
HABITS

THEIR MAKING AND UNMAKING

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BY

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A SELECTED BIBLIOGRAPHY AND EVALUATION OF
RECENT DEVELOPMENTS IN HABIT FORMATION BY
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INTRODUCTION

Knight Dunlap is widely thought of as an iconoclast. Many who know him in that role have overlooked that he has been a re-builder also. In this book he makes a frontal attack on several popular superstitions in the field of habit formation. The need for counteracting them is perhaps greater today than it was when the book was first issued. I shall list a few of them.

1. That the acquisition of a habit is generally accomplished by the subject's repeatedly practicing the act that is to be acquired.

2. That there is a generalized activity which may be called "learning" which is neutral with respect to *what* is to be learned—i.e., to increase speed, eliminate errors, select a better working set of effector-organs, etc. on the one hand; or, on the other hand, to attain an *understanding* or insight into the task that one undertakes, whether understanding it aids or hinders the acquisition of motor skill.

3. That thinking about an activity or imagining it accurately does not affect the rate or the degree of acquisition of skill.

4. That habits of thinking and feeling about a task are separable from habits of performing it.

5. That in order to eliminate a habit it is best to practice its opposite assiduously.

6. That intelligence-tests yield indirect measures of general intelligence; emotional stability tests, of general emotional stability; persistence-tests, of general persistence, etc.

7. That if a child is deficient in "intelligence" at one age, he will be deficient at any later age. In other words, that IQ

ratings measure "innate learning capacity," which is intrinsically unchangeable, being an irrevocable fact of history. (This article in the mental testers' creed was proclaimed in my own town the night before this preface was written. The orator holds an executive position on a board having the oversight of the care and treatment of the feeble-minded of one of the most populous of the United States.)

That these superstitions are still rank and growing is, I think, sufficient evidence that some book or supplementary textbook is needed to help immunize the brighter students against them. I know of no other work yet published that promises to be a better immunizing agent than this one has proved itself to be.

It is now possible to write about some of these superstitions more briefly and more simply. However a better treatment than Dunlap's is not to be found in any textbook yet published. It can be found in scattered articles published in professional journals, but these with their limited circulation have little influence on the average student. The main opportunity for improvement on Dunlap's writings that I can see lies in greater use of formal distinction between operational definition and hypothetical or formal definition of practical concepts, with greater emphasis upon the fact that an operationally defined concept (such, e.g., as "intelligence") has no meaning that is not expressed or logically implied in the defining operations unless and until some additional meaning is imposed on it by correlational procedure.

The future writers on this subject have an abundance of working materials published since the first issue of this book. They might well begin with an essay by L. B. Tuckerman * on operationally defined concepts indifferently called "hardness." It is in my opinion an excellent working model. For

* Tuckerman, L. B., The significance of the hardness test of metals in relation to design. *Proc. Amer. Soc. for Testing Materials*, 1943, 43: 847-854.

"hardness," like "intelligence," "generosity," "emotional stability," "arithmetic ability," etc., is intrinsically non-measurable by any procedure whatever, because it is intrinsically non-additive, and therefore non-averageable. When we apply such tests to candidates for training as apprentices, for example, we are not interested in isolating and testing the possibly non-existent traits after which the tests were named, but in some socially useful trait, such as success or failure in the course, or success versus failure in the vocation, just as the physicist is not primarily interested in determining whether these tests of hardness falsely so-called, enable him to predict whether a given metal will do what is demanded of it in practice.

If anyone should undertake this task, let him not feel that he is in any way opposing Dunlap. For the genesis of the argument is contained in this book. Those who have simplified it and made it more explicit in technical articles might never have been introduced to the problem but for Dunlap's critical writings.

In closing, I call up the controversy that was still active when this book was first issued, over what Dunlap called "negative practice." He asserted that under some conditions, which he did not perfectly specify, one could render an act non-habitual by intentionally repeating it. Thus he drew a reply in the well-known form *ipse dixit*, from the followers of a certain very eminent educational psychologist. That Dunlap did not concentrate his efforts and those of his colleagues on an attempt to determine more minutely the conditions under which the procedure is successful is to be regretted. For there is nothing new in the doctrine, and nothing intrinsically absurd. The treatment consists essentially in requiring the subject to repeat an habitualized act in circumstances that become more and more distasteful to him. Thus by repetition he establishes a habit of *revulsion* from the act, and this conflicts with his tendency to perform the act. In circumstances that have yet to be thoroughly explored, the revulsion may become stronger than the original habitual tendency and may serve to extinguish it.

Parents of some young children who are practicing on a musical instrument may find an example within their own homes. The child is required to forego something that he would rather do at the time. Consequently he may sulk, delay, kill time, etc., and thus devote not more than 10 minutes to genuine musical practice in an hour. Meanwhile, he is practicing a habit of resentment, of rebellion, of self-pity, much harder than he is practicing his keyboard work and his musical techniques. I have known children who eventually extinguished the habit of practice and who after many years will not yet try it again.

My own father demonstrated the usefulness of negative practice in breaking his young children from crying when they were disappointed. He might indeed begin by saying, "Now, now! I wouldn't cry. You are too big to cry . . . but, if you are going to cry, cry right. Make me some big salt tears. . . . Well, that's pretty good, but not enough. Cry me up some more—cry my hands full. . . . Well, that is better, but not good enough. Now as soon as you finish I'll show you some good crying." He would then pucker up his face in several hitches and then burlesque the sobs and the squalls that the child would make. Meanwhile the child attended raptly. Now he would say "That's the best way to cry; suppose you try it." Of course, the child could not now cry if he wished. I have borrowed his technique, and have tried it out on many children with no failures. Two trials are usually enough.

Inasmuch as Knight Dunlap and I have been rather closely associated in various scientific endeavors since early in 1910, and have maintained a strong personal friendship in the meantime, I have assumed the task of writing this introduction. The contents of the book were first prepared as a set of lectures given at the Johns Hopkins University and at the graduate school of American University, beginning some years ago. The final revision was made during the course at the latter institution, where I was then teaching. We discussed all the material in detail; as he remarked in his preface, I tendered many suggestions; as many of his close acquaintances might guess, he considered them all and accepted some.

The first issue was put on the market under most unfavorable conditions, arising out of the Great Depression. The book was always hard to get, and sometimes not available at all. Nevertheless it seems to have had an influence quite disproportionate to its circulation. Since it deals chiefly with principles rather than with detailed empirical fact, its usefulness should not be reduced by time.

I have used this book as a text in the regular sessions at the graduate school of American University and in summer sessions at Tulane University and the University of Virginia. It has proved to be interesting to the students, and provocative of original thinking. On this ground I heartily recommend it as a text for upperclassmen and for graduate students in psychology and education.

The effectiveness of the book has been enhanced by enlarging the bibliography and bringing it up to date. This work was done by Professor Joseph E. Morsh of The University of British Columbia.

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30 April, 1949

PREFACE

During a considerable period of my life, my primary interest has been in so-called "pure" psychology, that is, in the accumulation of facts and the discovery of principles concerning the operations of the mind, regardless of the practical applications of these facts and principles to the problems of life. Before application can be sound, we must have scientifically sound data on which the applications may be based. Hence, in the period when facts were not abundantly certified, and principles were not adequately formulated, it seemed obvious that psychologists who had the capacity for research on fundamental problems should bear down heavily in that part of the field.

I have always assumed, however, that the justification of "pure" science must lie in the ultimate utility of its products, although it is necessary, from both the scientific and from the ethical point of view, to proceed cautiously in the utilization.

During the last few years, it has been difficult for even the most cautious psychologist to avoid applications of psychology to personal and social problems. To him come "cases" whether he welcomes them or regards them as nuisances. Individuals in distress, in pressing need of readjustment to their environments, or of readjustment of their environments, cannot simply be turned away. In many cases, there is no one else to whom the sufferer may turn for aid, unless to the charlatan or the professional preyer on human weakness. Often there is disaster clearly imminent to the patient and others, if relief is not given immediately.

Cases of this sort, in increasing numbers, have drawn my attention and my energies towards applications. Through this work, I have been increasingly impressed with the importance of clarifying our knowledge of the principles of habit and learning, and have come to see the experimental problems involved in these topics in new lights.

The individual cases from which I have drawn data and conclusions, some of them treated by myself, some by colleagues, are not reported in this volume. All such cases are confidential in the highest degree, as regards patients, psychologists and institutions involved. The clinical details of such cases, moreover, are of no value to the non-professional reader. In no instance is a "case history" a guarantee of the adequacy of the conclusions drawn from it. On the contrary, the "case history" needs to be guaranteed by the soundness of the conclusions.

During the last fifty years, there has been a rapid growth in the study of habit formation and the general psychology of learning. A substantial amount of information has been collected through experimental research, but this information is still in the process of coordination and interpretation. Antiquated theories which are out of accord with ascertained facts are still conspicuous. "Principles" of learning, deduced from heterogeneous data without scientific analysis, are set forth in text-books for teachers, and constitute snares for those who are anxious to apply psychological results to education.

In particular, the interrelation of the processes of learning and unlearning, of habit making and habit breaking, has been neglected. It is the purpose of this volume to outline this interrelation in an elementary way, and to show how a fuller understanding of the learning process makes possible more adequate procedure in the breaking of disadvantageous habits.

I suppose that my treatment of the psychology of learning will seem to many readers to be radical, even revolutionary. It will be found that I have discarded the conventional formulations and flouted the "principles of learning" set forth in widely used texts. These revolutionary features, however, are merely the result of attempting to harmonize and interpret the products of the experimental work of many psychologists; and the revolt is directed only against the theories and the traditions which originated before the experimental era of psychology, and which need to be swept away in order that the experimental results may be understood. My function in this matter is that of an organizer, rather than that of an innovator. I am by no means under the illusion that my interpretations are final.

To the non-professional reader I suppose that the last four chapters which deal with practical applications will be of major interest. To such readers I would point out that these chapters do not hang in the air, but that the principles and the procedures they present are based upon the data which are summarized and interpreted in the preceding chapters. Useful educational and psychotherapeutic techniques are never the results of guesses or mere "hunches," but are laboriously elaborated from the body of psychological knowledge. Scientific innovations are almost always worthless, if not pernicious, unless they are really not innovations, but are evolutionary products of the scientific accomplishments of the predecessors of the apparent innovator.

In attempting in the earlier chapters to outline the critical evaluation of psychological data which lead to the final applications, I have wished to spare the reader the technicalities which might not only weary him, but might distract him from the main points. I have, therefore, not cited experimenters nor reported data in detail. In various places I may have erred by over-simplifications.

The reader who takes the chapters in order as printed will, I think, profit the most, and will find the presentation maximally interesting in the end. Some readers, however, may find the reading easier if they begin with the seventh chapter, and read the earlier chapters after reading the later ones. The fifth chapter, which doubtless many readers will pass over, is inserted because it will be, for the student of psychology, an especially important part of the total scheme which the volume attempts to present.

I am greatly indebted to Dr. Harry M. Johnson who carefully read the manuscript before its final revision. His criticisms, mainly constructive, are responsible for extensive improvements which were made in the text. The bibliography which is appended was prepared by Dr. Willis C. Beasley with definite bearing of each item on points discussed in the several chapters of the book. On this account, the bibliography is gratifying in its deviation from the usual type of book lists.

KNIGHT DUNLAP.

Baltimore, July 1, 1932.

HABITS

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CHAPTER I

THE PROBLEMS OF HABIT AND LEARNING

It is no great exaggeration to say that living is for the most part learning, and that the remainder of life is merely the carrying on in practice what has previously been learned. We begin to learn at least as soon as we begin to live; very probably the learning process commences some time before birth. It does not appear probable that we cease to learn until we are in the actual clutches of death.

If we limit the term "learn" somewhat narrowly, we can truthfully say that a definite process of learning is the formation of a *habit;* and conversely, a habit is a way of living that has been learned. For example, a child, in learning to talk, is forming habits of speech. Whatever speech habit he forms becomes a part of his way of living, until he changes the habit by further learning, or it is changed by something that happens otherwise to his speech machinery or to his nervous system. Habits, in their totality, make up the character of the individual; that is, they *are* the individual, as he appears to other people.

Psychology has distinguished three ways in which man responds, or reacts, to his environment; three ways, that is, in which his mental life is expressed. In the first place, man *perceives* his environment. He not only senses the colors, sounds, odors and other sensory data which his environment presents to him, he also perceives relations. He notes the differences between two colors, two sounds, two data of any kind. He perceives similarities, space relations of distance and form, time relations of succession and simultaneity, and many other relations. We commonly designate this relational feature of perception as *intellect*, to distinguish it from mere sensory perception; but intellect is not confined to perception. Intellect goes over into another department of mental life, that of thought or thinking.

Man *thinks* about his environment. He imagines, remembers and anticipates. In remembering, he is thinking about something which has appeared or occurred at an earlier moment; and he is thinking of it as past. That is, he is aware of a certain time-relation, that of the past. In anticipation, or anticipatory thinking, he is aware of another time-relation, that of the future. A great variety of relations are involved in thinking, in addition to the time-relations. Almost always, therefore, and possibly always, thinking is an intellectual process. In the higher forms of thinking, relations are involved in complex systems. For our primary classification, however, the time-relations are of fundamental importance. If you are thinking of something as future, you are *anticipating*. If you think of something as past, you are *remembering*. If you think of something without thinking of it as either past or future, you are merely *imagining*.

Man, however, does more than perceive his environment and think about it: he also *feels* about it. He feels elated, glad, sorry, grieved, angry or afraid. He feels excited or depressed. He feels in many other ways. It is difficult to separate feeling from thinking, and as a matter of fact, most of our terms which purport to describe purely the way we feel really describe ways of thinking rather than ways of feeling. Nevertheless, the facts remain that we do feel, and that there are different ways of feeling.

For convenience, we sum up these perceivings, thinkings and feelings by one expression: *being conscious*. To perceive is to be conscious; to think is to be conscious; to feel is to be conscious. If a person, because of a blow on the head or the effects of a drug, is assumed to be not perceiving, thinking or feeling, we say that he is *not conscious*, or *unconscious*. These are firmly established conventions of terminology, established in common speech, and recognized by psychology. Neither in common speech nor in psychology can we depart from these conventional uses of terms without fatal confusion.

Living, so far as psychology is concerned, is made up of these responses to environment. Yet we should note that one's own body, which in a peculiar way is himself, is also a part of his environment. One may perceive, think about and feel about his own body. The body is the medium or mechanism through which man responds to his environment; and yet the body is a part of the environment. This seems incongruous at first, but really offers no difficulty when once we recognize it as a fact.

All of these responses are literally things we do. When we perceive, we are doing something; when we think, we are doing something; and when we feel, we are doing something. Nor are these statements to be understood as metaphorical; they are literally true. Even in the sense of physiology, they are true, and their truth can be demonstrated by exact methods of the laboratory. There is no contrast between perceiving and doing, between thinking and doing, between feeling and doing. The apparent contrasts are due to our considering abstractly one aspect of a process while ignoring another. This abstraction is useful in its place, but we must never lose sight of the fact that all mental life is a process of action, or rather of *reaction*.

In accordance with this well-established psychological scheme, we may expect to find habits of the same three classes: habits of perceiving, habits of thinking and habits of feeling. The botanist has built up habits of perceiving plants and of thinking about plants in certain ways. The farmer has built up different habits of perceiving and thinking about them. The individual of every type has built up his ways of feeling about certain things and certain occurrences, and certain total situations. One man is irritated by matters which do not irritate another. One is afraid of what another does not fear. One is pleased by what displeases another. These differences are in some measure due to difference in sensitivity of sense organs, differences in bodily state, and other basal conditions; but in far larger measure they are matters of habit.

The habits of a teacher, a plumber, a nurse, a dancer, a person of any walk of life are to external appearances matters of mere physical doing. One man has acquired habits of skill with his hands in performing certain operations. Another man has acquired skill in another manual operation. Another man has acquired skill in movements of the feet and legs. Another has skill in public speaking, and so on. If we stop our analysis at their level, however, we never arrive at a real understanding of the habits. Always it is necessary to seek the full details of perceiving, thinking and doing, for the seemingly merely manual, pedal or vocal habits are fundamentally mental.

Psychology has made several distinct advances in the understanding of habits during the last generation, and one of these advances has been in respect to the relation of habits to hypothetical unlearned ways of responding, which were called *instincts*. Formerly, it was supposed that man was equipped, by mere processes of anatomical development, with certain ways of responding (the instincts), which were accordingly denoted as unlearned. To this stock-in-trade of unlearned ways of responding, man was supposed to add habits by learning. A grand illustration was offered in the life of the infant, who has (apparently) the instinct of nursing and the instinct of crying, but who learns to walk and to talk. A great many other instincts appearing at various periods of life, such as the sex instinct, the pugnacious instinct and the gregarious instinct have been assumed by various theorists.

Now our knowledge is greater in that we are not so certain of this sharp distinction between instinct and habit. It is true that the normal infant immediately after birth will perform the quite complicated operations of sucking and crying upon the proper stimulation being applied. But we cannot say that these are actually unlearned operations. It may be the infant has never either cried nor sucked previously, although as a matter of fact, many infants, at birth, show evidence of having sucked their thumbs *in utero.* If we assume that a habit is formed only by repeating the operation which is to become habitual, then since (by assumption) the infant has never carried out the operation before, he cannot have learned it; it is not a habit but an instinct.

Unfortunately for this simple interpretation, we can no longer accept the traditional theory of habit formation. It is true that some habits are apparently formed by mere repetition of the act. By repeatedly smoking, we form the smoking habit. By repeatedly speaking in a certain way we may acquire the habit of speaking in that way. But it becomes more and more clear that in many cases the habit of acting in one way is really formed by acting in another way. This common fact will be made clearer later, but the brief statement will indicate that we have good grounds for distrusting the apparent evidence that the infant's skillful performances immediately after birth are instinctive, in the sense of being unlearned.

As for the more complex instincts of later life, they have been abolished from psychology for even more weighty reasons. The so-called instincts are now seen to be merely arbitrary classifications of conduct, really classified not in accordance with any psychological facts, but merely in accordance with the external results accomplished. We saw the wolf chase the rabbit; catching the rabbit provides food for the wolf. "Aha!" we said. "The food-getting instinct at work." Whether the wolf was hungry or not; whether he sought food for himself or his mate or her young made no difference. We noted that men fight: "The pugnacious instinct." But fighting may be for food, for mates, for protection, for almost any purpose. The classification may be excellent so far as the outward effect of the movement is concerned, but is based on no fundamental psychological principle.

In all the so-called instinctive performances, moreover, learning is evident as the primary means of forming the type of response; that is, these performances are really habits. Even the vaunted "sex instinct" displays this characteristic clearly. Not only in man, but in the higher vertebrates generally, sex-behavior is learned by steps which are clearly discernible. In the case of the lower animal forms, and in insects, the case may be different, but we cannot at present be certain of that.

Our preliminary definition of learning or habit formation is, however, a narrow one, as we noted above. By a habit, is generally understood a stereotyped form of response: the doing of the same thing always in the same way under the same conditions. In many cases, however, no such stereotyping of action can be discerned. One may progressively change the performance, continuously varying it while constantly learning. This, for example, is the case while one is learning to dive. Day by day the type of performance changes; that is the indication that one is really learning. If the process goes far enough, a fixed or stereotyped performance may be reached. But learning has occurred even if the process be discontinued before that point. In learning to drive an automobile, certain minor operations of control of pedals and gear shift become stereotyped; but the proof of most efficient learning is found when one reacts properly in an emergency which has never before been presented, in which the appropriate reaction is anything but a habit; yet that adequate response has been truly learned.

It is true that in such cases we might describe the learning process as the formation of habits which are "broken" as they are formed, while progressively new habits are formed. Such a conception, however, is needlessly complicated, and is so evidently artificial that it is more useful to formulate a broad conception of learning which will not only include habit formation, but will cover also processes which do not so obviously lend themselves to descriptions in terms of habit.

The important consideration is that our responses or reactions, whether they be perceptual, ideational or affective, are not in general fixed, but are consistently being modified. It is only in the exceptional cases that the stereotyped response, or habit in the strict sense, serves. Many so-called habits are somewhat rigid; the responses occurring time after time in the same way, if the term "same" is loosely applied. When examined more narrowly, these habits are found to be really quite variable. A person, for example, may have formed the habit of waking at "a certain hour" in the morning. Yet, as a matter of fact, the exact moment of awaking varies; and the way in which the individual rouses varies still more. Even in two instances where the time of apparent wakening may be the same to the minute, the process may be a relatively sudden one from deep sleep in one case, a slower one in another.

Similar conditions hold for a wider range of habits. In fact, it is safe to say that any habit whatever is really a tendency to a response which varies constantly within certain limits, and the "invariability" of a habit is determined by the limits of measurement employed in describing the habit. Everyone, for example, may be said to have the habit of awaking after sleep. But when habits are described as broadly as this one, our scientific interest in the uniformity is less than our interest in the variability.

Our first problem, therefore, is the general one of discovering what factors or conditions determine the changes or modifications of response.

We may admit at the beginning that the same person may be expected to respond differently to stimulation patterns which are decidedly different, although it is true that some individuals will make practically the same response to two stimulus patterns to which another person will make two markedly different responses.

Illustrations of this sort of individual difference are easily found. If a singer sings certain notes "flat" or "sharp," some persons will respond in a quite different way to this stimulus pattern from the way in which they would respond to the pattern of the same melodic unit sung correctly. If the auditor does not actually wince, shudder or make any other "overt"[1] response, he nevertheless responds with a different perception, and usually with a different thought and a different feeling.

