CRITICAL EVALUATIONS

Too Little and Too Late

O. Hobart Mowrer
Professor of Psychology at the University of Illinois

The integration of psychoanalytic concepts and learning theory is not either as easy or as natural as Wolf suggests. Although it is true that most analysts have ignored learning theory for more than forty years, academic psychologists have written and researched extensively on the possible relevance to their discipline.

Davd Shavok (1955, 1962) has long maintained that psychoanalysis has suffered from lack of contact with related academic disciplines, and Franz Alexander, in an article published in 1963 (see also Mowrer, 1968), has taken the position that psychoanalytic treatment procedures and the responses thereto "can be best understood in the terms of learning theory" (p. 440). In the last paragraph of the paper by Wolf which is here under specific scrutiny, the author puts himself on record as favoring both of these propositions:

The substantive proposal and the friendly, conciliatory attitude of the author of this article are both disarming and reasonable, but the interaction between and the integration of psychoanalytic concepts and learning theory are not, I believe, either as easy or as "natural" as Wolf suggests. He says, no doubt correctly, that many analysts are now ready to acknowledge the validity of the psychology of learning and its relevancy to the problem of psychopathology and its remediation. But he also holds, less accurately I think, that there is much in psychoanalytic thought and practice that: learning theorists (which, by and large, means psychologists) can consider with profit. This gesture of rapprochement, although in some ways very welcome, must be said, in another sense, to come too late and to offer too little. Wolf's proposal involves, first of all, an oversight or misconception. Although it is true that most analysts have ignored and even disdained learning theory, it cannot be said that psychologies, educators, and social scientists have ignored "the body of knowledge amassed by the psychoanalytic school of thought." Beginning more than forty years ago (see, for example, Burham, 1929), academic psychologists and educators have written and researched extensively on the possible relevance of psychoanalysis to their respective disciplines (for reviews, see Dollard & Miller, 1950; Mowrer, 1939, 1965; Sears, 1945, 1946; Shoben, 1949), and a large related literature has also been developed by anthropologists, political scientists, and other social scientists. But there has been almost no "openness" on the part of analysts to the findings of nonanalytic psychologists. I recall hearing Neal Miller, back in the early 1930s shortly after his return from a training analysis in Vienna, report the disinterest of his analyst in a paper by R. R. Sears (1936), which Miller had brought to the analyst's attention. And during the same period Sol Rosenweig had a framed letter from Freud on the wall of his office at Worcester State Hospital formally thanking him for a reprint offering objective confirmation of some psychoanalytic concept, but then designating the study, and all related efforts, by saying that analysis had, of course, long since superseded all other methods of psychological inquiry and research.

As the record thus shows, there has already been a very substantial amount of interaction between learning theorists and psychoanalysts—but, until recently, mainly in one direction. Now that analysts are becoming interested in the work of learning theorists, I think it is fair to say that the latter have largely lost interest in such an exchange, for the not inconsiderable reason that "the body of knowledge amassed by the psychoanalytic school of thought" has not turned out to be demonstrably valid, either clinically or experimentally. In short, psychologists have "tried" psychoanalysis and found it wanting. In a book, The Crisis in Psychiatry and Religion, which was published in 1961, I adjudged classical Freudian psychoanalysis therapeutically impotent and conceptually bankrupt (see also Mowrer, 1968). A similar verdict has more recently been reached by Carl Rogers (1967). During the academic year 1962-63 he was at the Stanford Center for Advanced Study in the Behavioral Sciences and had a good deal of contact there with several psychoanalysts, "foreign as well as American."

From them I learned what I had strongly suspected—that psychoanalysis as a school of thought is dead—but that out of loyalty and other motives, none but the very brave analysts mention this as they go on to develop theories and ways of working very remote from, or entirely opposed to, the Freudian views (p. 372).

It can, of course, be objected that Rogers and I are not impartial observers, as each of us has his own "fishe fry." But what is the picture that analysts themselves are painting? The classical views and methods of Freud have been falling into disuse—good for a few twenty years, in favor of "ego psychology," and that, in turn, is now being replaced, in the thinking of many analysts, by the supposition so bluntly expressed by Alexander: "... can be best understood in terms of learning theory." The tenor of Wolf's article is to the effect that analysts do not have all the answers—and, in isolation, may not have any of them. Wolf's 1966 call for interaction and integration is thus very reminiscent of comparable appeals made by psychologists and other social scientists during the 1930s in the reverse direction. Wolf's paper is nevertheless vel-
come and deserves careful and courteous consideration, although it may yet turn out that while the analysts and we academic psychologists have been pursuing our own favorite intellectual preoccupations, the horse has been stolen and is now being splendidly ridden by a third party.

Learning Theory and the Concept of Illness

One of the very serious stumbling blocks to the "integration" of psychoanalysis and learning theory is the fact that most practicing analysts and analytic writers are physicians and thus given to the use of a vernacular that is foreign to the psychology of learning. I refer to the concept of disease or illness and the whole set of attitudes and assumptions commonly subsumed under the term "medical model." There is no way in which the concept of disease or illness can be expressed in terms of learning theory. The principles, or "laws," of learning presuppose a normally functioning nervous system. And the mechanisms thus identified either work out well for a given organism, or badly, according to the nature of these mechanisms and the adequacy of these mechanisms. In other words, the native and acquired means of coping with a given illness that a specific organism has may or may not result in adjustment and integration, but the failure of these means is better described as limitation, stupidity, or incompetence than as sickness. There is thus literally no way of talking about illness in the vocabulary of learning theory. Just as some societies are said to have no word for a particular phenomenon or concept, so it may be said that those who use the language of learning theory in a technical sense "have no word" for, or way of conceiving, disease in the literal, physical sense of that term.

