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#57

PSYCHOPATHOLOGY AND PURPOSE

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When I received Dr. Hoch's letter of invitation to address your group, I was, of course, pleased and honored, but I was also conflicted. It would be nice to present something new and perchance, even, exciting, but what came to mind was so far out of line with the consensus that I wondered whether it would even be taken seriously. Furthermore, your treasurer (Dr. Glueck) and I have spent many hours arguing over this question; and as a former analysand of his, I could hardly avoid the thought that my choice of topic was partly determined by some residual negative transference. In proposing a critical re-examination of received doctrine, it is difficult to draw the line between being challenging and being cantankerous. I hope I shall succeed in being one without being the other.

The thesis I invite you to consider with me this afternoon is this: The concept of "purpose," while it has an essential role in psycho-pathology, is currently over-emphasized; this overemphasis gives rise to needless theoretic puzzles and, more important, rationalizes psycho-therapeutic strategy and tactics which are sometimes useless and even countertherapeutic.

Freud's discovery that the concept of purpose could be powerfully applied in contexts where it had previously been thought irrelevant is justly regarded as a major intellectual achievement, and stands in no need of proof or eulogy by us. But, like other great ideas in the history of thought, it has been overgeneralized—mankind being incapable of grasping an important truth without making it into the *whole* truth (which, of course, it never is).

Bypassing philosophic questions and pseudoteleologic laws in physical science (e.g., "least action" principles), we can identify two broad classes of purposive concepts in biologic and social science. First, there is what the logicians call "motive talk," in

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which we explain a person's actions by referring to his drives, goals, motives, intentions, etc. Such explanations are usually signalled by connective phrases such as ". . . in order to . . . ," ". . . so that . . .," ". . . lest . . .," ". . . because he wanted . . . " and the like. The demonstration that these connectives are often appropriate even when the content of the dependent clause is nonreportable is perhaps most clearly and cogently made in Freud's 1915 paper "The Unconscious."

A second kind of purposive statement, which I shall dub with the neologism "biopragmatic," occurs in discourse about the biologic function of organs, substances, or processes. Even the most hardheaded anatomy professor is not offended by the question: What is this organ *for*? In experimental psychology, the perceptual constancies have a striking biopragmatic aspect. The apparent size of objects is a close correlate of their actual size. If our nervous systems were wired so that the geometric dependency of retinal image size upon object distance was determinative of experienced size, we would have a tough time getting around in the physical environment.

The laws of operant learning provide another example. A hungry rat in a Skinner box may be reinforced by delivery of a food pellet every time he presses the lever; or reinforcement may be given following only *some* of the lever pressings (i.e., on an "intermittent schedule"). The response strength turns out to be very delicately geared to the reinforcement schedule, so that slight alterations in the objective payoff-probability control the rate of responding. Continuous reinforcement maintains higher average rates than does intermittent. However, if *all* reinforcement is now withheld, extinction proceeds faster, and with far fewer total responses, following a history of continuous reinforcement. Biopragmatically, it is "sensible" for organisms to adjust their work output to objective payoff; however, if payoff probability *has been* =1 and suddenly falls to zero, the chances are that something funny is going on in the external world, and until things get back to normal it is biologically expedient to quit trying—and quickly.

Are such biopragmatic statements part of the corpus of behavioral science? My answer is no, because they do not actually appear in any theoretic derivation. They are *true,* but they belong to another science, namely, genetics (including evolution), which has the task of explaining how organisms with such biopragmatic properties came into being. Now this distinction is not one of mere philosophic precision, because it makes a difference whether you treat biopragmatic statements as laws of behavior (which they are *not)* or more like editorial comments imported from a nonpsychologic subject matter (which is what they are.) In general, the inclusion of biopragmatic assertions in behavioral science gives rise to more problems than it solves. A rat can be trained to put out 200 responses per pellet, a ratio which leaves him ahead of the game calorically. But if this schedule is suddenly imposed on a rat who has been performing at 20:1, he will extinguish, and can actually be starved to death in an environment where food is objectively available. A biopragmatic principle such as, "Organisms adapt to their environment," leads to paradoxes which a rat clinician might be tempted to resolve by *ad hoc* hypotheses (e.g., death instinct.) Whereas by confining our conceptual system to the substantive learning principles of reinforcement and discriminative control, we can *derive* the necessity of proceeding to large ratios by gradual steps. The important truth illustrated by this laboratory example is that the biologic "adaptiveness" or "utility" of behavioral laws is only *stochastic,* and cannot be validly applied to make deriviations about individual specimens and circumstances. Let me emphasize that we are not discussing rats who are somehow aberrant—any normal rat can be starved to death with food available by appropriate choice of the reinforcement schedule's parameters.