Some auditors, however, will make the same response to a melodic unit, irrespective of whether the notes are accurately sounded or are flatted or sharped. This characteristic of the auditor we commonly describe as a failure to discriminate, for by "discrimination" we mean actually the making of different responses to different patterns. Discriminative ability is a matter of degree. If the flatting of a note, for example, is very slight, although demonstrable by physical instruments, no auditor however musically trained can discriminate the flatted note from the unflatted. If the flatting is a trifle greater, some auditors will discriminate it. If still more flatted, still more auditors will make

[1] An *overt* response is one issuing in an action perceptible to another person, and a *covert* response is one which is not thus observable, although in many cases recording devices of sufficient sensitivity will actually demonstrate the occurrence of muscular and glandular changes in covert responses.

the discrimination. In the extreme case, a few auditors will be unable to discriminate two notes clearly an octave apart. Such persons are said to be "tone deaf," that is, to have no pitch discrimination. By rough tests, these tone-deaf persons can be picked out. By the refined methods of the psychological laboratory the discriminative ability for tone of "normal" persons can be determined through finding the least difference in pitch between two tones which will enable the reactor [2] to make with certainty one reaction to one tone, and another reaction to another.

In a given individual, discrimination is in part a matter of learning. It may be wholly a matter of learning, for all we know. The person with "untrained ear" (which does not mean really untrained, but only partly trained) may not be able to discriminate two tones which are a considerable fraction of a semi-tone apart. By further training, however, he may learn to discriminate two notes which differ only 1/40 of a semi-tone in certain parts of the scale. Other individuals may never succeed in reaching that point in fineness of pitch discrimination.

Although discrimination, the ability to respond differently to different stimulus patterns, involves learning, our more fundamental problems of learning center about another type of variation, namely, the changes in response to the same stimulus pattern.

Modification of response may be rapid or slow. If I have in my laboratory a reactor who is engaged in operating an adding machine, and without warning I discharge a pistol behind him, the reaction is usually violent, and almost all the muscles in his body are overtly affected. If, a few minutes later, I repeat the stimulus, the reactor again not expecting it, the reaction will for certain types of reactor be much less violent. For other types it may be more violent.

[2] By convention, the individual who is being measured for any psychological purpose is called a "reactor."

In either case, the first stimulus and response has modified the second. The reactor, in other words, has *learned.*

Modification may be still more rapid. A reactor unused to electric shocks, may, when given a shock-current of a certain measured amount, make a violent reaction. After a few such stimulations there may be no overt response to the same amount of current. Further stimulation may produce little further change. Learning, in other words, is relatively rapid for a short time, becoming thereafter very slight.

In some cases, however, the learning may proceed slowly over a long period of time. This is the case in learning to play billiards. While both of these illustrations are drawn from the field of motor learning,[3] the same variations occur in other sorts of learning. Thus one usually learns the answer to a riddle by being told it once. But learning to do "mental arithmetic" may be slow and up-hill work.

So far, we have described habit formation as a positive process: a building up of a response tendency to a point of dependability and effectiveness. This is really only one side of the process. Many habits are detriments rather than assets, and an important feature of the adequate control of life is the breaking down, or unmaking, of "bad" habits. Some of these "bad" habits are wholly detrimental. Others are advantageous in one phase or period of life, but become detriments in later periods. In either case, the efficient destruction of the habit is an important matter. Some of these "bad" habits will be discussed in detail in later chapters. Here we need only point out that much of what we have said about learning applies with equal force to unlearning. Instead of describing living as being primarily

[3] There is no real fundamental distinction between "motor" learning and types which are described otherwise, but the old terms are sometimes convenient. "Motor learning," which we shall later discuss under the name of *skill,* is merely that sort of learning in which the results of the learning are observed or measured in terms of muscular contraction or its direct effects.

learning, we might well say that it is *learning and unlearning.*

If we now classify the factors which modify responses whether as learning or unlearning, we find:

First that time may produce a change. I have learned to play chess; but after several years of disuse, I find I can no longer think of the proper openings. I do not move my pieces in the way in which I formerly did. In other cases an actual improvement in performance may occur after a sufficiently long time has elapsed since the last preceding trial or performance. Now, of course, "time" alone does not produce these changes. What we mean by saying that time modifies the response is that during the elapsed time some unknown changes have occurred in my organism, perhaps in my nervous system, perhaps elsewhere, such that the same stimulus pattern no longer produces the same response. Not only are the specific changes unknown, but also the causes of these changes are unknown.

Second. Although the neural or organic changes which have occurred during a certain time may be unknown, we may have knowledge or indications of the causes of these changes. They may be due perhaps to a change in the general health of the reactor. In general, we do not expect the same response from a man when he is ill as when he is well. Or, the causes may be more specific. The ingestion of alcohol or drugs, the occurrence of fatigue, the eating of a full meal or other definitely describable occurrences or conditions may modify the individual's responses. Modification for which causal factors of these sorts are apparent, we do not ascribe vaguely to "time elapsed," but to *organic changes.*

Third. There are the modifications which are properly called *learning.* In these cases, we assume that time is involved, for learning does consume time, however brief. We assume also that the learning operates by producing organic changes, perhaps in the nervous system; such changes, whatever they are, being the immediate determiners of the modification of response. These unknown neural changes are known to be due to previous response. Otherwise we would not class them as learning. The change in the reactor's response to the second pistol shot (under the conditions above specified) is due to the first response. The progressive changes in the performance of the billiard player, the chess player, the student of arithmetic, are due in each case to the previous responses the learner has made.

It is not to be assumed that the *learning responses* are responses of the same kind as those which are *learned.* The billiard shots through which one learns to play billiards are, in one sense, of the same type as those he makes after he has become proficient. They are muscular movements which propel the cue longitudinally against the ball. But considered and measured more carefully, the early shots turn out to be different from the later in very important respects. By one type of response, apparently, the reactor learns to make a quite different type of response.

This learning of one thing by the doing of another is more conspicuous in those motor cases where thinking is explicitly involved. The billiard pupil profits immensely by having the theory of the shots explained to him. His thinking is really a response or series of responses, but it is a response of a type different from that of making a shot. So also, the man learning to dive makes progress through planning, or thinking of the proper performance before attempting it. Actually, such combinations of thinking and perceptual response in "motor" learning are not exceptional. In almost all learning processes thought is an important factor, and in many learning processes it is the essential factor.

Learning, then, proceeds through responses, which may or may not be similar in type to the responses which are ultimately learned. Far from the general truth is the old assumption that "we learn a response by performing the response." In fact, in many cases, the reverse process, of unlearning a motor response which has become a habit, is best carried out through the deliberate repetition of the motor performance which is to be disintegrated or unlearned.

The importance of adequate knowledge of the condition of learning can scarcely be over-estimated. The progress of the student through school and college is a progress of learning. Every artisan and artist goes through a prolonged period of learning his trade or art. But these are only minor features of the total learning the individual must accomplish. The infant between birth and school-age completes a learning program of enormous range. We learn our social adjustments, our political and business abilities, our cookery and the materials of our culture. We learn our language, our literature, our civilization. As social conditions, political conditions, economic conditions and sports change, we have an enormous amount of relearning to do. We learn our adaptations to our wives and husbands, without which marriage is a total failure. Our living, in short, is mostly learning, and success in life is almost wholly by success in learning.

Our practical problem of learning may be summed up in one question: How can we learn most usefully? This includes a vast list of minor problems, few of which can be solved completely at the present time. Because of the vital importance of these practical problems, every scrap of scientific knowledge about learning that we can acquire is of value. The present need is to acquire still more scientific knowledge, without being troubled by the question of how we propose to apply it. When we have an adequate body of scientific knowledge on any subject, the application presents no great difficulty. The real difficulty is in accumulating the scientific results, and the real danger is in the attempt at application of what we do not really know.

Such premature applications are necessarily the applications of theory rather than of fact, and are usually detrimental.

In the scientific study of learning, therefore, we must temporarily ignore the question of practical application. We must accumulate all the information possible. Observation and analysis of common learning problems is useful up to a certain point. Such observational work shapes up definite problems. But these problems can be solved only by experiment.

In experiment, we start from a basis of previous observation or experiment and formulate a tentative *hypothesis*. It seems probable or possible that such and such is true. Adopting this as a working hypothesis, we now devise *methods* by which the truth or fallacy can be tested. Then we have to devise experimental *technique* (including the use of apparatus) by which the method can be applied to the hypothesis. The making of the test is the experiment, which is literally the *trial* of the hypothesis.

If the results of the experiment brand the hypothesis as *true* or *false*, the results are positive. If the results do not indicate the truth or falsity of the basal hypothesis, they are *negative*. If there is no definite hypothesis involved, or if there is *no experiment*, the work itself may furnish useful *observations*.

In applying both the experimental method and the preliminary observational method to the learning problem, we must eventually consider learning of all types. We cannot certainly infer from one type of learning to another until we have proved experimentally that they follow the same principles and laws. The results of experiments on one sort of learning, however, supply at once important hypotheses concerning other sorts of learning. In this way the study of one type of learning may throw light on another type. Hence the study of the learning of infants and children throws light upon the learning of adults. Hence also study of the learning of animals, vertebrate and invertebrate, may well be expected to throw light on the learning of humans, as well as being intrinsically interesting. Furthermore, many lines of experimental work can be carried out on children only with great difficulty and expense. Much time, labor and outlay can be saved by carrying out the experiments first on suitable animals, such as rats, dogs, and chickens. The results of experiments with animals alone do not enable us to infer with certainty to the facts and principles of human learning, but from the animal results, we are enabled to clarify our problems, to develop our methods and to plan our experiments on human beings so that they can be carried out more efficiently and economically.

For this reason, psychologists studying the learning process are intensely interested in animal learning, and animal psychology is undergoing an extensive and elaborate development. The reader need not be surprised therefore if he finds that we draw on the results of experiments on the learning of animals, along with the results of experiments on human beings.

CHAPTER II

THE FUNDAMENTAL PRINCIPLES OF LEARNING

WE learn by acting, by perceiving and by thinking. So far as we can now determine, no other ways of learning are possible. It is true that there are other ways of modifying our responses, but these are not ordinarily classed as learning and should not be so classed, since the extension of the term to cover processes which are not demonstrated as yet to be of the same nature as those processes now covered by the term may result in confusion. We may, for example, change the responses of a rat or the responses of a man by removing parts of the brain, or by administering alcohol, strychnine or other drugs. The essential response mechanism being thereby changed, the stimulus patterns which otherwise would produce certain reactions now produce other responses. The man who, when in his normal conditions, stops his car when the red light shows on the traffic semaphore, as he learned to do, may, when intoxicated, drive through without stopping. This variant response is not, in established parlance, something he has learned; it is the modification of his learned results by a transitory change in the neural mechanism. A change has occurred; but we may reasonably hold that the process of change is not learning, and should not be confused with learning.

The constitutional features of the mechanism, including those which are the results of learning in the proper sense, are being modified throughout life by processes of growth, nutrition and other metabolic and perhaps structural changes. The results of learning are, therefore, never stable, and the net effects cannot be described in terms of learning alone. Any measurement of learning effects, by whatever standards and criteria, is significant only in terms of the general constitutional conditions under which the learning has occurred. Learning which gives a certain efficiency in response under given conditions may give a higher or a lower efficiency later under other organic conditions, even if the external stimulatory conditions are not essentially different. Action or conduct, at any time, under any stimulatory conditions, must always be figured as determined by two factors: (1) previous learning; (2) the organic status of the moment.

The distinction between thinking, perceiving and "acting" is not so sharp as it might appear to be. These are all *reactions* or responses, characterized typically by (1) the stimulation of sense organ (2) the excitation through the sense organs, of "nerve current" or nervous discharge. This excitation is conveyed inward to the neural centers (the brain and the spinal cord) and from there outward to the muscles and glands. If the muscles contract or relax conspicuously as a result of the nervous discharge, we have "action" in the popular sense of the term. We may designate such a response as an *action-response.*

Action-responses may be perceptual; that is, they may be processes of perceiving. For example, I may perceive an approaching missile through a response which involves, as its final phase, putting up my hand to ward off the missile. Other action-responses may be processes of thinking, as when I think of an object by speaking its name aloud. Still other action-responses are theoretically possible in which there is neither thinking nor perceiving. Neither thinking nor perceiving always involves terminal action. I may perceive a fly on the window pane without performing any act which is noticeable. I may think of a cow in a pasture with no conspicuous action of even the speech muscles. In a great many such perceptions and thinkings, muscular action actually occurs, but is too slight to be readily observed. In some cases, apparently, there is no muscular action at all. Such thinkings and perceiving are considered, nevertheless, to be true reactions, so far as the neural feature of the process is concerned.

Seldom, if ever, do reactions of the type described occur in pure form. In most of our responses, there are thought features and perceptive features, and there is usually some external action. Only by abstract analysis can we separate such a response into its several factors.

For convenience, we may distribute responses of various sorts into four classes: (1) Responses in which the action is overt and definite, but the thinking involved is minimal. Some of these responses are commonly described as "impulsive actions," and some are described as "reflexes," although the term "reflex" is used in a variety of other senses. If a sudden noise occurs behind a person, he may start; the start being due to contractions of various muscles of the trunk, arms and legs. This response is usually called "impulsive," and is a perceptual reaction, the thinking involved being slight, if thinking is involved at all. If a missile approaches the person's eye he will blink. This response is also perceptual, and is commonly called a "reflex action" or simply a "reflex."

(2) At the other extreme from the responses designated as "impulsive" or "reflex" are those in which thinking conspicuously occurs, but action is slight. Some one, in your hearing, mentions water, or you hear water running, and you suddenly remember that you left the water running in the sink, but you do not change your actions in any conspicuous way. There may be observable, but slight, action involved in some responses of this class. Perception does not occur in a response of this class; but a perceptual response may precede it as in the illustration given.

(3) In many responses there is action gross enough to

be readily observable, together with definite and important thinking. If I am asked for a certain reference, I may think that I have it on a pad on my desk, while at the same time I turn in the appropriate direction and put out my hand toward the pad. Such responses may be complex, and analyzable into two parts, one of which, if it occurred alone, would be merely perceptual, ending in action, while the other would be a thought-response.

(4) In some responses, the action is a part of the thought process itself. In speaking fluently, the thought-response itself eventuates in the action of speaking. That is, one may think by means of an action-response. One may think the answer to a question by the speaking of the answer. This type of response is to be contrasted with that in which one speaks and thinks alternately, rehearsing in thought what is spoken in words immediately afterwards. This is a partial duplication of processes which is often inadvisable, but which at times is useful. Sometimes, if one is skillful, he may think and speak by a complicated response which includes much more than would be necessary for the adequate speaking of the words. This is the way in which the stammerer usually attempts to proceed, but in which he does not succeed. Instead of thinking a sentence by saying it, he is attempting to say it while thinking how he should say it, or thinking that he will have difficulty in saying it. The ultimate cure for the stammerer lies in his learning to think by the simple speaking operation as normal people usually do. After he has achieved this, he can add the more complicated processes to his repertoire.

The foregoing classification of responses is arbitrary, as classifications always are, there being no actual line of division between the several classes. Certain responses cannot be readily put in any classes, because they are intermediate between the types of two classes. Admitting this arbitrariness, however, all responses of the individual are included somewhere in this scheme. Can we learn through all of these responses? If not, which are possible means? If so, which is the most efficient? These questions are vital for the theory and practice of habit-making and habit-breaking; and from the answers we may derive fundamental principles of learning. Let us consider first the responses in which thought is minimal; namely, those which are designated as "impulsive" and "reflex," beginning with the "reflex."

Reflex actions or "reflexes," as we have above used the term, are perceptual responses or they are complex responses including components which, if separated, would be perceptual responses; but they include negligible thought elements, if any. Among these reflexes are the *tendon reflexes*, of which the knee-jerk is the best-known example. Aside from the tendon reflexes, the blink reflex, above described, the salivary reflex, the Babinski reflex, the iris reflex and the vestibular reflexes are familiar.

If the leg be supported by a rest under the knee, so that the lower leg hangs free and if the tendon just below the knee-cap be then struck smartly with the edge of the palm, or with a small rubber hammer, a sharp forward kick or jerk of the lower leg usually results. Some persons show no visible movement of the leg; but if the hand be placed on the leg above the knee (the palpation method of observation), a slight contraction of the muscle, not sufficient to move the leg, can be noted. In some individuals, however, no knee-jerk can be observed, even by the palpation method. Injuries to certain parts of the nervous system, or diseased conditions thereof, may prevent the knee-jerk from appearing; and it was formerly supposed that the absence of the knee-jerk is always a symptom of a pathological condition. We know now that many physically sound persons have no knee-jerk, a fact which may be important for our consideration of the learning process.

When food is taken into the mouth, resulting in stimulation of gustatory receptors on the tongue and olfactory receptors in the nose, a flow of saliva is produced. This flow is in small part due to muscular contraction of the ducts of the salivary glands, and is in large part due to increased activity of the glands themselves. The odor alone of food substances usually produces the same result. The sight of a cut lemon or of someone sucking a lemon or even hearing a lemon mentioned or thinking of lemon juice may produce the salivary reflex.[1]

The iris reflex is elicited by flashing a light into the eye, whereupon, in all healthy persons, the circular muscle fibers of the iris contract, diminishing the size of the pupil. The Babinski reflex is a spreading of the toes of an infant produced by stroking the sole of the foot when the infant's leg is firmly held. If the leg be left free, some other response may occur. After early childhood the Babinski normally disappears, and its presence in an older child or an adult is sometimes a symptom of a pathological condition.

The vestibular reflexes are produced by stimulating the non-auditory part of the inner ear, either by irrigating the meatus of the ear with warm or cool water or by rotating the reactor for a number of turns. By a single stimulation, several different reflexes may be produced, including the production of nystagmus, which is an alternate drift of the eyes in one direction and quicker movements in the other direction, continuing for varying periods of time.

Some of the vestibular reflexes, we know, can be modified by what appears to be a learning process. They may perhaps be considered as habits of a relatively fixed order: if so they are habits which can be changed. By daily irrigation of the ear with water of a temperature which at first produces violent nystagmus, the nystagmus can be made to

[1] In the days of small street bands, employing brass instruments, mischievous boys have been known to put a band out of operation by sucking lemons in the view of the musicians.

decrease in violence and in duration, and can eventually be eliminated. By rotating a person through ten complete revolutions in twenty seconds at uniform rate, we can produce nystagmus following the stopping of the rotation, and lasting from fifteen to thirty seconds, according to the individual. If a person is given five or six sets of rotations daily, with rest periods between the sets, the nystagmus is progressively decreased, and may in ten days or two weeks completely disappear. Apparently, however, this modification of the reflex to rotation does not modify the reflex to irrigation of the ear, and *vice-versa*. The processes of modification seem to be learning processes; but we cannot be quite sure that this is the case.

The Babinski reflex, as we have said, tends to disappear with increasing age of the child. Is this a mere matter of growth or is learning involved in the modification? At present, we cannot answer this question. The iris reflex, on the other hand, does not seem to be modifiable except through changes in conditions of health. The blink reflex is modifiable in two ways, and sometimes rapidly. If daily stimulation of the blink is practiced, through the falling of a small hammer on a glass plate fixed just in front of the eye, the blink which at first occurs with every fall of the hammer eventually disappears, although with a different type of apparatus, without the glass plate, the blink may still occur. If, before much practice has been given, and the reactor is blinking with every fall of the hammer on the plate before the eye, a small bell is struck each time the hammer falls; then after a few falls of the hammer, the sounding of the bell alone will produce the blink. A "substitution of stimuli" has occurred; the blink reflex has been so modified that it is now produced by a stimulus which previously would not produce it.

The salivary reflex is very easily modified in this substitutional way. Apparently, the individual in early life salivates only to the taste of food or the feeling of it in his mouth. The salivation to the smell of food is apparently learned; the salivations to the sight of food, to the mention of food and to the thought of food are certainly learned.

The knee-jerk is modified with difficulty but apparently can be modified in the substitutional way. Prior to training, the knee-jerk is elicited only by stimulation of the tendon or the muscle directly. By combining another stimulation (*e.g.*, an electric shock applied to the toe), with the tap on the tendon, the other stimulation can eventually be substituted for the tap. Whether the action of the jerk in response to the tap-stimulus can be modified is not certain, but it seems probable that by an intensive course of training, the leg may be made to jerk backward instead of forward when the tendon is tapped. Whether repeated elicitations of the response will cause it to disappear, or decrease, has not been ascertained.

By the proper training process, we see some reflexes which at first occur only upon certain specific stimulations, can be made to follow other stimulations; and as we have indicated, the effectiveness of the various stimuli which produce the salivary reflex may be due to learning. The blink reflex, however, occurs to a number of different stimuli, even early in life. A visual stimulus from an object approaching the eye produces a blink. A light flashed in the eye, a sudden loud sound, a puff of air on the face may have the same result, with no certain indication that that variety of effective stimulations is due to a substitution through learning.

The conditions for the different reflexes seem, on the basis of the foregoing descriptions, to be decidedly diverse. There is no single reflex type to which all the reflexes conform. Where modifications are possible, however, these modifications do seem to depend on conditions which are alike for the majority of reflexes. We will first state these conditions, and then explain them:

(1) A reflex, if it is modifiable, is not a narrowly limited response. It does not involve only a small group of muscles or glands, and it is not determined by a stimulus of a single specific sort. There seem to be no exceptions to this rule.

(2) Thinking has an important influence on the modification of reflexes. The vestibular reflexes appear to be the only exceptions to this rule.