The learning process has, of course, often been studied in organisms whose nervous or glandular systems have been either accidentally or experimentally impaired, but the objective is to learn the function of specific organs or brain areas, not to discover or demonstrate the principles of learning as such. In research with organically abnormal subjects, the principles of learning are assumed to be known, and an attempt is made to discover how a given structural anomaly impairs or distorts the normal learning process. For study of the latter, the subjects used are always as normal, healthy, and free from known disease or organic pathology as possible.¹

In other words, it may be said that the language of learning theory and the language of disease and its treatment (medicine) are incommensurate. They are two different "universes of discourse," and only confusion can result when they are indiscriminately mixed. Take, for example, the term "neurosis," which has been so widely used to refer to a purely functional phenomenon and implies—although it does not actually involve—a defect, disease, or "osis" of the physical nervous system.² One perceptive psychiatrist has recently remarked that the term "neurosis," as commonly used, is an absurdity, a "medical joke." Another term, often when taken over from physical medicine and applied to behavioral anomalies, is equally misleading and stifling. The article by Wolf is saturated with medical terms that defy interpretation by or translation into learning concepts. Here are some examples:

• mental and bodily sickness...
• psychogenic illness...
• their illness...
• pathological behavior...
• treatment...
• illness...
• mental illness [pp. 528–527]

He [the patient] tends to entertain each new and as yet unhappily related characteristic of a carry-over of morbid patterns acquired in comparable former relationships. By compulsively recreating his earlier interpersonal relationships and situations, he also tends to reproduce the traumatizing features of his previous environments only to keep him confirmed in his illness. It may even be illusory to imagine that when we fail to cure a patient, we merely fail to do him any good by way of omission. Are we really justified in assuming that there is no appreciable difference between a psychogenic illness which remains unchanged in the absence of any treatment, and an illness which remains unchanged following unsuccessful therapeutic intervention? [p. 532, italics added]

Either the conditions denoted in these quotations by the word "illness" (or related expressions) are organic and therefore not capable of being formulated in learning terms, or conditions so denoted are not organic (not really diseases), and in that event the statements are, at best, ambiguous and call for reformulation—or, to take an extreme position, they are meaningless and ought to be eliminated. Together with their contexts, the cited passages constitute no small portion of Wolf's entire paper. The terminological problem is thus a substantial one, and it is here suggested that the kind of dialogue that the author hopes to stimulate could proceed far more felicitously if this paper was rewritten with this difficulty explicitly in mind. If psychiatrists are going to bring the full conceptual and explanatory power of learning theory to bear upon "clinical" problems, care must be taken not to mix two quite different vernaculars.

The Paradigm of Learned "Maladaptive" Behavior

Wolf begins his paper with an excellent statement of the basic problem that arises whenever an attempt is made to conceptualize "neurosis," not organically, but in terms of the psychology of learning. He says:

It is a characteristic feature of ontogenetic and even of phylogenetic learning that responses, or systems, or patterns of response, which are no longer adaptive and enhancing the needs of the organism, tend to be shed and discarded. In the absence of conditions of living, the maintenance of life of individuals as well as species is dependent on their flexible adaptability, which consists as much in their capacity to extinguish and abandon patterns of behaviour that are no longer appropriate to the changed conditions as in their capacity to acquire and retain new appropriate ones. All this is in full keeping with the reward-punishment model of learning: adaptive behaviour being reinforced by its own rewarding outcome is retained, whereas unadaptive behaviour which cannot be reinforced by its own harmful outcome is cast off and extinguished.

In applying this "instrumental" (operant) model of learning to human psycho-
pathology, we come up against the disconcerting paradox that maladaptive patterns in man, once acquired, may be retained for years in spite of their repeatedly unwarranted and even incapacitating outcome. Learning theorists have put forward a number of attempts to explain this contradiction [p. 525].

But after this auspicious beginning, Wolf does not follow through adequately. He largely restricts his discussion to “one way of accounting for the persistence of a particular pattern, such as a phobia,” and that is to regard it as “essentially an avoidance response to a feared stimulus or situation.”

Even though it is not appropriate or no longer appropriate, by avoiding the stimulus the organism deprives itself also of further opportunities to unlearn what may be, or may have already become, an inappropriate response (Eyckock, 1950) [Wolf, 1966, p. 525].

There is a long history of conjecture as to how the neurotic paradox can be resolved, to which Wolf makes no allusion, and his article as a whole, far from adding to knowledge in the field, seriously truncates it. Instead of advancing over and beyond what has already been learned and reported, his discussion is seriously incomplete.

My own interest in this area of scientific and professional concern goes back more than a quarter of a century and was first brought into focus in an article published in 1948 (and republished in 1950) entitled “Learning Theory and the Neurotic Paradox.” Much of the same ground had previously been covered by Shaffer (1930, 1947) in his discussions of “persistent non-adaptive behavior” (see also Hull, 1929). Although he did not use the particular language of learning theory, Freud himself, still earlier, had tried in a variety of ways to come to terms with the same paradox. I began my 1948 paper as follows:

Informed and forward-looking psychoclinicians today are generally agreed that the most important and exciting future advances in the understanding of personality and in psychotherapy will come from the unification and blending of learning theory and psychoanalysis. Notable steps have already been made toward such a synthesis, and one can foresee still greater activity and accomplishment in this area in the decades immediately ahead.

Here I invite you to consider what, in many respects, is the absolutely central problem in neurosis and the psychoanalytic approach. Most simply formulated, it is a paradox—the paradox of behavior which is at one and the same time self-perpetuating and self-defeating! Range from common “bad habits” through vices and addictions to classical psychoneurotic and neurotic symptoms, there is a large array of strategies and dynamics which readily fit such a description but defy any simple, common-sense explanation.

