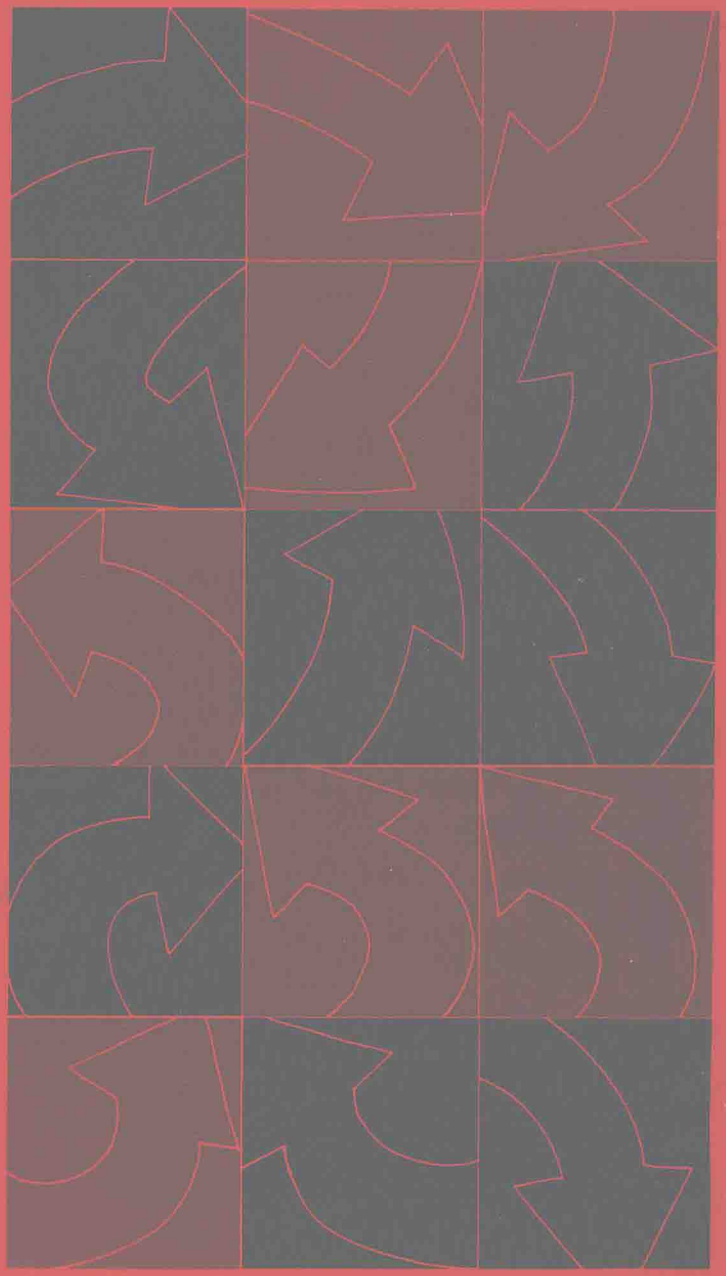
Erasmus L. Hoch

Experimental Contributions to Clinical Psychology



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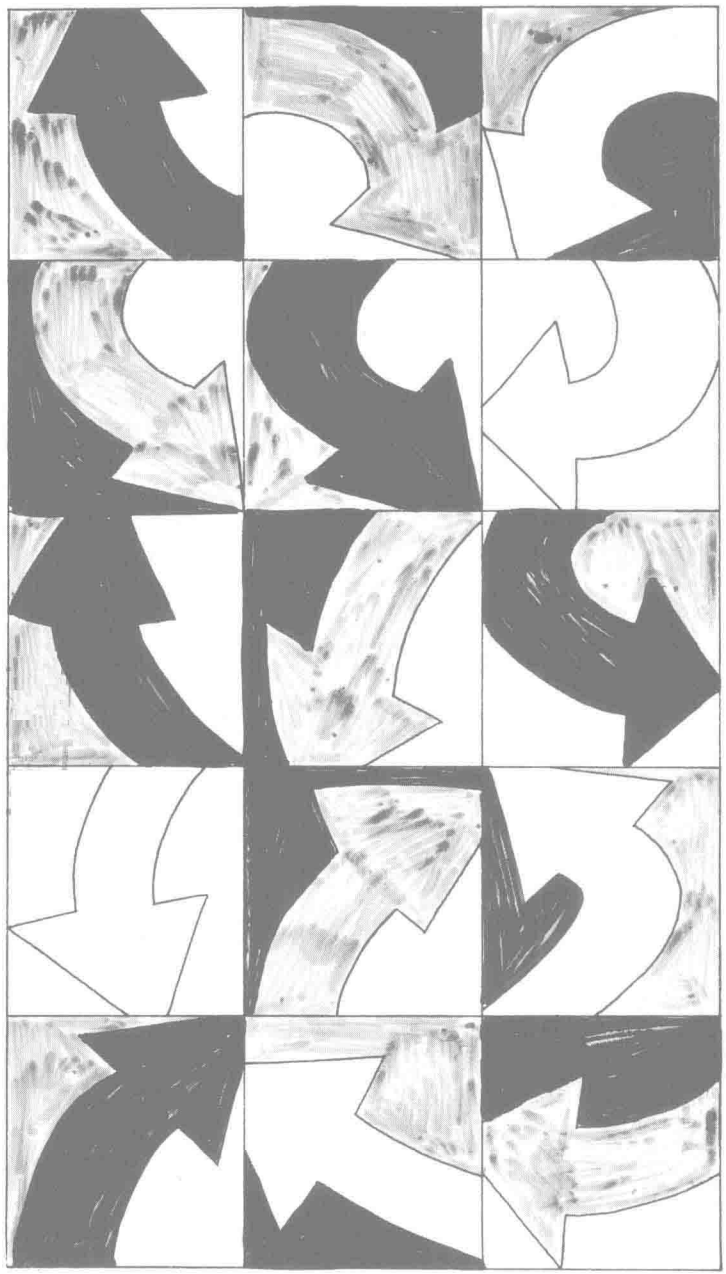
Erasmus L. Hoch

The University of Michigan



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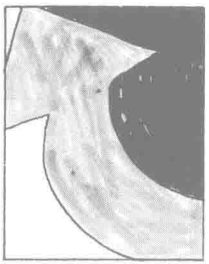

Experimental Contributions to Clinical Psychology

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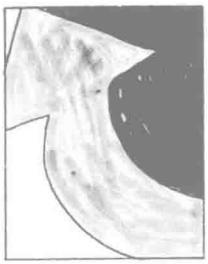


Preface

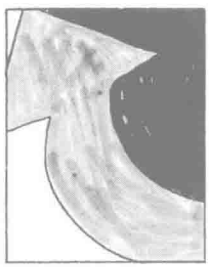
This book rests upon the premise that communication among psychologists of *every* persuasion will help the whole field of psychology to grow faster than if each of us were but tending his own scientific patch. It attempts to show the interdependence of clinical psychology and other areas of psychology and to illustrate the advantages of attacking a particular problem in psychology from several different directions.

My earlier, briefer introduction to the field (Hoch, 1971) attempts to tell interested readers what clinical psychology is about. This second book, which encompasses the previous one, assumes that the reader is already converted and would like to know more fully what this field entails.

Part One of the book is devoted to some methodological considerations. Part Two presents some of the dilemmas faced by the investigator who carries on research with human subjects. Part Three, on research and theory, is intended to illustrate the variety of resources from which clinical psychology may draw in its quest for broader understanding. Part Four, on theory and practice, deals both with answers that *seem* to have been found and with questions that continue to be asked.