When the knee-jerk is elicited by tapping on the tendon, the extensor muscle of the leg contracts with conspicuous results. Muscles of the other leg, of the arms and of the trunk also contract, as is shown by registration of the electrical currents from these muscles by means of a sensitive galvanometer. The action, in short, is not confined to a single muscle of the leg stimulated, but involves the muscular system rather widely. The food stimulus does not merely cause salivation; it stimulates the flow of gastric juice in the stomach. It affects the muscles of respiration, and we have reason to suspect that the action pattern is still more extensive organically. With regard to the blink, we have little direct information, but the blink appears in the general muscular response occurring when one is "startled" from any cause; which strongly suggests that when the blink is the only conspicuous result of a response, a wider pattern of bodily activities is really involved. In eliciting the Babinski reflex, stroking the foot produces not only extension of the toes but also movements of the legs and arms, although these other occurrences are quite irregular.

From the side of stimulation, the situation is equally clear. The knee-jerk is dependent on tendon stimulation, in the sense that, prior to specific training, the tendon stimulation is an indispensable condition. Without tendon stimulation, the reflex does not occur. When it occurs, however, a wide range of stimulations to sense organs in other parts of the body contribute to the determination of the energy and the speed of the response. For example, by causing a person to exert muscular strain in his arms, we can intensify the knee-jerk. The reflex is in fact so susceptible to stimulations of the various sense organs and to the effects of thought processes that successive stimulations of the tendon, using the same force of blow, will not produce a uniform series of knee-jerks, no matter how solicitously we may attempt to make all the other conditions uniform.

Even the vestibular reflexes are highly variable from moment to moment, although we may make the specific vestibular stimulations uniform. Apparently here, also, the reflex is determined not by the primary stimulus only, but also by a variety of stimulations of other sorts, including visual, auditory, tactual and visceral. Moreover, it has been demonstrated that vestibular stimulation affects not only the eye muscles, but the entire musculature of the arms, legs and trunk, and certain of the visceral muscles. It alters the heart rate and the blood pressure and increases the kidney secretion.

So far as the reflexes are concerned, learning or habit formation seems possible only through integrative action of the nervous system; that is, through the tendency of processes in any part of the nervous system to involve various other parts of the system also.

Turning now to the effect of thinking on the modifications of the reflexes, we note that except for the vestibular reflexes, modifications occur most conspicuously in those reflexes which are most readily influenced by thought processes directly. The salivary reflex and the knee-jerk are clear illustrations of such reflexes. The Babinski reflex is very likely of the same type. The iris reflex seems to b' directly influenced by thinking. From these considerations, we might be led to suspect that the thinking which occurs in connection with the process of modification is the real modifying factor, if it were not for the vestibular reflexes, which are apparently not influenced by thinking. In this connection, however, the following considerations are important:

(1) We do not as yet know certainly that the vestibular reflexes are not influenced by thinking.

(2) We are not yet certain that the modifications which are possible in the vestibular reflexes are actual learning processes. It is still possible that the reduction of nystagmus through repeated rotations or repeated warm or cold applications to the ears, is due to changes produced in the sensory organs in the vestibule, and not to changes in the brain.

In regard to the importance of thinking for modification of the reflexes, we are in no position to make final inferences. A vast amount of experimental work on reflex modification remains to be done. The apparent importance of thinking in such modifications is strongly suggestive, however, in connection with the known facts concerning learning involving response above the reflex level.

Impulsive responses are not, in general, more variable than are reflexes; but the integrative feature is more pronounced, and the modification of these responses is more clearly dependent upon thought processes. In fact, these features furnish the only practical basis on which we could distinguish an impulsive act from a reflex, and probably constitute the only important differences between responses of the two classes.

Among impulsive response we class the "start" to a sudden stimulus, the putting up of the hand to ward off unexpected missile, the drawing back from a suddenly appearing object or situation, exclamation of pain, surprise or joy and a host of other responses less readily named.

These impulsive responses are all capable of elicitation by a variety of stimuli. One starts at something heard or seen, at a touch or at the thought of something. Defense or withdrawal movements are produced by words as well as by visual or tactual presentations. Impulsive actions are all determined, not only by the specific stimulation which mark the beginning of the response, but also by concurrent and preceding stimulations of all sorts. All impulsive responses are modifiable, and in all cases the modifying processes involve thinking.

The impulsive start at a sudden noise is usually modified by repetition, although the modification may endure only for a short period. Even the second repetition of the stimulus after a brief interval may produce a start less violent than that of the first. This is not the result of the mere first response, but the result of the thinking which follows the first response, and which so modifies the mechanism that the second response is different. On the other hand, where different thought-processes are involved, the start may be more violent upon repetition of the stimulus.

At the other extreme, it seems much more probable that thinking can produce a modification in thinking, without overt action being necessary. Especially is this true in sequential or serial learning. Suppose I ask you to compose a series of six syllables in which the three vowels, *a*, *i* and *o* are used in rotation, to compose this silently without any vocalization and to recite it to me only after it is complete, and when you are certain that you can recite it without hesitation. This involves your learning the series to the point at which you can think it through without break, but learning it by thinking only. That you can now make it the basis of an action series does not matter. What I am pointing out is that at the last thought repetition, before you try to recite it, you have learned it.

That other types of modification of thinking by thinking occur may be taken for granted without extended discussion. It is evident that we can learn by thinking. It is not evident, though it may possibly be true, that we can learn without thinking. On determination of the actuality or the non-actuality of learning without thinking depend important features of our eventual animal psychology, for if it turns out to be true that learning occurs in human beings only through thinking, the extension of the rule to the lower animals would be in order and our conceptions of the mental processes not only of the rat in learning the maze, but also of the angleworm and the snail, which also can learn to traverse mazes, would be profoundly altered.

In the light of these conclusions, what about the intermediate and more common types of response? In these, both action and thinking are involved. Does the learning efficiency vary directly with the relative amount of thinking involved? The situation is not so simple. The thought element is the most important element in all learning in which it occurs, but the learning depends, apparently, on the definite relations of the thinking to the end result, to the particular response which has just occurred and to the future responses. Moreover, particular types of thinking may be more efficient than others, and in determining types, we must include the elements of feeling. The action also is important, not in itself, but as combined with thinking.

It is commonplace that the reaction which produces an unpleasant result is not so likely to be repeated as the reaction which produces a pleasant result. The feeling is influential, if definitely associated with a thought. That feeling in itself, without thought, is of any importance is improbable, except in so far as feeling may be an organic condition which is generally favorable or unfavorable to learning. From present information, we may infer that perhaps mild feeling is favorable and intense feeling possibly unfavorable, but we have no indication that any specific type of feeling is any different in its effect from any other, except the feeling which is involved in desire.

Both anticipatory thought and retrospective thought seem important in learning. Whether the one is generally more important than the other cannot yet be decided. Anticipation with a specific feeling constitutes *desire,* and without this specific determinant it may be called *purpose.* A purpose to learn is effective and, admitting that the effectiveness of a particular purpose may depend upon the stage and type of learning in connection with which the purpose occurs, it would nevertheless seem to be a fact that, other conditions being constant, the more specific the purpose the more effective it is for learning. If we wish to maintain the most favorable conditions for learning, we make our instructions to the learner as specific and as clear as possible, that he may understand the goal and the process; that is, may be able to make his purpose or his desire or his mere anticipation as definite as possible.

Retrospective thinking is important in the recognition of errors and successes. The learner, throwing darts at a target, under usual conditions observes the amount of error, or the approximation to success, of each shot. This might be said to be a merely perceptual matter. In every such case, however, there is a definite standard of achievement which has been thought about, and in respect to which each shot is "judged"; it is not merely a shot, but a shot approximating in certain measure to the previously conceived standard. The perception of the locus of the dart is not a process on the merely perceptual level, but a more complex process into which enter thought elements which are based upon the original thinking of the standard. It is for this reason that we call such observations "judgments."

The more dynamic phase of anticipatory thinking is found in desire and purpose. For the most adequate learning we currently assume that a desire or purpose to attain a certain performance is a valuable asset, and that the learner who merely *anticipates* a result as one which will *probably* be attained is less efficient, and one who anticipates as a mere *possibility* still less efficient. Actually, we have little experimental evidence on this most important point; but in lieu of the specific proof, it is not unreasonable to adopt the current assumption.

We do not know whether desire for the immediate result of the learning process (immanent desire) or desire for an ultimate or secondary result (transcendent desire) is the more effective for the production of learning. That both transcendent and immanent desires are effective for responses generally, as well as in learning, we admit as soon as we note that desires "spread" regressively.

If I desire to please my teacher, and I know that achieving a certain thing will produce that result, I have a secondary desire to achieve that which, in itself, I might have no desire to achieve.[2] We call the primary desire as involved in such a regression the *motive*. The nature of this transfer has not been investigated. The regression may not be real, in the first instance, but only apparent through the fact that real desires of ultimate results are effective in producing mediate or contributing results. The question is not important for our present purposes. The important question is: Does learning proceed as well when the primary desire involved is transcendent, as it does when the desire is immanent? Since we cannot answer this question at present, we insure by utilizing both types as fully as possible. If we can arouse an immanent desire, a desire on the part of the child, for example, to learn that which we desire him to learn, we do so. If we cannot, we work through a transcendent desire, such as the desire to escape punishment, or the desire to receive an extrinsic reward. It is, however, a practical question of much moment whether the combination of both is stronger or weaker in its effects than the immanent desire alone. We might assume that the addition of an immanent desire to a pre-existing transcendent one is

[2] As regards the particular achievement, the primary desire, which is the motive to please the teacher is transcendent; the secondary desire is immanent.

an improvement; but as a matter of fact we have no proof of this. In any event, there is some reason to believe that the manner of combination of immanent and transcendent desires is highly important.

The result of our exposition up to this point is to demonstrate clearly that the process through which we learn is not, in general, the process we learn. If we complicate the knee-jerk in order to modify it, the response which we finally set up is a modified form of the knee-jerk, from which the complicating processes employed in the learning may be absent. If we should produce in a given reactor a reversed knee-jerk, where the movement of the foot would be produced by retraction of the lower leg, instead of by extension, we might proceed as follows:

We might instruct the reactor to retract the foot voluntarily every time he receives a tap on the tendon. In this case, the learning process involves the purposive or anticipatory thought of retraction, with, perhaps, at least transcendent desire and the judgmental notice of the actual result with its conflicting muscle patterns. If the experiment should be a success, then after many practice responses, the reactor would respond in the formerly anticipated way without reinstating the purpose or desire, and even without noticing the result of the response. Whether this particular course of training would succeed or not is immaterial. It is the *type* which all actual courses of learning follow. The responses which are employed in learning are not the responses to be learned.

Elsewhere, the relation between the learning responses and the learned responses may be somewhat different, while conforming to the same law. We wish to train a man to salivate when we press his shoulder blade. We set up a stimulation condition in which both pressure on the shoulder blade and the smell of food are employed. This produces a response which may have been already learned; but the learning we have in view is something different. After enough repetition, the shoulder-blade pressure alone produces salivation. This is what was to be learned, and has been learned. The response employed in the learning was different from the response learned, both in respect to the stimulus and, therefore, in respect to the neural pattern; it was probably different also in its action-pattern.

Again, if the reactor is to learn to throw darts so that fifty thrown in succession land in a certain circle in the target, he begins by throwing wild. Of the first 500 darts thrown, only ten per cent may land within the prescribed circle. Are these throwings the only ones involved in the learning? Certainly not. If, as may happen, all of the throws which land the dart in the prescribed area occur in the first half of the practice 500, he profits by the 90 per cent of throws which went wild, as is shown in the next 250 throws. In the total learning, the darts thrown wrong probably have been as effective as an equal number of those thrown "successfully," provided the thought factor has been adequate. By response of certain types, the reactor, as in other cases, learns to make responses of certain other types. In this illustration, the most obvious difference between the learning response and the learned response has been in the action patterns; but as in the other illustrations, there is a difference between the neural patterns also.

The principle that learning process and learned process are essentially different is of great importance, but it is usually overlooked in discussions of learning. If we ignore it, the learning process cannot be understood, and false principles will result from the attempt at analysis. On the other hand, we must remember that the principle has its limitation. The differences between the learning response and the learned response are variable and have zero as their limit. In certain cases, the learned and the learning may be not different within our limits of measurement. This is the case with "habit formation" as conceived by William James and by many of his followers.[3] This type of learning does occur, but it is not typical of learning; rather it is the infrequent limiting case. Instead of serving as the explanation for learning in general, this type of learning requires additional explanation, and is perhaps the most difficult of all learning to explain. According to certain theories, repetition is a factor in itself, although the function of thinking and feeling and desiring are admitted as accessory. That learning often occurs without repetition, as the result of a single performance, is not denied, and this exception offers no difficulties. The theory of repetition does not mean strictly repetition, but *performance.* One performance (it may be assumed) tends to "fix" the response, but in most cases will not fix it completely. If the one performance has some effect, however slight, a second will add to it, and by sufficient accumulation of the slight effects of single performances, the work is done. It is admitted that some learning proceeds more rapidly than others. The effect of a single performance in one case may be relatively much greater than in other cases, so that fewer repetitions are required to produce "fixation." In the limiting case, of course, one performance is sufficient. There is thus no opposition between learning by trial and error and by "insight." Both would be provided for by the repetition theory, if the repetition theory could be made to work at all.

The repetition theory, however, explodes conclusively as soon as we analyze learning adequately and discover the principle of difference between the learning response and the learned response. Yet repetition is necessary in most cases of learning. How does it really operate?

The answer is to be sought through asking another question: Repetition of what? Of the particular response to be learned? As we have just shown, this is not essential,

[3] See Chapter IV.

and is usually impossible until learning is practically accomplished. The repetition which is essential to learning is the repetition of *trial*, not of success. By repeated trial, resulting generally in a great variety of responses, we arrive finally, in fortunate cases, at the response which has been anticipated, or else at some other response which differs from the initial type resulting from the trials. This sort of repetition, which is demonstrably a feature of learning, is not that conceived in the old repetition theories.

If now we summarize the relation of the response which is to be learned to the learning process, we can say that in the more efficient learning process, the ultimate response is thought of, or else its results are thought of. The response to be learned, in short, is an ideal, which is present in the learning, and which is actualized in the result. Is this the good old "ideo-motor action"? Not at all! Ideo-motor action, as James conceived it, is the thought of an action, or the thought of the result of the action, producing the action immediately following the thought. Two processes are involved: First, the response which is the thought, and then the response which terminates in the action. Such a sequence is possible, but must not be confused with the response in which the thought of the action, and the action, are included in a single response. If I have learned to think "baked beans" by saying the words, and if this is my habitual way of thinking of baked beans, then my procedure, when I say "baked beans," will not be ideo-motor action at all, as James conceived it. I will not have the thought of baked beans, followed by the words; I will have thought of baked beans by saying the words. When I do think by one response, and this leads to doing something, through another response, I am merely demonstrating the serial association of responses; and it makes no difference whether the two responses associated are two thoughts, two actions, an action followed by a thought or a thought followed by an action. The last of the four cases, which is James' ideomotor action, is merely a particular form of a much broader phenomenon. All such sequences, however, are learned. An action does not call up a thought or a thought call out an action, unless the two have previously been associated, and such association is one particular type of learning.

In the learning process with anticipation, the idea or ideal of the final response to be learned does not tend to produce the response immediately. It may tend to produce the attempt; but the attempt will ordinarily result in some other response. Only after learning is complete can the ideal call forth the actual response. During the learning process, the ideal produces only learning.

We must bear in mind, however, that neither anticipatory thinking nor retrospective thinking can be said to be essential to learning, aside from the question of "mechanical" learning. The simpler form of thinking, which is conventionally called "mere imagination," is probably effective, although less efficient than the more complex form. If we should eventually have to assume that the rat "thinks" in learning the maze, this would not compel us to attribute either memory or anticipation to the rat.

The conception of learning response as different from learned response has far-reaching implications which raise troublesome problems. How wide may this difference be? How great may be the gap between the responses involved in practice and the response to which this practice ultimately contributes? Can the difference be so great that we would not easily recognize the two as related, and yet the actual relation implied by learning exist? If so, we are very apt to overlook many phenomena of learning, and build up false theories to explain phenomena which are really to be explained on the basis of learning. Painful as the realization may be, it is necessary to admit that the gap between learning and learned may indeed be so great as to escape ordinary observation. It is highly probable that many of our developmental theories are founded on ignorance of actual learning relations.

Let us turn for a moment to the old distinction between instinct and habit, or, better, the clearer distinction between learned response and instinctive response. This distinction is based on the logical, and logically unobjectionable, assumption that a learned response is one which has been formed by modifying a preceding response. But the process must start somewhere. If we start with no responses, we have nothing to modify. Therefore, there must be, in the animal, a certain initial equipment of unlearned responses. To this supposition there can be no logical objection. The difficulty arises when we attempt to decide which those responses are.

A criterion has been found in the old and utterly illogical contrast between reflexes and more complicated acts. The reflexes from which the contrast is drawn are the same responses which we have above designated by this term. To make the contrast, however, it has been customary to define the reflexes as *unlearned responses.* As we have shown above, the distinction between the responses which are customarily listed as reflexes and other responses is not absolute, but is a matter of gradation; and many reflexes are so modifiable that there is extreme difficulty in ascertaining what the original, unlearned form of a reflex was. To maintain a contrast between unlearned reflexes and responses which are in part due to learning, we must in some way identify the unlearned, original reflexes. As an escape from this difficulty, proponents of the contrast view have often resorted to a re-definition of reflexes as responses occurring for the first time in the individual. This definition logically abolishes the contrast between reflexes and other responses with which we started, and substitutes a different contrast, but still a contrast between the learned and the unlearned.

The definition also assumes the theory of learning by repetition only. Prior to the "first occurrence" of a response, there can have been no repetition of the response, *ergo* there can have been no learning of the response. In view of the principles of learning above developed, we might well at this point consider the theory of learning as the modification of reflexes as too inconsistent to merit serious consideration. There is, however, some illumination of the principles of learning to be obtained by analyzing this theory further.

What is the criterion of a first appearance? Merely the fact that we have not noticed it earlier, or have not recognized it. At birth, the infant cries. This is the first appearance of this particular complex response; hence it is claimed that it has not been learned. It is classed as instinctive. But can we be certain that the same movement pattern has not occurred *in utero?* It would not be "crying," because by crying we mean more than the response; we mean the setting in vibration of the air by a certain response. If the infant is not surrounded by air it cannot "cry"—but it can make a response which is very similar, as a response. Perhaps the infant does not make this response before birth: It may be that for the infant floating in the amniotic liquid the response would be fatal. Nevertheless, it is probable that all the muscles involved in crying have been exercised, and so through other responses involving the same muscles, the ultimate crying response has been learned. The response which produces learning is not the response which is learned.

A somewhat clearer illustration is furnished by anothe response which occurs shortly after birth—the sucking response (sucking reflex). Here is something which has been seized upon as "instinctive" or non-learned. The infant has never had the mother's nipple in its mouth before; the sucking is therefore clearly a first performance. Here is a beautiful reflex. How complicated it is, involving what fine coordination of muscles! How efficiently it is performed! Few other reflexes illustrate so well the perfection of organization of response by mere maturation of a mechanism, without any learning.

Unfortunately for the beauty of the illustration, infants are born which show signs of having sucked the thumb *in utero*. That other infants have made sucking movements *in utero*, even without their thumbs in their mouths, is highly probable. The "first performance" shown after birth thus loses its value at once as a criterion of instinctive performance.

It is well known that infants make responses involving the limb and trunk muscles for some months before birth. In many cases these movements are so violent that they may be distinctly felt, not only by the mother, but by the observer's hand laid on the mother's abdomen. Sometimes, the results of the movements are even visible. That a wide range of lesser movements occur before birth is quite evident. How early these begin is the question. Learning is apparently proceeding at an important rate. The response in learning is not the same as the response learned. We cannot be certain that the modifications of the tissue arrangements in the blastula, as a feature of growth, are different in kind from those which occur in learning generally. The important step in deciding the issue is to determine whether mechanical learning is possible.

It is not useful to push our speculations far. We might accept as highly probable the beginning of the learning process early in the embryonic life. How early, we do not at present need to guess.

If we turn to the so-called "instinctive" or reflex performances which make their "first appearances" somewhat later than birth, we find the same difficulties. The child begins, somewhere in the early months of life, to utter sounds of a character different from wailing. It begins perhaps with "ga" or "kow," the child's first speech consonants being usually guttural.[4] Is this an "instinctive" performance? Well, many throat and lip movements have preceded it, and the vocal cords have been well exercised. That these performances do not contribute to the incipient speech response is too improbable to be assumed. It is true that there is a "first time" at which the child says "ga." But *the learning response is not the same as the response which is learned.*

The kitten may efficiently catch its first mouse. The fledgling bird, pushed from the nest, may fly, although the first flight may be awkward and inefficient. Learning is obvious in the improvement of the flying response, but even so the first flight is a remarkable performance. To one who has watched the wing movements of young birds in the nest, the assumption that these responses have not contributed to the ultimate flying seems far-fetched. The previous performances of the kitten have just as clearly contributed to the development of the mouse-catching response. *The learning response is not the same as the response which is learned.*

But, it may be argued, the kitten will catch a mouse, and the young rabbit will not. Here would seem to be a definite proof of instinctive action. This is true. There is no denying heredity. One animal will learn what another will not under the same conditions. There is no doubt of the existence of instinctive action. The truth of the matter, which clears up the whole difficulty, is that all responses are instinctive in the sense that they are dependent upon, or determined by, heredity. The unlearned responses (if there be any) are instinctive. But the learned responses, in their utmost complexity of modification, are equally in-

[4] This is not in accordance with the traditional view that infants commence speech with labial's (m, p), but the traditional view happens to be wrong, as careful study of infants has shown.

stinctive. As soon as we abandon the spurious contrast between "instinctive" and "learned," the term "instinctive" has useful meaning—not before.