Another point of this example is that the organism is a repository not only of *motives* and *emotions,* but also of *habits.* There is a vast experimental literature dealing with the acquisition, shaping, control, activation, and extinction of habits, and the psychopathologist who considers this body of facts irrelevant to his concerns is profoundly mistaken, because there are behavioral phenomena which cannot be understood, especially in their quantitative aspects, except in terms of learning principles.

Let us turn now to clinical material. Since Freud, we are accustomed to ask, "*Why* is the patient doing this?" anticipating an answer formulable in motive talk. My thesis is that this is sometimes a mistake. It is worth noting that Freud expressed a somewhat similar conclusion in *Beyond the Pleasure Principle.* The "repetition compulsion" strikes most of us as a rather poor explanatory construct because it represents an abandonment of that insistence upon finding motivational explanations which made Freud's early discoveries so illuminating. The "death instinct" is Freud's metaphysical effort to subsume what he reluctantly recognized as nonpurposive clinical phenomena under a principle which, although counterintuitive and no longer biopragmatic, still preserves an odd kind of teleologic character.

In understanding a trait or symptom, the question “What is the patient getting, or avoiding (or ‘trying’ to get or avoid)?” may be inappropriate for a variety of reasons, of which time permits me to mention only three. First we have the distinction between classic (Pavlovian) conditioning and instrumental learning by reward (Thorndike’s “Law of Effect”), corresponding to Skinner’s distinction of respondents and operants. Salivating when you hear the dinner bell is a respondent. It does not manipulate the environment, it does not bring about the presence of food, and it belongs to that class of responses, occurring in smooth muscle and gland, controlled by the autonomic system. By contrast, saying to the waitress, “I’ll have a hamburger,” is an operant (like lever pressing), which acts upon the external environment to bring about the presence of food, occurs in striped muscle, and is controlled by the skeletal nervous system. Respondents are “conditioned” in the original Pavlovian sense, and the question “What does the dog ‘get out of salivating?” is not behaviorally meaningful (although it obviously has a good biopragmatic answer). Operants, on the other hand, are learned and maintained by reward, and it *does* make behavioral sense to ask what the organism gets out of them.

The operant-respondent distinction is crucial for understanding the relation between psychosomatic phenomena and conversion symptoms. The former are respondent and are therefore not “purposive.” The latter are operant and are “purposive.” The question “What is Mr. X getting out of his stomach ulcer” is not only scientifically unsound but partakes of a certain element of injustice. I don’t think it is always correct to say that the patient is getting *anything* out of his ulcer, although of course even an ulcer (like a conversion symptom) may provide secondary gains. But there is no good evidence that conditioned respondents can be strengthened, or their strength maintained, by reward; so that the secondary gains of a psychosomatic symptom are pragmatically of less importance because they do not, theoretically, contribute to symptom maintenance.

Even in the operant case, one can be misled into asking pseudoquestions by over-teleologizing the reward concept. The late, great psychologist E. C. Tolman once argued in print against the Law of Effect on the curious ground that it could not explain a man’s persistence in smoking Luckies when Old Golds would satisfy the smoking drive equally well! This is a beautiful example of what can happen when a bit of biopragmatics is allowed to metastasize, and the fact that such an able mind as Tolman’s could fall into the trap testifies to our pervasive and ineradicable tendency to think teleologically. The Law of Effect (or, the principle of operant reinforcement) does not refer to hypothetic occurrences (what *would* happen *if* something else happened) but states that organisms tend to emit the responses that have been reinforced. There isn't any basis for expecting the fellow to shift from Luckies to Old Golds, nor is there anything paradoxical about his persistence. His learning history is that he has been asking for Luckies, getting Luckies, and smoking Luckies. This is the behavior that has been emitted, and it is this behavior that has been reinforced. He hasn't been asking for Camels or Old Golds, and consequently that behavior has not been reinforced. The Law of Effect tells us that he will do what he has been rewarded for doing; what he *could* get rewarded for doing, as seen from the vantage point of the outside observer, is behaviorally irrelevant.