I hope that, in reading this book, the student with predominantly clinical interests will gain a healthy respect both for the clinician as a behavioral scientist and for the non-clinician as a contributor to the clinical enterprise. In emphasizing the similarities rather than the differences between the schizophrenic and ourselves, Harry Stack Sullivan (1947) reminded us poignantly that ". . . above all else, we are all much more human than otherwise." I would like to suggest in this book that, regardless of our areas of specialization within psychology, we are all much more psychologists than otherwise.

For their helpful reviews and suggestions, I would like to thank my colleagues of The University of Michigan who read portions of the manuscript: Professors Benno G. Fricke, Harry F. Gollob, Jesse E. Gordon, Ralph W. Heine, E. Lowell Kelly, John E. Milholland, Warren T. Norman, James D. Papsdorf, J. E. Keith Smith, Stephen B. Withey, and Research Associate Frank M. Goode, as well as former colleagues, Professors George L. Geis of McGill University, Irwin Katz of the City University of New York, Sarnoff A. Mednick of the New School for Social Research, Harold L. Raush of the University of Massachusetts, and Sherman Ross of the National Research Council. Special thanks are due Professor Edward L. Walker of The University of Michigan, whose editorial support was much appreciated, and Louise H. Kidder of Northwestern University, who offered the kind of critical comments on the entire manuscript that only a student can. I am very grateful to Adrienne Harris, who offered her invaluable editorial talents with delicacy and therapeutic consideration for an author's feelings. For her painstaking secretarial efforts in the preparation of the manuscript, I am indebted to Ruth Rowry.



