designed to alleviate specific difficulties through the use of specific therapeutic procedures practiced by highly skilled professionals. Psychotherapy seen in this fashion did not evolve until the end of the nineteenth century, and it is with the ramifications of this development that we are most concerned in this chapter.

Psychotherapy Within Medicine

The Origins and Legitimization of Psychodynamic Therapy

While the importance of psychological factors and their effects on disease were recognized by the ancient Greeks and had been commented upon by physicians through the ages, reassurance, support, and even counseling about personal problems were seen as an ancillary part of medical treatment. The practice of psychotherapy could not become a medical specialty in its own right until it was seen to go beyond these general common-sense principles. The study of hypnotic phenomena in the early nineteenth century came close to providing a specific method of treatment, and the increased recognition of functional disorders helped identify the maladies to which it could most appropriately be applied. However, despite a flurry of interest occasioned by such controversies as those of the Salpetriere and Nancy schools, the apparent similarities of hypnosis and suggestive therapy to faith healing probably prevented their widespread acceptance by

medicine. Though some few neurologists enjoyed good reputations and seemed to have considerable success in the treatment of functional disorders, the practice of psychotherapy as a truly independent enterprise could not develop until it was conceived of as a specific treatment, based on principles other than suggestion and not readily inferred through common sense. The formulation of such principles required, in turn, the evolution of the concept of psychological causality. It remained for Sigmund Freud to develop the rationale necessary to make psychological treatment a specific therapy.

Freud and Josef Breuer's early psychotherapeutic efforts employed hypnosis in a novel way as a means of exploring unconscious memories. The existence of such memories and their effects had been recognized previously and had played an important role in the thinking of several of Freud's contemporaries (Whyte, 1960). What was crucial about Freud's studies in hysteria from our point of view was the emphasis on a specific mechanism: in his view the effect of psychotherapeutic treatment was caused not by suggestion but rather by the recall of unconscious repressed material and its interpretation.

Hypnosis had proved to be an effective technique to facilitate the recall of material; nonetheless, Freud eventually abandoned it in favor of free association. The reason he originally gave was the difficulty of inducing hypnosis in all patients, an explanation that hardly explains why the

procedure ought not to be used for those individuals who are able to enter trance. Undoubtedly his decision was determined by various additional factors such as a growing awareness of the mechanisms of transference and countertransference. Contrary to some assertions, Freud was highly sophisticated and skilled in the use of the hypnotic technique, (Kline, 1972) but whatever his reasons for abandoning hypnosis, his decision to do so was probably crucial for the eventual acceptance of psychoanalysis as a specific form of therapy. The sole reliance on free association as an apparently rational procedure for investigating repressed memories made it more plausible that his results were due to a specific therapeutic effect, and this clearly distinguished them from the therapeutic success of others who relied on "suggestion." He developed a theory, couched in the mechanistic metaphors of the day, that discussed the effect of psychic energy—libido—and tried to show how these energies would be appropriately reallocated as repression was lifted. He outlined a developmentally oriented rationale for the etiology of neurosis, analogous to the characterization of physical anomalies of development by the emerging discipline of embryology. Though his insistence on the sexual etiology of all neuroses caused great controversy, he nonetheless succeeded in distinguishing between the enterprise of psychoanalysis, on the one hand, and either faith healing or common-sense advice and persuasion on the other. This distinction is crucial to the concept of psychotherapy as it has evolved over the years, and by drawing it Freud

created a new discipline.

These comments should not be taken to deny the substantive merit of his incisive clinical observations, particularly as they relate to the analysis of transference (Gill, 1972), the recognition of countertransference, the mechanisms of defense, and the significance and multidetermined causality of symptoms. Nor do we minimize his contribution in adding a new form of clinical inquiry as a primary data source. The intention here is neither to question nor to evaluate critically the contribution of psychoanalysis, but rather to indicate its crucial historical importance for the acceptance of psychotherapy as a specific treatment —quite independent of its scientific validity.

Though the medical profession was slow to recognize the significance of Freud's contributions, his theories provided a radically new framework for perceiving man. The impact on the arts and the humanities was rapid and dramatic. Psychodynamic concepts address themselves to the nature of man and his basic motives. These concerns are central to the writer, to the artist, to the anthropologist, and also to the historian and the social philosopher; though the psychological causation of illness had long been recognized by the great poets, it now became the focus of much artistic work and literary criticism. The acceptance of psychodynamic thinking had profound consequences for the artist's view of man, and changed the focus of interest

from what an individual does to why he does it. These same insights helped to brand many of the values, beliefs, and perceptions of the Victorian era as false and artificial. Thus, altruism became self-serving, patriotism an excuse for a variety of infantile feelings, loyalty became suspect, and so on—a reevaluation of attitudes that undoubtedly gained enormous impetus from the general disillusionment that followed World War I. The influence of Freud's ideas outside of medicine was probably even greater than that within it; we are probably too close as yet to fully appreciate their effect. If Marx created Economic Man, Freud created Psychological Man (Rieff, 1956).

The impact of psychoanalysis on society at large was probably of considerable import for the development of psychotherapy itself. The medical establishment refused to bestow its recognition, but this recognition was accorded by significant segments of the public so that until the Second World War most patients who sought psychoanalytic aid did so directly without referral by other physicians. Indeed, Freud seriously contemplated establishing psychoanalysis as a separate discipline, undoubtedly influenced by the hostile attitudes of many of his medical colleagues in Austria. It is interesting to note that only in the United States was psychoanalytic training limited almost exclusively to physicians, and it was here that psychoanalytic thinking was most readily integrated into psychiatry.

Freud’s impact on contemporary American psychiatric thought has been

so great that it is difficult to imagine how there could have been any American psychiatry without him. It is worth remembering, however, that until the 1940s the practice of psychotherapy—in the sense of *dynamic psychotherapy*—hardly played a significant role in the day-to-day work of either the state hospital psychiatrist or the neuropsychiatrist. Psychoanalysis was taught outside of academic channels in separate institutes, and psychotherapy was, with a few notable exceptions, largely taught by psychoanalysts. Most residencies in psychiatry included no formal training in psychotherapy, and the practice of psychotherapy did not become the principal occupation of psychiatrists until relatively recently.

The Basic Tenets of Psychodynamic Therapy

This is not the place to discuss the details of psychoanalytic thinking or techniques, but only those views about the nature of the psychotherapeutic process that have become generally accepted by virtually all psychodynamically oriented therapists (that is, by psychotherapists who, whatever their different theoretical persuasions, accept the notion that psychopathology, and indeed many facets of normal personality, are rooted in the "dynamic" conflicts of intrapsychic forces). These beliefs are, for the most part, shared by the patients who seek psychotherapy; they have, in fact, become part of the belief system of those subcultures within contemporary society from which the overwhelming bulk of patients for psychotherapy are

drawn. These views contrast sharply with the concepts underlying the practice of other branches of medicine. Most psychodynamic therapists feel that these beliefs must be understood and accepted to make psychotherapy a meaningful and rational enterprise.

An admittedly incomplete list of such basic tenets might include the following assertions (whose validity, or for that matter even testability, are not at present under discussion):

1. An individual is never fully aware of the reasons for his feelings and actions.

2. All behavior is multidetermined and motivated.

3. Symptoms are expressions of important psychological needs and motives. They satisfy these motives in a variety of ways but usually without the patient's awareness. While often initially obscure, when understood they generally turn out to be important and meaningful communications by the patient to those around him.

4. Symptoms are an expression of an underlying difficulty and ought, therefore, not to be suppressed. Instead, what requires treatment is the underlying problem.

5. Appropriate treatment involves making the patient aware of those unconscious motives which play a dynamic role in his adjustment. This growing awareness will ultimately lead to

psychological growth and development on the one hand and the elimination of specific symptoms on the other.

6. The process of treatment requires the patient to honestly express his feelings and thoughts to his therapist. He must not hold them back or censor them, regardless of how unacceptable, demeaning, or frightening he may find them. These feelings and thoughts often include some that could not be expressed under other circumstances. They must be shared with the therapist even if, or rather especially if, they concern the therapist.

7. The therapist's role is to help the patient understand, but not to suggest or to give advice. He is to help the patient grow by helping him become aware of what he really wants by bringing into consciousness motives, feelings, and ideas that were not previously available.

8. Among the various techniques that are used to facilitate the therapeutic process are encouragement of free expression, free association, the interpretation of dreams, appropriate inquiry into feelings and thoughts associated with various experiences and events, and the analysis of discrepancies between what individuals do and the reasons they provide for their behavior.

9. The process of therapy is seen as a mutual enterprise, analogous to a learning experience rather than to other forms of medical treatment. Its effects are dependent upon the active participation of the patient and the changes within his

awareness that follow insights derived from treatment.

10. The process of therapy will involve considerable effort and some suffering on the part of the patient. He is likely to feel worse before he feels better, and some sessions will involve the experience of intense dysphoric affects.

The Concept of a Definitive Treatment

In addition to these general beliefs about treatment (for a more extended discussion, see Orne and Wender [1968]) there are also various shared beliefs about the nature of psychological difficulties. According to most psychodynamic therapists, various symptoms reflect basic underlying personality problems that, in turn, are best understood developmentally. Psychoanalysis is usually seen as that process by which these developmental difficulties can be *definitively* cured, analogous to the way a surgeon is able to treat physical anomalies. It is important to recognize that the (psychodynamically oriented) psychotherapeutic ethos postulates not only the mechanism that produces the malady and the process that heals it, but further asserts that its treatment is definitive rather than supportive and that, when cured, the patient is, in principle, cured once and for all.

The view of therapy as a definitive, once-and-for-all treatment was probably very significant in leading to its acceptance by both the lay public and the medical profession in the United States (Orne, 1968). It would seem

that such a view of man was highly consonant with the Protestant ethic (Weber, 1930) at a time when fundamental religion had ceased to be an active force in many lives. A value system that holds that change is basically good, that man is infinitely adaptable and capable of growth, that everyone can and indeed should grow up to he successful and prosperous, and that the failure to achieve in such a manner is sinful, provides a ready climate for a therapeutic system that ascribes problems to faulty development and promises to rectify such faults definitively once and for all. It hardly seems accidental that psychoanalysis and psychodynamic psychotherapy became accepted in those areas where the Protestant ethic had been firmly established, but was largely ignored in those areas which held to a more traditional Catholic view of the world. After all, if man is seen as unchangeable and his job is to find his proper place in a fixed universe and make his peace with God, if poverty and illness are givens and accepted, to be bravely endured with proud humility, and if character is seen as largely predetermined at birth, it hardly seems reasonable to expect psychotherapy to be effective.