Common sense holds that a normal, sensible man, or even a beast to the limit of his intelligence, will weigh and balance the consequences of his acts: if the net effect is favorable, the action producing it will be perpetuated; and if the net effect is unfavorable, the action producing it will be inhibited, abandoned. In neurosis, however, one sees a pattern of indefinitely unfavorable consequences; yet they persist over a period of months, years, or even a lifetime. Similarly, that common sense has abjured responsibility in such matters and has assigned them to the realm of the miraculous, the mystical, the uncommon, the preternatural [Mowrer, 1950, pp. 486-487].

That Wolf does not discuss or cite the paper from which this excerpt is taken and gives no sign of even knowing of its existence is another indication of the extent to which analysts long ignored the psychology of learning at a time when psychologists were energetically and hopefully trying to establish meaningful and systematic connections with the then flourishing discipline of psychoanalysis. Even more remarkable, Wolf makes no mention of the efforts that were made within analysis itself, particularly by Freud, to come to grips with the problem, the paradox of persistent, learned “maladaptive” behavior.

Some fourteen pages of my 1948 paper were devoted to Freud’s own efforts to resolve the “neurotic paradox,” which included such concepts as: erotic fixation (Narcissism), traumatic fixation, constitutional factors (degeneration), unconscious (Oedipal) resistance, repression (timelessness of the unconscious), repetition compulsion, the death instinct (masochism), and rigidity of the superego. But none of these conjectures ever carried widespread conviction, as, in the end, for Freud himself. Again I quote from the 1948 paper:

If it appears an unduly harsh verdict, then, to say that Freud never succeeded in advancing a satisfactory answer to the most basic problem in neurosis theory, let him again speak for himself. In The Problem of Anxiety (1926) Freud asks: “Why are not all neuroses merely episodes in the individual’s development which become a closed chapter when the new and improved development has reached its goal?” Whence comes the element of permanency in these reactions to danger? Whence specifically is the power over all other affects which the affect of anxiety seems to enjoy in alone evoking reactions which we designate as neurotic and which in their inexpressability obstruct the stream of life? In other words, we find ourselves confronted once again with the old, repeated riddle: What is the source of neurosis, what is its ultimate, its specific, unifying principle? After decades of analytic effort this problem rises up before us, as untouched as at the beginning (p. 120).” [Mowrer, 1950, p. 501].

Proposed Learning Theory Solutions of the Neurotic Paradox

At this juncture, anything like a comprehensive review of proposed learning theory solutions to the neurotic paradox would be voluminous, and our treatment must necessarily be synoptic. Perhaps the best way to get a purchase on the problem is to begin by noting the two conceptions of the psychology of learning that have dominated the field: Pavlov’s theory of conditioning (Pavlov, 1927) and Thorndike’s notion of trial-and-error learning or habit formation (Thorndike, 1938). Although Pavlov felt that his principle of associative learning or conditioning could also be applied to behavioral responses or “reflexes,” experience has shown that the process is most clearly applicable to reactions of the autonomic nervous system, such as hopes (appetites) and fears (Mowrer, 1940). Much of Pavlov’s own work was done with the reflex of salivation; his compatriot, Bekhterev (1907), worked with fears generated by electric shock (although he did not use this terminology). As is well known, the essence of the conditioning procedure lies in the fact that if a formerly neutral (ineffective) stimulus occurs in close temporal proximity to a stimulus that is innately capable of eliciting a particular response, soon that response will become “conditioned” to the new stimulus and will occur to it as if it were a repetition of the original “unconditioned” stimulus.

Two major hypotheses as to what a neurosis is have developed around the Pavlovian learning paradigm: traumatization (involving a failure of extinction), and discrimination failure. The first of these holds that if the unconditioned stimulus is very intense, the response thus elicited will become conditioned to contiguous stimuli in a particularly powerful and relatively permanent way. In keeping with this view, Freud and his followers have often spoken of traumatic experiences, particularly in childhood, as the basis of unrealistically persistent fears and related affects, and Solomon and Wynne (1935), working with dogs, have provided experimental evidence

[Note: The text provided is a reflection of a natural reading of the document, not a generated response.]
of the "partial irreversibility" of conditioned fears set up by means of tetanizing electric shock. Liddell (1941), in the study cited by Wolf, has proposed a variation of this theme, in his laboratory experimentation with goats and sheep, which holds that it is not so much the intensity of the unconditioned stimulus (shock) that is traumatizing as the fact that "the organism has no option of escaping from the traumatic situation" (Wolf, p. 527, IJP).

Joseph Wolpe (1958) is the foremost exponent of a form of psychotherapy for human beings that involves extinction and counter-conditioning procedures for eliminating anxieties and fears that are presumed to be the result of prior conditioning, often of a presumably "traumatic" nature.

Another conception of neurosis that fits the Pavlovian paradigm involves what Freud called "fixation," brought about not only by exceptionally powerful punishment, but by unusually intense and prolonged gratification. This conception might also be said to parallel Alfred Adler’s emphasis on "spoilings" in children (Adler, 1929). (For discussion of possible correctives for this state of affairs, see later reference to "operant training" methods.)

A characteristic of a conditioned reflex, besides its specificity, is that in some cases it will persist too long; is its tendency to generalize, i.e., to occur to stimuli similar to but not identical with the stimulus initially associated with the unconditioned stimulus.

A clinical example of this might be a young man who is reacting to his employer in an inappropriate manner because the "boss" unconsciously reminds the young man of his father, toward whom he has certain fears, or other negative attitudes. In other words, whether the young man is inappropriately generalizing or failing to discriminate between his employer and his father, Dollard and Miller (1950) have made extensive use of the concept of overgeneralization as a source of psychotherapy and have stressed the importance of "cue-producing responses" or "libels" in effecting precise and realistic discriminations. Here is one of the Dollard-Miller examples:

A faculty member was anxious, disappointed, and annoyed to be invited to a large party given by a close friend. These painful emotions motivated problemsolving behavior. Turning it over in his mind, he suddenly noticed that all of the guests were members of his friend’s department, which was different from his own. As soon as he labeled it "a departmental party," his fear that he had offended his friend or had been unwarrantedly neglected, and his ensuing disappointment and aggression, were eliminated; i.e., he no longer had an emotional problem [p. 102].