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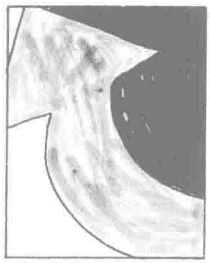

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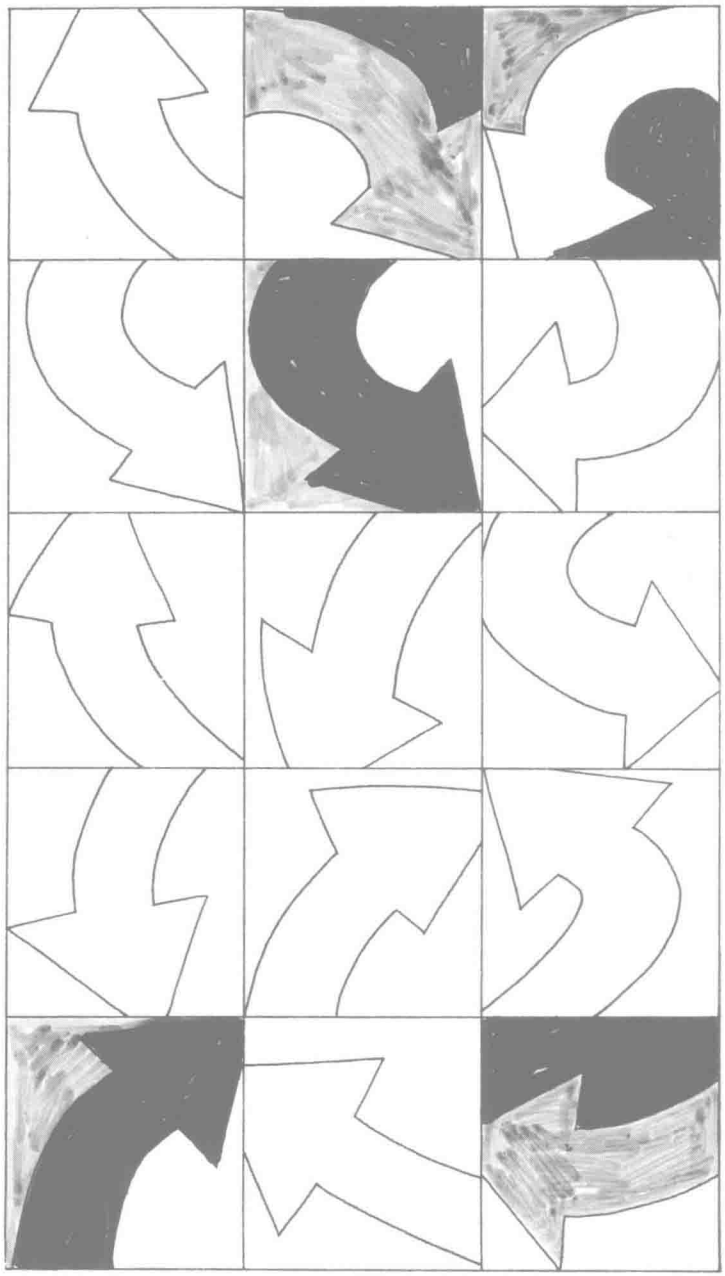

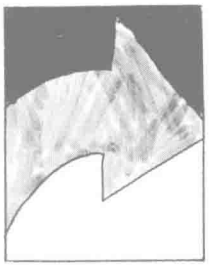

1
The Nature of the Problem

Few people would venture to diagnose a neighbor's chest pains as an incipient heart attack. But should he complain that his wife is hard to get along with or that his children are out of hand, most are ready to offer advice, if not to prescribe solutions. People-watching is a universal hobby and every man a self-proclaimed expert, unfortunately. The road to amateur psychology is paved with good intentions and, alas, with bad theories, as the following story will attest.