It is interesting to note that the same country that supports the greatest number of psychotherapists *per capita* is also the country that, more than any other, believes in man's infinite capacity for self-improvement, be it through higher education, correspondence courses, or primers on "How to Win Friends and Influence People." This same claim of nearly limitless human

plasticity is a cardinal tenet of early behaviorism, a theoretical posture that took America by storm during the twenties and thirties and is still a dominant force today:

> "Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist, merchant-chief and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors." (Watson, 1924, p. 82).

Watson and American psychodynamically oriented therapists undoubtedly disagree about the means whereby man can be altered, but that he is alterable both believe, and believe without question. The fact that America so readily accepted both behaviorism and the psychoanalytic view seems related to this one basic tenet that both views share.[5]

Given the Protestant ethic, it was important that psychotherapy was seen as leading to a genuine change, for it could then be conceptualized as a definitive treatment rather than as some kind of crutch. The former seems worth suffering for; the latter is seen as degrading and leading nowhere. The promise that psychotherapy, however long, can ultimately produce a person who is truly well seemed to justify whatever was required.

A different view of psychotherapy was recently put forward by Frank, who suggested that the problems might be conceived of as analogous to the

common cold (1968). If this were so, it would not prevent the patient from catching another cold at a later time, even if the treatment was originally effective. This position readily explains the difficulty of documenting the effectiveness of treatment, and such a view is congruent with many clinical observations, but most therapists find it unacceptable, probably due in part to discomfort at the thought of practicing anything other than definitive treatment. Indeed, many therapists believe that most patients share their own bias: that they would prefer a course of treatment that extends over several years, but results in a definitive cure, to an intermittent form of therapy that produces equal symptomatic relief hut requires them to consult a therapist three or four times a year for the rest of their lives. This is not a matter of faith in the efficacy of a relatively small number of ongoing visits—even if both patient and therapist were to believe that both procedures are fully and equally effective, the two forms of treatment must inevitably produce two different ways in which both participants see the patient. In the one case, the patient is defined as a person with a continuing and never-ending need for the therapist; in the other, the patient is defined as someone who will eventually become well. The implications of these attitudes for the practice of supportive therapy are considerable.

Psychotherapy and the Medical Model

We have noted that the tenets of psychodynamic psychotherapy make it

a procedure that, in many respects, is radically different from other forms of medical practice. Over the years considerable controversy has developed as to whether the medical model is appropriate for psychotherapy. Both the theoretical and pragmatic importance of who shall practice psychotherapy, how', and why, can hardly be overrated. The question of whether it is a medical treatment, a form of education, a method of behavioral engineering, a religious practice, or simply a personal service (in effect, a commodity) are issues where the conceptual model, scientific fact, the societal value system, and pragmatic public policy must intersect. While these various factors often seem independent, they are not. The kind of research that is carried out will modify the conceptual model, the kind of practice that exists may modify the kinds of research, and the patterns of both practice and research will necessarily be affected by public policy. Both the underlying rationale and the practice of psychotherapy have broad implications; in consequence, it will be carried out differently in different social systems. In our own society, it has been profoundly affected by political considerations as well as by the value systems of various social groups.

The Historical Relationship Between Psychotherapy and Medical Practice

We must consider the role of the physician in society to understand why it was so important that psychotherapy was originally perceived in a medical

context. In modern society, illness is defined as a medical problem, diagnosed and treated by physicians. A broad range of deviant behaviors are treated rather than punished, if it can be demonstrated that they were the direct consequence of a physical malady. For example, extremely violent behavior is not prosecuted, given the diagnosis that it is consequent to a brain tumor; and it is medical opinion that determines whether the tumor does explain the patient's behavior. Similarly, if a soldier falls asleep on guard duty or refuses to fight, he is excused, if accepted medical opinion holds that his actions are the result of illness. Again, it is a physician who decides whether an individual is ill enough so that he cannot work, when he is ready to return to work, and so on. Just as medical decisions affect the attitude of our social institutions, so they do our own: medical opinion forms the basis for our decision to view a particular behavior, in others as well as in ourselves, as reprehensible or as sick.

The physician's role as the final arbiter of when an individual is ill, and of what actions can be explained and excused because of this illness has long extended to psychiatry. Indeed, one of the alienist's major responsibilities was to advise the courts whether an individual was suffering from mental illness, to help determine competence, and, finally, to arrange for commitment if this seemed necessary. The profound legal and social implications of the psychiatrist's decisions were as readily accepted as the pronouncements of any other medical specialist. His competence was

legitimized by his medical training and experience and by his adherence to the approach of medical science in reaching his conclusions. While, in retrospect, one can undoubtedly see many inadequacies, it is probably fair to say that his judgment was the best available, and its probability of error was no greater than that of other medical judgments, a point easily overlooked in present-day criticisms of past (and perhaps present) psychiatric practice.

A further factor is the patient's belief in the therapist's competence, a belief more readily accorded to physicians than "laymen." The importance of this belief in the therapist's competence to provide help is, after all, a common thread in all psychological healing. That this competence derives from scientific rather than from religious credentials has been crucial to the further development of the field, especially since psychotherapy was accepted most eagerly by those segments of society for whom religion had ceased to be of preeminent significance.

Finally, the medical role provided a tradition of ethics for the therapist that was generally very helpful. The medical tradition demanded that the physician treat all sick individuals, even those society might have condemned. Regardless of his personal attitude he was expected to provide the best care he was able to give. Within such a framework it was accepted by the physician and society at large that it was his obligation to alleviate suffering and preserve life. This ethos allowed the psychotherapist to avoid explicit value

judgments about his patients and to develop a point of view that holds moral judgments in abeyance and seeks to evaluate causality, an approach often held to be particularly conducive to the psychotherapeutic process. This attitude was facilitated by the already widely accepted view that transactions between doctor and patient are confidential, and that the physician's first responsibility lies with the patient.

The uncritical acceptance of the medical model ultimately involved psychotherapy in some contradictions. Since treatment was carried out by physicians, the person treated was by definition a patient, resulting in a doctor-patient role relationship with all of its sociological implications. It soon became clear in the context of psychotherapy that it was the patient's responsibility to participate actively in the process of treatment. But the therapist nonetheless accepted responsibility for the procedure, since the age-old tradition of medical responsibility was taken for granted by all concerned—the patient, the therapist, and the community. On the one hand, the ethos of psychotherapy emphasized that it was vital that the patient make decisions, accept responsibility, decide what is best for him; on the other hand, the physician never abrogated *his* obligation to look out for the patient's best interests and never really abandoned the implicit assumption that *he* knew what was in the patient's best interests. These inherent contradictions have been eloquently emphasized by Szasz (1961).

The Conceptual Implications of the Medical Model for Psychotherapy

Psychotherapy evolved not by way of the alienist and the mental-hospital psychiatrist but rather through the treatment of psychoneurosis by "nerve specialists." These were neurologists to whom patients with unexplainable disorders were referred, and they developed the criteria by which functional illness was distinguished from neurological problems. They tried to describe psychopathology in a manner analogous to the description of organic pathology. In so doing they provided a medical model not only for the initial evaluation of the patient's symptoms but also for the schemata by which the therapist might understand the nature of the underlying disease. Much of the appeal of psychoanalysis derived from its assertion that it was the science that sought to do for psychopathology what organic medicine had so successfully done for the treatment of organic pathology. Freud made extensive use of structural metaphors in the development of his theories. The descriptions of the id, the ego, the superego, the conscious, the unconscious, and the preconscious have a quality that might lead a naive reader to localize them within the brain. Similarly, in discussing psychic development he writes as if he had discovered psychobiological processes analogous to those which were being discovered in embryology. Again, he discusses the organs of ego functioning as though these were analogous to the physical organ system; the libido theory is discussed in a manner analogous to hemodynamics. Yet

another example of this postulated parallelism between mental and biological mechanisms is the implied analogy between unacceptable, and unconscious, ideas that fester until brought into the light of day, and infections by foreign bodies that are handled by surgical incision and drainage.

Thus, Freud and his followers saw themselves developing a scientific basis for specific treatments of psychopathology, and this view remained largely unchallenged by other dynamic psychologists regardless of the technical differences between theories that engendered so much affect within the field.[6]

By the end of the nineteenth century the extension of the medical approach to the treatment of functional ailments was unquestioned. Even the rather virulent disagreements among schools of psychotherapy had an acceptable historical analogue in disagreements between medical authorities about physical ailments, which did not in and of themselves undermine the prestige of the various medical practitioners.

One often overlooked consequence of this historical development is that psychotherapy based its legitimacy not on its outcome but rather on its scientific rationale. The early disputes among the various psychodynamic views are polemics about the nature of the postulated mechanisms, hardly ever about the more concrete issues of technique or outcome. Only rarely did

the parties to the disputes refer to the success of their treatment as evidence for the validity of their theories. The early history of psychodynamic psychotherapy clearly reflects the peculiar dual attitude of medicine: on the one hand, it is an empirical art whose practitioners use their best judgment to employ whatever treatments have been shown to he effective; on the other hand, it is a scientific discipline whose members try to identify the underlying mechanisms of disease in a rigorous scientific fashion.

It was an ambitious undertaking indeed to build a science of psychopathology by constructing the mental analogues of physical processes. The resulting system was familiar to medical practitioners who recognized the analogies to the biological and physical sciences of the day. Interestingly enough, the link to psychobiology was not kept up to date; it was a link to scientific conceptions of the late nineteenth and early twentieth centuries that was never abandoned and inevitably became a fettering chain. While the brain sciences evolved and developed less ambitious but testable formulations that facilitated and, in turn, were facilitated by continuing laboratory research, the conceptual categories of psychodynamic therapy remained rooted in the late nineteenth century. This surely is one major reason why it has been so difficult to integrate new findings from other sciences that deal with brain and behavior with the constructs upon which psychotherapy was based.

In sum, psychodynamically oriented psychotherapy has been rooted in medical traditions and has continued to work with a translation of the conceptual framework that dominated medicine at the turn of the century while (with the notable exception of the psychosomatic movement) it became increasingly alienated from physical medicine, on the one hand, and relevant biopsychological research on the other.

Beyond the Medical Model

The last three decades have seen a series of major challenges to the whole system of beliefs and practices that constitute the core of psychodynamically oriented psychotherapy. These eventually led to the development of several alternate models of therapy. Historically, these challenges began as two questions were raised ever more insistently during the forties and fifties: (1) who should practice psychotherapy, and (2) does this practice have any effect?

We will begin our discussion by considering these issues as they were treated during the two decades following World War II.

Who Should Practice Therapy?