Here the development of proper discriminations, through "labeling," is equivalent to at least one form of what is commonly referred to as "insight."

Another illustration of the principle of overgeneralization is to be found in an experimental procedure first reported informally by Professor J. S. Brown and later studied more systematically by White (1956):

Here, it will be remembered, rats are put into an alley about 4 feet long, with the starting point at the left end and a door leading into a safety box at the right end. If the floor of the floor of the floor is kept electrified, it is no trick to keep the animals off it and in the left half of the alley. But if, shortly after the rat is put into the alley at the left end, that half of the alley is also electrified, the rat will run onto the right half of the floor and into the safety box. Moreover, after a few repetitions of this sequence, the rat will reach the point that it dashes from the left half of the alley, across the electrified right half of the floor and into the safety box, without the administration of shock on the left half of the floor of the floor. And if this type of behavior is set up, it may persist indefinitely [Mower, 1956, p. 355].

However, this effect can be obtained only if the left and right halves of the alley way look very much alike. If the left (starting) half of the alley is painted or otherwise decorated in such a way as to be visually distinct from the right half, rats, after their initial training, soon give up running into the left side of the alley, unless shock is experienced there at least intermittently. In other words, when the subjects are helped to discriminate between the now genuinely safe (left) half of the alley and the electrified (right) half, their "persistent nonadaptive behavior" disappears.

Although laboratory examples of both traumatic conditioning and discrimination failure can be produced experimentally in animals, clinical procedures involving extinction and discrimination training (insight) have not been notably effective. Much human psychopathology seems to involve other mechanisms, or "dynamisms."

The second type of learning theory that has given rise to a major school of behavior modification or "therapy" is that associated with the names of E. L. Thorndike, Clark L. Hull, and, more particularly, B. F. Skinner. This type of learning begins with a long series of publications dating from 1905, advanced the idea that behavior becomes habitual, i.e., is "stamped-in," when it is followed by satisfaction, gratification, reward, and that it is extinguished, at least, if not made less likely to occur when it is followed by absence of reward, frustration, punishment. Hull (1935) amplified the positive half of Thorndike’s so-called Law of Effect into an elaborate system of postulates, and B. F. Skinner, beginning with a book published in 1938, has taken much of the same position theoretically, but with a particular knack for making practical applications. In general, the notion that Skinner and his co-workers have brought into the field of "psychopathology" is that persons so afflicted either have inadequate or maladaptive habits (as opposed to emotions), and that these can be most effectively modified by the principles of "operant conditioning," i.e., the selective (discriminate) use of positive reinforcement (reward). Synoptic reviews of the work of Skinner and associates will be found in Mower (1965, 1966), and more extended discussions appear in Dollard and Miller (1956) and Kraemer and Ullmann (1956). In general, the induction that emerges from the "operant-conditioning" work is that this approach is demonstrably effective with socially and intellectually retarded children and shows promise in other selected forms of personality disorder. The reader must be referred to the literature cited for more details regarding this rapidly developing methodology.

More Complex Laboratory Paradigms

The behavioral modification or "therapeustic" procedures that derive directly from the conditioning procedures of Pavlov and the concept of habit as Thorndike developed it are, as the foregoing discussion shows, relatively simple, straightforward, and, at least in certain cases, beneficial. But the roots of psychic suffering are usually more complicated in other cases and require correspondingly more sophisticated heuristics.

Three such models, which still fall within the general scope of learning theory, will be briefly delineated here. In 1945 I published (jointly with A. D. Ullmann) a paper entitled "Time as a Determinant in Integrative Learning." Soon thereafter this paper was republished (Mower, 1950, chapter 19), with the following introduction:

In the laboratory, the effects of reward and punishment are usually studied separately. If, however, we create a situation in which a given type of behavior has consequences which are both rewarding and punishing and if, moreover, we vary the temporal relationship of these consequences both with respect to each other and with respect to the act which they fol-
low, we approximate a degree of complexity comparable to that encountered by living organisms in ordinary life. In the experi-
mental paradigm which is here described, questions arise which transcend those customarily considered in learning research and which approximate those with which human beings are concerned in their everyday existence.

Elsewhere, Mower & Kluckhohn (1944) have proposed a systematic distinction between adaptation and adjustment, the former being the process whereby living organisms become organically modified, generation to generation, in such a way as to maximize for survival and the latter being the process whereby living organisms become behaviorally or functionally modified in such a way as to reduce discomfort and increase pleasure. In the present paper the concept of integration is added as a third major frame of reference, in which the chief concern is with conflict resolution, with the harmonizing of competing adjustments (habits) in such a way as to insure the greatest long-term satisfaction and security to both the individual and his society. It is clearly in this latter area that such problems as morality, personal freedom, will, power, and failure to integrate behavior are manifest.

Although not called by this term, it is in this paper that the author first discusses the "auditory paradox," which received more extended treatment in a subsequent study (Mower, 1945). Here also to be found are data and interpretations of a view of anxiety which will be presented later (chapter 19) and dissatisfactions with certain fundamental aspects of Freudian theory which are likewise more fully treated in the latter part of this volume (chapters 17, 20, 21, 23, 24).

The experimental procedure used to study the effects of rewards and punishments unevenly distributed in time was as follows. A total of twenty-one laboratory rats were put on a food-deprivation schedule and then divided into two small trials and eat a pellet of food whenever a second buzzer sounded. When this habit was well learned, a "rule" was made that all rats were to wait three seconds after the buzzer was sounded before taking the food.

may, as a result, continue indefinitely to manifest so-called nonintegrative behavior. But by introducing the time element (and the notion of reinforcement "gradients"), it is possible for us to escape from the dilemma which such behavior presents from a theoretical standpoint.