On Observation, Labeling, and Inference

Several years ago, after sitting with a group of colleagues in weekly committee meetings over the period of a year, I was struck by the sudden realization that one could divide this group of ten people in a very simple way. In the comfortable atmosphere of the conference room, some members of the committee *invariably* put their feet on the conference table while others *never* did! And, as it happened, of the ten people involved, five fell into each group, so that the box score for the year looked like this:

"Tablefooters"	*"Non-tablefooters"*
B— —	F— —
H— —	Ho— —
M— —	K— —
N— —	Ma— —
W— —	Wy— —

In mildly scientific fashion, several members of the committee were independently asked to list names in each category. The result was 100 percent agreement. The

layman, acting as self-styled psychologist, might now be tempted to proceed from this observation to fancier efforts, first using conventional labels (for example, "inhibited" versus "uninhibited"), and then moving with zeal to further inferences, perhaps even toward a miniature theory. Let us imagine for a moment where this could lead.

Someone might assume, for example, that putting one's feet on the table represents a kind of carefree abandon, an easygoing, relaxed temperament, the ability to translate inner peace into corresponding outer behavior. It would not seem farfetched, therefore, to hypothesize that the "tablefooters" (the uninhibited, by this token) were the product of a permissive, accepting, tolerant early environment, in which freedom, mental health, and other good things prevailed.

That's fine (although the facts would have to be checked). But what is interesting, yet vexing, about this psychological enterprise is that someone else might proceed along quite different lines to quite different conclusions. This latter, more pessimistic "theorist" might begin by assuming that one does not usually put one's feet on the table. Such behavior, he would argue, represents not an easygoing nonchalance but rather a defiance of social convention. Therefore he might hypothesize differently about the two groups. He would see the tablefooters as rebellious rather than uninhibited. Their early upbringing, the "theorist" speculates, must have been rigid and authoritarian; their present behavior must represent defiance of authority, whether blatant or subtle, conscious or unconscious. Again, such speculation would have to be checked against the facts, insofar as they can be determined.

The same observation, then, can lead to two equally plausible lines of theorizing. This makes "psychologizing" at once intriguing and frustrating, especially when some skeptic suggests that, lest we proceed too blithely, we should first investigate some more prosaic facts. Is it perhaps true that the non-tablefooters are aged 25–35 and the tablefooters 48–63, so that the latter, as senior citizens, can afford to take more liberties than the former, who, as assistant professors on trial, need to be on their good behavior? Or is the age distribution the other way around, with the young men being the tablefooters, so that perhaps the older, feebler group does not put its feet on the table for reasons that are arthritic rather than psychological?

Most discouraging of all for the amateur psychologist would be to have someone point out that, with such a high conference table, it becomes uncomfortable for the non-tablefooters (all of whom prove to be under 5′6″) to put their feet on the table, in contrast with the tablefooters (the shortest of whom is 6′1″). The committee in this case did not happen to consist of giants and dwarfs, but it could have.

This is not exactly an example Freud might have chosen. It is, however, the stuff of which the layman's collective wisdom is sometimes made as he ventures into the realm of psychological conjecture. For this is an area in which the untrained can often sound very interesting but turn out to be very wrong! The intellectual hop-skip-and-jump from observation to hypothesis to theory can easily wind up as a technical flop.

Without getting overly prophetic, let us look for a moment at another way in which the untrained or the careless can leap at psychological inferences and end up in an awkward position.

Psychodynamics and Levels of Abstraction

We cannot go too far wrong if we stick closely to observed fact (although even here some classic experiments on testimony [Münsterberg, 1908] make one wary). If a woman wears a dress that comes to five inches above her knees, we can safely state that (by definition) she wears miniskirts. That is correct, but not particularly interesting in itself. However, we may conjecture about the "psychodynamics" of her choosing this mode of dress, particularly when the woman, although comely, is 40 years old. Such speculation becomes more interesting; unfortunately, it also becomes more risky. Let us follow the process.

We can observe, although perhaps not as directly or as objectively, that the woman is a rather shy, or at least a somewhat reserved, person. Others who know her agree that she speaks softly, is very polite, and tends not to assert herself in a group. We cannot measure such attributes as readily as the length of her skirts, but at least there is a consensus among several observers that this is so.

We have, then, departed a little from the more objectively determinable facts, but not much. We are now at the more interesting point of contrasting the seemingly modest makeup of the woman with her somewhat sexy appearance. We learn, however, that we have paid a small price, inasmuch as one of our colleagues doubts that she is really as reserved as she appears on the surface. He has, for example, observed on her desk a catalog of high-priced bathing suits, featuring mainly the bikini type. Putting two and two together, then, our colleague is willing to move yet another step up the abstraction ladder—that is, to make an additional inference that this outwardly modest person is really unconsciously seductive. Now the situation becomes decidedly more interesting psychologically; it becomes at the same time more shaky scientifically. In fact, of six people who know the woman well, three are willing to countenance such a hypothesis, while three are not. Thus, we pay a price for moving

from a clear but dull fact (she wears miniskirts—defined operationally as dresses five or more inches above the knee) to an interesting but possibly erroneous inference (the shy lady is a temptress).