Within the psychoanalytic movement it was soon recognized that the technical aspects of the therapeutic method had no direct relationship with

other forms of medical practice and that medical training, oriented as it was toward organic pathology, might even interfere in some ways with the development of an appreciation for subtle psychological factors. Furthermore, it was evident that some highly gifted nonmedical individuals were eminently capable both as practitioners and theorists in the new discipline. This potential readiness to accept lay practitioners was strengthened by the fact that psychoanalysis was taught outside of universities and did not depend upon other medical specialties for referrals or consultation. It was only the integration of psychoanalysis into the mainstream of American psychiatry during the 1940s that kept it, and thus psychotherapeutic practice, within the confines of medicine for many years. Much the same happened in Europe when psycho-dynamically oriented psychotherapy was subsequently reintroduced there under American auspices, but eventually a number of forces combined that served to legitimize psychotherapists with other professional backgrounds.

With the increasing acceptance of psychotherapy as the appropriate form of treatment for psychological difficulties, it soon became clear that the number of therapists was insufficient to meet the demand. As long as psychotherapists were expected to be psychiatrically (and preferably psychoanalytically) trained physicians, the problem simply could not be solved. The relative shortage of physicians in the United States provided a limited pool from which potential medical psychotherapists could be drawn.

However, the increased emphasis on psychiatry in medical schools, the ready availability of training stipends for residents, and the greatly enhanced social and economic prospects of future psychiatrists served to attract many young physicians to this specialty. Furthermore, special training stipends for established physicians made it possible for them to acquire specialty training, an option a good many physicians availed themselves of later in life. While the number of psychiatrists rose, the public demand for their services rose yet more. If one considers the relatively small number of patients who can be seen by psychiatrists in long-term treatment, a continuing scarcity of medical psychotherapists was well-nigh assured.

In child psychiatry, nonmedical therapists had long been accepted as competent practitioners—largely due to the orthopsychiatric movement. Treatment was typically carried out in collaboration among psychiatrists, psychiatric social workers, and psychologists. Well-trained psychiatric social workers were recognized as competent therapists. Initially, there was considerable conflict about the appropriateness of psychologists as psychotherapists. Some highly respected medical psychotherapists did, however, emphasize the competence of nonmedical professional workers and, despite official pronouncements to the contrary, there was a progressive inclination toward their acceptance, especially in institutional settings with a surfeit of patients and a shortage of staff.

Independent of medical psychotherapy, a different psychotherapeutic tradition evolved by way of the guidance-counseling movement in schools. Academically trained guidance counselors had credentials as scientific experts based on their work with objective tests that were designed to help individuals choose appropriate professional careers. The acceptance of this group initially came by way of the educational establishment and the widespread use of psychological tests in both school and industry. In the context of adjustment to various difficulties in school, many counseling psychologists had long carried on psychotherapeutic activities with troubled students. The work of Carl Rogers represented a major change of focus. Initially called "non-directive therapy" (1942) and subsequently "client-centered therapy," (Rogers, 1951) it was rooted in entirely different, largely humanistic, traditions from those of psychodynamic therapy. Rogers argued that man is essentially healthy and that to help the "client"—not the patient—recognize and effectively deal with his difficulties, it was necessary only that the therapist create a context of positive regard in which to reflect the client's feelings. Rogerian therapists saw themselves as contributing to the client's growth and development, as strengthening his feelings of self-worth and helping him to better cope with his problems. In sharp contrast to the medical psychotherapists, Rogerians neither attempted to diagnose the underlying pathology nor did they feel called upon to take medical responsibility.

More or less concurrent with the growth of the Rogerian movement

(just after World War II) there was a general upsurge of interest in clinical psychology. Many academic psychologists became interested in applying both psychological techniques and theories to clinical problems. The problems that interested this group were different from the earlier traditions of clinical psychology, which had applied laboratory methods to the diagnosis of special deficits. In principle, these academic clinical psychologists were sympathetic to many (though certainly not all) of the basic premises of the psychodynamic approach. Their object was to put dynamic psychology on a rigorous scientific basis. As a consequence, several gifted scientists in outstanding academic settings made serious efforts to relate psychoanalysis to facts and theories developed by academic psychology and by the social sciences, using methods and concepts from each of these.[7]

These academically trained clinical psychologists also had applied interests. Initially they concerned themselves with the development of various diagnostic tests, especially projective techniques, that were quite different from the aptitude and intelligence tests created and perfected by the American testing movement between the two world wars. These projective methods (such as the Rorschach, the Thematic Apperception Test, sentence completion, and so on) were widely used as a means of rapidly evaluating the interplay of dynamic forces within the patient. Their focus was not so much on traditional descriptive diagnosis but rather on diagnosis in psychodynamic terms that were directly relevant to the psychotherapeutic process.

Considering this focus it is hardly surprising that the interest in and emphasis on projective tests came to the fore in the period following World War II. Ultimately, however, these clinicians wanted to do more than help in diagnosis: they wanted to participate in the therapeutic process itself. With progressive insistence they sought training as psychotherapists. The eventual acceptance of psychotherapists who were clinical psychologists rather than physicians was partially based on the value many medical therapists attached to the contribution of clinical psychology to diagnosis in what was seen as a quasi-objective evaluation of psychodynamic factors. Many therapists hoped that psychological tests would ultimately provide them with information analogous to that which the pathologist gives to the surgeon. Because of these contributions, the psychologists' demands to take clinical training in institutional settings were progressively met (though with varying degrees of enthusiasm). It was a relatively small step from the practice of psychotherapy in institutional settings to psychotherapy in private practice.

The importance of clinical psychology for present views about psychotherapy can hardly be overrated. In contrast to social workers, some of whom became practitioners and accepted a secondary role within the medical setting, psychologists had their own tradition and derived both respectability and power from their own academic discipline. Most important, the traditions of psychology were entirely different from those of medicine. Trained in scientific analysis and often highly articulate, some clinical psychologists

recognized the apparent contradictions in the medical orientation of the psychotherapist and had both the intellectual and social skills to make their own views heard. Their success in asserting their right to be practitioners is evidenced by the licensure or certification of psychologists in many states, specifying that psychotherapy is part of the practice of psychology.

Though psychologists were critical of the difficulties encountered in testing various aspects of psychoanalytic theory, and different individuals questioned one or another aspect, we should reiterate that on the whole they accepted the psychoanalytic approach and that this approach formed the basis for the theory and practice of post-World War II clinical psychology. While effectively questioning the need for medical training, there was, with the exception of the work of Rogers, still no serious challenge to the medical model that formed much of the conceptual basis for the practice of psychotherapy.

Some Comments on Training Requirements

There is no doubt that a dispassionate analysis of the actual activities of a traditional analytically oriented psychotherapist will reveal few instances where his medical training is directly relevant. What alternative academic training, however, is relevant to the actual day-by-day tasks of the therapist? Psychological training, probably most widely cited as a specific alternative

form of background, is hardly more relevant to activities of the psychotherapist (and only slightly more so to those of the behavior modifiers to be considered later). Perhaps the only curriculum that focuses primarily on training that is directly relevant to the practice of traditional psychotherapy is that of some schools of psychiatric social work. Such a curriculum would, however, be rejected by many as too brief, too applied, and lacking in sufficient depth and breadth.

It is easy to overlook the fact that all professions require training that is not necessarily relevant to the practitioner's day-to-day activity. As professions are upgraded, the amount of education not directly relevant but nonetheless required inevitably increases. For example, it was once possible to become a physician as an apprentice to a practitioner. Later, medical school became a requirement, then an internship was added. Subsequently two years of college training were required for admission to medical school; more recently, practically all students complete college prior to entering medical school, and some residency has become all but mandatory. It is only when a profession has achieved a very high status and there is a shortage of practitioners that it voluntarily (under much social and political pressure) revises this trend and questions the relevance of some of the requirements.

In fact, neither medical schools nor graduate schools in psychology qualify the graduate as a practitioner of psychotherapy; rather, they provide a

general background of information, some fraction of which may prove useful. More important, however, they provide the graduate with a legitimacy that is helpful both in terms of his self-image and the ease with which he will be accepted as a competent member of the healing professions.

With the development of community mental health centers that tried to provide psychotherapeutic help to all who desired it, it was soon evident that there were simply not enough trained mental health workers. In response to this need (justified partly by the work of Rioch [1963]) as well as in response to other social pressures, mental health aids became the providers of primary care in most mental health centers. Similarly, many programs sought to provide mental health training to members of the community without "appropriate" prior education. Thus, while up until the late 1950s psychotherapy was practiced almost exclusively by individuals with some formal advanced degree augmented by further supervision and training, the stated need for such training in order to practice within recognized institutional settings decreased dramatically and inevitably affected the ease with which totally untrained individuals could lay claim to mental health skills.

Does Psychotherapy Have an Effect?

Up until the fifties the issue was, as we have seen, not *how*

psychotherapy should be practiced, but *who* should practice. During the last two decades a much more basic question was raised even more sharply: does this practice have any effects, and, if so, what exactly are they?

The Attempt To Evaluate Outcome

If previously we have sometimes used the terms *psychotherapy, psychoanalysis, dynamic psychology,* and even *psychiatry* almost interchangeably, it merely reflects the tremendous influence of psychoanalysis upon post-World War II psychotherapy. It was hardly conceivable to speak of a well-trained psychotherapist without implying that he was a dynamic psychologist, which, in turn, implied that he was psychoanalytically oriented. To be sure, there were disagreements between different psychoanalytic points of view and between psycho-dynamically oriented therapists and more orthodox analysts, but these were minor family quarrels, for there was a broad set of shared assumptions that formed the basis of psychotherapeutic practice, and none of these assumptions was more central and none more unchallenged than the assertion that, if properly practiced, psychotherapy works.

An early attempt to address this issue was made by Rogers and his students whose work was characterized by an emphasis upon research. By using verbatim tape recordings, reasonably sized samples of patients, and

independent judges to analyze and objectify the interview protocols, they sought to clarify the mechanisms of the therapeutic process. Their efforts were made somewhat easier by the relatively brief duration of the Rogerian treatment process and by the relatively mild problems it seeks to treat, but this fact does not diminish the historical importance of these studies which were the first serious attempts to evaluate the outcome of psychotherapy and to understand and document some of the factors that affect it. The great impact of the Rogerian movement upon academic psychologists was partially due to the fact that Rogers and his students tried to verify the change they sought to obtain.

Rogers had demonstrated that it was possible to study the psychotherapeutic process, but his work was largely outside of the therapeutic mainstream. Rogerian treatment presented an alternative to the medically oriented model of psychotherapy, but it was not considered appropriate for the treatment of serious pathology and had relatively little impact on the practice of clinical psychology and practically none on medical psychotherapists. However, there was a gradually increasing dissatisfaction with the unwillingness of psychotherapists to take a hard look at their results, and to subject the therapeutic process to more rigorous analysis.