A similar error occurs in the thinking of many clinicians when trying to understand neurotic behavior. A patient ought, we think, to be doing so-and-so because this would lead to need gratification. Why doesn't he? In contemporary psychodynamics, the first place we look is for something which is "in the way," i.e., we look for some kind of counterforce or impedance which prevents the individual from emitting the healthy response and getting the healthy gratification. Now often there are such roadblocks, especially in the early phases of treatment, But even if there aren't, the clinician who thinks biopragmatically will search until he *finds* them—which, given the complexity of behavior and a moderate ingenuity, he can almost always do. Whether or not the patient will *buy* his interpretation, and whether, if he does buy it, it has a therapeutic consequence, is another question.

But, as we have seen, responses may fail to occur for other reasons; and one of the commonest causes of response failure is the quantitative inadequacy of reinforcement. Once we abandon the assumption that maladaptive behavior always reflects the influence of interfering forces and therefore should, so to speak, "clear up by itself" when these adverse influences are lifted by the therapeutic process, we can recognize that, in addition to incompatible habits and disruptive affects, there are also failures to have acquired or maintained instrumental responses at sufficient strength. The therapeutic task then becomes partly one of building up such healthy responses. Of course, to attempt this while the counterforces are still present makes no more theoretic sense in terms of the experimental psychology of learning than it does in terms of, say, classic analytic theory. I am not suggesting that we throw overboard what we know about the role of defense in maintaining maladaptive behavior, returning to some kind of Couéism or other suggestive-suppressive therapy. I am concerned with a kind of problem which I am sure every psychotherapist present has met repeatedly, namely, the patient who has worked through a great deal of material, freed himself from many neurotic defenses, learned to tolerate less distorted derivatives, but who persists in not doing the obvious things that he himself says would now be in order, and who sometimes even raises spontaneously the mysterious question of why he doesn't get around now to doing them. This clinical problem is *not* effectively approached by searching for some yet uncovered counterforces or by postulating excessive amounts of *thanatos,* and the like. Instead, we recognize that the individual's interfering habit systems have been considerably reduced by psychotherapy, but that he lacks sufficiently strong instrumental response chains of the gratification seeking type to get the behavior out and to keep it going under the reinforcement schedule of adult life. The stochastic nature of socially mediated reinforcement schedules here becomes particularly important. Very little of our interpersonal behavior is on a total reinforcement schedule. When we tell a joke, people are not always amused; when we go to a party, we are sometimes bored; when we accept a patient for therapy, the patient does not always improve; and so forth. When an individual has been impeded by neurotic counterforces from acquiring a stable and well differentiated system of instrumental responses, removal of the impeding factors constitute a necessary but not a sufficient condition for development of healthy behavior. Uncovering psychotherapy is primarily aimed at reducing defense, which means in learning-theoretic terms that it tries to extinguish *avoidant* operants and the conditioned anxiety respondents which underlie them. Neither of these changes has any intrinsic tendency to strengthen operants of the *adient* class. Relying upon the uncovered impulses to do this is often like expecting the rat to switch without stepwise restraining gradually to a new schedule, merely on the grounds that he is hungry. Moral: *Drive is not enough.*

Weak operants are in danger of extinction because a respectable payoff probability may easily fail to materialize in a short run of trials. Suppose, for example, that you are treating a patient whose neurosis has greatly restricted his heterosexual history. As the treatment frees him up a bit, he starts tentatively exploring. If his long run odds of a "success"-experience are, say, twenty percent, there is one chance in nine that he will have a run of ten consecutive failures—which might lead the healthiest male to get a little discouraged. The patient is not being sabotaged by his *thanatos,* or his will to defeat the therapist, or by his unconscious guilt; he is merely a victim of the binomial theorem.