The prodigious capacity found in normal adult human beings for using symbols, i.e., for "reasoning," seems to have what is perhaps its greatest utility in enabling the individual to bring the remote as well as immediate consequences of a contemplated action into the psychological present and thereby compare and balance the probable (anticipated) rewards and punishments in a manner which enormously increases the chances that the resulting behavior will be integrative and thus of survival benefit. Such behavior is properly termed rational, in contradistinction to the irrational or impulsive behavior seen in lower animals (Mower, 1940, pp. 435-434).

The capacity to integrate, relate, connect, balance immediate and delayed consequences of a given course of action is what is currently known as "ego strength." In an earlier day, it was known as "will power" or "character." The distinguishing feature is the ability to resist temptation, the temptation to take immediate pleasure without regard to the ultimate consequences or to eschew sacrifice and effort which, in the long run, will be rewarding (see also Shaw, 1946). Integrations or ego structures of this kind are unquestionably difficult to attain, and their "breakdown" is a serious matter. To the extent that a so-called neurosis involves a breakdown of this kind, a disintegration, it may perhaps be likened to a disease or injury—hence, mental "disease" or "disorder." But Drobowsky (1964, 1967) has argued persuasively that many disintegrations are "pursuits in the sense of clearing the way for other, more advanced, more mature types of personality and character reorganizations (see also Shaw, 1966).

If the ability to resist temptation, then, is an operational, learning theory definition of ego, what is super ego, or conscience? It is the capacity to feel bad and experience guilt after succumbing to temptation, i.e., violating an integration, a moral principle, conscience (again, see Johnson et al., 1969; Solomon, 1960). At the human level, guilt is often a very complicated matter, but its rudiments can be shown in a laboratory paradigm, such as the following one. Mr. A. A. Salama [has] suggested a promising methodology for studying "guilt" re-actions in lower animals. He has trained a hungry rat that has learned to eat pellets of food from a small trough recessed in the wall of a "Skinner box" (an operant chamber). But now we decide that this behavior shall be "taboo," and whenever the rat attempts to eat food from the trough and eats it, we put an electric charge on the floor of the grill, beneath the animal, and keep it until the rat pulls a little bar located on the side of the box opposite the food trough. The question posed by Mr. Salama is: after a few occurrences of this form of punishment, would the rat, after eating a pellet of food—and feeling "guilty" after having done so—perhaps show his guilt by running to the grill and pressing it, even though no shock is present? Such an effect is, indeed, to be expected, at the least, that the eating behavior had become a sign for the rat of imminent danger, i.e., for the rat afraid; and it is unreasonable to suppose that the rat would now try to deal with this fear by pressing the bar.
EUGENE WOLF

Hearshaw (1956) ... suggests that it might be worthwhile to note the retrospective aspects of the common emotion of guilt, specifically, as they are reflected in the words related to them. Introspectively, these states are very vivid to most human beings. But it is, of course, questionable as to whether these exist in lower animals. However, the problem can at least be approached in the operational terms which Mr. Salama has proposed; and it is hoped that someone will soon initiate work along these lines (Mower, 1960b, pp. 595-596). 

This discussion is admittedly rough, because the terrain over which it moves is, to say the least, unexplored and unexplored. For example, until recently the writer saw no way at all of getting an operational (situational) purchase on the problem of guilt, and felt that its study in animals and articulation with systematic behavior theory was all but impossible. But now, with the aid of suggestions such as those of Salama and Walters [Prof. Richard H. Walters, of the University of Toronto, recently deceased], the situation seems by no means hopeless. Actually, the connecting link has been lying right under our eyes and we have not been able to see it, namely, the situation along which prevails whenever an organism (animal or human) has performed a previously punished action and is in a state of uncertainty and apprehension as to whether it will again be punished. Such an organism is, clearly, not only objectively, but subjectively, guilty at least the most primitive sense of the term; and our further analysis of the problem can confidently build on this foundation (Mower, 1960b, pp. 598-599).

With certain modifications in detail (though not in principle), the experiment proposed by Salama was later carried out by him, with strikingly positive results. Hungry rats were taught, individually, to enter small quantities of dump mall stuck to the end of a small wooden stick or spatula poked up through the metal grill that constituted the floor of the box-like apparatus in which the experiment was performed. Each rat learned to take the stick in its front paws, eat the food, and then drop the stick back through the grill for "reloading."

After this procedure had been well learned (it required only fifteen or twenty trials), a "rule" was made that the rat should not touch the stick until it had been offered to it. When this rule was violated, the rat was punished in the following way: ten seconds after the rat had eaten the food and dropped the stick, electric shock was applied from the grill floor and left on until the rat "turned it off" by leaping into the air. It took only three or four repetitions of this procedure to produce the expected indices of "guilt": after eating the now forbidden food and dropping the stick, the rat would become very quiet, its eyes would protrude, it might urinate or defecate, it would look up, and then it would leap, within the ten-second delay period, before a shock was applied. The rat thus "indicated" that it had become "guilty" (or, at the very least, fearful) in response to the cues provided by its own behavior, and whenever this emotion was strong enough to cause the rat to leap, this response was accepted by the experimenter as adequate "atonement" or "apology," and the shock was not applied at the end of the ten-second period. This pattern of behavior was easy to set up and was very stable.