Far and away the most widely publicized and devastating attack on psychotherapy was launched by Eysenck, (1952) who argued that there was

no evidence to show that psychotherapy had any effect whatever. He summarized the few outcome studies, pointing to the lack of controls of most and the negative findings of those few which were controlled, and concluded that there was no scientific evidence for any effect of psychotherapy let alone a beneficial one. Other studies, most notably the work of Frank and his associates (1963) had shown that it was difficult to demonstrate specific therapeutic effects, that individuals who were treated in outpatient clinics did not improve significantly more than controls who were awaiting treatment, and that the relatively sparse improvement that could be seen initially failed to persist over time.

It was possible, of course, to deny in each instance that psychotherapy was given a fair test: to argue that the investigators had failed to evaluate truly skilled psychotherapists, that they employed unreasonably brief periods of treatment, that they evaluated symptomatic improvement rather than the resolution of the underlying illness. But mere argument was obviously not enough, and there was an increasing willingness by psychotherapists to subject their treatment efforts to systematic inquiry (Kernberg, 1972). For an excellent summary of out-come-evaluation studies of psychotherapy, see Strupp and Bergin, (1969) Bergin and Garfield (1971), and for a careful and sympathetic review of psychoanalytically oriented studies, see Luborsky and Spence (1971).

It is not possible here to review the work that has been carried out and is yet in progress in this area. Suffice it to point out that reviewers conclude that some treatment effects have been documented. Bergin (1963) concludes that outcome measures evaluated after psychotherapy show considerably greater variability than those taken from control patients, which suggests that psychological treatment makes some patients better and leaves others worse. This conclusion is not exactly comforting, but at least it suggests that there is some effect rather than none at all as Eysenck had claimed. In a careful recent review Luborsky (1972) was able to document that in long-term therapy the therapist's training turns out to be of considerable importance; furthermore, that the bulk of outcome studies support the effectiveness of psychotherapeutic treatment.

Some Difficulties in Evaluating the Evaluations of Therapy

There are serious problems in conducting appropriate research on the outcome of psychotherapy. These include the highly variable natural history of the difficulties under treatment, the importance of apparently unrelated patient attributes and resources, and the myriad of adventitious concurrent events that affect the psychological status of the individual. Furthermore, while each of a number of different indices that have been employed to evaluate outcome has some face validity, they correlate poorly with each

other, which creates difficulties of method and interpretation. However, the most serious problems are conceptual and hinge on the definition of the psychotherapeutic process. What is the proper control group, the group that did *not* receive therapy? Can we possibly specify an appropriate control without a clear conception of the precise phenomenon under investigation? To specify a control group we must at least be able to specify what psychotherapy is *not.* Much of the difficulty with outcome research is that it has not squarely faced this issue: it has sought to determine whether a technique works without properly specifying either the technique or the outcome to be evaluated. A test of the specific effects of psychotherapy is not equivalent to tests that try to determine, say, whether a meaningful human relationship can lead to emotional growth (For a more extensive discussion of these issues, see Fiske, Hunt, Luborsky et al., 1970).

It is not surprising that many of the implicit questions about the nature of the psychotherapeutic enterprise become especially clear when one attempts to do meaningful research on psychotherapy. Only then are we forced to confront the lack of consensus about what constitutes psychotherapy; once given this lack, we have to face the fact that we can specify neither the nature of the treatment, the appropriate outcome measures, nor the appropriate controls. These difficulties are hardly novel and have often been commented on, but it is interesting that until fairly recently concerted efforts to deal with these issues did not occur.

It is worth noting that when outcome studies ask a reasonably specific and modest question, they may get a specific answer. For example, several studies have shown that in hospital settings a certain kind of patient tends to be selected for psychotherapy: the so-called YAWIS patient—young, attractive, white, intelligent, and successful (Hollingshead, 1958). Other studies have shown that the therapist's empathy and warmth are positive factors affecting outcome (Truax, 1967). Again, a specific hypothesis was proposed (Orne, 1962; Orne, 1968) that suggested that successful dynamic therapy depends partially upon whether there is a set of psychodynamic assumptions that both patient and therapist share (see also Goldstein, 1962). Since most patients in private psychotherapy have friends in treatment, it was argued that persons who know no one else in treatment might well encounter difficulties: their therapists might mistake an unfamiliarity with the psychodynamic point of view for a negative attitude and an inability to verbalize. Accordingly, an anticipatory socialization interview was developed by Orne and Wender, (1968) specifically designed to overcome this potential handicap. In two studies, (Hoehn-Saric, 1964; Sloane, 1970) a single anticipatory interview of this kind produced a significant positive effect on the outcome of therapy. In both instances, the therapists were unaware that some randomly selected patients had received special anticipatory socialization instructions. Nonetheless, the salutary effects of this pre-treatment session could be demonstrated some months after termination.

The difference between these and most other psychotherapy studies is that they focused on the effects of a clearly specifiable factor upon the psychotherapeutic process. Unfortunately, such specificity is very difficult to achieve regarding precisely those variables which most therapists view as particularly important for treatment outcome.

An Alternate Model: Behavior Therapy

Some of the effectiveness of Eysenck's (1952) attack on psychotherapy as a treatment procedure was probably due to his forceful attempt to propose a viable alternative: behavior therapy. The behavioral approach to psychological problems, based on principles of conditioning and learning, dates back to the work of Pavlov (1927) and Watson (1924). It met with considerable success in the treatment of specific symptoms, such as in the case of enuresis by Mowrer (1950). But this approach has received wide attention and gained increasing acceptance only within the last fifteen years. Like psychoanalysis it proposes a meaningful rationale for the development of symptoms: they are learned responses that must be unlearned and replaced by other, more adaptive reactions. Certain earlier theorists such as Miller and Dollard (1941) had tried to create a theoretical bridge between psychodynamic thinking and their current versions of Hulls (1943) S-R (stimulus-response) reinforcement theory: in effect, they tried to interpret various psychodynamic mechanisms in terms derived from the animal

conditioning laboratory, with particular emphasis on avoidance learning. But to the modern behavior therapist (e.g., Wolpe, 1958; Yates, 1970; Krasner and Ullman, 1965) the focus is on the symptoms, for in his view the symptoms as such *are* the pathology. Here was a radical change from the medical model of disease that views symptoms only as an external manifestation of an underlying process. If symptomatic difficulties are seen as maladaptive learning, then the learning process itself becomes sufficient to explain the symptom. Further, from such a point of view, if the symptom is unlearned, nothing more remains to be treated.

While psychotherapy took its legitimacy from its medical roots, behavior therapy saw its legitimacy in its scientific roots. It looked to rigorous laboratory research with animals and man for its scientific underpinnings, and developed a number of treatment techniques that sought to modify maladaptive behavior. These techniques were at least said to derive from animal research. Thus, examples are systematic desensitization (Wolpe, 1958) based on classical conditioning, a wide variety of techniques using shaping procedures,(Ferster, 1961; Lovaas, 1966) implosive therapy, (Marks, 1972; Stampfl, 1967) aversive conditioning, (Rachman, 1969) the token economy for the treatment of hospitalized patients, (Ayllon, 1968) and a wide range of related techniques to manipulate overt and covert behavior (Krasner, 1965; Rimm, 1974; Yates, 1970). These and others became part of a burgeoning literature that showed much of the excitement that characterized

the early years of psychoanalysis.

In some regards the development of behavior therapy is curiously reminiscent of the early years of psychodynamic therapy, and it appears to recapitulate some of its growing pains. For example, there are a number of competing approaches within the behavior-modification movement, some more strident than others in claiming to be the only truly scientific approach. Different approaches are often based on different theoretical positions within the field of learning (some leaning on P. Pavlov, others on E. R. Guthrie, still others on neo-Hullian view's, many on B. F. Skinner), and sometimes opposite techniques, both of which seem to take their rationale from the same theory and to yield good results (implosion therapy versus systematic desensitization). Again, there is considerable divergence of opinion on whether theory derived from laboratory studies should take precedence over the clinical facts observed during therapy itself. Some therapists argue that the empirical findings evolved in the process of behavior therapy are the most relevant and should be used to clarify the theoretical formulations based on laboratory research. Finally, there is considerable disagreement about the evaluation of therapeutic results: some insist on outcome studies with statistical comparisons between treated persons and appropriate controls, while others insist with equal vigor on the systematic study of single cases in which specific interventions can be shown to have systematically altered specific kinds of behavior.

The initial appeal of behavior therapy was based in large part on what seemed a striking clinical effectiveness. Thus, Wolpe (1985) reported that 188 of 210 neurotic patients seen in his practice recovered or were greatly improved after an average of thirty treatment sessions, a truly remarkable cure rate approximating 90 percent. A further factor was the assertion that behavior therapy involved a specific form of treatment unlike the "talking cures" of psychodynamic therapy, which were attacked as "nonspecific"—an ironic recapitulation of the controversy between Freud and the French hypnotists in which *he* laid claim to specificity for his procedure. A number of controlled studies sought to show that therapeutic techniques such as systematic desensitization are specific and that the results could not be accounted for in terms of subject expectancies, therapeutic relationships, and similar nonspecific factors. One of the most influential studies of this type (Paul, 1966) compared the results of systematic desensitization with those of short-term psychotherapy in an elegant experimental design. A number of dynamically trained therapists, committed to psychotherapy, were trained to carry out systematic desensitization. Each therapist treated randomly assigned individuals—students with stage fright who were patient volunteers —with either dynamic psychotherapy or systematic desensitization. A third group of subjects was exposed to a placebo pseudotreatment. Finally, this study also included yet two more control groups. One was a group of students with stage fright who were contacted about participating in the study, and

after having agreed to do so were placed on a waiting list. Another group was made up of students who were matched for the severity of stage fright but were never contacted until after the study and served as an inert control group to evaluate spontaneous change over time.

The surprising findings were that despite the presumed psychodynamic bias of the therapists, systematic desensitization proved to be significantly more effective in relieving stage fright than any other method. Interestingly enough, subjects treated by dynamic psychotherapy expressed more positive views toward their treatment, though according to objective behavioral criteria this method was no more effective than the placebo control or, for that matter, the waiting-list control group (though all three of these groups changed more than the uncontacted inert controls). This unusually well-controlled study certainly seems to document that systematic desensitization is a therapeutic intervention that has clear-cut specific effects above and beyond the nonspecific components of subject expectations and related placebo elements. A follow-up study (Paul, 1967) indicated that, far from suffering from substitute symptoms, persons who received behavioral treatment for stage fright also showed a general improvement on a number of questionnaire measures, which suggests that the specific effects of behavior therapy may generalize to other fears and difficulties.

The impact of such studies on psychology was prompt and dramatic.

Within a few short years the focus and training veered sharply from psychodynamic psychotherapy and personality assessment toward an interest in behavior therapy.