Returning now to our problem of "impulsive" or "reflex" action, and freeing it from the conception of "unlearned" responses, we find another difficulty in the popular antithesis between "voluntary" action and "involuntary" action. Certain impulsive actions are described as "involuntary." Reflexes are often said all to be involuntary, although it is not claimed that all non-impulsive actions and all non-reflex actions are voluntary. This distinction, if it be valid, might have some significance for the learning problem, in that the function of voluntary and involuntary action in learning might be different. We must consider the matter carefully.

CHAPTER III

VOLUNTARY AND INVOLUNTARY ACTION

In the learning of useful modes of action, as well as in the unlearning or breaking of bad habits, improvement and progress are dependent wholly or in part on the "will" of the individual to improve. In our psychological discussions, however, we avoid using the term "will," not because we ignore the importance of the facts to which the term "will" is popularly applied, but because the term itself is a theological one, having implications or fringes of meaning which tend to confuse the understanding of psychological statements in which the term might be used.

Even scientists, for example, frequently become embroiled in discussions of the theories of the "freedom of the will," and its theological opposite, the theory of "determinism"; neither of which has any real meaning either in science or in daily life, but which sometimes seems to have meaning because the term "will" is also used to designate actual features of life. For this reason, we use the terms *volition* and *voluntary*, with their cognates, which signify everything to which "will" could be applied in psychology, and which have not as pernicious associations at the present time as does the term "will."

Responses which we designate as *voluntary* are those which we could equally well describe in various cases as responses which the individual desires to perform or decides to perform or chooses to perform. Responses which the individual performs in spite of the fact that he desires not to make them, or chooses or decides not to make them, we usually designate as *involuntary*. Still other acts, which apparently are performed without any desire or choice, for or against them, we designate as *non-voluntary.*

Responses of all three types are involved in learning and in unlearning, and sometimes it is necessary to distinguish carefully between them. In the breaking of certain bad habits, as we shall see later, it is especially important that the patient shall find means by which certain acts shall be made voluntary at certain times and be allowed to proceed involuntarily at other times. Hence it is necessary at this point that we should briefly discuss the nature of voluntary and involuntary action, in order that there shall be no misapprehension later.

If we extend our study of action into the field of physiology, we encounter a source of confusion in the classification of striped muscle as "voluntary" (such as the muscles of the arms, legs and trunk), and the smooth muscle as "involuntary" (such as that of the stomach and intestines). This is confusing because responses which involve striped muscle may be either "voluntary" or "involuntary," and some of the smooth muscles are under as definite "voluntary" control as are any of the striped muscles, although other smooth muscles are not ordinarily under voluntary control.

The glands, in general, would be described as "involuntary," or their actions said to be "not under voluntary control," yet certain glands are as distinctly under voluntary control as are many of the striped muscles. If I desire to "make my mouth water," *i.e.*, produce a flow of saliva, I can do so. Some persons can shed tears "at will." The action of the salivary glands is, for the greater part of the time, involuntary; but so, as a matter of fact, is the action of the arm and leg muscles.

Indirectly, of course, I can control the action of the smooth muscle of my stomach. By whirling around, for example, I can check the rhythmic "hunger contractions."

I can also control to a certain extent the beating of my heart; by running, I can accelerate it; by lying down I can slow it. Such methods of control, however, are not generally classed as voluntary. The control of the response, in these cases, is through other responses. It is indirect, whereas voluntary control is assumed to be direct; and these responses are said to be voluntary in which the essential factor of choice, decision or desire is involved in the action itself, and not in an antecedent action or actions.

Is this distinction a valid one? Probably not. It appears that the voluntary control, in many types of response which are universally said to be voluntary, is as indirect or remote as is the control of the heart beat or the stomach contractions. In order to bring this point out clearly, it is necessary to analyze the simpler cases in which desire, choice or decision are involved in the response.

Let us suppose that while walking you notice a shiny object on the sidewalk. The idea of picking it up occurs to you, and also ideas such as that it may be nothing but a fragment of metal or glass, not worth examination. Suppose that you do pick the object up. Here is an act which would readily be called voluntary, an act in which the occurrence of decision is apparent. Suppose that after picking it up you are still in doubt as to whether it has value. You think of putting it in your pocket for later examination, and you also think of flinging it away. Whichever course you take, you perform another voluntary act, in which *choice* may be noted.

The two cases are, after all, much the same, except that in the first there is the thought of a definite action, with complicating thoughts of an inhibiting sort. The period of deliberation or hesitation, which, however fleeting, seems to occur in both cases, is in this first case (as we later analyze it) the deliberation of "do it or not do it." In the second case, there are two equally positive and definite courses of action thought of, and the deliberation is: "do this or do that." Such, apparently, is the only essential difference between the "choice" situation and the "decision" situation.

If these actions are typical of voluntary action, it is apparent that the salient feature in such action is the anticipatory idea or anticipatory ideas: the idea, that is, of a certain act which may be performed, whether immediately or in the more remote future. The feature of secondary importance is the hesitation or deliberation. So far, our analysis merely follows the more brilliant analysis which William James made. Idea, deliberation, action; this is the sequence involved. But is this all? James held that an additional factor distinguishes the true voluntary act from the non-voluntary; namely, *acceptance* or *approval,* which we may readily identify with *desire.* If this definitional concept be adopted, then volition, or voluntary action, is ideo-motor action *plus* desire.

Such deliberative, voluntary action does occur. There is no doubt of that fact. It is not so certain, however, that either desire or deliberation is essential to what we usually classify as voluntary. In many cases the desire is slight, if it occurs at all; and the hesitation, if any, is so fleeting as to be indeterminable. Now, this may be said to be merely a matter of terminology, a matter of arbitrary decision as to just what, in the total range of action, we are to designate by the term "voluntary." But shall we give the term a broad extension or a narrow one? The broader application seems the more adequate. The important point to be considered is that there is really no dividing point between what James calls ideo-motor action and the more complex actions involving deliberation. There is a gradation of intermediate forms, and any division into two groups is arbitrary and the classification of particular acts as belonging to the one type or the other would be in many cases determined solely by the accuracy of our observations.

Further, we may note that hesitation or deliberation is an interference with action, an impedance, and establishes a temporal as well as a logical remoteness of initial from final parts of the sequence. If the voluntary character of response is to be identified with this deliberation or inhibiting factor, then volition is a negative factor in action, not a positive factor. Again, the idea which precedes the deliberation and choice is, according to our modern notion, a response of fundamentally the same type as any other response. In making remoteness of sequence a characteristic of voluntary action, therefore, we are preparing the way for considering such sequences as are involved in the ordinary method of control of heart rate or stomach contractions as if they were the most complete and outstanding types of voluntary action, instead of being remote or doubtful examples.

The most adequate view of the range of actions which our term voluntary seems to include is that the "normal" type of voluntary action is the immediate one, in which the idea (of the act or end) is not a response in advance of the act, but is an essential feature of the response of which the act itself is the external feature and is separable from it only by abstraction. From this point of view, the delayed response where deliberation intervenes, and other remote sequences of response, are also voluntary actions, deviating from the normal type in their complexity.

Voluntary action, then, is either merely action in which the idea of the act itself (or of its result) is essentially involved, or it is a series of acts in which the idea of the final act is involved in the first one. This definition includes James' ideo-motor action, and properly so.

Immediate objection will be raised to this simple point of view. The knotty problem of "the will" is not solved, or even fairly presented, but is merely ignored by definition. A striking illustration will be brought from the field of hypnotism. The responses of the hypnotized patient are exactly what we have described as voluntary action. The idea of the action "suggested" to him by the hypnotizer is followed immediately by the action or is a part of the response. The patient has the idea, either by performing the act, or else through a thought-act which is followed immediately by the action itself in the simple (ideo-motor) fashion. Here is apparently the *reductio ad absurdum* of our point of view, for the responses of the hypnotized subject would be the purest illustration of voluntary action, as we have described or defined it; whereas it is commonly assumed that the subject's actions are strictly non-voluntary, his "will" being in abeyance. The conclusion that the hypnotized patient's responses are of the purest voluntary type is indeed correct. The paradox of the situation is merely the result of an inherent inconsistency in the popular view, which it is our business to remove.

The hypnotized patient's conduct is of the type which we follow in most of our responses. We think of doing something and we do it. No ideas of alternatives occur. The interesting question about the hypnotized patient is: why, in his temporary condition, do the alternatives which would occur, or the inhibitory ideas which would occur in his "normal" condition, not occur?

The responses of the hypnotic patient are "appropriate" to his ideational sequences, but not "appropriate" to external conditions (*i.e.*, appropriate to the circumstances as he views them; not to the circumstances as we view them). Such conduct, however, is familiar in everyday life. How often we observe a person acting "foolishly" but voluntarily, even deliberatively. Decision or choice is based on knowledge, that is, on the ideas which occur at the time. But a person who acts on very limited knowledge, we always recognize, may act as voluntarily as a person who acts upon more copious and more adequate knowledge.

The dilemma is now clearly presented. Either we must identify voluntary action with knowledge, and declare that the voluntariness of action is definitely the index of the amount or degree of knowledge involved in the action, or upon which the action is based, or, we must assume that the hypnotic patient acts in the purest voluntary way. The first horn of the dilemma has often been grasped. It is the identification of "will" with intelligence (in the older sense of the word "intelligence"). This point of view, however, breaks more violently with the general features of the common conceptions of "will" or voluntary actions than does the choice of the other horn of the dilemma. For the identification of "will" with knowledge or intelligence abolishes the whole system of popular notions which have grown up about volition, and should bring about the abolition of all the terms. We should, as a minor example, drop the investigation of the difference between "voluntary" and "involuntary" muscles, and begin the investigation of the question which muscles are "intelligent" and which "ignorant." The whole vital problem we have been laboriously discussing; namely, the distinction between responses which are "voluntary" and which are "non-voluntary" would be phrased in the question: Which responses show the effects of a high degree of intelligence or wisdom; and which a sad state of ignorance or feeble-mindedness?

If, however, we adopt the opposing view, we do straighten out (abolish, if you please) a certain popular conception as to the actions of the hypnotized person, but we leave intact the general view of volition which has become imbedded in our science as well as in our daily praxis of life.

The situation which confronts us in the dilemma we have been discussing is very much like that which confronts us in the Euclidean and hyperbolic geometries. We can adopt the former or the latter. Either course is reasonable and may be consistent, if we never forget our postulates and axioms. But the Euclidean geometry is the one we follow in our praxis, for we find it useful. So, in respect to the choice of hypotheses of volition, if we adopt the "common-sense" one, we are no more logical than if we adopt the philosophic alternative. But the common-sense dictums of voluntary action we will find useful; and no use has been found for the other. Therefore, we shall assume that the responses of the hypnotized patient are the purest type of the voluntary, and that more complicated responses of everyday life are to be classed as voluntary in so far as they present the essential ideational feature which is simply manifested in hypnotic action.

CHAPTER IV

PHYSIOLOGICAL THEORIES OF LEARNING

If a person of average education were asked to define learning, he might say: "Learning is training the brain." This statement is as true as are most brief statements about complicated matters, but it is far from being the whole truth, and cannot be certified as containing nothing but the truth. If the same person were next asked to tell in what consists this training of the brain, in so far as the brain itself is concerned, or to indicate how a brain that has been trained in any particular way differs from one that has not been trained in that way, he would admit that he does not know. He may add the statement that the problem is one for the psychologist or the physiologist, and disclaim being either. The fact is, neither the psychologist nor the physiologist knows the answer to this question.

Philosophers, in the past, have sought to explain learning by assuming "impressions," "traces" or "engrams" which are left in the brain by perceptions, thoughts and actions. Such explanations are merely restatements of the problem in figurative language. All one means by saying that an "impression" is left in the brain is that somehow something has happened to the brain, so that it functions differently thereafter. Although psychologists and physiologists in the past have used the language of these philosophers in their descriptions of learning, and may use the same language informally today, they recognize that the language is metaphorical, emphasizing a problem but supplying no explanation.

Strictly speaking, the study of the brain and of the details of its function is in the province of the anatomist and the physiologist. The psychologist also is vitally interested in the brain. He is interested indeed in the entire nervous system and in the muscular and glandular systems as well. He is not, however, as a psychologist, concerned with the study of the internal activity of these tissues. He is interested in whatever facts the physiologist can report concerning the physiological functions, and in the theories the physiologists construct; and he is interested in comparing these facts and theories with physiological facts and hypotheses.

It has been sometimes supposed that, through physiological investigation of the functions of the brain, facts might be discovered which would be of direct value to psychology. It has even been assumed that the facts of brain activity are the facts for which psychology has been searching. More generally, it has been believed that psychological findings are valid only in so far as they are directly based on physiological findings. Historically, these expectations have not been fulfilled, and we no longer expect much assistance from this quarter in solving the problems of psychology. Little is known about the physiology of the brain, although a great deal is known about its anatomy and histology. Much that passes for brain physiology is merely a mass of psychological facts of perception, feeling and thinking, translated into terms of the anatomy of the brain in accordance with various arbitrary assumptions. Since we have not yet a complete psychology of these mental processes, and since the assumptions are often either mere conjectures or archaic beliefs, the physiological theorizing could hardly be expected to be more than tentative. Attempts to base psychology on the physiology of the brain have been sadly disappointing.

The better understanding of the physiology of the brain and the nervous system really depends on an advance in psychological knowledge, and the application of this knowledge to the problems of physiology. Psychology, accordingly, has a large responsibility for future contributions to the physiology of the brain. It is mainly from this point of view that psychologists are interested in physiology; and the problems on which a number of individual psychologists have been actively working might otherwise seem to be merely physiological. Contributions have begun to accrue from these labors, such as that of Franz and those who have followed his lead in the investigation of the localization of functions in the brain, and that of Weaver and Bray on the ear, which has developed a technique that physiologists are actively employing.

Under the influence of the older psychology, which considered learning as merely the fixation of a habit, several theories of brain action in learning have been constructed by philosophers, physiologists and psychologists, and are now being painfully eliminated. The most important of these has been the theory of "brain-paths," which was later re-formulated in terms of "bonds" between the individual nerve cells in the brain.

The essential features of the "brain-path" theory are well presented by William James in the chapter on *Habit* in the first volume of his *Principles of Psychology* (1890). This theory was by no means new, but had been outlined by many English, German and French philosophers and physiologists, from whom James drew his ideas and to some extent even his phraseology. We can trace the hypothesis through various philosophers and physiologists as far back as Descartes.

With the introduction and acceptance of the "neuron theory" of neural structure and function, according to which the entire nervous system is made up of innumerable nerve cells or *neurons*, anatomically separate, but functioning in coordinated groups, it became possible, and seemed

(7) The foregoing statements are over-simplified. The situation is complicated by the fact that in the brain one neuron may excite not merely one further neuron, but may excite several neurons simultaneously, thus starting several branches of the original chain action. Conversely, one neuron is not always excited by a single antecedent neuron, but may be excited by the simultaneous actions of several antecedent neurons. The nerve chains are thus really organized into complicated *neural patterns;* the number of different patterns being beyond estimation. This complication is of vital importance for any theory of brain action.

Confining ourselves to the simplest cases, it is evident that nerve current entering the brain anywhere may run over a definite pathway (that is, a succession of neurons) within the brain and emerge to any group of muscles, as determined by chance, by previous learning or by constitutional "set." The "brain-path" theory assumes that the nerve current having once taken this course, the "pathway" is made more permeable, that is, the chance is increased that on the second entrance of a nerve current over the same afferent route it will follow the same "brain-path." By further repetitions, the pathway is "deepened" and the probability of its being taken is still further increased. By many repetitions, in the absence of strong countervailing forces, the probability may be raised almost to a certainty. The habit is established.

The detailed theory of the fixation of the pathways in terms of "bonds" is as follows: The nerve current, at a given transitional point, has the choice of at least two routes. The neuron which is excited is in contact with two other neurons and might, theoretically, excite either. For some reason, it excites one rather than the other. Let us call the neuron primarily excited A; the two which it might excite B and C. It happens to excite C and not B. This increases the probability that when neuron A acts again, it will excite

C and not B. A "bond" is said to have been formed between A and C. Each recurrence of the exciting of C by A "strengthens the bond." Assume such a process (mysterious though it may be in its nature) occurring at each transsitional point, and we see how the "brain-path" is formed and deepened. This theory of "bonds" obviously is the old "brain-path" theory revamped in terms of neuron chains.

This "brain-path" theory has been rejected for two reasons, either of which would be sufficient. In the first place, while the theory apparently fits some simple cases of habit formation, in which a response, once made, comes to be repeated in a mechanical way without essential modification, it does not agree at all with the more numerous and more important cases of learning, described in Chapters I and II, in which a response is continuously modified during the process of learning, so that at the end of the learning it may be distinctly different from its initial form. We cannot assume the changing of the outward result without a corresponding alteration of the inner brain pattern. Instead of the discharge over a given pathway making that pathway more probable for the future, it seems in such cases to render it less probable; at least it is clear the route of discharge through the brain is progressively changing.

In the second place, the theory would set aside for certain responses certain neuron chains, and for other responses, other neuron chains; and it is fairly certain that the brain cannot operate in that way. For example: by the "brain-path" hypothesis, if neuron A discharges to neuron C a number of times, and the "bond" between these is thereby strengthened, neuron A would thereby be ruined so far as concerns its possibility of use in another response, in which discharge to neuron B would be required. We have good reasons to suppose, however, that many of the brain neurons must participate in many different responses. Looking at the response from the point of view of the final muscular action we know that the same group of muscles must participate in many different responses, which leads us to the conclusion that the brain cells which are involved in the nerve-current discharge from the brain to these muscles must participate at different times in different responses. Looking at responses from the point of view of sensory stimulation strengthens this conclusion. The first neuron in the brain which receives the afferent current from the point of origin in the sense organ must (on any simple theory) discharge in some responses to one further neuron, in other responses to a different neuron. Or, if we assume the discharge of the first neuron to be always to the same second neuron, the next neuron, or one of the further ones in the series, must exercise this variable function. The formation of "bonds" between successive neurons would be successful only on the assumption that the neurons participating in the formation of one habit did not participate in the formation of any other habits.

The complexity of function of the brain cells is greater than is permitted under this simple theory. While we cannot say that *all* neurons in the brain participate in every response, it appears that vast numbers of them do participate in a great number of responses, and we can say that the brain *tends* to act as a whole, although the tendency may never be fully realized. These facts are sufficient to let the "brain-path" or "bond" theory of learning out completely. For the present, the only available theory of brain action in learning is that the brain does act in learning; but that the nature of its action is unknown. Many more details must be acquired by painstaking experiment before it will be worth while to construct a more definite theory.

Action as a whole, which we now ascribe to the brain, we designate conventionally as *integration*. If integration is a fact, so far as the brain is concerned, we should expect it to be recognizable in the muscular patterns of movement in which the primary response ends. In any response, whatever the stimulus, and whatever the more obvious or more useful muscular action might be, we should expect to find the whole muscular system tending to participate. As a matter of fact, we do find experimentally that this total response tendency is present. A response which apparently involves only one group of muscles, such as the arm and finger muscles when picking up a pin from the table, really involves muscles of the trunk and legs. This wide integration may not be useful, but it does occur.

A theory of learning which may be called "physiological," because it has been developed by a physiologist, and which apparently involves definite physiological assumptions, has attracted much attention within recent years. This is the "conditioned reflex" theory of Pavlov, which, like many older theories, is an attempt to derive all responses, however complex, from a group of original reflexes with which the animal is supposed to be provided, and to reduce all learning to the modification of reflexes, and the further modification of these modified reflexes.[1]

The term "conditioned" is apparently equivalent to the older word "modified," although Pavlov's interest in the modification of reflexes is mainly restricted to one type of modification, namely, that produced by what is more commonly called "associative learning," in which, as we noted in Chapter II, the stimulus which normally produces one response becomes associated with another response.

Pavlov and his assistants have experimented on animals, chiefly dogs, modifying reflexes of several kinds, and modi-

[1] Publications from Pavlov's laboratory appear in Russian. Some of his own writings have been translated into English, and it is fair to assume that from these, and from summaries of other work which have been prepared by persons presumably competent, a fair indication of the views of Pavlov and of the results from his laboratory may be secured. The exposition and criticism in this chapter apply strictly only to Pavlov's views as they have been presented to English readers. With views which have not been so presented, we have no concern.

fying other processes which we do not ordinarily class as reflexes, but which Pavlov seems so to class. The most conspicuous work has been with the salivary reflexes of dogs. Using a technique devised by DeGreef, the duct of one of the salivary glands of the dog is brought out to the surface of the face, so that the saliva, when secreted, is discharged externally instead of into the mouth, and can, therefore, be collected and measured. In this way, the familiar facts of salivary secretion, and the modification of the reflex action by the process of associating a new stimulus with the reflex, to which we have already referred, have been subjected to detailed investigation. It is true that some of the experimental results, quite aside from Pavlov's theories, are looked upon with suspicion by men in other countries; but many of the results are in accord with facts which have long been known, and Pavlov apparently depends upon these facts to illustrate his theory of learning.

The salivary reflex was probably chosen as the response on which to work on account of the ease of measurement of the salivary flow in the animal which has been subjected to DeGreef's operation. The salivary reflex, however, is peculiarly suitable for elementary experiments on reflex modification, on account of the variety of stimulations which influence the secretory activity of the salivary glands.