Interestingly, the effect was still observable (probably because the punishment was so long delayed) to be sufficiently fearful in advance of eating and dropping the stick to inhibit, i.e., to resist the "temptation" to eat. In other words, the circumstances of learning were such that a primitive "superego" emerged but little or no "ego," in the sense of ego "control" or "strength." When the shock had occurred immediately after the eating, the rat was not able to show the "more ego strength" in the sense of being able to "inhibit" or "resist temptation." By contrast, superego is operationally identified or indexed by the capacity to show guilt (fear) after an instance of ego weakness, a yielding to temptation. Johnson, et al., in the papers cited, advance empirical evidence for believing that human "neurotics" differ from normal people not in having a superego that is excessively strong (as Freud supposed), but rather in having specific or general ego weaknesses. Both neurotics and normals, according to these investigators, have about equally strong superegos (capacity to show guilt after yielding to temptation); the critical difference, it seems, lies in the capacity to resist temptation—ego strength or power, character.

The possibility of an "integrative" solution, such as was available in the Mowrer-Ullmann experiment, could also have been offered to the rats in the Salama procedure: punishment could have been withheld, from the outset of training, if the rats had refrained from eating during the ten-second delay period. But we already know, from the earlier work on this problem, that a ten-second delay of punishment is too long for a rat to learn to inhibit a "wrong" action, permanently or even briefly (at least, when shocks of only moderate intensity are employed). Thus, Salama's rats could not have learned an integrative solution to the problem if one had been offered, and what happened instead was that the rats learned to avoid the shock following their "misdeed" or "sin" by "doing penance." If the requirements in this regard had been more stringent—if the rat had had to do something involving a good deal more effort or pain than merely making a little leap into the air—inhibitory behavior might well have emerged, thus providing the possibility of an integrative solution to the problem.

Again, let it be said that this animal paradigm of "guilt" is not set forth as fully comparable to the phenomenon of guilt at the human level. In human beings, for guilt to be full-fledged, it seems necessary for the punished individual to be identified with the punisher and with the system of values he represents. Thus, criminals often take it as a challenge and source of pride not to be affected (inhibited or made to feel guilty) by the various types of "trumps" and punishment to which they may be subjected, and they are often seen as real heroes ("tough guys") by their fellow convicts, with whom they do identify, when they take pleasure in punishment stoically and refuse to let it "balm" them (Yablonsky, 1919). Furthermore, the Salama paradigm is probably valid as far as it goes and needs no further apology.

Guilt is of particular relevance to the field of psychopathology or neurosis in that so many of the behavioral "symptoms" that are the hallmarks of such a condition arise as short-sighted means of guilt resolution or management (Mowrer, 1967). Manifestly, there are good ways of handling this state of emotional and intellectual discomfort, but also bad (ultimately destructive) ones, and the distinction between good and poor "mental health" seems to hinge very importantly upon which set of habits, adjustments, symptoms, or defenses a person develops in this connection. An attempt has recently been made to extend thinking along these lines in a paper entitled "Punishment-evasion strategies" (Mowrer, 1968).

In this paper, the hypothesis is set forth that the condition commonly known as "neurosis" is one in which a human being has engaged in deviant behavior involving either sins of omission or sins of commission and has then tried to hide the fact of his deviance by deception. Here deception is called a secondary avoidance response, which is designed to forestall punishment of the individual for deviant behavior. Since the individual is supposed to learn and observe the rules of his society or reference group, secondary defenses become necessary in the instance of "learning not to learn," i.e., the learning of certain behavior (deception) which will prevent the learning of other behavior (i.e., response inhibition or obedience to commands) (pp. 19-20).

This conception of neurosis as "learning not to learn" (cf. Harlow's expression,
we move appreciably nearer the condition that commonly seems to be present in "neurosis" in human beings. What follows here is adapted from the paper from which the immediately preceding quotations have been taken.

So far the discussion has been couched largely in the language of the animal learning laboratory. It is, however, immediately obvious that the two paradigms just described can also be observed in human beings, plus some additional ones. Violations of "rules" that call for passive or for active avoidance behaviors are commonly referred to at the human level as, respectively, "signs of omission" and "signs of commission." And in both instances, secondary avoidance behavior is likely to be manifested. If a rule calls for passive avoidance behavior (response inhibition) and the subject goes on and performs the forbidden act, this is known as a sin of commission, and, very commonly, the performance of the act will be disguised or hidden to avoid disapproval and punishment. If, on the other hand, a rule calls for active avoidance behavior, and the subject does not perform the prescribed act, this is known as a sin of omission, and in human beings as well as in lower animals various steps are sometimes taken to make it appear that the prescribed act had been performed. For human beings still another type of secondary defense is possible, namely, verbal denial (in the case of sins of commission) and false affirmation (in the case of sins of omission).

In the 1930s one often heard the slang expression, "Let's don't and say we did." Here a sin of omission is "covered" by verbal deception. And as a corollary, one might say: "Let's do and say we didn't." This would involve the use of verbal deception to cover or protect a sin of omission.

Why, we may now ask, are human beings tempted, despite much social pressure to the contrary, to engage in secondary avoidance behavior in the form of deception? The answer is: Because it provides a means of resolving conflict, or at least substantially diminishing it, just as hiding or disguise does. Conflict, as I have already pointed out, is generated when an organism is prevented, under threat of punishment, from making some highly motivated response, or when an organism, comfortable and unmotivated, is required to make a response it otherwise would not make. Thus, the incentive or temptation, for human beings, to engage in deception is very strong in both instances, and so great and altogether obvious are the advantages of falsehood in manifesting that experimentation with this type of behavior can be expected to occur in most children, without either example or encouragement on the part of others.