It is, of course, too soon to evaluate behavior therapy. Some aspects, however, are worth noting. The remarkably high cure rates originally reported (Wolpe, 1958) have never since been obtained by these authors or others. Despite the emphasis on scientific principles and theory, the relationship between therapeutic practice and theory is often tenuous. London (1972) has recently pointed out that behavior therapy is, in fact, a clinical art, and while it derives scientific legitimacy from scientific psychology, its procedures cannot rigorously be adduced from commonly accepted theory. One may well ask whether academic psychology only provides for the behavior therapist what medicine gave to the psychiatrist: respectability and a general world view.

It is somewhat unfortunate that the dialogue between dynamic psychotherapists and behavior modifiers has been characterized by a lack of understanding of each other's position, a tendency to criticize the most poorly formulated aspects of the other's view, and a reluctance to come to terms with some of the genuine points of disagreement. While a detailed consideration of the relationship between psychotherapy and behavior modification is beyond the scope of this discussion, some similarities as well

as some differences deserve emphasis.

There are certainly some genuine differences. One concerns the basic conceptual model from which the therapy is said to derive. As previously noted, dynamic therapists share certain convictions. These include an emphasis on unconscious motivation, the dynamic meaning of symptom formation, the use of the transference relationship, the importance of insight, and the working through of basic conflicts in order to modify the mechanisms underlying maladaptive psychopathology. Behavior therapists share a set of different convictions. In their view, symptoms must be understood in terms of learning and any attempt to modify them must ultimately be based on learning principles. Given this belief, they regard it as crucial to explore in detail the environmental contingencies that aggravate or suppress the occurrence of symptoms (recognizing full well that more than one antecedent event may elicit a given symptomatic behavior and that a number of different environmental contingencies may serve to suppress it). Their efforts to modify symptoms may be based on breaking the link between certain environmental events and symptomatic behavior by deconditioning or counterconditioning procedures, by establishing competing operant-response patterns, or by modifying the environment to prevent certain contingencies from occurring. Finally, they believe that one can transfer the control over undesirable behavior (broadly defined) to other contexts and place it within the control of the individual himself.

Another difference concerns the professed goal. Stated in extreme form, the dynamic therapist wants to cure the underlying problem of which the symptom is but an external manifestation; the behavior therapist wants to remove the symptom that, in his view, is the illness itself.

When considered more closely, the actual differences between the two groups are not as large as they appear in polemics. This holds for their goals as well as the means by which these goals are implemented. Some of the similarities are obscured by the enormous difference in the language each group employs (Sloane, 1969). In fact, there have been some attempts at translation. Thus, Miller and Dollard have tried to account for many of the events that occur in dynamic therapy from a learning theory point of view (1941). Analogously, it may be possible to analyze much of what occurs in behavior therapy in dynamic terms (Wilkins, 1971). Similarly, the distinction between the focus on the underlying illness as opposed to the symptom is in actual practice rarely clear-cut. Many dynamically oriented psychotherapists show little concern for dynamic diagnosis, while virtually all behavior therapists carry out a detailed assessment of the contingencies associated with the symptom, which, in fact, provides much of the data upon which clinical diagnoses are ultimately based. Much has been made of another difference: the interest of many behavior therapists in discovering the natural history of a patient's symptoms in his everyday environment as contrasted with the dynamic therapist's preference for exploring the patient's problems

retrospectively, far removed from the actual event. Even here exceptions abound: dynamic, conjoint family therapy is based on bringing the conflicts into the therapeutic session so that they may be studied and dealt with as they occur, and the pioneering work of analysts such as Spitz (1945) and Engel (1956) was also based on systematic observation of primary data. In contrast, Wolpe's behavior therapy is practiced exclusively in an office setting.

Similar comments can be made about several other distinctions that are often raised in debates, but that on closer inspection turn out to be quite blurred in actual practice. Contrary to some early characterizations (both by the behavior therapists and their dynamically oriented adversaries) most behavior therapists are *not* naive peripheralists who look only at gross, overt motor actions; today their concerns extend to all sorts of private, covert behaviors as well; similarly for the assertion that behavior therapists do things *to* the patient while dynamic therapists try to help the patient do things *for* himself. This, too, is a caricature. Some dynamic therapies—especially the briefer ones—are at least as manipulative as behavior therapy; an example is the use of "paradoxic intention" (Haley, 1963). Nor is there a dearth of behavior therapists who explicitly see their task as helping the patient gain control over his own behavior: among others, therapists whose conceptual roots are in social learning theory (e.g., Bandura, 1969; Kanfer and Karoly, 1972; Mischel, 1968) do exactly that. In these cases, even the

techniques are not as different as are the terms in which they are described. The behavior therapist insists on the detailed analysis of the contingencies that evoke certain kinds of behavior to help the patient gain control over his own behavior; the dynamic therapist tries to help the patient become aware of why he does certain things in the psychotherapeutic context in the hope that this will increase ego control. Isn't there a parallel? Some behavior therapists (notably Lazarus, 1971) are trying to make some of these underlying similarities explicit and, with varying degrees of success, have begun to build bridges, both conceptually and in technique. Only too often, unfortunately, their efforts are rewarded by severe attacks for departing from orthodoxy, reminiscent of the vehement polemics during the early days of psychoanalysis.

This is not to deny that there are some genuine differences in points of view that transcend mere semantic issues but rather to emphasize some of the useful points of contact. In any case, all practitioners of whatever persuasion should applaud the behavior therapist's demand for the precise contingencies under which certain behavioral events occur; likewise his concern about specifying exactly what is to be done and why and how; and, similarly, his attempts to make the research effort an integral part of clinical practice. Of course, it still remains to be seen how effectively the scientific ideals of the present generation of behavior modifiers will be retained in the face of increasing clinical responsibility. Only time will tell how effectively

this movement will resist the pressure of increasing professionalization, codification, and a concurrent decrease in the concern with the whys and wherefores of therapeutic results.

Alternate Goals: Cure or Happiness

Recent Trends Toward Increasingly Broader Goals

As stated before, many of the controversies between adherents of different psychotherapeutic approaches hinged on different conceptions of the goal. Again and again the pendulum has swung from goals that were narrow and precise (get rid of the symptom) to goals that were much broader, more ambitious, and correspondingly imprecise (remove the underlying disorder). The psychodynamic therapists argued that the behavioral approach was too superficial. But if they are right (which symptom-oriented therapists, of course, would not concede), they face the immense task of specifying what this underlying disorder is and how one can know for certain that it is no longer there. There is an unpalatable trade off: as the goal becomes broader and more ambitious, it becomes progressively more difficult to define.

The tendency toward a broadened definition of therapy's goal is virtually built into the dynamic approach, given its assumption of a rather

general, underlying disorder. But recently a similar trend can be seen in behavior therapy as well, though justified on different theoretical premises. One manifestation is a growing interest in the modification of behavior patterns that are so pervasive that one might well describe them as general personality characteristics, such as assertiveness, (Wolpe, 1958) self-control, (Kanfer, 1972) and depression (Beck, 1967).

This widening of goals was probably most pronounced in the actual day-to-day clinical practice of both groups, whether dynamically oriented or based on behavioral models. Both groups gradually broadened the scope of their activities to encompass increasingly vague and diffuse complaints. While initially they treated only those persons with readily defined problems, whose need for help was clear and self-evident, therapists increasingly lent their services to anyone who asked for them (on the assumption that if the patient asks for treatment he probably needs it, even if the clinician does not really know why).

In part, the extension of the therapeutic goals seems forced upon the practitioners by the realities of the clinical situation since the number of patients with clear-cut, specifiable focal complaints is limited, whereas most troubled individuals present themselves with a variety of difficulties, none of which is necessarily that which prompted the visit. Regardless of the clinical realities, however, the extension of goals exacts an inevitable price: as the

definition of what one wants to achieve becomes increasingly vague, it becomes ever more difficult to determine whether one has achieved it.

Many dynamically oriented therapists, perhaps in response to some of these difficulties, had gradually changed their criteria of whom to treat; the concern was less with whether someone needed treatment and more with whether he would be likely to benefit from treatment. Concurrently, the therapeutic emphasis shifted from the cure of disorders that cause pain and suffering (and interfere with an individual's ability to effectively function in his environment) to the more ambitious goal of helping an individual to achieve his potentialities and increase his personal satisfaction. In a sense these ultimate goals were defined much in the way Freud defined the ultimate goals of analysis: to enable the person successfully to love, work, and play. But these goals are radically different from those of traditional medicine, which is concerned with the treatment and prevention of disease and illness. In general medicine there is concern for physical fitness, but the analogy is superficial at best, for the physician's concern about fitness focuses on its importance for preventing illness rather than on what this fitness may contribute to an individual's happiness and the healthy gratification of his wishes as ends in themselves. Thus, psychotherapy, while continuing to use medical metaphors, progressively moved from goals consistent with those of other medical disciplines to goals that were more characteristic of the educator, the philosopher, and the moralist.

The ever-increasing scope and ambition of the therapeutic purpose is most dramatic in those therapeutic movements which have explicitly abandoned either of the two conceptual models that served to set some limits, the medical model or the learning-theoretic one. One such movement is existential therapy whose adherents explicitly seek to deal with ultimate questions of meaning and purpose, questions that—at least explicitly—had rarely been raised by traditional psychotherapists.

Existential therapists were not primarily concerned with either unconscious conflicts or with symptoms; instead, they tried to deal with the feeling of aimlessness, purposelessness, anomie, and ennui that had become widespread. In many ways their concern was in response to problems that had become especially common during the post-World War II period. Long-established values were widely challenged, and never before had such large segments of the population enjoyed material comforts and security without corresponding responsibilities. The combination of leisure and security, and a limited number of obligations and commitments in the absence of either meaningful challenges or a guiding value system, led many persons to ask themselves what they really wanted to do and whether it was really worth it. Such "existential" concerns undoubtedly contributed to a wide range of aberrations in individuals—the widespread use of hallucinogens and other sensation-seeking behavior are excellent examples of behaviors related, at least in part, to these issues. But, except for those individuals who suffered

from true depressions, these problems were different in kind from the incapacitating symptoms, the inability to function, or the acute suffering that had previously characterized most individuals seeking therapy. There was a new kind of patient population, some of whom sought out a new therapy: they typically lacked focal symptoms and they were generally able to cope with their environment. As often as not there was a surfeit of resources, physical and mental, rather than a lack, and the patients sought help not to escape pain but to escape ennui. Existential therapists argued that they were dealing with a new kind of difficulty, where neither the concept of disease nor that of focal symptoms was really relevant. They saw their task as helping people to seek their own solutions to their own problems—not by trying to resolve them historically but by dealing with "where they are at."