Pavlov's specific application of the terms "conditioned" and "conditioning" may be indicated as follows: A dog, which has been fed on a particular kind of food (let us assume milk) can be demonstrated to have a flow of saliva (a) when the food is put in his mouth or (b) when the food is smelled or (c) when the food, in its usual form, is seen. When a food new to the animal (let us say meat) is first presented, neither the smell nor the sight of the food will produce a salivary effect. After the dog has eaten the new food several times, the *a*, *b* and *c* responses are all obtainable from this food. In ordinary language, we would say that the animal has learned the salivary responses to the sight and smell of the food. Pavlov expresses this in less common terms by calling the *b* and *c* responses "conditioned" reflexes, and calling the process of learning them the "conditioning" of the reflexes. The *a* response (to the food in the mouth) he calls an "unconditioned" reflex. Correspondingly, he calls the food in the mouth the "unconditioned" stimulus, and the sight and smell of the food, after they have, by the learning process, been made capable of exciting the salivary response, he calls "conditioned" stimuli. Before the learning process, these are "neutral" stimuli.

In these terms, some of the familiar phenomena of learning have been reformulated by Pavlov, and it has been made to appear that all forms of learning, and not merely learning of the associative type with which Pavlov and his assistants have worked can be described in terms of "conditioning." These new terms have recently had a considerable vogue, and, as frequently happens with new terms, have seemed to many persons in some mysterious way to "explain" learning.

It is true, as critics have pointed out, that when Pavlov's accessible writings are paraphrased in the usual language of psychology, the greater part of the novelty of his doctrines disappears. No doubt Pavlov has himself been misled into supposing that he has discovered new points of view, where he has merely invented terms. There is, however, more than neologizing in Pavlov's work.

The extensive experimental work done in Pavlov's laboratory, while not of major importance for the psychology or the physiology of learning, has resulted in interesting confirmations and extensions of our knowledge. We have known, for example, that stimulations of a variety of types affect the salivary secretion. Pavlov has shown how great this range really is, and has provided definite illustrations of the effects concerning which our previous knowledge was rather sketchy. If he has, in James' phrase, "elaborated the obvious," such elaboration seems well worth while. The especial benefit which this work has conferred is the increase in interest of physiologists and psychologists in studying the modification of reflexes.

Pavlov's experiments are not different in type from experiments which had previously been performed, except in the particular technique applied to the salivary apparatus. The results of his experiments obviously furnish no basis for a theory of learning which is not provided by any of the great mass of results from experiments which psychologists have carried out. Pavlov's work has revealed no modification of response which is not of a familiar type. There is nothing especially "physiological" about the work, and in spite of Pavlov's insistence that he is studying the brain, it is difficult to see that he is, in the more striking parts of the investigations, studying the brain in any way different from that in which Small, Thorndike and Watson, in their experiments on animal learning, may be said to have been studying the brain.

The fact remains, however, that Pavlov has been strongly convinced that he has made a decided innovation in the field of learning, quite aside from the technique of his salivary measurements; and his enthusiasm has persuaded many others that he has introduced revolutionary notions, although they are unable to ascertain exactly what these notions are.

We have ascribed to Pavlov the proposal of a theory of learning, although it is actually difficult to prove that he holds any theory at all. If we should assume that his theory is merely that all responses are either reflexes, in the usual meaning of the word reflex, or are modifications of such reflexes, we would be ascribing to him a theory which has been put forward by many philosophers since Descartes, the sterility of which we have analyzed in Chapter II. Such a theory would be no innovation.

On the other hand, we might point out that Pavlov uses the term "reflex" in an exceedingly loose way. He uses it at times to designate what we ordinarily call a reflex. On the other hand he speaks of a "freedom reflex" where he means the total activity by which the animal seeks to escape from captivity; and he postulates other astonishing reflexes. He explicitly identifies reflexes with instincts. He speaks of "acquired reflexes." It is not unfair to say that by a reflex Pavlov means any response, of whatever type, and however complex, for which he can assign no previous development through learning. A fair consistency can be brought into Pavlov's translated writings, if the word "reflex" is everywhere deleted, and the word "response" substituted. When interpreted in this way, Pavlov's statements may seem to involve no new theory of learning, but merely to present the familiar problems of learning, with some experimental contributions to be added to what others have made.

A still different interpretation is put upon Pavlov's statements by some of his American followers, namely: that he holds that all learning is associative, that is, proceeds by the substitution of one stimulus for another in a response already established. This interpretation ascribes to Pavlov a naïve conception appropriate to the early periods of scientific investigation, the sterility of which will be made more evident in Chapter V. It is hardly polite to suppose that Pavlov really holds such a view, although it may be implied by some of his more confused statements.

Assuming that there is an important theory behind Pavlov's presentations, this theory is not difficult to find. The key is supplied by his terminology, which although illogical and confusing, may be supposed to be determined by a central idea, an idea not clearly expressed, and apparently not clearly understood by Pavlov himself. At any rate, there is a theory of learning which has been suggested by various physiological facts, but apparently has not been completely developed by anyone. This theory is the most plausible explanation of Pavlov's statements, even if we cannot prove that he actually endorses it. It is a theory of learning based on reflexes; and it may reasonably be described as a physiological theory. It would account for Pavlov's belief that he has developed a physiological approach to the problem of learning, through the reflexes.

The reflex on which this theory is based is a response which is not found in the normal vertebrate animal. It is found chiefly in the animal which has been subjected to certain operations on its nervous system for purposes of physiological experiment, although it may result from accidental injuries or from disease.

The knee-jerk may again serve as a useful illustration of a reflex. In normal cases, not subjected to specialized training of the reflex, it is elicited only by stimulation of the patellar tendon, below the kneecap. The usual stimulus applied to this tendon is a blow, which can be controlled in force and can be applied to a designated spot on the tendon. From the tendon to the spinal cord extend neurons, which enter a segment of the spinal cord in the lumbar region (which is approximately the "small of the back"). From this same segment issue other neurons which run to the quadriceps muscle of the leg, the muscle to which the patellar tendon is attached. The neurons connecting the cord segment with the patellar tendon [2] carry nerve current from the sensitive endings in the tendon to the cord; they are hence *afferent*. The fibers connecting the cord segment

[2] In describing the knee-jerk we have adopted the view that the essential stimulation is applied to the sensory nerve-endings in the tendon. This view is not in favor at present, the more orthodox theory being that the effect of the blow, mechanically transmitted to the quadriceps muscle, is to excite sensory nerve terminals in that muscle. The pathway of the re-

with the quadriceps muscle carry nerve current from the cord to the muscle; they are accordingly *efferent.*

Within the cord segment, the afferent and the efferent fibers are in contact with other neurons, some of which extend to other parts of the spinal cord, and some of which merely form connecting links, within the segment, between the afferent fibers from the tendon and the efferent fibers to the muscle. These are called *central neurons.* When the patellar tendon is struck, the afferent neurons, which have their sensitive terminals in the tendon, are irritated, and discharge nerve current into the cord. This irritates central neurons, which in turn irritate the efferent neurons which discharge to the quadriceps muscle, and cause it to contract, thus moving the lower leg in the characteristic manner.

This discharge of nerve current, beginning in the tendon, passing through the cord, and ending in the leg muscle, is not the whole story. The afferent neurons irritate also other central neurons, which carry the discharge to other parts of the cord, and there irritate still other neurons, some of which carry the discharge to muscles in various parts of the body; some of which may carry the discharge into the brain, causing further discharges from the brain to the muscular system. The irritation of the efferent neurons, discharging to the quadriceps muscle, is not controlled by the afferent current from the patellar tendon alone. Some of the central neurons, connecting with these efferent neurons but extending to other parts of the cord, may be irritated by neurons transmitting nerve current from sense organs in various parts of the body, and so may influence the efferent discharge to the leg muscle.

flex would accordingly be from the muscle to the spinal cord and from the cord back to the muscle, instead of from tendon to cord and cord to muscle as above described. For purposes of our exposition, we may tentatively adopt either hypothesis, since the conclusions are the same in either case.

There are still further complications. The blow on the tendon does not stimulate merely nerve fibers ending in the tendon. Other nerve endings terminating in the skin, in the tissue between the skin and the tendon and in the tissues of the knee joint are also irritated, and discharge their currents into the spinal cord. The contraction of the quadriceps muscle, moreover, is not the only factor producing the movement of the lower leg. Contraction or relaxation of other muscles in the leg, under the excitation of efferent nerve current supplied to them, may oppose or assist the work of the leg muscle. Clearly, the knee-jerk, in the normal man, and the corresponding leg reflex in a dog or a cat, is a complex process; and the facts that it is modifiable by training, that it is variable and that it is a conscious response are intelligible on this basis.

If we should completely sever the spinal cord both above and below the lumbar segment we have described, and if we should sever all neurons issuing from or entering the segment, with the exception of those entering from the patellar tendon, and those issuing to the muscle, and if these operations should be performed without injuring the excepted neurons or the neurons in the cord segment which connect them, the knee-jerk would still be producible, but would be radically altered in its characteristics. It would become invariable and unconscious, and would not be susceptible to modification by training, although it would still be subject to fatigue. Further, there would be no effects produced by the tendon stimulation anywhere in the body, other than the contraction of the quadriceps muscle, which would move the lower leg.

Under the abnormal conditions described, the knee-jerk might be called a *mechanical reflex*, or *a physiological reflex*. The term "mechanical reflex," therefore, designates a reflex which may occur in an animal surgically injured in certain ways, but is only an abstraction so far as the sound animal is concerned. The complete operation which we have described has apparently never been performed. Various approximations to it have been performed, however, and the results of these operations leave us in no doubt as to the validity of the above description of the completely mechanical reflex.

Other tendon reflexes might be isolated and rendered mechanical by operations corresponding to those we have described. Theoretically, the gustatory-salivary reflex and the olfactory-salivary reflex might be isolated, but the operations are probably impracticable. By an exercise of the imagination, however, every reflex in the animal may be conceived as a response which is theoretically isolated from all other responses, that is, as a mechanical reflex.

If these mechanical reflexes are considered as elements of response, and if it be understood that there are glandular reflexes as well as muscular, every response of which the animal is capable can be conceived as composed of these elements of response. Every muscular and glandular action-pattern, however complex, may accordingly be looked upon as if it were composed of elementary actions in which elementary mechanical reflexes terminate. Every stimulation-pattern, involving stimulation of the visual, auditory, tactual and whatever other sensory receptors, may be looked upon as a composite of stimuli, each of which might arouse one of the mechanical reflexes. Every change in response-pattern which occurs through learning may then be conceived in terms of the substitution of stimuli or substitution of actions.

We may take as an illustration the child's learning to write. At a certain time, a total stimulus-pattern which includes not only the stimulation of his eye-receptors by the paper, pen and desk, but also stimulations from his muscles and joints, from his skin and from his visceral organs produce a response, or a series of responses, terminating in actions of various muscles, among which are conspicuous the actions of the arm and hand muscles which propel the pen in an irregular track. Months later, a stimulation-pattern which may be for the moment considered as identical with the first pattern, will produce a quite different writing movement. Here, we may say, the actions of certain mechanical reflexes have disappeared, and the actions ordinarily produced by other mechanical reflexes have been substituted.

For another illustration, we may take the child's learning to read. At one time, the child will say "dog" when he sees a dog. Later, he will say the same word when he sees its printed representative letter-sign. The action in the two cases may for the moment be considered as the same. There has been, however, a substitution of stimulus elements in the stimulus-pattern.

Neither of the conditions described can be realized. There is no way in which the stimulation-pattern can be made even approximately constant in the first case, nor the action-pattern made approximately constant in the second case. The learning process in both cases has changed both action-pattern and stimulation-pattern. We can allow for this discrepancy, however, admitting that, while as a result of any learning process, there has been substitution in both the stimulus-pattern and the action-pattern, we may abstractly consider the change in each pattern separately. Since we are dealing with abstractions anyway, this procedure would seem permissible. It is simpler, however, to avoid such illustrations, and to deal only with reflexes, in discussing which the discrepancy between statement and fact is more easily ignored.

Although mechanical reflexes do not appear in the adult animal, it might be assumed that they are actually found in some early stage of infancy, or in the embryonic stage of the animal. Such findings would give a definite significance to the mechanical reflex abstractions as applied to the adult animal. It happens, however, that such studies of human infants and of embryonic and young animals as have been made indicate the opposite of this assumption. In early stages of development, animals appear to be more highly integrated than they are in later life. Responses of limited type develop from earlier responses which are more general. "Total responses to total stimulus-patterns" appears to be more nearly the rule in the infant than in the adult. The animal at an early embryonic period develops responses to extensive stimulus-patterns, and these responses involve actions of the total muscular system. From these total responses there are developed those which are more specific in their stimulus-patterns, and more specific in regard to the muscle groups involved, their ultimate development being in the reflexes. The earlier the stage of life, the more conspicuous is the absence of mechanical reflexes.

In more general terms, the mechanical-reflex way of "explaining" learning consists in assuming a definite number of mechanical reflexes, each initiated "primarily" by a specific stimulus and eventuating "primarily" in a specific action. The stimulus-pattern for any actual response is then assumed to be a mosaic of the primary stimuli of certain of these mechanical reflexes; the action-pattern of any response is then assumed to be the mosaic of the primary actions of a certain number of the reflexes. The difference between any two responses is then the enumerative difference between their action-patterns together with the enumerative difference between their stimulation-patterns. Where the difference between the two actual responses is due to learning, these pattern differences, expressed in terms of the hypothetical stimulus elements and action elements, represent the learning.

To the mechanical-reflex theory as a merely logical device, we can see no objection. Since it is expressed in terms which represent actual physiological processes, although these processes occur only in the abnormal animal, we may readily enough agree to call it a physiological scheme. We cannot see in it, however, any vestige of an *explanation* of learning. It merely states, in a way which does not appear useful, the *results* of learning. It avoids, of course, any reference to actual processes occurring in the nervous system, and it is not found useful by physiologists engaged in actual investigation of neural functions. This scheme, vaguely conceived, and confused with principles of actual response, seems to be the basis for Pavlov's theoretical interpretations. If it is not his basis, then his point of view, as judged by available accounts, is even more confused than we suppose. Pavlov does not appear, in the work from his laboratory which is most emphasized, to be engaged in physiological investigation of the nervous system but rather in investigating problems of animal learning of the classic psychological type, in which it is admitted that the brain plays a vital, but mysterious, rôle. Our interpretation, therefore, seems plausible, for the mechanical-reflex scheme can be made a basis for the statement of the results of animal learning where we are not concerned with the detailed functioning of the nervous system. It is not to be assumed, however, that such a method of statement has any scientific utility.

A useful physiological theory of learning may be developed at some time in the future. In the past, attempts at theoretical constructions have been too much dominated by analogies between the results of learning processes, and the results of processes of modification in non-living objects and substances. The "brain-path" theory was obviously shaped by such analogies.

The flow of a stream deepens its channels, and as the channel is deepened, the probability that the stream will continue to flow in the same channel is increased. By analogy, the transit of neural discharges through the brain over a given route should make the selection of that route more probable for the future. Folding a piece of cloth weakens it along the line of the fold, and makes easier thereafter folding it along the same line. Repetition of a response by an animal should, by analogy, make the response easier for the future. Arguments for the "brain-path" theory have depended largely on these and similar analogies.

Varied patterns may be composed in mosaic by appropriate combinations of the same finite number of pieces of colored glass. From a few chemical elements a large number of substances may be produced. By analogy, the varied responses of an animal might be compounded from a finite number of fixed elementary responses. The fixity of the elementary responses turns out to be somewhat mythical, after the mechanical-reflex theory of learning is elaborated, but unless the theory is analyzed with logical rigor, this inconsistency can be ignored.

Reasoning by analogy is always fallacious; and it seems especially futile to attempt to build up a physiological theory by such a process. The modifications which can be impressed on non-living objects give us no direct clue as to the nature of the modifications which are made in an animal through its own activity. There may be a continuity between the living world and the non-living world, but the nature of that continuity has not yet been ascertained. We have not even discovered the nature of the difference between the animate and inanimate. It is to be hoped that future constructions of theories of learning will not depend on analogies for their bases.

CHAPTER V

THE PROCESS IN LEARNING

THE succession of responses of whatever type, through which the learner goes while learning, is conventionally called *practice*. When you are learning the complicated process of playing a certain composition on the piano you practice diligently: you go through certain responses employing the arms, fingers, and feet, at first with visual stimulation from the written score, later without this visual stimulation, but with plenty of stimulation nevertheless from the auditory sense-organ, from arm and leg muscles and joints, and with an indeterminate inclusion of thought responses. Earlier in your musical career, endless practice through finger exercises was involved. In learning to swim, or in learning to recite a stanza of poetry, in learning to do anything whatever, varying amounts of practice are required.

There are cases, it is true, in which something is learned by one response. In being introduced to a person not previously met, you may, if fortunate, "learn" the individual's name so that it is not forgotten for a long period thereafter. Cases such as this are limiting cases, in which the principles we have laid down are fully involved, and offer some theoretical points of interest. But they are far from typical, and we can explain their occurrence only from the further light we derive from the more difficult and complicated cases, in which practice is required. There are certain other cases in which, from a superficial point of view, practice seems not to be involved. When a mechanical puzzle is put before you for the first time, for example, after examining it carefully, you may solve it in one consecutive operation, and may by that operation have "learned" the puzzle so that you can repeat the operation which demonstrates the solution at any time thereafter. Such learning is sometimes called "learning by insight," and might seem to be learning of a sort different from the ordinary type, in which many attempts have to be made before the correct solution is found, and even more different from certain other and apparently simpler forms of learning. The actual relation of "insight" to various other types of learning may perhaps be brought out in this chapter.

Of especial importance in the total field of learning is the apparent distinction between knowledge—learning and skill-learning. By skill-learning we mean the acquisition of response, or system of response, terminating in muscular activities which produce immediate adjustments to the environment, or changes in the environment. This is the acquirement of skill, and that which is acquired is abstractly spoken of as skill, or a skill. Tennis playing, knitting, musical performance, etc., are properly called skills.

Other types of learning, in which the responses learned do not immediately affect the environment, are commonly called the acquisition of knowledge, or knowledge-learning. For example, in learning geography, history, and many school subjects, in acquiring information of various sorts, we are really forming and modifying perceptual and ideational responses and systems of response which ultimately determine adjustments to the environment, or modifications of the environment, but do not produce these effects immediately. In learning a poem, one is actually learning the system of response which will recall the poem. But this recall, if silent, has no immediate effect on the environment. In learning to distinguish varieties of plants and animals of different species and varieties, the perceptions which are learned may not have any discernible effects on the environment immediately.

We must note, however, that the knowledge-response *may* have an immediate environmental effect. If I recite the poem aloud, it affects other persons, causing them to change their activities; it may even produce a graphic record on a phonograph disc. So, if I perceive a plant by speaking its name aloud, or by a response terminating in a hand-and-arm movement, as in picking up the specimen, immediate effects are produced. The point is: the recall or reinstatement of the knowledge-response does not necessarily produce an immediate environmental change, whereas the reinstatement of the skill-response does essentially have a direct environmental effect of a measurable sort.

We must note further that some acquisitions seem difficult to classify since they involve processes which are intermediate between knowledge and skill, or essentially involve both. Acquisitions of this type, perhaps, are made in mathematics as in the extraction of the square root of a number, and the bisection of an angle.[1]

Finally, we find that in many cases skill and knowledge are definitely and usefully related in the learning process, the knowledge acquired having a definite value in the learning of the skill, and the skill acquired in other cases assisting in the acquisition of knowledge.

In the solution of puzzles of the "mechanical" type, which require particular hand-and-finger movements, we have a characteristic type of learning in which a *skill* is acquired, but the skill is based on, and facilitated by, *knowledge*. After solving the puzzle a large number of times, the skill may become almost entirely independent of knowledge. One may be able thus, on again approaching the puzzle, to solve

[1] We must also note that the difference between skill and knowledge does not coincide with the distinction between perception and thought. It is possible that all skills are of the perceptual order, but knowledge is of both the perceptual and ideational.

it without thinking about it. The first learning is often strikingly impressive in that after once performing the proper manipulation, perhaps through many trials and errors, one is able to operate the puzzle in a minimum time and without error. This achievement is really the learning of certain thought-processes, and the learned thought-processes are what we call knowledge.

The learning of knowledge is peculiar in that it involves, in many cases, this "fixing" of the knowledge by a single successful thought-operation. This same peculiarity may be noted in cases which involve no effects of motor processes on external objects; that is, no "skill," but only "pure knowledge." In solving a problem in geometry by mere thinking, without the use of pencil and paper, one success often constitutes learning.

In the case of skill, on the other hand, a lengthy training process is required before the operation which is learned is made independent of or relatively independent of knowledge. In the simpler forms of knowledge-learning, the same need of repeated practice occurs. In "memorizing" a lengthy series of words, or a number of verses of poetry, repetition of practice as measured by the completion of a unit of work (one reading of the series for example) is required. One does not learn the long series in even a few readings, but may require many.

If, however, we measure in terms of "success," then, in memorizing a series, "success" is the ability to recite it perfectly without looking at the printed page, and one successful performance may mark complete learning, as it does in the puzzle solving, or the solving of the geometry problem.

In considering skill, we note that a skill is acquired by many complete faulty performances, leading to an eventual "correct" performance. The rat in learning a maze, the human individual in learning to throw darts, makes many trials, each of which is a complete performance, before he learns the correct one.

We can, however, consider "success" in skill-learning as a correct performance. In that sense, a single correct performance may be the mark of final learning, as in the puzzle solving. One apparent difference between knowledge-learning and skill-learning would, therefore, seem to disappear. The difference is indeed less than it first appears to be, but it still seems to be important. In particular, wherever a sudden and marked improvement occurs in learning, we always suspect that ideational factors are involved; that knowledge which has been acquired is the actual basis of the improvement.