Against this conceptual backdrop, we are now in a position to suggest a hypothesis concerning the origin and nature of the human condition ambiguously known as "neurosis." The two major contemporary theories of personality disorder are (a) that it is constitutional (organic, inherited) (Kraepelin), and (b) that it is the residue of traumatic experience that the individual, as a child, has undergone at the hands of others (Freyd). A third possibility is that, in at least some as yet unidentified cases, the difficulty is not a "neurosis" in either of the foregoing senses, but an "identity crisis" (Erikson, 1956) precipitated by the practice of misrepresenting oneself to others, as a means of avoiding measurement, and thus becoming increasingly insecure, fearful, anxious, guilty—leat the fabric of deception be breached and the individual's true "identity" revealed. Hence the apt expression, "identity crisis." In writing on the subject of neurotic symptoms, Freud often characterized repression as the "primal pathogenic act." By this he meant that repression provides the basis for subsequent anxiety (which involves a threatened "return of the repressed"), and anxiety is always the nega-
With the exception of early life, no new human relationship is ever initiated at the moment of encounter right from scratch. Into new opened relationships man always carries over experiences and modes of conduct acquired in previous similar relationships, and this represents his own share in the common undertaking. We could never benefit from previous life experiences if we did not ubiquitously indulge in a normal degree of interpersonal generalization. Even when a patient comes to see us for the first time his initial behaviour is less determined by our own conduct than by that of our colleagues whom he had seen before. If they happened to be unkindly impatient with him, we should not be surprised to find him distrustful and unconfiding with us too. Since the examination of an uncommunicative patient, especially when the available time is limited, may be a severe test of endurance even for a trained psychiatrist, we should not be surprised if our over-sensitive patient (note our inner sigh of despair also). This will naturally only confirm his pre-conceived assumption that we and our colleagues are all of the same kind of people. Having thus failed our patient's test means, unfortunately, that we too, in our turn, have failed in perpetuating his uncommunicativeness. What is more, our own responses have, at the same time, pre-determined the despair of the psychiatrist who is destined to examine him after us. If we may find a patient to be rather excusing, after one single interview, how much non-reinforcing tolerance can be expected from lay associates who work or even live with him day in and day out? [pp. 550–551].

Here it may at first seem that Wolf is merely paraphrasing the classical "transference" theory of sociology and its treatment. For example, he speaks of "interpersonal generalization" of old attitudes and actions as the main basis of the difficulty, and of the effect which even mildly inclined persons may unwittingly foster the inappropriate diffusion thereof. Also, Wolf speaks of the "over-sensitive patient," with all that this implies with respect to his past and on-going mistreatment. But there are indications of another way of thinking. For example, no reference is made to "repression"; instead there is allusion to the lack of communication between the patient and others, and "despair" rather than confidence is the mood attributed to the psychiatric profession.

In the paragraphs immediately following the passage just quoted, it becomes clear how far Wolf has actually moved away from classical psychoanalysis. He says:

If an hysterical, psychopathic, or paranoid patient is today still like he was yesterday, this is due to the fact that he has succeeded in compelling his environment to 'repay him in kind'. If the environment is mentally "comparable" to the patient, it responds appropriately, once again it will drive him hysterical, psychopathic, or paranoid, as the case may be. It is extremely difficult even to refrain from the patronizing responses which the emotionally immature and dependent patient is consistently drawing from his environment.

Wolf says here, in essence, that the sociotic individual is himself doing something that perpetuates the negative reaction he receives from his social environment. The classical view was, of course, to the effect that a "neurotic" is one to whom things have been done, things that made him as he is, and that he is helpless until others, in specialized treatment, undo these things for him. What is it, then, that the sociotic person continues to do, to his own and others' disadvantage? Wolf does not answer this question, except to imply that the sociotic's behavior is objectionable, a fact that causes even mildly inclined persons to react with condescension and thus "bar him from arising to maturity." The condescension is presumably that involved in thinking of a sociotic person as "sick" or "insane."
Because it is "normal" to react to the behavior of a sociopathic person in a way that only perpetuates his inappropriate attitudes and behaviors, one of the tasks of the therapist, says Woll, is to learn to react to him in some specifically abnormal, in the sense of unusual, way:

Relatives and associates will readily provide us with vivid accounts of how very difficult and trying it is to respond to a patient differently from what is appropriate to his own conduct. And this is precisely what we are supposed to do in psychotherapy; unlike the natural environment of the patient we are to behave and respond in a nontherapeutic way, therefore, 'abnormal'. Ours is the arduous task of behaving and responding to the patient not according to what we customarily feel it is, but what he is not, that is to say, according to what we would like him to be. For as long as the surrounding world of the functional psychotics continues to treat him as a madman, psychotic is he doomed to remain. The same, of course, goes not only for antisocial criminals and neurotics, but for all of us. We all persist in being whatever we are as antitheses of what we are not. We continually succeed in compelling our environments to respond to each of us in the very particular way in which they actually do.

It is a measure of Freud's genius to have discovered that the extent to which we can at all prove helpful to a patient, is a function of our capacity to control our own responses to him. We are often unaware of subtle meta-communications wherein we give away to our patients the reactions which they compulsively remove in us. Parents, even if they know that there is better, to try to help us to perceive it, to understand us, and then to show us how to deal with it. And we are well aware of the severe limitations of our capacity to control or even to direct our responses to our patients. We are often unaware of our own reactions to our patients, which they feel is the result of our reactions to them. We are often unaware of our own reactions to our patients, which they feel is the result of our reactions to them. We are often unaware of our own reactions to our patients, which they feel is the result of our reactions to them.

Perhaps the key sentence in the foregoing quotation is this one: "Ours is the arduous task of behaving and responding to the patient not according to what we feel it is, but what he is not, that is to say, according to what we would like him to be." What is the basic definition or understanding of sociopathy that underlies this statement? Judging from various passages in Woll's paper, it seems to be that a sociopathic person has certain unrealistic fears, deriving mainly from childhood, which, because of continued self-protection and "defenses," have not been extinguished. On this supposition, it is the therapist's job really that of responding to the sociopathic according to what he is not, i.e., as not fearful? This can hardly be the direction of the author's reasoning. Is not his intent rather to suggest that the therapist has to love the unlovable, accept the unacceptable, and thus provide an interpersonal atmosphere which in which defenses become unnecessary and fears can gradually and "spontaneously" be extinguished? Love and acceptance undoubtedly have their place in a "therapeutic community," but modes of treatment predicated solely upon the acceptance-and-permissiveness principle do not have an impressive record of accomplishment, a fact suggesting that the underlying "diagnosis" is probably in error.