A similar shift had taken place in the focus of group psychotherapy, which, during the forties, had become strongly dynamically oriented and had been widely used as an alternative or an adjunct to dyadic therapy. Extensive training was required of group therapists that, in addition to the usual credentials of qualified therapists, included much supervision in group experience. Group therapy was seen as clearly health-related, both in terms of its goals and in terms of the orientation and training of its practitioners.

Competing models of group psychotherapeutic efforts, however, also developed concurrently from other traditions. Especially influential was the

work of Kurt Lewin on group dynamics, which led to sensitivity training or the "T-group." Conceptions such as "feedback," the giving and receiving of interpersonal perceptions, and "participant observation" were some of the techniques used by the group leader to help the group learn the dynamics of its functioning. The T-group was seen as a means of enhancing interpersonal understanding, modifying prejudice in a variety of areas, and facilitating productive creative interactions. In contrast to traditional group therapy, however, it was practiced primarily by individuals trained in group dynamics, with group members who defined themselves as normal. The settings as well were far removed from the healing context, ranging from industry to schools, hospitals, and even police departments. While using some dynamic insights and much of the technology of behavioral science, the goals tended to orient around specific problem areas. Initially, at least, group leaders were trained with considerable care and for the most part felt responsible in helping to prevent any serious difficulties arising from interactions. A further focus and direction with these groups was usually provided by the institutional aegis under which they were organized.

Under the stimulus of the humanistic tradition, the T-group, renamed by Carl Rogers the "basic encounter group," became increasingly prominent in the early sixties. Now the purpose had become the search for a meaningful experience on the part of the individual participant, a growth experience that need not be either health related or important for the achievement of some

greater group goal. The basic encounter group, which proved to be a powerful but by no means totally harmless method of modifying experience, (Lieberman, 1973) is then a particularly clear example of the shift away from psychotherapy toward experiences with the avowed purpose of growth. Once having made this shift, the practitioner of the new method found it progressively easy to disclaim all responsibility for the client's welfare. He was not responsible to an institution nor did he have culturally defined responsibility as a healer. Quite to the contrary, the Hippocratic view that the healer should not only strive to help his patient but that above all else he "must do no harm" was explicitly and purposively rejected as inappropriate and harmful paternalism.

The practice of any form of psychotherapy inevitably must come to terms with some of these broader issues of goals. Many thoughtful psychotherapists talked of providing emotional-corrective experiences, and many likened the process to other forms of education. Clinicians often saw how the resolution of various conflicts helped release previously blocked creative forces and recognized that the process of psychotherapy must involve emotional growth as it seeks to alleviate symptoms and alter those patterns of behavior which most of us would characterize as psychopathology. Nonetheless, as much as the therapist and patient might strive to release the latter's creative potentials, such a goal was accomplished in the context of treating an underlying disorder (or, in terms perhaps more

palatable to at least some behavior therapists, the modification of behavior patterns defined in exceedingly molar terms). On the face of it, it seems extremely reasonable and appropriate to move only a small step further and think of the therapeutic process exclusively in positive terms. Would it not seem best to abandon the authoritarian vestiges of medical tradition and the pretense of treating mental illness—especially if it is only a myth? Instead of the role of patient, repugnant to many, the role of an individual seeking fulfillment would seem acceptable to all. With this apparently small step therapists began to promise not merely the relief of symptoms or the treatment of underlying disorders, but a far more ambitious goal. The terms varied, but in essence they now offered the hope that individuals could obtain that ultimate (and most impossible to define) of all gifts—happiness.

However reasonable the shift of emphasis might seem, the consequences have been both unexpected and troubling. Freed from the constraints of medical and professional traditions, a myriad of new therapies, each stridently asserting its effectiveness and superiority over more traditional procedures (and over each other) suddenly emerged. The range of "therapies" or "growth experiences" available to all comers expanded from encounter groups to marathons, to nude marathons, to sensory experiences. Still different techniques are now advocated to help "let it all hang out," from Gestalt therapy to extreme forms of Reichian treatment, screaming, Rolfing, energetics, sex therapy, and brutality therapy. Similarly, the meditative

disciplines that were once practiced in highly structured settings were now widely popularized in an "instant" form, whether as transcendental meditation and its variants or in the use of biofeedback to achieve instant *samadhi* (see London [Psychol. Today, 1974]). All of these various procedures (should they still be called *therapies*?) are characterized by an emphasis on rapidly achieved experiential change, by the rejection of traditional concepts of illness, and by the explicit abrogation of responsibility for the consequences of treatment. The purpose of treatment is simply to make the individual "happier." If the process happens to precipitate serious difficulties —*caveat emptor.* The patient who had become a client was now simply a consumer, and as such it is he who decides whether he is in need of the treatment -—the therapist merely "does his thing" for a fee. For the instant therapies the justification for the treatment's effectiveness is no longer in its theories, in the scientific evidence, or even in the reflected glory of the medical or psychological doctorate of its practitioners. To the contrary, there is a common theme of anti-intellectualism and a rejection of science and what is sometimes called the "engineering approach," often expressed with mystical overtones and an emphasis upon feeling, on "vibes," and the free expression of instinctual needs.

We have previously stressed the difficulty of evaluating whether therapy "works." In great part this reflects the difficulty of defining the therapeutic goal. As this goal is defined ever more broadly, its definition

becomes correspondingly vague and the difficulty of evaluating outcome increases in turn. When the purpose of treatment expands to the wide horizons sought by the new therapies, the determination of whether the treatment works has become more than difficult . . . it is impossible. We can, therefore, say little or nothing about the effectiveness of the new treatments. We can only suggest some of their sociopsychological roots.

Some Implications of the Broadened Goals: When Is Treatment Necessary?

We have argued that as the therapeutic goals of therapy become increasingly broadened it becomes increasingly difficult, if not impossible, to evaluate whether they have been met. This does not deny the desirability of goals, such as emotional growth, or the possibility that they may be approached in a responsible fashion. Suppose one grants, for the purpose of discussion, that goals such as growth and even happiness can be defined so that their attainment can be determined. Suppose one also grants that a given treatment can indeed achieve these effects, and that this can be documented. If so, we will still face a variety of social and even political issues that hinge upon the question of when such treatment is necessary—and what do we really mean by necessary. These issues tend to come most sharply into focus as one considers the availability of therapy and who shall bear its cost.

Whether treatment is indicated and how much treatment is enough has

always been a difficult matter to decide. However, as long as the financial cost was borne by the patient, the financial sacrifice itself helped minimize the problem. The question of whether treatment was really necessary hardly seemed relevant as long as the therapist was prepared to offer it and did not see it as detrimental and so long as the patient himself sought the treatment. This question becomes more thorny, however, when the financial burden is borne not by the patient himself but by others (e.g., his family). In the past, therapists considered such issues ethical matters, to be resolved on an individual basis in line with their own integrity while trying to take into account the needs of all persons involved. As long as psychotherapy functioned within the framework of the medical model, it could rely on traditional guidelines of how such matters might be resolved. Questions about the burden an individual's medical needs impose upon his family were by no means new and have been of considerable concern to thoughtful physicians for a long time. A reconsideration of this issue, however, is necessitated by the increasingly widespread view that adequate medical treatment is a right and should be freely available to all. This view has gained general acceptance throughout the world and various forms of third-party payment now account for the overwhelming bulk of medical expenses even in the United States.

Third-party payment systems such as medical insurance have serious implications for the practice of all forms of psychotherapy. In recent years,

psychotherapists have increasingly come to accept the patient's decision that he requires treatment as an adequate justification for administering it. But this approach is altogether different from the way in which medical, third-party payment systems determine the kind and duration of treatments whose costs they are willing to pay. The patient asks for medical assistance, but it is the physician's responsibility to establish the nature of his illness and to determine whether there is a viable procedure for its cure or alleviation. If such a procedure is available, but in limited supply (e.g., long-term dialysis treatment for kidney failure), its allocation is generally based on some rational principle that considers both need and prognosis.

An analogous set of decisions may ultimately have to be made about the need for psychotherapy. The question of whether someone should receive psychotherapy will have to be considered in terms of the patient's needs, the likelihood of achieving the therapeutic goal, and the availability of appropriate treatment resources. This will require us to identify those psychological problems which are sufficiently serious to be analogous to those physical problems which urgently require treatment. We will undoubtedly find that for other psychological problems treatment is desirable (as a matter of health) but not urgently so, analogous to physical disorders that should be treated on an elective basis. We will finally note a set of problems that may trouble the patient and for which he seeks treatment, but for which treatment is not a health necessity. Such problems also have a clear

analogue in medicine—cosmetic (as opposed to reconstructive) plastic surgery. Interestingly enough, the question that determines whether a procedure is paid for by health insurance is not whether the procedure is a medical one but only whether it is necessary for health. Thus some problems of appearance may be so disfiguring as to interfere with the individual's normal functioning and thus justify cosmetic surgery for reasons of health, but this is probably true in only the most extreme cases. On the other hand, an individual whose nose is well within the norm may ardently wish to change the shape; he does not as a consequence suffer from a health-related problem, no matter how intense his desire may be. Under the circumstances, no health insurance will pay the cost. There is, of course, nothing that prevents the patient from making a private arrangement with a plastic surgeon, to modify his appearance at his own expense.

A similar distinction may be useful for psychotherapy. Such a distinction would have to establish when psychotherapy may be considered health-related (and thus appropriately reimbursed by third-party payments) and when it is essentially elective or "cosmetic" in nature. Growth experiences that fall in the latter category do not thereby become any less desirable, but must still not be confused with health necessities. If health professionals choose to provide such experiences as a personal service, they are not, in so doing, carrying out a function vital to an individual's health. Once the distinction is clearly drawn, both the public and other health professionals

are likely to show a similar ambivalence toward such treatment as that often seen toward the practice of cosmetic plastic surgery. Such a distinction may also have implications for determining criteria of outcome effectiveness. If we are considering psychotherapy for health, the criteria should probably be similar to those used to judge health-related procedures: the person's ability to function and the prevention of dire consequences that might otherwise occur. But if we are considering psychotherapy, not as a health-necessity but as a form of psychic self-improvement, the criteria may well be similar to those whereby cosmetic surgery is judged almost exclusively: the patient's satisfaction with the procedure.

Another analogy with medicine may help clarify the role of the psychotherapist in the development of programs of mental hygiene and other forms of prophylaxis. There are certain safe and effective prophylactic procedures against serious diseases; it is the public-health physician's responsibility to ensure actively that such procedures are carried out. He has a clear-cut responsibility to treat communicable diseases and to prevent their spread. The public-health physician may also be concerned with various measures to improve the population's physical fitness insofar as these have profound effects on matters of health. But this does not mean that the public-health physician feels called upon to become a physical-education teacher, nutritionist, or environmental engineer, though he recognizes his responsibility to bring relevant matters to the attention of each of these.