With reference to the stimulus-pattern we may distinguish two sorts of practice. *First*, that in which the sensory stimulus-pattern is repeated with no significant modifications [2] for the successive responses. In throwing darts at a target, for example, we may in each throw have the visual stimulation from the target, and from objects in the room generally, constant. We may also control the room so that the lighting is constant and the sound-pattern from inside and outside is constant. The hand, arm, feet and trunk may be each time in the same positions. Absolute control of these factors is of course impossible, but we may approximate to a control standard in such a way that the variations are minimal. In certain psychological experiments on learning, a high approximation to stimulation control, in the attempt to keep the stimulus-pattern as nearly constant as possible, is an essential part of the experiment.

Second, we may have the stimulus-pattern significantly different in the successive responses. Such a condition would obtain in a dart-throwing experiment where the noise

[2] The determination whether any modification is significant or not is dependent primarily upon the fineness of measurement which is employed, and the purpose for which determinations are made.

of traffic just outside the open window varies greatly, where persons are moving about in the room, and where the lights in the room vary in intensity, and even in direction of flux. Such variation in conditions may be unavoidable in certain experiments and may be deliberately permitted in others. In certain other cases, variations in the stimulus-pattern may be purposely brought about. In learning for practical uses, it is often of the highest importance that there shall be large variations in the stimulation-pattern during practice. It is true, the non-variation of pattern is a limiting condition of variation, and the limit is seldom attained. For an adequate consideration of the facts, however, it is important to distinguish practice where the stimulation conditions vary considerably from practice in which the variations are of such small order that they produce no measurable difference in the responses.

If we consider the action-pattern, we note that again there are different sorts of practice possible. The action-pattern may in some cases be practically the same time after time. In learning verse by the laboratory method, one reads the assignment of verse over time after time in a prescribed and controlled way. The same action-pattern is repeated with no significant modification. In learning to throw darts, on the other hand, a great variety of action-patterns are involved in the successive responses, and the same is true of the rat's performances while learning the maze.

All this is merely a more detailed exposition of the principle we have earlier laid down, namely, that in practice, or the process of learning, the same response may be repeated with increasing probability of its recurrence; or a succession of responses may occur, each making its recurrence less probable. When we consider, however, that in the true sense, two responses are "the same" only when identical stimulus-patterns lead, through identical transit-patterns (that is, through identical pathways in the nervous system) to identical action-patterns, we begin to wonder if there really are any cases in which learning occurs through repetition of the same response, or whether the repetition of response, as required by the old "brain-path" hypothesis, is not merely an ingenious fiction. For the present, however, we shall continue to admit this as a possible alternative, although, as we have already shown, it is at most a minor feature in our actual learning.

In earlier publications, the author suggested three alternative theories of learning, which were designated the *alpha* hypothesis, the *beta* hypothesis, and the *gamma* hypothesis respectively. The *alpha* hypothesis is the one implied in the old "brain-path" theory, namely, that the occurrence of a response increases the probability that when the same stimulus-pattern[8] occurs the same total response will again occur.

The *beta* hypothesis proposed is the reverse of the *alpha*, namely, the hypothesis that the occurrence of a response lessens the probability that on the recurrence of the same stimulus-pattern, the same response will recur.

The *gamma* hypothesis is that the occurrence of a response in itself has no effect on the probability of the recurrence of the response.

In terms of these hypotheses, it is evident that in the preceding discussions we have been implying that for some learning the *alpha* hypothesis holds, and for some learning the *beta* hypothesis holds. We might go further and insist that very probably there are cases in which the *gamma* hypothesis holds.

When considered more narrowly, as we must now do, it appears that for almost all learning, the *alpha* hypothesis

[8] We need not assume the improbable absolute identity of two patterns. In any consideration, identity is merely the absence of differences not included in our scale of measurements. The same consideration applies to total responses.

fails, unless we construe the identity of stimulus-patterns and the identity of action-patterns in very special ways.

The response in any case of practise, such as throwing a dart, includes not merely the part of the action-pattern and antecedent transit-pattern which propels the dart in a given direction with a given force. The response includes also the transit and action details (if any) which are involved in the perception of the target, the perception of the dart, and of the hand and arm, the thought of what is to be done, and what has been done in preceding throws, the desire to throw effectively and affective experiences of various sorts. The actual response in the practice includes all of these.

If, however, we abstractly separate this response into two parts: *W*, the parts which actually propel the dart and *Z*, all the rest of the response, and consider only the *W* part of the response, it is possible to say that neither the *alpha* nor the *beta* hypothesis holds; the repetition of the response, considered as *W*, has no effect on the probability of recurrence of the same "response." The effective factor in determining the probability is the *Z* part of the total response.

This way of viewing the response was satisfactory for the sharp raising of the problem, and for the attack on the traditional view, which, as a matter of fact, depended for its illusory plausibility on the separation of the actual response into *W* and *Z*. It is now time, however, to place the problem on a sound basis. The separation of the response into *W* and *Z* is not possible, even as a logical device. The contraction of the muscles which propel the dart (to continue with that illustration) is not the result of a particular part of the transit-pattern, but the result of the total transit-pattern. The same consideration holds for any practice situation. The thought, desire, perception and affective factors which determine the effects of a response do not "accompany" the response; *they are integral parts of it.*

Strictly then, although the *beta* hypothesis applies to a great many responses in practice, the *alpha* hypothesis and the *gamma* hypothesis may actually be valid for certain other responses. A certain total response may (for all we know to the contrary) have no effect on the probability of the recurrence of the same total response. A certain other total response, involving perhaps the same hand-and-arm action, as in dart throwing, but in which these actions are perceived in a different way, through the coöperation of different perceptual, conative or ideational factors, may render the recurrence of that particular total response more probable. In an important number of cases, however, the effects of practice are to change the response, and the real importance of the practice, the utility of the learning, is in that modification. We shall now proceed to show this more definitely.

That the "purpose" in acquiring skill is to modify the response progressively should be clear from our earlier exposition. We need now to consider that although in some learning we progressively modify a response to a repeated stimulus-pattern, in other learning the modification is of a different character. In certain cases we are learning through the modification of response to a changing stimulus-pattern. Further, in practicing with a constant stimulus-pattern, we are learning to make a prescribed response to a different stimulus-pattern.

The last condition may be made clear by reference to our old laboratory stand-by, the dart-throwing experiment. Suppose we could give the learner several days of practice under stimulus conditions which are practically constant, which we will call *A;* he then progressively modifies his responses to greater and greater efficiency. But is he merely learning to respond to the stimulus-pattern used in practice? He is not. If we switch now to a different weight of dart, with a different-sized target, and change the lighting conditions and other extraneous stimulation, installing thereby a new stimulus-pattern, *B*, we will find that if he had antecedently little efficiency under the altered conditions, he has learned to throw better under these conditions than he could before. If, on the other hand, he had already acquired high skill for condition *B*, the practice under condition *A* may actually have reduced his skill for condition *B*.

A similar situation exists for billiards and pool. A beginner at billiards will profit by practice at pool, but after he has attained môre proficiency in billiards, practice in pool will impair his skill in billiards.

The other point is that practice toward a certain end of motor performance often must proceed with a variety of stimulus-patterns. This is at once evident when we consider learning the proper operation of brake, clutch and gear shift in driving. In starting a given car, adequately warmed up, on a level, the essential muscular activity should be the same in one case as in another, if there are no other cars or pedestrians to be considered. Yet the stimulation conditions in a garage, at various street corners and on country roads are almost infinitely varied. While we may do initial practice under simple standardized conditions, the bulk of actual learning must be under the varying conditions indicated.

It will be noted that a certain part of the total stimulus-pattern under the varying conditions cited will be approximately the same—this part is that furnished by the car itself. Here we note a widely prevalent condition of learning. We practice reacting to a total stimulus-pattern (*ST*), in which there is a limited sub-pattern (*Sa*), which is constant, while the remainder of the pattern (*Sr*) varies. This type of practice is not merely an interesting laboratory stunt; it is the type which is imposed upon us in much of actual life.

Reverting to the automobile illustration, we note that the action which is learned is not ever a fixed act, but is highly variable. The actual work on brake, clutch, gear shift and accelerator in starting the car varies in an exceedingly complex way in accordance with the cold or warm condition of the motor, the slope of the road, the position and movements of other cars and pedestrians, and the need of proceeding straight or turning a corner. It is not even a matter of learning a finite number of different total responses, for situations which have not previously encountered are constantly occurring, and the driver who has not learned to meet these as yet unusual situations is unfortunate. Through practice on previous problems the driver has learned to respond to other situations, but these other situations are endlessly variable.

The variability of the responses which are the goals of learning has still other dimensions which are of especial interest for the understanding of the process of learning. Even where the action which is learned is capable of description as a fairly fixed or invariable one, it is in many cases only a part (Ma) of a total action-pattern (MT), the remainder of which (Mr) is widely variable.

Many learning situations, accordingly, require practice which may be represented as follows:

$$Sa + Sr \quad <N> \quad Ma + Mr$$

Where Sa is reasonably constant and Sr indefinitely variable, Ma in the ultimate response is a definite variable represented in practice by a series of approximation $Ma_1\ Ma_2\ Ma_3$, etc.; and Mr is widely variable both during practice and subsequently. N is the neural transit-pattern, *i.e.*, the complicated pathway in the nervous system connecting the stimulus-pattern with the action-pattern, and varies with every variation in ST and MT.

After inspecting the complication of the learning process, we can readily understand why, in the present elementary state of our knowledge of the function of the nervous system, it is futile to attempt to continue a "physiological" theory of learning. We can still more readily see that the "physiological" theories which have been continued are mere chess games, in which the pieces represent nothing real, but are given arbitrary values which determine a fictitious scheme arbitrarily defined as "learning."

When we consider still more complicated learning processes, our distrust of the "simple" explanation of learning is still greater. Suppose, for example, we consider the synthesis of two already learned responses into a third, equally unitary, but different response. For illustration we may use the associative learning which Pavlov calls "conditioning a reflex." Let us suppose that a dog has already learned to secrete saliva on the visual presentation of food. Let us suppose that the dog has already learned to perceive sound, that is, to make a definite response to sound. This may be the cocking of his ear, tensing of his leg muscles, or what not.

It is of course not to be assumed that the acts mentioned (secreting saliva, cocking the ears) are the total action-patterns in either case. When you present food to the dog, he secretes saliva (Ma_1), but there is other action (Mr_1), which completes the total action-pattern (MT_1). When you strike the bell (if that is the auditory "stimulus" employed), the cocking of the ear, which we may call Ma_2 is a part of the total response. We may call the remainder Mr_2 and the total response MT_2. The total stimulus pattern (ST_1) in the first response includes the food (Sa_1) and much more (Sr_1). Similarly, the total stimulus (ST_2) in the second response includes the sound (Sa_2) and other stimuli (Sr_2).

Our procedure now is to combine the two partial patterns (Sa_1 and Sa_2) in a third pattern (ST_3), which is obviously analyzable into $Sa_1 + Sa_2 + Sr_3$. Presumably, the consequent action-pattern (MT_1) is $Ma_2 + Ma_3 + Mr_3$. We say "presumably" because no one seems to have been able to determine whether Ma_1 and Ma_2 are or are not essentially changed through their being evoked together.

If we repeat this procedure for a sufficient number of times, we obtain results which can be certified, together probably, with other results to which no one seems to have paid any attention. The total result is revealed when we revert to the presentation of a stimulus-pattern including the sound, but not the food. This is the pattern $ST_4 = Sa_4 + Sr_4$. The action-pattern consequent upon this stimulation is $MT_4 = Ma_4 + Mr_4$. That is, the dog, upon sound stimulation, secretes saliva; what else he does is not recorded. Perhaps Mr_4 includes Mr_2. This is just as important a result as any other, but the experimenters do not happen to be interested in it.

The formulation as given above is not complete, but we may recapitulate as follows:

$$\text{(I)}\quad ST_1 = Sa_1 + Sr_1 \quad \langle N_1 \rangle \quad MT_1 = Ma_1 + Mr_1$$

$$\text{(II)}\quad ST_2 = Sa_2 + Sr_2 \quad \langle N_2 \rangle \quad MT_2 = Ma_2 + Mr_2$$

$$\text{(III)}\quad ST_3 = Sa_1 + Sa_2 + Sr_3 \quad \langle N_3 \rangle \quad MT_3 = Ma_1 + Ma_2 + Mr_3$$

$$\text{(IV)}\quad ST_4 = Sa_2 + Sr_4 \quad \langle N_4 \rangle \quad MT_4 = Ma_1 + Mr_4$$

The incompleteness of the formulation is in the fact that Ma_1 in Response IV is not the same as Ma_1 in Response I and may not be the same as Ma_1 in Response III. We could, of course, represent this by using different symbols in the three cases. We have not thought it advisable to make the formulation excessively complicated. Mr_4, moreover, is very probably different from either Mr_1 or Mr_3. This is represented in the formulation, along with the fact that the neural (N) pattern is different in each of the four responses, which is evident not only from the differences in the action-pattern resulting from the transits, but also from the differences in the stimulation-patterns. In other words, we are practicing the dog in one response, in order to produce a quite different response. Obviously, a very complex learning process is involved, and it looks simple to the superficial observer of the responses merely because he ignores the complexity of the actual response and notes only certain simple results in which he happens to be interested.

It appears now, that the "conditioning" of reflexes is among the most complicated of learning processes. Far from furnishing an explanation of the wide range of learning processes, it can only be explained after we have obtained much more extensive information on various sorts of learning than has so far been collected. It is probable that information concerning the simpler types of learning (for there are learning processes in the human being far simpler than the "conditioning" of the dog's salivary "reflex"), will be particularly necessary and applicable in explaining these more complex learning processes.

Dogs are not as efficient[4] learners as are human beings, especially in some of the more complex types of learning. It has been found important in "conditioning" the dog's salivary response, to keep Sr_1, Sr_2, Sr_3 and Sr_4 as nearly equivalent as possible. In other words, the dog is trained by responding in practice to stimulus-patterns in which only one restricted subpattern is varied, the remainder of the pattern being kept approximately constant. If Sr_1, Sr_2, Sr_3, Sr_4 are made different, and especially if Sr_4 varies during the successive practice period, the "conditioning" is seriously interfered with, and may be prevented.

The human being will "condition" his salivary response efficiently under far more variable conditions of Sr_3 and Sr_4, and the response learned will be produced with a still

[4] "Efficient" here, as elsewhere, means: approximating to arbitrary standards, which in this case are standards set for the dog by the experimenters. "Efficiency" in approximating to standards set by the dog would be another matter.

different Sr_s. In any case, however, variations can be found which, when introduced into the *r*-factor of the stimulus, will seriously interfere with the learning, or with the response after learning.

With *Mr* and *Sr* irregularly variable, and *Ma* varying as *Sa* varies, the formula:

$$ST = Sa + Sr \quad <N> \quad MT = Ma + Mr$$

is applicable to many types of learning. In fact, our most important practical learning is of this type. The botanist, for example, in "recognizing" or identifying a plant is making use of previous learning of that sort. The plants previously studied and the plant being identified present varying stimulus-patterns (*ST*) in each of which a certain feature (*Sa*) is relatively constant. The identification is a response terminating in an action (*MT*) in which a certain feature (*Ma*) agrees with a feature of previous responses, but a part of the action-pattern (*Mr*) is again a variable, which may be highly important for other purposes, but not for the identification of the plant. The botanist is justified in ignoring *Mr* for his particular purpose because he is using the learning; he is not studying learning.

Another application of the formula may be made to the type of learning which is essential in literal translation from one language to another. I see, for example, the word *oeil*, but I say (or write) *eye*. If, instead of perceiving through the complete speech response, I perceive through some response not terminating in oral or graphic action, the conditions are essentially the same. I make to the stimulus *oeil* the perceptual response I formerly made to *eye*. This is what we commonly call *paired association*. Obviously, I first learned to perceive "eye" properly as an *Sa* combined with a variable *Sr*. I do not, however, have to learn to perceive *oeil* antecedent to my learning to perceive it as *eye*. In some cases, students do proceed as in the more complex case of "conditioning," but this is not necessary. The formulation therefore is:

$$\text{(I)}\quad ST_1 = Sa_1 + Sr_1 \quad <N_1> \quad MT_1 = Ma_1 + Mr_1$$
$$\text{(II)}\quad ST_2 = Sa_1 + Sa_2 + Sr_2 \quad <N_2> \quad MT_2 = Ma_1 + Mr_2$$
$$\text{(III)}\quad ST_3 = Sa_2 + Sr_3 \quad <N_3> \quad MT_3 = Ma_1 + Mr_3$$

Where Sa_1 is the printed word *eye*, Ma_1 is the spoken word *eye*, and Sa_2 is the printed word *oeil*. Sr_1, Sr_2, Sr_3 include, of course, other printed words. The procedure involves not the "substitution" of stimulus components or "substitution" of action components, but the simpler process of the addition of one stimulus component for practice followed by the dropping of another stimulus component if the practice has been sufficient.

The learning process is a vastly varied and unusually complicated one. We should be prepared to appreciate the difficulty in studying it, and should be neither astonished nor discouraged by the relatively small headway which has been made in experimental analysis of the total problem. In absolute terms, great progress has been made since the experimental study of learning was commenced in 1880. The magnitude of the problem, however, is so great that the really substantial achievements seem small in comparison.

The learning of the English equivalent of the French word may be a poor illustration of the simpler type of learning, since it may be objected that one usually has formed, or forms, a response to the French word which is different from the one to be learned. In other words, one "pronounces" (incorrectly perhaps) the French word.

Let us turn therefore to a form of laboratory procedure which has been devised to avoid just that complication. We present to the learner a card bearing a simple pictorial design, such as the outline of an irregular polygon which it would be difficult to name, together with a printed word, which may be *eye*. The card bearing these two stimuli is presented for a fraction of a second, several times in succession if necessary, and then a card is presented bearing only the design without the word. The learner, however, can respond to the stimulus by speaking the word *eye*, just as if it were printed on the card.

What has happened? The learner had, of course, learned to speak the word *eye* as a response to the printed word. Adding the device had no effect (apparently) on the action-pattern. The word *eye* is spoken, as if the device were not present. In orthodox (but inaccurate) phraseology, the same response is made to the new total pattern as was formerly made to a part of it. Then we reduce the stimulus-pattern by removing the word, but now the converse result is found. The learner's action-pattern in the response is the same to a part of the original total pattern as was previously made to the total pattern.

This is the characteristic form of associative learning which has long been familiar to psychologists. Superficially it would seem that:

1. Adding a certain new detail to the stimulus-pattern does not change the response.

2. Subtracting another stimulus detail does not change the response. This is the way in which the process was described by the older psychology.

In terms of our modern principles of learning, starting from one response already formed (Response I), we utilize a different response (Response II), obtained by adding to the first stimulus-pattern; and by performing the response we increase the probability that a still different response (Response III) will occur.

This type of learning is simpler than the "conditioned reflex" in that only one previously learned response is involved. It is simpler than many other types of learning in still another way, namely, that modification of the practice response is not essentially involved. Of course, if in successive practice performance of Response II, *Sr* changes, the response changes; but these changes are not essential to the learning, although they may inhibit it in some cases and aid in others. In the acquisition of skills, on the other hand, the progressive changes in the practice response are the essential features of the learning.

The neural transit-pattern is an essential part of a response, and by the general hypothesis which puts the responsibility for learning on the brain, the transit-pattern is the part of the response directly involved in the learning. We know that a stimulus-pattern cannot be changed without changing the transit-pattern. Obviously, therefore, when the response

$$Sa_1 + Sr_1 \quad < N_1 > \quad Ma_1 + Mr_1$$

is changed to

$$Sa_1 + Sr_1 + Sr_2 \quad < N_2 > \quad Ma_1 + Mr_2$$

we have essentially changed the neural pattern of the response. N_1 and N_2 cannot possibly be the same. Also, when we next change the situation to

$$Sa_3 + Sr_3 \quad < N_3 > \quad Ma_1 + Mr_3$$

we have again changed the response in its most essential feature. This is entirely aside from the question as to the variation in *Mr*.

In another form of laboratory learning, we have the conditions which are set up in the "conditioning" of a "reflex." We use two words, or two objects, which are already stimuli to two different responses. For example, we present on a card

ROSE AIM

We add another stimulus factor, corresponding to the hunger in the dog, by verbal instructions emphasizing the response to *aim*, rather than the response to *rose*. After a certain number of presentations, we present a card on which is the single word:

ROSE

whereupon the learner will promptly respond by saying *aim*.

This procedure is closely akin to that of the "conditioning reflex." We do not call it "conditioning," since we have long had the established term "paired association," and see no reason to substitute therefore another term, especially as the newly minted one has confusing implications.

A still different type of learning process appears when the objective is merely the synthesis in a single response of features of several other responses. We have this objective before us for example in learning to pronounce (or to use without pronouncing) the word *axolotl* when we first meet it. We have, however, already learned words involving *ax;* other words involving *olo*, and still others involving *tl.* We do not need to analyze these compounds, or to recall the familiar words. Through a few practice responses, the past results of our previous learning "fall together," and shortly we can say the word. A distinct and important learning process (probably the process which is sometimes called "insight") is involved here; but it is different from "conditioning" and from the other forms of paired association. It is, apparently, simpler than even the simpler form of paired associations. It may be, indeed, the simplest form of learning.[5]

Another type of learning, different from the formation of a single response, is illustrated by serial learning. In its

[5] This formation of a response can be combined with the associating of it with a symbol in one learning task. It certainly is the primary or fundamental form of learning, for it is that which the individual must utilize in infancy and early childhood, and upon the results of which all further learning is based.

simplest form, the objective is to form a serial habit out of a number of responses already learned. We present to the learner, in the laboratory procedure, for example, the following words:

MUSK	FLAT	HELP	WISH
HAND	JUMP	KELP	RISK

The learner has already learned these words as words. His task is to learn them as a series, so that eventually they can be recalled in the proper order.