Let us assume instead that the sociopath's basic difficulty is, to revert to Erikson's expression, that he is in an identity crisis, and that the salient feature of such a crisis is more or less extensive depressive that has been practiced as a means of hiding more or less extensive personal deviance. Is the therapist's task then that of "behaving and responding to the patient not according to what we would like him to be," or that he is not? The person's basic problem is dishonesty, hypocrisy. Is it therefore really helpful and "therapeutic" to ignore this diagnosis and assume that the person is honest ("according to what he is not")? The procedure of choice, it seems, is to approach the sociopathic individual quite realistically, on the assumption that he probably is a consummate hypocrite and therefore incapable of being helped. Thus he is denied the help that he needs. This is not the way of the helping person, not "control," appears to be the posture most clearly indicated (Drakeford, 1967; Jourard, 1964; Mower, 1961, 1964, 1966a, 1966b).

In this context, it appears that the therapist should be of two kinds: contractual and attitudinal. Let us consider the following example. Suppose that two men, A and B, enter into a formal business partnership. A now gets into an identity crisis with respect to B in either of two ways. A can receive and cheat B contractually, by stealing from B, and then B will help him. The discrepancy thus created by partner B between the kind of person he pretends to be and what he actually is involves one kind of identity crisis, one way of losing one's integrity and
Statistical Techniques Are Premature

Leon Salzman, M.D.
Professor of Clinical Psychiatry, Georgetown University Medical School

Much of the criticism of psychotherapy comes from sources that have neither clinical training nor experience with illness. They are mostly made by academic or clinical psychologists who try to test the validity of a theoretical concept by statistical study of the outcome of certain procedures. This naive oversimplification of the present unsettled state of psychodynamic theory and therapy leads them to commit major errors.

In recent years there has been a considerable outpouring of attacks on psychoanalysis as a theory and as a practical method of behavioral therapy. Particular criticism has come from the behaviorists, who claim significant statistical results from various types of conditioning and desensitizing techniques. They propose a formulation for the symptoms of a mental illness on the assumption that the symptom is the illness itself. In addition to the enthusiasm with which they view conditioning as a therapeutic technique, they also state that they utilize a model that is capable of being tested and appraised objectively, and is therefore truly scientific. Psychoanalysis, they say, with its problems of subjectivity and small sampling, has no place in pure science.

It is interesting to note that these conditioning critics use the phobia as a paradigm of the symptom to be studied. While the Freudian definition and formulations for this defense are attacked, they proceed to the issue of learning and human behavior. Particular purpose, namely, that a phobia is an excessive fear reaction that is persistent and unadaptive. By implication it is the result of the conditioning and inappropriate anxiety to neutral or nonthreatening stimuli. However, fear reactions are not phobias, nor are phobic reactions necessarily persistent, unadaptive, or unrelated to neutral or nonthreatening stimuli. Using their definitions, they then proceed to cure what they call a phobia by conditioning. Surely it is not remarkable that a conditioned response can be deconditioned! The crucial question, however, is whether the phobia is a simple, conditioned response, or whether it is a complicated psychological avoidance technique, designed to deal with a situation of object that threatens the individual with loss of control and humiliation. The latter definition is the one that Freud and most psychoanalysts mean by a phobia. This is not a specific response, but a defensive technique in an obsessional or hysterical personality structure, and it prevents the individual from facing any symbolic situation that will cause him to lose control.

Despite their claim of preciseness, the behaviorists persist in utilizing a simple label for phenomena which, while they may seem to be similar, are in fact different, such as fear and anxiety and avoidance and phobia. Until they are able to define their concept of phobia more carefully and accurately, it can hardly be compared to the term as it is used by psychoanalysts who have the same difficulties in definition. The behaviorists treat bad habits, and generally do this quite effectively. They may also alter the manifestations of a true phobia by eliminating a situation in which the phobia may occur without, however, altering its dynamic purpose in the personality structure. This is a valuable and sometimes useful technique, but it should not be confused with the treatment of a phobia, or its cure, and a theoretical construction based upon such a premise cannot enlighten the behavioral scientist as to the true nature of this disorder.

In this article Dr. Wolf has done a fine job of presenting the fallacies of the behaviorist argument. The best scientific method alone does not make a project scientific. The best scientific method available must grow out of an understanding of the issues involved. Much of the criticism of psychotherapy unfortunately comes from sources that have neither clinical training and orientation nor experience with illness. They are mostly made by academic or clinical psychologists who try to test the validity of a theoretical concept by a statistical study of the outcome of certain procedures. Though their research design may have scientific validity, the implicit assumption of the unquestioned validity of disease entities or categories such as phobias or schizophrenia, and the naive method of oversimplification of the present unsettled state of psychodynamic theory and therapy, lead them to commit major errors. What presumes to be a scientific approach in contrast to the classical, subjective, and unscentific approach becomes a travesty on the scientific method.

The attempts to minimize bias and pre-judgment in the therapeutic process by emphasis on behavior as the index of process instead of on subjective interpretaions are certainly valid. This introduces bias in the selection of the patient in the development of human skills and defects. Dr. Wolf demonstrates very well the relationship of learning and conditioning to the distortions of human function, which we call neuroses. In a brief and cogent fashion he presents the viewpoint of many behavioral scientists who have long been aware of the need for integrating learning theory into the psychoanalytic process. This is no longer an either-or matter. Distorted patterns of behavior initiate or disturbed relationships, or are the consequences of a malevolent atmosphere or set of experiences, and such patterns persist for reasons that are clearly defined by conditioning theory. The process of therapy is one of relearning or re-experiencing, which is