Indeed, he will carry out the necessary epidemiological studies to help clarify the relationship between physical fitness and illness, and will identify procedures that may have potential application. Preventive medicine has made great strides because its programs were based upon a firm scientific foundation; its advance was due to this rather than any premature activism on the part of its practitioners. Preventive medicine has long since given up the goal of making Olympic athletes of us all. Its effectiveness increased as it assumed more modest goals. Psychotherapy would do well to adopt an analogous point of view, devoting less effort to merchandising its knowledge and more effort to acquiring and to validating it.

Some Final Comments

The psychodynamic approach seemed to provide a new way of studying mental phenomena by way of dreams, free associations, and parapraxes that led to the discovery of such basic clinical phenomena as transference and countertransference, the meaning of symptoms, and the various defenses. It seemed as though a new scientific way had been found by which great universal truths could be identified with relevance to all aspects of human life and experience. The theoretical foundations for dynamic psychotherapy were formulated at the turn of the century, but the acceptance of the psychodynamic view followed neither from the accumulation of hard evidence nor the demonstration of effectiveness in clinical practice; rather, it

was more the result of a fortuitous link between a novel view toward man and the prevailing *Zeitgeist* of the American culture. Psychodynamic ideas did not find broad public, scientific, and medical acceptance until the early forties. The fact that these ideas were promulgated under medical/scientific auspices in no small part contributed to their acceptance.

The degree to which the psychodynamic viewpoint had been accepted in the United States was invariably commented upon by European psychiatrists, surprised by the influence that the ideas of their American colleagues had on the general public. The general awe in which psychiatric opinion was held led psychiatrists to assume the role of social philosophers, using the credentials of medicine and science as their justification for commenting upon everything from the consequences of rock music to the suitability of presidential candidates. The limitations of psychotherapy as a treatment, however, were obscured by the respect accorded to psychiatrists' comments about matters far removed from medical practice.

The last fifteen years have witnessed the gradual erosion of consensus within psychiatry about the conceptual models underlying psychotherapy, the kind of training required to practice it, the procedures by which it is carried out, and the ethical responsibilities the therapist assumes in undertaking treatment. A concurrent change in attitude within academic medicine, the scientific community, and among the public at large has

gradually taken place, resulting in a decline of esteem from that in which the theory and practice were once held. It gradually became clear that psychotherapy was unable to live up to the promises made for it by its most enthusiastic protagonists. Its claim to be a scientific discipline is no longer undisputed, and even when accepted is no longer universally seen as an undisputed good. On the one hand, serious challenges from within the scientific community deny the validity of these claims. On the other hand, ironically enough, some segments of the public are prepared to reject the psychodynamic approach (and no doubt the behavior therapist's as well) because they see it as too scientific, too rational, and not sufficiently concerned with feelings, emotions, and love.

Psychotherapy had become widely accepted in contemporary society, largely because of its claim to scientific legitimacy on the one hand, and the fact that it was practiced by highly skilled practitioners, members of the traditional healing profession who had completed rigorous and arduous training. But the legitimacy based on training is rapidly eroding, nor is membership in the established healing professions any longer accepted as an essential prerequisite for psychotherapeutic practice. The rapid proliferation of more or less bizarre therapeutic procedures creates the serious risk that the public will come to regard them all as equally effective or ineffective. Some acceptable basis must be found for choosing among them and rejecting some while accepting others (Luborsky, 1975). This will require a

widespread concern for relevant empirical information among practicing therapists, which has thus far been largely lacking. Instead, psychotherapists have tended to become alienated from their own conceptual base and have shown little concern for the potentially relevant literature in psychological or psychiatric journals. (In this regard, psychotherapy is very different from other medical disciplines, where reports of relevant new findings rapidly affect both the conceptual models and clinical practice.) In some instances, psychotherapeutic practice has tended to ignore solidly based clinical research with obvious relevance for practice.

If these trends continue, psychotherapy will become a discipline unto itself, essentially unrelated to psychology or medicine, and functionally autonomous from efforts to elucidate issues that are obviously crucial to an understanding of the psychotherapeutic process (from any point of view other than the parochial one adopted by the therapist himself).

Psychotherapy thus finds itself at a crossroad. We must begin to recognize that the original enthusiastic reception accorded to psychodynamic views was based on a promissory note—the clinical and scientific evidence promised for later delivery. It happens to be the present author's conviction that the major insights derived from the dynamic view are basically sound and have to be incorporated into whatever (health-related) psychotherapies will evolve in the future. Unfortunately, such affirmations of personal credo

are no longer enough. Even if shared by virtually all practitioners (as they were a few decades ago) they can never provide the firm platform on which a solid future can be developed. We have to begin to recognize the limitations of the data obtained from the private, dyadic relationship upon which the bulk of our theory has been based. Interesting as they may be, such data are inevitably obtained under special circumstances in which neither patient nor therapist is a truly objective, disinterested observer, and while this allows for the study of transference and countertransference, it also introduces the problems of self-fulfilling prophecies, (Merton, 1948) experimenter-expectancy effects, (Rosenthal, 1966) and demand characteristics, (Int. J. Psychiatry, 1968) well-documented in other areas of research. Observations obtained in a psychotherapeutic context can be uniquely important in formulating hypotheses, but they cannot provide a rigorous test of these hypotheses until they are objectively evaluated.

Of the crises in psychotherapy that we are now facing, perhaps the most serious is the proliferation of therapies—many of them increasingly implausible and irresponsible— without any objective means for discriminating among them. In an increasingly egalitarian society, pronouncements from authorities and legal regulations become increasingly ineffective in preventing abuses, and we will be compelled to develop reasonable means by which objective evaluations can be carried out.[8] Unfortunately, meaningful outcome studies are both very expensive and

extremely difficult to execute. Worse yet, judging by present events, the proliferation of new techniques will undoubtedly exceed the rate at which they can be evaluated, nor will any negative study, regardless of merit, serve to convince the protagonists of any new treatment to abandon the procedure. Such an approach will not as such do much to advance our understanding. It is analogous to executing outcome studies on every new combination of drugs that any individual claims to be effective in the treatment of some disorder. The science of pharmacology did not advance in this manner. Instead, it depended on an understanding of how different groups of active ingredients function *in vivo*, *in vitro,* and in the presence of different kinds of pathology. In clinical pharmacology, outcome studies become relevant only after a great deal of pharmacologic information is already known; they are rarely, if ever, considered in the absence of any plausible pharmacologic mechanism. Unfortunately, the basic science that underlies psychotherapy remains to be developed. There is reason to believe that this development will occur only if psychotherapy resumes contact with the psychological sciences in general and with those relevant biological disciplines that may shed light on the relationship between brain, behavior, and experience. For the only way in which psychotherapy can become truly effective will be by recapturing the zest for learning that characterized its early years while acquiring a newfound eagerness to disprove—if possible—its own pet beliefs.

We are not a new discipline any longer. Modern psychotherapy

antedates modern physics, biochemistry, molecular biology, behavioral genetics, and many other highly developed disciplines. We can no longer excuse the lack of hard clinical and scientific data cither by the newness of the field or by the complexity of its problems. Our task is to build an applied science upon the foundations of the relevant basic sciences, to incorporate the lore of our art (and some of its beliefs that have stood the test of time will probably also stand the test of rigorous, empirical scrutiny) within a solid discipline that truly fulfills Freud's promise to create a rational science of the irrational. Only when this is accomplished will we be able to boast, along with other cumulative disciplines: we can do what yesterday's giants could not . . . because we stand on their shoulders.

Bibliography

Arey, L. B., W. Burrows, J. P. Greenhill et al., eds. *Dorland's Illustrated Medical Dictionary*, 23rd ed. Philadelphia: Saunders, 1957.

Ayllon, T. and N. H. Azrin. *The Token Economy: A Motivational System for Therapy and Rehabilitation.* New York: Appleton, 1968.

Bandura, A. *Principles of Behavior Modification.* New York: Holt, Rinehart and Winston, 1969.

Beck, A. T. *Depression: Clinical, Experimental, and Theoretical Aspects.* New York: Harper & Row, 1967.

Bergin, A. E. "The Effects of Psychotherapy: Negative Results Revisited," *J. Consult. Clin. Psychol.,* 10 (1963), 244-250.

Bergin, A. E. and S. L. Garfield, eds. *Handbook of Psychotherapy and Behavior Change: An Empirical*

Analysis. New York: Wiley, 1971.

Bruner, J. S. "Personality Dynamics and the Process of Perceiving," in R. R. Blake and G. V. Ramsey, eds., *Perception: An Approach to Personality,* pp. 121-147. New York: Ronald, 1951.

Cobb, L. A., G. I. Thomas, D. H. Dillard et al. "An Evaluation of Internal-Mammary-Artery Ligation by a Double-Blind Technic," .V. *Engl. J. Med.,* 260 (1959), 1115-1118.

Ellenberger, H. F. *The Discovery of the Unconscious.* New York: Basic Books, 1970.

Engel, G. L. and F. Reichsman. "Spontaneous and Experimentally Induced Depression in an Infant with Gastric Fistula," *J. Am. Psychoanal. Assoc.,* 4 (1956), 428-452.

Evans, F. J. "The Placebo Response in Pain Reduction," in J. J. Bonica, ed., Advances in Neurology, Vol. 4., *Pain,* pp. 289-296. New York: Raven, 1974.

Eysenck, H. J. "The Effects of Psychotherapy: An Evaluation," *J. Consult. Clin. Psychol.,* 16 (1952), 319-323.

Ferster, C. B. "Positive Reinforcement and Behavioral Deficits of Autistic Children," *Child Devel.,* 32 (1961), 437-456.

Fiske, D. W., H. W. Hunt, L. Luborsky et al. "Planning of Research on Effectiveness of Psychotherapy," *Arch. Gen. Psychiatry,* 22 (1970), 22-32.

Frank, J. D. *Persuasion and Healing: A Comparative Study of Psychotherapy.* Baltimore: The Johns Hopkins Press, 1961.

----. "The Role of Hope in Psychotherapy," *Int. J. Psychiatry,* 5 (1968), 383-395.

Frank, J. D., E. H. Nash, A. R. Stone et al. "Immediate and Long-Term Symptomatic Course of Psychiatric Outpatients," *Am. J. Psychiatry,* 120 (1963), 429-439.

Gill, M. M. "Hypnosis as an Altered and Regressed State," *Int. J. Clin. Exp. Hypn.,* 20 (1972), 224-237.

Goldstein, A. P. *Therapist-Patient Expectancies in Psychotherapy.* New York: Pergamon, 1962.

Guralnik, D. P., ed. *Webster's New World Dictionary,* 2nd Coll. ed. New York: World, 1970.

Haley, J. *Strategies of Psychotherapy.* New York: Grune & Stratton, 1963.