Recognizing that habits and reinforcement schedules are just as important in understanding learned behavior as drives or affects, suggests the modification of psychotherapeutic procedures along lines of response strengthening and shaping through positive reinforcement. Specifically, we ought not assume that whenever the anxiety signal is sufficiently extinguished and the defensive system has been sufficiently worked through by interpretative methods, the drive system of the organism will somehow automatically do the rest. There is no theoretic reason why we should expect this to happen; and it is quite apparent that many psychotherapists today are acutely conscious of the clinical fact that it frequently does not happen. Since our clinical experience is in such excellent accord with theoretic expectations, it would seem appropriate to modify and adapt our procedures so that they will be more in harmony with the principles which have been discovered by the experimental study of the learning process.

The technical suggestions which arise upon adopting this view would require detailed discussion, and rather than give a misleading picture by characterizing them generically, or even by listing them *in extenso,* let me take only two specific tactical examples. The first of these involves the amount and kind of positive reinforcement administered by the therapist himself. Because of the notorious fact that ordinary unsophisticated reassuring tactics of the type used by the patient's friends and family are ineffective, there is a widespread professional opinion that explicit positive rewards ought not to be administered by the therapist. The complications which can arise from an unsuitable use of tactics such as encouragement are too well known to need discussion. But we should surely distinguish between the unskilled and blanket reassuring procedures employed by the patient's peer group (without *first* reducing the neurotic counterforces), and the skillful application of verbal rewards at suitable stages of therapy. Therapists show here a kind of double standard. When they talk about an approved therapeutic tool, such as interpretation, they wish its efficacy to be judged in terms of its skillful and optimal use. But when asked to consider interview tactics such as "priming" or reassurance or encouragement or explicit approval for doing something healthy, they tend to denigrate them as "mere symptomatic treatment," assimilating all such to the unskilled operations of nonprofessional helpers. An experimental psychologist would expect that the proper dosage of reward for behavior in the healthy direction, even if objectively *un*successful on a given environmental trial, should help to build up the desired response strength and keep the behavior coming out sufficiently so that the patient has a fighting chance of getting a few pellets.

A more controversial tactic is task setting. While Freud discussed task setting in connection with phobias, he did not develop his own technique along these lines; furthermore, the task setting experiments of certain other workers (such as Ferenczi) brought the whole concept into disrepute. On learning principles, one would expect task setting to be an extremely powerful ancillary procedure at certain stages in therapy; and under some circumstances, with some patients, it might even be a necessary condition for getting a patient over the hump. Here also, the contraindications and numerous dangers (e.g., with respect to the therapeutic relationship) are admitted. However, the existence of dangers and complications ought not to rule out therapeutic experimentation, especially since our current methods can hardly be considered so powerful as to be beyond improvement.

A third situation in which the motive question is inappropriate resembles Freud's repetition compulsion, and is admittedly more speculative than the cases of respondent conditioning or low operant strength. On present evidence, we cannot exclude the possibility that at least some "negative" psychic conditions are simply the result of a thoroughly overlearned, massively conditioned CNS state. The patient is not seeking or avoiding—he is just repeating, and that's the end of the story. Whether this type of central "state repetition" occurs, and whether it obeys the laws of respondent conditioning and extinction, are factual questions of great clinical importance. If the baffling and discouraging behavior of some of our patients is thus mediated (especially if the "statistical physiology" of the situation leads to maintenance of a steady state, or to a kind of automatic selfreconditioning by sheer repetition and without primary reinforcement), the implications for therapeutic tactics are quite different from those derivable from either an impulse-defense or a hedonic-control model.

In summary, I have tried to suggest some of the theoretic and experimental reasons which lead me, as a clinician and erstwhile rat psychologist, to believe that contemporary psychodynamics systematically over-generalizes the concept of purpose and neglects other variables whose control over behavior is well established. It is my further conviction that when this error is rectified, the full conceptual power of Freud's emphasis on motives will be manifested because it will no longer be attenuated by misapplication; and that our ability to help patients will thereby be materially increased.

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