Intrigued by the process of psychological inference and where it might lead, the most widely read and imaginative of the six observers suddenly gets a burst of clinical intuition that a new complex has been discovered. In a poetic flash he even coins a name for it—the "sea anemone syndrome"! This woman, so modest in her demeanor, yet dressed in attire quite risqué for a 40-year-old, is more complicated than she herself suspects. Like the beautiful sea anemone that coyly attracts unsuspecting prey, she is unconsciously bent upon seducing and destroying men. Bravo!—says the proud theorist. Nonsense!—say four others. It occurs to the remaining member of the group of six that the price of inference might be pictured as shown in Figure 1.

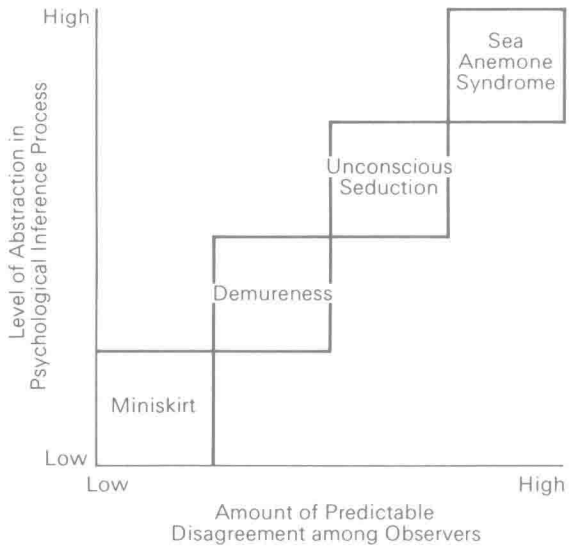

Figure 1

This hypothetical example represents a fanciful but not atypical situation. The amateur "psychologist" indulges in inferential speculation as a pastime; the clinical psychologist does so of necessity. There is a difference. The former proceeds with temerity, the latter with caution, fully cognizant that the price of psychodynamic theorizing is sometimes error but hopeful that he can minimize error by seeking data that will confirm or disconfirm his hunches.

The Clinical Psychologist's Dilemma

One's image of a clinical psychologist might well be that of someone who administers projective (personality) tests to mental hospital patients or spends his professional life in a private office helping neurotics cope better with life—both worthy objectives. Indeed, a dictionary of psychological terms (English & English, 1958) defines clinical psychology as ". . . that branch of psychology which deals with the psychological knowledge and practice employed in helping a client who has some behavior or mental disorder to find better adjustment and self-expression. It includes training and actual practice in diagnosis, treatment, and prevention, as well as research for the expansion of knowledge" (p. 90).[1]

Apt as the definition is, it needs elaboration. The clinical psychologist of today is concerned with adjustment as well as with maladjustment. His interest lies in behavior modification generally, rather than in treatment specifically. He shows less concern with testing *per se* and more concern with the general problem of assessment. In short, he is interested in observing carefully and measuring accurately, to the end that clearer description, better prediction, and more adequate control of behavior may be attained. In clinical psychology this is more easily said than done. The strength of a phobia (an irrational fear) is not indexed as readily as the strength of the connection between words such as "light-dark" versus "light-heavy" (although some [Lang & Lazovik, 1963] do not see the former as such an impossible task). Nor is the degree of gain from psychotherapy measured as easily as the reaction time on a visual-motor task (for example, pressing a button as soon as a light goes on). And constructs like "repression" (unconsciously excluding something from awareness) are not as readily specified as the hunger of a rat that has been carefully kept at 80 percent of normal body weight for a month and run in a maze 22 hours after its last meal.

The above discussion is not to imply, however, that the usual principles of inquiry do not apply in clinical work, nor that methodological ground rules can be waived. Clinical problems may be different in character, but they merit rigorous study nonetheless. Techniques for their analysis are sometimes relatively new, and often still awaiting development, so that the clinician may often have to take on an unenviably difficult task. Nevertheless, as a behavioral scientist, he is held to the same canons of evidence that govern his colleagues in other areas.

There are some interesting ironies in this picture. When an unmanned space vehicle landed on the moon, scientists were able to learn something about the moon's com-

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