In other experiments, nonsense syllables (one syllable paralogs) are used, for example:

NAF KEX RUX PUM RIN FAB, etc.

Here the learning is more complex since the paralogs are not already learned, but required to be learned as well as serially associated. As might be expected, the learning is more difficult, that is, by the repetition criterion, it requires more readings of the series.

The present author, in 1914, printed a diagram which presumed to represent the response processes involved in serial learning. In this diagram the stimulus for each word, during the reading, was shown as the printed word together with the kinesthetic stimulation resulting from the saying of the preceding word. This may be transformed into the scheme earlier used in this chapter as follows:

$$Sa < N_1 > Ma \rightarrow Sk_1 + Sb < N_2 > Mb \rightarrow Sk_2 + Sc < N_3 > Mc \rightarrow Sk_3 + Sd$$

etc., where the *Sr*'s are omitted for brevity; *Sa*, *Sb*, *Sc*, etc., represent the successive printed word-stimuli of the series; and Sk_1, Sk_2, Sk_3, etc., represent the kinesthetic stimuli in the muscles resulting from the successive action-patterns of saying the words. In recalling the series, after practice, the printed word-stimuli, except the first, are omitted, and the series is composed of:

$$Sa <N_1> Ma \rightarrow Sk_1 <N_2> Mb \rightarrow Sk_2 <\mathrm{N}_3> Mc \rightarrow Sk_3, \text{etc.}$$

The procedure in each of the successive responses would then be comparable to the modifying of single response by the simpler of the two paired association methods.

This explanation, with modifications of the author's original diagram, has been adopted by many other psychologists; but the author is at present somewhat in doubt as to its sufficiency. The process in serial learning is actually much more complex than the scheme suggests, and its complexities are not yet adequately analyzed.

In the foregoing exposition, the "learning" of a word has been employed as an illustration, as if in the use of words they were always spoken. The author was perhaps responsible for the introduction into psychology of the conception of the perception and thinking of a word as a response, whether spoken or not. One extension of this point of view, to which the author has never subscribed, has interpreted it to mean that words, if perceived or thought, are actually always spoken, not necessarily audibly, but at least by minimal movement of the vocal organs which has been called "implicit speech." Now speech, covert or overt, is really not required by adults in these cases, although it may be necessary for young children in certain stages of their development. In adults the speech response proper is largely replaced by another type of response, which, however, functions in exactly the same way in which speech does, so that the illustration we have used may still be valid. Not all perceptual responses are language response, of course; and these other responses, which in children, and often in adults, involve action-patterns of the arms, legs, and other muscular units, are replaced largely in adult life in the way in which speech is replaced. This substitution of response obviously is important from the learning problem, but takes us too far from our main discussion to permit us to enter upon it here.

We have considered several types of learning:

1. The formation of a response, as in learning to say "axolotl," and in the still more basic formation of response where there are no antecedent materials, as in the formation of vocables by the child.

2. The formation of a response which includes the essential actions of a variety of practice responses but only a selection from their stimulus-patterns.

3. The modification of a definite response, already learned, so that a part of the stimulus-pattern produces a response with the essential action-pattern.

4. The combination of two responses previously learned into a third (including the "conditioned reflex" as a particular case).

5. The serial connection of response.

6. Serial connection while the responses to be connected are being learned.

These are the fundamental forms of learning of maximal importance in practical life, in the order of their probable importance. They are complicated in human life by thought processes of various sorts which assist the learning.

There are still more complex forms including: (a) Trial and error learning. (b) Analytical learning and (c) Synoptic learning. In these forms, specific complex responses and series of complex responses are learned, but the important feature is the domination of the whole process by the idea of a goal.

The rat, in learning the maze, apparently utilizes all of the forms from 1 to 5. But these are means to an end, which end is the utilizing of various means of attaining the food. He may, after having learned to attain the goal by a succession of responses of a certain sort, be able thereafter to attain it by a radically different set of responses.

In learning the method of operation of a novel piece of machinery, we analyze the relation and operation of the different parts, utilizing the various modifications of response above described, but that which is learned is something beyond the responses employed in learning. After having learned the structure and operation of the machine, one is able to take the machine to pieces and put it together again, to change its operation, and in various ways apply a vast amount of previously learned materials to the situation. While learning proceeds as the results of previous learning, the results of learning the new make available at once a vast amount of the old.

Synoptic learning, that is, learning the "sense" or "substance" of a situation or a text, involves the making of multiple responses, in order that a certain result thereof may be later reached by simpler processes. For example: that which is expressed in a long series of printed words in a text is learned, by study, in such a way that it may be summarized in a shorter wording, or may be reproduced in a number of alternative wordings.

These forms of learning, complex as they are in many cases, are sharp illustrations of the principle that by practice in one set of responses we may learn quite different responses. There is no conflict between this principle of learning and the fact that, through practice of certain responses, we learn those responses themselves, that is, make them habitual.

We are now ready to give final consideration to the cases of learning and unlearning to which the *alpha* and the *beta* hypotheses were first applied. These are the cases in which the learning appears merely to fix the practice response, in a way conforming to the traditional description of habit formation; and the cases in which, by repetition of a response which has already become habitual, the response is abolished. For convenience, we describe these procedures as *positive practice* and *negative practice* respectively.

The relation of positive practice to its results becomes intelligible when we represent the responses by the formula $ST = Sa + Sr < N > MT = Ma + Mr$. If Sr and Mr are irregularly variable throughout the practice and the subsequent performances of the response, or if both are constant throughout, we have habit formation corresponding to the *alpha* hypothesis. If, however, Sa and Sr contain features which are constant throughout the practice, but are absent after learning has reached its goal, the learning conforms to the *beta* hypothesis. Such a condition may obtain where Sa—Sr during learning includes purposes, desires and judgments which are effective factors in the learning, while performances subsequent to learning occur without the involvement of these thought factors. Quite clearly, we are here utilizing in practice a response of one sort, in order to be able to make subsequently a response of another sort. Quite clearly, also, the *alpha* type of learning occurs only in the cases where the learning is really never finished, but the habit exists only as a continuous learning process.

Negative practice has long been utilized as an unlearning procedure but its importance and psychological significance was first appreciated through recognition of the failure of the *alpha* hypothesis as applied to many learning processes, and was demonstrated in application to practical problems of memory, which are discussed in a later chapter, and to the elimination of persistent errors in typing.

The non-professional typist and the learner frequently make persistent errors, such as the transposition of *the* into *hte*, and these errors are ordinarily eliminated with difficulty. It has been found, however, that even a small amount of practice in writing the word in the wrong way will eliminate the error. Similar negative practice has been found effective by many persons who make persistent errors in playing a piano composition, although the error eliminated under one condition, such as in private practice, may reappear in other conditions in which it has become habitual, such as playing before an audience. A specific disposition of negative practice to meet the particular difficulty is then required.

In negative practice the determining factors are the thoughts and desires involved in the practice, which would be represented in our formulation as the *Sr—Mr* factors. Obviously, learning or unlearning through negative practice are merely striking examples under the general rule that the responses in practice are not the responses which are learned.

CHAPTER VI

THE CONDITIONS OF EFFICIENT LEARNING

EXPERIMENTAL work on human and animal learning has been actively pushed in psychological laboratories since 1900. A variety of problems have been attacked, and although none of the fundamental problems can be said to have been solved, great progress has been made toward the solution of many problems, and the partial solutions have not only given additional stimulation to the work, but also have contributed much to practical educational work, and to our understanding of the learning process. Some of the work has upset theories of slight foundation, and has thereby cleared the way for more fertile investigation. It must not be forgotten that the proof that a problem, which initially appears simple, is in reality exceedingly complex is a distinct advance in knowledge, and an essential step to the solution of the problem. The more important problems which have been attacked are:

1. *The range of learning ability* in the animal kingdom. Here it has been shown that not only the higher vertebrates, but more lowly animals, such as the snail and the angle-worm, and even the protozoa, or single-cell animals, such as the amœba, can learn. In fact, there is some indication that plants, of the types which respond to contact or to light by opening and shutting their blossoms or their leaves, can learn. We may for the present be sceptical regarding these indications, but it is possible that further research may demonstrate a considerable extension of learning and habit formation in the vegetable kingdom.

2. *The importance of different types of stimulus-patterns*

in learning has been much investigated, especially with animals. Learning to respond in definite ways to auditory, visual and other sensory cues has been studied in a variety of the vertebrates. The sensory stimulation employed has been chiefly visual, and a great part of the work has been done in rats because of the convenience and relatively small expense of working with these rodents.

3. *Discriminative learning*, in the attempts to establish thresholds of discrimination, has been widely investigated in animals. A rat, for example, is trained to select a food box marked with one visual pattern (such as a triangle, point down) and to avoid another pattern (such as a triangle, point up). Or, a rat is trained to go from a brightly lighted compartment into a dimly lighted compartment as soon as the brightness in the one or the other compartment is changed (the two being initially equally lighted). This habit having been established, the brightness differential between the two compartments may be progressively reduced until the habit breaks down.

The variety of differences to which the animals are trained to respond is great. Through these experiments information is gained as to sensory perception and discrimination of various animals: color and light perception and discrimination, pitch discrimination, etc., etc.

4. *The effects of amount of investment* involved in learning. With human individuals and animals, in general, the amount to be learned increases the required investment of time, if other factors are approximately constant. A student will spend more time in learning a stanza of sixteen lines than in learning a stanza of four lines of the same grade of poetry. A rat will take a longer time in learning to run a maze with eight blind alleys than one with only four, and so on. But the time apparently does not increase in proportion to the material to be learned.

A single reading of the sixteen-line stanza requires four times as much time as a single reading of the four-line stanza, since there are four times as many lines read. If the ratio of time to material were constant, we should, therefore, expect that it would require the same number of readings to learn the one stanza as required for the other. This is not the case. More than twice as many readings (that is, eight times the time) may be required for the learning of the sixteen-line stanza as are required for the four-line stanzas of a certain type of poetry.

Experimental work has shown that the ratio of time increment to material increment depends on a number of factors, including (a) the absolute amount of material; (b) the method of learning; (c) the type of material; and (d) the particular individual who is learning.

(a) The learning of eight lines of verse may require no more than twice the time (*i.e.*, the same number of readings) as for learning four; but if the number of lines is increased to sixty-four, the time required may be far more than eight times that required for eight.

(b) The method of learning is highly important for the results, and much work has been done on human and animal learning in determining the efficiency of different methods. The adequate utilization of desire which was discussed in the preceding chapters is a problem in method which is especially pertinent. Many other details in method are, however, important, of which the following are illustrative:

The distribution of work determines the labor of learning. If a certain task (for example, the learning of a stanza, a word series or a nonsense syllable series) is set, this may be learned in one sitting, the learner reading and re-reading the stanza continuously until it is "learned." Or the learning may be divided into short sittings. In an illustrative case, the learning in one sitting may require thirty repetitions. If, however, the work is broken up into daily periods, in each of which five readings are made, the total number of readings is materially reduced. A rat in learning a maze requires fewer trials and a shorter total time spent in learning, if given one trial a day than if given two trials a day, and fewer with two a day than with three a day.

In certain cases and within certain limits, the wider the distribution of the work, the greater the efficiency of the learning. From this, unfortunately, hasty generalizations have been drawn, implying that this relation of efficiency to distribution is a general rule, which it is not. The actual efficiency of different temporal distributions depends on the total amount to be learned, the type of material, the method of learning, the particular individual and what he does in the intervening period. For small amounts of material, the one-sitting method is most efficient. For one type of material or one method, a certain distribution is optimal, and further scattering and limiting of the work periods is less efficient. For another type of material or another method, another distribution is optimal. Two human learners, or a human learner, and an animal learner, may require quite different distributions for optimal results. The prescription for individuals of given ages, with given type of material, given method of study, and given duties of other sorts must be worked out separately for each case. The optimal distribution for rats learning a certain maze cannot be adopted for children learning arithmetic. The optimal distribution for arithmetic may be different from that for geography.

In particular, the relation of the work done in one period to the work done in other periods is especially important. If the successive periods are occupied by repetition of the same learning process (as in learning a set of verses, where the whole set is read at least once in each work-period), so that the latter periods are essentially repetitions of the former, we have one condition, for which the optimal distribution may be determined. If, on the other hand, it is a question of longer or shorter geography lessons, correspondingly spaced, the situation is entirely different. The successive study periods are occupied by new materials, not by further study of the materials studied in the preceding periods. The conditions are essentially different from those in the verse-learning, and results of the experiments under the one condition cannot be directly applied to the work under the other conditions. Unfortunately, most of the "principles of learning" formulated for school work, and supposedly based on psychological experiments, entirely overlook these essential conditions, and although they may accidentally be correct, they have no scientific basis.

(c) Increasing the stanzas of verse to be learned from eight to sixteen lines may increase the time required in a certain ratio; but if a series of words not making sense requires the same amount of time in learning as does the eight-line stanza, doubling the number of words in the word series will not give the same time increment as doubling the number of lines in the stanza.

(d) Two human individuals may require different amounts of time to learn the same material by the same method. Increasing the material in a certain ratio for both learners will not, in general, increase the time required in the same ratio for the one learner as for the other. If a person's learning is to be compared with that of an animal, an even greater discrepancy is to be expected.

5. *The emotional and affective factors* in learning are admittedly important, but so far their investigation has been largely restricted to the preliminary study of simple types of motivation and to the particular questions of the effects of reward and punishment, with some speculation upon the pleasant and unpleasant feelings evoked. Food, appetite, thirst, sex-urge, the urge to escape from confinement and social drives and impulses have been investigated as features of motivation. The studies have been confined

almost exclusively to animals, and the investigations are still in their initial stages. These motives are certified as adequate to initiate and maintain the learning process, but no one motive is essential.

The effectiveness of punishment and reward in learning are matters of common knowledge as regards both animals and human learners. Reward, by food, or other means, is widely employed as means of motivation, but the detailed function of reward in the promotion of learning is still a matter of dispute. (I) Although the affective satisfaction of reward always occurs subsequent to practice, it may actually increase the probability that response which previously led to satisfaction will occur in the next practice period. The rat, for example, which has eaten in the food box which he enters as the final step in his running the maze, may be assumed to experience pleasant feelings while eating, and as the result of the eating. Now, these "pleasant effects" are, from the physiological point of view, chemical changes in the tissues which may affect the nervous system. It is necessary to assume, also, that the running of the maze leaves neural effects which are important for further practice work; otherwise there would be no progressive learning, no habit formation. That these latter neural conditions may be directly affected by the neural effects of the "pleasure" is conceivable. On the other hand, we have no evidence that "pleasure" actually operates in this way.

(II) The effect of eating may be exercised through the motivation which is essential to the learning process, not merely preserving it for the later practice periods, but even increasing it. The fact that the rat, in completing previous trials in the maze, has experienced the food satisfaction probably does not increase the food appetite on succeeding trials. This particular example of motivation was based on food appetite, but it is not simply food appetite. It is appetite directed to a specific goal. The rat does not continue merely to be hungry. He must have the desire directed toward possible satisfaction at the end of a definite type of activity; otherwise, his maze-running would remain throughout on the chance level of his first run, and there would be no improvement through practice. The effects of the satisfaction on the motivation of later practice periods are conjectural, but we cannot deny the possibility even for the rat or the earthworm.

(III) In human beings, the effect of satisfaction on later motivation is clearly produced through modification of the thought processes involved in the learning. A reward, either promised, or accidentally discovered in preceding trials, produces the definite thought of a goal to be attained by procedure either prescribed or to be discovered; and if the reward produces satisfaction or pleasure, the learner tries to obtain it. The motivation of an appetite or desire, effectively combined with an objective, is thus definitely established and maintained. This result was conjectured for the rat, and is actual for the human. That thought processes (not necessarily, of course, of the human type) may operate in the lower animal is a live possibility which may prove to be the explanation of the effects of reward on the animal. Unless an effect of reward on a level below the ideational can be demonstrated for the human being, we may have to take very seriously the possibility that the usual process in the human being is the only process for all animals.

The effects of reward not as a sequence to practice, but during actual practice, should throw light on this problem. Experiments on rats have been made on this point; but, so far, no aid to learning through reward administered during the "trial" rather than at the end have been discovered. If no exception to these negative results can be found in exhaustive experimenting, an important step toward the determination of the effects of reward will have been made.

The effects of punishment are not simply the converse of those of reward. Punishment for failure to learn may indeed promote learning. In the human learner, the effect is obviously produced through motivation. By the anticipation of punishment, with its attendant unpleasant feeling, the learner is not only motivated to keep trying, but a reinforcement of the goal idea and desire is secured. Obviously, this operates through thought processes in the more conspicuous cases. Perhaps it operates in this way only.

Punishment can not be usefully administered, like reward, at the end of a "trial" or practice period. Theoretically, a trial could be continued for a certain length of time, and if "success" is not attained, the animal could be punished. It is conjectural whether this will work at all with animals, and it is an inferior method with human beings, in spite of their superior thought processes. Punishment, to be efficacious, must be administered during practice. This can be done in various ways. The learner can be continuously "punished" during the trial, escaping from punishment by success. For example, the rat may be placed in a maze which is so wired that he is continually receiving mild electric shocks, from which he can escape only by finding the terminal compartment. In this case, we might theoretically build up an "escape motive." As a matter of fact, this method is efficacious only if the animal can solve the problem very simply. If a two-compartment problem box, with an open door between the compartments, be so wired that by closing a switch the current is turned on the grid floor of either compartment, and the rat, wherever he stands in the compartment, receives a shock; he will learn to run into the other compartment. In the early trials, the rat will dash frantically about the compartment, escaping into the "safe" compartment finally, by chance. After from forty to one hundred trials, the rat will learn to bolt immediately into the other compartment as soon as the shock is administered, and most rats learn to turn around facing the door, ready for a quick get-away on the next shock.[1]

For more complicated problems, as in a maze with several alleys, this method of punishment will not work. The rat becomes thoroughly confused, and will resign himself eventually to sitting and "taking" the shock.

The effective way of administering punishment in most rat learning is to arrange electric shocks to be received by the rat only when he performs some act which is "wrong," and so that the rat ceases to receive the shock as soon as he ceases the "wrong" response. For example: when the rat enters a blind alley in a maze, he receives a shock from the grid with which each of the blind alleys is provided. By withdrawing from the blind alley, he escapes from the punishment. In no other part of the maze will he receive a shock. This punishment does not produce a motive to learn the maze, but merely a motive to keep out of certain alleys. Hence the learning must depend on positive elements, such as food, appetite, etc., although the added punishment may be an asset, the motivation to keep seeking the food being supplemented by the motivation to keep out of certain alleys, leading eventually to the habit of keeping out of these. Punishment even under these conditions, however, is not always advantageous. The invariable effect is to excite and confuse the rat; and if the punishment is too strong or too frequent or prolonged, or the difficulty in finding out how to avoid it is too great, the punishment may hinder learning.

In any learning, activity is essential. The human learner must keep trying. The rat must keep going in the maze. Sometimes, however, the rat sits down and remains for a

[1] The reader can be assured that the "shocks" used in these experiments are very slight; about four hundredths of a milliampere of current (.00004 to .00008 ampere), which can be barely perceived by a human being, and are not in any wise painful, although the novelty of the stimūlation makes the shock unpleasant to the rat. A shock five hundred times as great has to be continued over thirty seconds before it does the rat any physiological harm.

long time in one spot. Hunger, or other discomfort, may spur him to activity. The rat which has just been fully fed tends to be inactive. His learning capacity has not been lowered, but he tends to sit still, and hence does not learn. Hunger, accordingly, is an asset in rat learning, quite aside from any motivation it may produce. Even when some other form of motivation is employed, as when an animal of the opposite sex may be found in the terminal food box, the rat may not begin to learn without the discomfort of hunger or some other exciter. Raising the temperature of the maze above the normal may produce effective discomfort, but to be as effective as moderate hunger, we should have to employ a temperature which would be prejudicial to the rat.

After learning has been begun, with sex reward, and hunger as a stimulant to activity, the hunger can be omitted, and the rat will continue to learn. The motivation has been built up about the sex reward, and may function without an additional exciter. This throws an interesting side light upon the problem of reward, strongly reinforcing the suggestion that, in the lower animals as in man, thought processes (not necessarily involving memory) may be of primary importance in learning.

No direct inferences as to the effects of punishment in human learning can be made from animal results. Punishment seems to be efficacious with children under conditions similar to those which render it destructive with rats. On the other hand, to assume that punishment would be useful with human individuals in circumstances similar to those in which it is effective with animals would be a fallacious assumption. The greater development and complications of thought processes in human beings make the direct comparison in the field of punishment impossible. The only benefit we can derive, for human application, from the results on animals so far, is a principle of caution: "Go easy on punishment"; "in case of doubt, don't punish."

6. *Mechanical learning.* We have already indicated the serious doubt whether mechanical learning, *i.e.*, learning independent of thought processes and desires, is possible either in man or animal. Direct attacks on this problem are desirable and some have been attempted.

(I) The determination of the effect of guidance on learning seéms to be one means of attack. In the case of a man in a maze, he may learn through his own efforts, or he may learn by being taken by the hand and led through the maze time after time. This proves nothing in itself, because effective thought processes are operating during this guided practice. In a variety of other human problems, guidance, by verbal instruction, by diagram or by taking the subject's hand and putting it through certain required movements, is efficacious. This does not establish anything but a form of economy in human learning which may be immensely valuable.