Hoehn-Saric, R., J. D. Frank, S. D. Imber et al. "Systematic Preparation of Patients for Psychotherapy: 1. Effects on Therapy Behavior and Outcome," *J. Psychiatr. Res.,* 2 (1964), 267-281.

Hollingshead, A. B. and F. S. Redlich. *Social Class and Mental Illness.* New York: Wiley, 1958.

Hull, C. L. *Principles of Behavior.* New York: Appleton, 1943.

Kanfer, F. H. and P. Karoly. "Self-Control: A Behavioristic Excursion into the Lion's Den," *Behav. Ther.,* 3 (1972), 398-416.

Kardiner, A. and R. Linton. *The Individual and His Society.* New York: Columbia University Press, 1939.

Kernberg, O. F., E. D. Burstein, L. Coyne et al. "Psychotherapy and Psychoanalysis: Final Report of the Menninger Foundation's Psychotherapy Research Project," *Bull. Menninger Clin.,* 36 (1972), entire issue.

Kline, M. V. "Freud and Hypnosis: A Re-evaluation," *Int. J. Clin. Exp. Hypn.,* 20 (1972), 252-263.

Kluckhohn, C. and H. A. Murray. *Personality in Nature, Society, and Culture.* New York: Knopf, 1949.

Krasner, L. and L. P. Ullman, eds. *Research in Behavior Modification.* New York: Holt, 1965.

Lazarus, A. *Behavior Therapy and Beyond.* New York: McGraw-Hill, 1971.

Lieberman, M. A., I. D. Yalom, and M. B. Miles. *Encounter Groups: First Facts.* New York: Basic Books, 1973.

London, P. "The End of Ideology in Behavior Modification," *Am. Psychol.,* 27 (1972), 913-920.

----. "The Psychotherapy Boom: From the Long Couch for the Sick to the Push Button for the Bored," *Psychol. Today,* 8 (1974), 62-68.

Lovaas, O. I. "A Program for the Establishment of Speech in Psychotic Children," in). K. Wing, ed., *Early Childhood Autism,* pp. 115-144. Oxford: Pergamon, 1966.

Luborsky, L. "Another Reply to Eysenck," *Psychol. Bull.,* 78 (1972), 406-408.

Luborsky, L., B. Singer, and L. A. Luborsky. "Comparative Studies of Psychotherapies: Is it True that 'Everybody Has Won and All Must Have Prizes'?" *Arch. Gen. Psychiatry,* 1975, in press.

Luborsky, L. and D. Spence. "Quantitative Research on Psychoanalytic Therapy," in A. E. Bergin and S. L. Garfield, eds., *Handbook of Psychotherapy and Behavioral Change,* pp. 408-437. New York: Wiley, 1971.

Marks, I. M. "Perspective on Flooding," *Semin. Psychiatry,* 4 (1972), 129-138.

McGlashan, T. H., F. J. Evans, and M. T. Orne. "The Nature of Hypnotic Analgesia and Placebo Response to Experimental Pain," *Psychosom. Med.,* 31 (1969), 227-246.

Merton, R. K. "The Self-Fulfilling Prophecy," *Antioch Rev.,* 8 (1948), 193-210.

Mesmer, F. A. *Mémoire sur la découverte du magnétisme animal. (Précis historique écritc par M. Paradis en mars* 1777.) Paris: Didot, 1779.

Miller, N. E. and J. Dollard. *Social Learning and Imitation.* New Haven, Conn.: Yale University Press, 1941.

Mischel, W. *Personality and Assessment.* New York: Wiley, 1968.

Mowrer, O. H. *Learning Theory and Personality Dynamics.* New York: Ronald, 1950.

Noyes, A. P. and L. Kolb. *Modern Clinical Psychiatry,* 5th ed. Philadelphia: Saunders, 1959.

Orne, M. T. "Implications for Psychotherapy Derived from Current Research on the Nature of Hypnosis," *Am. J. Psychiatry,* 118 (1962), 1097-1103.

----. "On the Social Psychology of the Psychological Experiment: With Particular Reference to Demand Characteristics and Their Implications," *Am. Psychol.,* 17 (1962), 776-783.

----. "On the Nature of Effective Hope," *Int. J. Psychiatry,* 5 (1968), 403-410.

----. "Pain Suppression by Hypnosis and Related Phenomena," in J. J. Bonica, ed., Advances in Neurology, Vol. 4., *Pain,* pp. 563-572. New York: Raven, 1974.

Orne, M. T. and P. H. Wender. "Anticipatory Socialization for Psychotherapy: Method and Rationale," *Am. J. Psychiatry,* 124 (1968), 1202-1212.

Paul, G. L. *Insight versus Desensitization in Psychotherapy: An Experiment in Anxiety Reduction.* Stanford, Calif.: Stanford University Press, 1966.

----. "Insight versus Desensitization in Psychotherapy Two Years after Termination," *J. Consult. Clin. Psychol.,* 31 (1967), 333-348.

Pavlov, I. P. *Conditioned Reflexes.* London: Oxford University Press, 1927.

Rachman, S. and J. Teasdale. *Aversion Therapy and Behavior Disorders: An Analysis.* Coral Gables, Fla.: University of Miami Press, 1969.

Rieff, P. *The Triumph of the Therapeutic: Uses of Faith after Freud.* New York: Harper & Row, 1956.

Rimm, D. C. and J. C. Masters. *Behavior Therapy: Techniques and Empirical Findings.* New York: Academic, 1974.

Rioch, M. J., C. Elkes, A. A. Flint et al. "National Institute of Mental Health Pilot Study in Training Mental Health Counselors," *Am. J. Orthopsychiatry,* 33 (1963), 678-689.

Rogers, C. R. *Counseling and Psychotherapy.* New York: Houghton, 1942.

----. *Client-Centered Therapy.* Boston: Houghton, 1951.

Rosenthal, R. *Experimenter Effects in Behavioral Research.* New York: Appleton, 1966.

Shapiro, A. K. "The Placebo Effect in the History of Medical Treatment: Implications for Psychiatry," *Am. J. Psychiatry,* 116 (1959). 73-78. Simmons, L. W. *Sun Chief.* New Haven: Yale University Press, 1942.

Sloane, R., A. Cristol, M. Pepernik et al. "Role Preparation and Expectation of Improvement in Psychotherapy," *J. Nerv. Merit. Dis.,* 150 (1970), 18-26.

Sloane, R. B. "The Converging Paths of Behavior Therapy and Psychotherapy," *Int. J. Psychiatry,* 8 (1969), 493-503.

Spitz, R. A. "Hospitalism," in *The Psychoanalytic Study of the Child,* Vol. 1, pp. 53-74 New York: International Universities Press, 1945.

Stampfl, T. G. and D. J. Levis. "Essentials of Implosive Therapy: A Learning-Theory-Based Psychodynamic Behavioral Therapy," *J. Abnorm. Psychol.,* 72 (1967), 496-503.

Strupp, H. H. and A. E. Bergin. *Research in Individual Psychotherapy: A Bibliography.* Washington: NIMH, 1969.

Sullivan, H. S. *The Interpersonal Theory of Psychiatry.* New York: Norton, 1953.

Szasz, T. S. *The Myth of Mental Illness.* New York: Hoeber, 1961.

Truax, C. B. and R. R. Carkhuff. *Toward Effective Counseling and Psychotherapy: Training and Practice.* Chicago: Aldine-Atherton, 1967.

Watson, J. B. *Behaviorism.* New York: People's Institute, 1924.

Weber, M. *The Protestant Ethic and the Spirit of Capitalism.* Translated by Talcott Parsons. London: Allen & Unwin, 1930.

Whyte, L. L. *The Unconscious before Freud.* New York: Basic Books, 1960.

Wilkins, W. "Desensitization: Social and Cognitive Factors Underlying the Effectiveness of

Wolpe's Procedure," *Psychol. Bull.,* 76 (1971). 311-327.

Wolpe, J. *Psychotherapy by Reciprocal Inhibition.* Stanford: Stanford University Press, 1958.

Yates, A. J. *Behavior Therapy.* New York: Wiley, 1970.

Notes

1 The substantive research upon which the theoretical outlook presented in this paper is based was supported in part by grant #MH 19156 from the National Institute of Mental Health and by a grant from the Institute for Experimental Psychiatry.

I am deeply indebted to Henry Gleitman for his many conceptual contributions and extensive editorial comments as well as some particularly apt descriptive metaphors and to A. Gordon Hammer and Lester B. Luborsky for their detailed criticisms and incisive suggestions, and to Frederick J. Evans, Merton F. Gill, John F. Kihlstrom, J. Martin Myers, Emily Carota Orne, and Sydney E. Pulver for their helpful comments in the preparation of this paper.

2 Unfortunately, while it is easy to recognize someone else's treatment as faith healing, it is extremely difficult to recognize such components in one's own practice. The chiropractor readily dismisses his patient's former treatment by a Christian Science practitioner as faith healing as he manipulates the spine and administers megavitamins to "definitively" treat his patient's complaint of fatigue.

3 It is worth noting that with the current regulations governing medical research it would be extremely difficult if not impossible to carry out such a study, and it is likely that a great many more patients would have been exposed to a great many more risks with this procedure.

4 Placebo effects will of necessity play a role in the psychotherapeutic process, as they do in any other form of treatment (an issue that has been discussed by Shapiro [1959]).

5 Freud himself was far less sanguine about man's ultimate plasticity, but environmental determinism became dominant within psychoanalysis as it was transplanted to the United States.

6 Freud stayed closer to the biological theories than neo-Freudians who focused more on social factors and the here-and-now. Nonetheless, these were differences of degree. Even Harry Stack

Sullivan, (1953) who focused very heavily on environmental factors and rejected much of the biological metaphor, nonetheless describes the identification and resolution of parataxic distortions much in the manner used in describing some physical disorder that required resolution. The medical model shared by these workers seemed so ingrained as to have been a background phenomenon largely outside of awareness. It is likely that Adler was least biological in the infrastructure of his views, and this may account for his disproportionate impact on educational thinking, and the tendency for psychiatry, until very recently, to have largely ignored his contributions.

7 For example, the Department of Human Behavior at Yale, where the work of Miller and Dollard (1941) represented a concerted effort at synthesis; the Department of Social Relations at Harvard, only slightly less psychoanalytically oriented, resulting in the close collaboration of Kluckhohn and Murray, (1949) and "the new look in perception" (Bruner, 1951) that focused on dynamic factors in what had previously been seen as traditional areas of psychology; at Columbia the work of Kardiner and Linton, (1939) and so on.

8 The establishment of the Society for Psychotherapy Research has been a recent salutary development in the effort to share information and develop meaningful methods by which various treatments can be evaluated.