Guiding the animal should produce important results. We might, for example, lead an animal through a maze. It would be difficult to guide a rat, but one can easily guide a dog or goat. It has not been experimentally done. As a substitute, rats in the maze have been prevented from entering blind alleys by closing their doors until the rat has learned to run quickly through the only passages left open, from starting box to food box. If the entrances to the blind alley are now opened, a considerable disturbance is produced. The rat has to learn again because the situation has become novel. The relearning is brief, however, and further work will show whether the rat trained in this way learns, eventually with fewer total trials (guided and unguided), than the rat trained with the blind alley overt from the beginning. Other methods of restraint from errors have been tried, but these introduce more serious disturbances. Further work must be done with blind rats, where the disturbance due to opening the blind alleys will perhaps be less serious. Whether the results will eventually lead to important interpretations can not yet be predicted.

Another approach to the problem which has been discussed, but not sufficiently worked experimentally, is in the use of anesthetized animals. Can an animal learn under general anesthesia? That an animal might not learn under deep anesthesis does not preclude learning under slightly lighter effects. This should be settled.

Another approach is through operations on the brain. Experimental work, inaugurated by Franz and confirmed by Lashley, has destroyed the old (but still physiologically popular) doctrine of brain localization, which had already become suspected by the psychologists. We are now certain that different parts of the brain have the same kind of function, that the manner of their interconnection is the determinant of the rôle the various cells play, and that the present function of one group of cells can be taken over by another group by a rearrangement of the interconnections. But the basic problem of the function of the brain in learning has not been attacked. We know that animals can still learn after considerable areas of the cerebrum or cerebellum or both have been destroyed. But we do not know how the learning after large destruction proceeds as compared with learning in normal animals.

While it is important to know what animals can learn after brain injury, that is, what objectives they can learn to accomplish, this teleological information is not sufficient. The more important problems concern the types of responses they employ to accomplish these objectives, and the way they learn these responses. On these points we have little information as yet. We know, for example, that a rat after certain injuries can learn to run a maze. That does not inform us that the maimed rat is equivalent to a normal rat except that he somewhat gains a socially equivalent end-result. What we must know is the type of responses he employs in running the maze, and how he learned these responses. In both respects, certainly the rat with extensive brain injury differs from the normal rat.

For further and definitive work, some animal other than the rat may need to be employed. The investigations which are needed are on the type of learning, the type of response learned and the limits of learning; (1) in animals completely deprived of the cerebellum; (2) in animals deprived of the entire cerebrum, or as much as will permit the maintenance of the animal for a period of months; (3) in animals deprived of maximal amounts of both cerebrum and cerebellum. We have some evidence that animals approaching some of these conditions can accomplish certain results (walking, for example), which normal animals accomplish, and that the responses by which they accomplish these results are different in important ways from the responses of normal animals. We need much more information on this point, covering as wide a range of processes as possible, and information as to how the operated animals learn these performances. This may illuminate many of our psychological problems, including the problem of "mechanical" learning.

7. *The nature of the responses most important for learning*. From the comparison of normal and blind animals in learning the maze, Small surmised that visual responses were of little if any use to the normal rat in such performances, and was led to suggest that the learning of the rat was really kinesthetic for the most part. Later experimenters with blinded rats have obtained somewhat conflicting results as to their maze-learning ability, and the problem is still a live one. In some of these investigations, as in much other animal work, the sterility results in part from the use of the "average rat," an entirely fictitious animal.

In comparing the maze learning of a group of normal rats with that of a group of blind rats, properly selected as to age, sex and previous treatment, we may find that the "average blind rat" learns the maze in approximately the same number of trials and the same total hours and minutes as the "average normal rat." This result is of little importance, since there literally is no "average rat" of either class. Nor, to use another method of statement, is there an "average performance" of the group. There is an average of the individual measure of performance, but unfortunately, the average even when conceived in these terms, does not give useful information in this case. The actual performances are measures of performances of individual rats, and these individual performances must be scrutinized much more closely than can be done by consideration of the average.

What occurs with some equated groups of rats is that some blind rats learn the maze more quickly than any of the normal rats, and some learn more slowly than the slowest learner among the normal rats. This is the really important result, and the average does not tell us that. Even the measures of variability (mean variation, standard deviation, probable error) of the scores do not give us the information. Much more detailed comparisons are necessary to bring out the vital facts.[2]

The total information we desire from the learning of blind and seeing rats is that the learning process is really a very complex one, even for the rat, and that different rats may learn the "same" thing (as teleologically defined) in quite different ways. An immense avenue of investigation is opened up by the results, and that is all.

[2] The most flagrant example of the fallacy of the "average rat" (or "average performance") is the report of an experiment in which a large group of rats were individually permitted to choose either the right or the left of two exits from the starting box. (The experiment was assumed to involve a choice of a different kind, but it was actually the choice just described.) Taking all the choices of all the rats in all their trials, it was found that there were approximately an equal number of choices for each of the two exits. This result indicates precisely nothing. If we knew how many of the rats chose always the right, how many the left, and how many chose sometimes one, sometimes the other, that information would be of some importance.

The too simple interpretations of the results with blind rats led to Small's supposition, further backed by Watson and others, that learning is primarily a matter of kinesthetic response. Watson's work with rats which had been deafened, their olfactory sense organs destroyed, as well as being blinded, confirmed his conviction. From this point, others have not hesitated to go on to the conclusion that the rat, in learning the maze, is merely linking together a series of motor processes. The final chain would be: so many steps ahead, a right turn; so many steps ahead, a left turn, and so on to the end. This would be what the rat is really learning, the stimulus-pattern for each unit in the finally welded series being the kinesthetic stimulation due to the preceding movements. Visual, olfactory, etc., cues might assist in forming the series; but their function would be minor in most cases, merely initiatory at the outside.

The same considerations would apply to much of human learning. Perhaps it would apply to all. Where language is involved, learning might be due to the enchaining of vocal movements in producing speech. Language is, of course, much more employed in thinking, than the laity supposes, although the language is in many cases not audible.

This "kinesthetic" theory of learning has had a wide appeal to psychologists, and the present writer was indeed decidedly inclined to it at one time. Further consideration of experimental results have, however, shattered the theory, and it should not now receive serious consideration. Kinesthetic learning is possible, and may sometimes play a rôle of importance. Kinesthesis as an aid to learning is of more extensive importance. But seldom, if ever, is that which is really learned a series of movements.

A sufficient illustration is found in the performance of maimed rats in the maze. Some rats, which have learned the maze up to a standard of running directly and rapidly from the starting box to the food box, have had a front leg removed, and the tendon in the contralateral hind leg cut, so that they could not walk or run normally, but could only proceed by hobbling, jumping or by a crawling-sliding movement. On being placed in the maze, these rats "ran" it as accurately as before, with no delay other than that imposed by a less efficient type of locomotion. If these rats had merely learned to "run" the maze by learning a particular series of movements while they were normal, they could not "run" it after they were injured, since they cannot perform the movements they formerly employed in tracing the maze. Obviously, what has been fundamentally learned is something radically different from a series of movements, however useful the movements may have been in the learning.

A wide range of observations on animals confirms this conclusion. The rat's learning is a far more complex affair than was formerly supposed, and the lack of appreciation of this complexity has contributed to the puzzling results of many early and now discarded experiments.

8. *The transfer of training.* In consideration of the wide range of learning tasks with which the individual is confronted in early life, and the considerable differences in results of practice in different tasks which we have noted, the question of transfer of training becomes important. In learning one task, can one acquire proficiency in another? This is the question. In practicing with one set of materials, are we learning at the same time others with which we are not at present dealing? Before the experimental era in psychology, several different theories on this point were in vogue.

The most familiar theory was that of "Formal Discipline," which is still in vogue in some quarters. According to this theory, a sort of "general learning" is not only possible, but even necessary. In learning certain things, one is virtually learning a great number of other things. Not every task has the same general potentiality, of course. It is necessary to select those which have the major general learning effects, or "disciplinary value," as it was called. The Greek and Latin languages, classic literature and mathematics were once supposed to be the "disciplinary" subjects *par excellence*. The student studied these subjects, it was assumed, not so much for the linguistic, literary or mathematical knowledge itself as for the "mental training" they conferred.

This exaggerated point of view was possible only in an age when the nature of the learning process was little understood. It is possible only for those who narrowly conceive of the mind as a collection of "faculties" or "intelligences" which are like instruments (hammers, saws, sewing machines, etc.), which when built up, adjusted and oiled are capable of being applied to a considerable variety of jobs. Perhaps a better analogy would be with a muscle which, when properly "trained" by some suitable gymnastic system, is capable of work of a number of types.

"Training the mind," "developing the faculties," "increasing mental capacity" are expressions which were taken in a quite literal sense even a generation ago. We still use the expressions, but only in a metaphorical sense. Our conception of the mind has radically changed, and with this change the doctrine of formal discipline has disappeared, except in quarters where the dust of the past lies thickly. Perhaps as frequently happens, we have now gone too far in another direction. There was no scientific basis for the dictum of "formal discipline." We suspect that the bases of some of the views which are antithetical to it are as slight.

Of course, the real reason for the original emphasis on the ancient languages and the classics was in their actual usefulness to educated men. With the passing of that usefulness, the topics were retained as bases for education partly from the force of tradition, partly because they are really difficult topics. If the mind is to be "trained" or "disciplined," it must be given severe labor for its training. No pugilist would train for a fight by moderate walking and gentle calisthenics.

The obvious result of experimental work on learning is to show that the results of practice in any line are most conspicuously demonstrated in that very line. Conversely, to obtain the best training in any one, one must study in that line, if possible. One best learns mathematics by studying mathematics, not Greek. One best learns German by studying German, not French. One best learns tennis by practicing tennis, not golf. And so on for all topics, apparently. Concerning this, there is no question. Hence the primary basis for the selection of subjects for study today is the need for, or advantage of, knowledge or skill in these subjects.

On the other hand, there is no doubt of the importance of preparation for the study of certain topics. One seems to need no special preparation for the study of a language; the best learning in these is done by children, who have had no formal preparation. Nevertheless, a knowledge of Latin *does* assist the adult in the acquisition of French and Italian, if he has not learned those languages in childhood. Even in English, the adult who has made much progress profits continually in further learning by a knowledge of the Greek and Latin roots of English words. In algebra, a knowledge of arithmetic is essential, and without algebra, analytic geometry (at least in the Cartesian form, as ordinarily presented) is impossible. Even the playing of tennis is necessarily prepared for by the child's learning to walk and to use his arms accurately and forcibly. It is clear that one topic of study does contribute to the study of another topic; in learning one thing, we are laying the basis for the learning of another.

If we analyse the illustrative cases, however, we see that "formal discipline" or "general training" is not necessarily involved. In algebra we use the actual skill and knowledge of arithmetic. In studying English, we are dealing with words we must understand, and these words are in part made up of words that we have already learned to understand. In every case, apparently, where learning one thing contributes to the learning of another, we utilize in the second topic the actual knowledge or skills we have acquired in the other. It is not in any case a matter of "general" training, but of very "special" training. "General training," like the "average rat," is a fiction.

So far, we are on safe ground. We might then go so far as to conclude that learning anything gives us skill or knowledge of, or skill in, that thing only, and in nothing else. Practicing certain coördinated movements, for example, improves those movements only and no other movements, except in so far as the muscle vigor may be increased by exercise. (But there is no general "mental vigor" to be so fostered.) This is the doctrine of "identical elements." The doctrine that there is no transfer of learning from one department of practice to another, except in so far as the identical facts or factors which are learned in the first appear in the second—this doctrine may be sound. It does not appear so from the experimental results.

The doctrine of identical elements is apparently supported by experimental work on what is called "bilateral transfer." It is known that the training of one hand in operation of process and speed improves the performance of the other hand, not used in the practice. The "training" of the right hand, speaking metaphorically, is "transferred" to the other hand.

The experiment of Norcross on learning the adding machine furnishes perhaps the clearest illustrations of bilateral transfer and its conditions. Individuals having no previous experience in operating an adding machine were initiated into the technique by having the operation of the machine explained to them. They were then tested for speed and accuracy of operation of the machine, first with one hand, then with the other. Then, for a number of days, each individual was given practice in operating the machine with one hand only, either the right or the left. Substantial improvement was made, of course, both in speed and accuracy. Finally, the other hand, not used in practice, was again used in a test; and it was found that nearly as much improvement was shown with this hand as with the one used in practice.

Superficially, the results would not seem to square with the dictum of identical elements. Movements of the right hand are not "identical" with movements of the left hand. In many respects they are antithetical. Further experiments, however, revealed the identity in the two situations. Other individuals were given the preliminary tests for speed and accuracy, but instead of practice in operating the machine, they were allowed merely to watch the practice, day after day, on other individuals. Still other individuals, after the preliminary tests, were given practice in operation of the machine, without reading any numbers, merely listing numbers at random. Still other individuals were given practice in reading the numbers from the standardized number sheets which the others had used, glancing at the keyboard while reading, but making no hand or finger movements.

All these individuals showed improvement in the final test. Obviously, the major practice effects are not in the finger movements themselves, but in the eye movements and other processes involved in reading the numbers and locating the keys, and in the general adjustment to the problem and situation in which they were placed. These factors are identical, whichever hand is used. They are comparable to what the maimed rat had learned in the maze.

We may conclude, therefore, that "bilateral transfer" is not transfer at all, to any vital extent. It is true there may be processes carried on in the left hand corresponding to processes involved in operating the machine with the right hand. These processes, if they occur, are too slight to permit recording with pneumatic devices applied to the non-used hand and fingers. In Norcross' work, no movements of the unused hand or finger could be detected. Evidence of sub-motor nerve and muscle current might perhaps be picked up by a delicate galvanometer; in any case the actual practice effect on the non-used hand must be so minute as to have no bearing on the results.

If we turn from the deceptive "bilateral transfer" to other experimental fields, we find more perplexing results. Practice effects are found everywhere. They are the bane of laboratory psychology, which must always be watchful lest they confuse results. In some cases, there seem to be effects of practice on practice; learning itself seems to be learned.

In studying the various factors involved in memorizing words, sentences and syllables, it is necessary to have individuals learn a considerable number of series of various lengths. We may, for example, compare the learning of eight-word lists with the learning of sixteen-word lists, as described in an earlier section. Each learner is required to learn a number of eight-word lists and an equal number of sixteen-word lists under carefully controlled conditions. Our comparison of the results is apt to be vitiated, however, by the fact that every learner, toward the end of the experiments, learns eight-word lists much more rapidly, that is, with fewer readings, than he learned them in the earlier part of the experiment, and the same improvement is shown in the sixteen-word lists. Apparently the learner has learned to learn. He can learn certain items now much more easily, because he has learned other, different, items previously.

This effect of practice runs through almost all topics of learning. In school subjects, a large part of the increased facility of learning a particular subject which appears in the student's progress in the subject is due to the utilization in later learning of the items of knowledge acquired in earlier learning. It is quite possible, however, and we now think it probable, that a part of the facility is accounted for by learning to learn. We can account for this, perhaps in terms of specific attitude acquired, adjustment to the learning situation, and increased interest and motivation, but these explanations are frankly *ad hoc*, and may reasonably be suspected of insufficiency.

A crucial point is raised when we inquire whether this learning occurs in *all* topics. If, in some topics, the interest and specific adjustment fully account for the increased facility, then we should expect the increased facility in all topics in which adjustment factors are involved. As a matter of fact, there are some types of activity in which practice produces no improvement whatever, except for a slight initial improvement obviously due to the gaining of full comprehension of the problem, and orientation to it.

The conventional "tapping test" and "steadiness test" furnish excellent illustrations of this lack of learning. In the tapping test, the reactor taps as rapidly as possible on a brass plate with a stylus held in the hand. The taps are electrically registered, and the score is in terms of the number of taps in a prescribed time, or the time taken for a prescribed number of taps. The reactor may show a marked improvement in speed on successive days, demonstrably due to improvement in method of holding the stylus, and type of tap movement adopted. These factors may be speedily eliminated by proper instructions to the reactor. The change of movement to a "tremor" often gives an apparent practice effect later, if the apparatus is used as described.

If a "double tapping plate" [3] is used, the reactor being required to tap alternately on the two plates, the "tremor" movement is excluded, and the other adaptations may be made negligible from the first. There will be variations in speed from day to day, but no general change, except as the health of the reactor may change. In some cases, if the test is given for a long series of days, there is a falling off, due apparently to lessening motivation. But this lessening of motivation in a prolonged experiment occurs in the word-learning experiment also, where the increased facility continues. In the steadiness test, the reactor is required to insert the end of a long thin stylus in a small hole in a metal plate, and keep it there for a prescribed length of time, without touching the edge of the hole if possible. A trial or two may be required for adaptation to the test. The test has some applicability to the determining of physiological conditions, drug effects, fatigue effects, etc., precisely because of its low-practice factor.

These illustrations are not strictly comparable with the word-learning test, except that they do show the ease with which "adaptation" may be eliminated. Various other responses, in which this factor is almost as quickly eliminated, do show practice effects over long periods of practice. In the "simple reaction time" measurements, for example, in which the reactor is required to make a single movement of the finger to a single stimulus, such as a brief sound, or a flash of light, the adjustment of the reactor to the situation seems to be completed in a few days of work. But even after ten days' work, with one hundred reactions each day,

[3] This is a heavy base, on which are mounted two brass plates, each 8½ inches square, separated by a hard rubber strip which rises slightly above the level of the brass plates, so that the tapper can not slide the stylus from one plate to the other. Each of these plates is electrically connected to a separate recording device. The reactor holds a stout metal stylus by a properly shaped hard-rubber handle, and taps alternately on the two plates. The stylus is connected by a light flexible cord to the electrical source, so that each tap on each plate is recorded.

during which the reactor has made notable improvement in speed of reaction, he will make still further improvement with further practice.

On the whole, we may suspect that the improvement in learning shown by increased facility in learning, and due to previous learning of other items, is not due entirely to adaptation to the situations, but is to a certain extent a learning of learning. In characteristic cases, however, this "transfer" occurs between materials of the same kind, although not identical. Learning one word list increases the facility in learning another word list—different words, but still *words*. Our vital problem is the determination whether this transfer occurs between materials of different type, or rather between the learning of material of one type, and the learning of material of another type. Does learning word lists, for example, facilitate the later learning of speed of reaction? Does learning to solve mazes of a particular type increase facility in learning to operate an adding machine, or to translate sentences into a code? Does the learning of arithmetic help the learning of geography?

This problem still remains a problem. Much experimental work has been done in attempts to solve it. In some cases the results are apparently negative. In other cases, they seem to show the "transfer." In many cases, the difficulty in controlling the several factors involved in learning, other than the hypothetical "transfer" factors, are so great that the results are not capable of clear interpretation. In few cases has the work been comprehensive enough to enable us to decide whether the results are indicative.

Further, even with more adequate work on specific pairs of types of learning, we shall not expect to reach a conclusion until a great range of types has been covered. Transfer may occur between certain types, and not between others: from maze-learning to word-list-learning and not from either to learning to sort perforated cards by touch.

The complexity of the problem is indicated by the more definite results from experiments on interference of learning. In many cases, the learning of one topic seriously inhibits the learning of another. If each of the nine digits is paired off with a color; 1-red; 2-blue; 3-tan, etc., the pairings may readily be learned, so that the presentation of the digits in any order "calls up" immediately the colors paired with it. If now a different pairing of the same names with the digits is made, the learning of the new pairs will be more difficult on account of the previous learning. Even if the digits in the second task are paired with a different group of words—names of trees, for example—the first learning interferes with the second, unless time is allowed for eliminating the results of the first learning.

With rats, whose learning ability is more limited than is man's, not only does the learning of one maze or one problem box by the rat seriously inhibit the learning of another maze or another problem of the same type but different details; but even the learning of a maze, far from facilitating the learning of a different problem (such as learning to lift the latch of a latch box), more often actually inhibits the second learning.

We cannot infer from the animal results that "transfer" does not occur in human learning. In fact, transfer does occur in the human learning, from the learning of one set of material to the learning of a different set of the same sort, in some types of learning at least; whereas in the rat, nothing but inhibition results. We can infer, however, from the prevalent inhibition in animal learning, and the demonstrated inhibition in certain types of human learning, that the relation of transfer and inhibition in the human learning are highly complicated, with no possibility of inference for the results with one pair of topics to results from another pair of topics.

In the meantime, while one can be certain that the doctrine of "formal discipline" with its assumption of "general training" and "mental discipline" is well disposed of, we cannot go to the other extreme as yet and conclude that "transfer" occurs only through "identical elements."

9. *The "part" and "whole" methods in learning.* In synthetic learning, in which the result is not the production of a response of simple type, but the welding together of many features in a series, or into a more complex response, it is possible to learn the material as a whole, or to learn it as a number of parts, which are then integrated into a whole. In learning to drive a car, for example, one might learn steering as one job, the control of the accelerator as another job, the use of the brake as another, and the use of the horn and hand signals as another. Having learned these elementary processes, one could then learn to combine them into the requisite total control as the final step. This would be the "part" method. One might, on the other hand, attempt full control of the car from the beginning, synthetizing or integrating these various tasks as they are learned. This is the "whole" method.

As a matter of fact, both methods are actually used by the majority of learners. Some persons, moreover, who learn the greater part of driving before actually touching any of the car mechanisms, by the perceptional and ideational method of observing drivers' performances, noting what is required in particular circumstances, observing the traffic and road conditions and deciding what a driver must do, and noting how he does it, use the "part" method to a large extent. It is a question, however, which method, or what combination of the two methods, is the most efficient.

Something of the same problem appears in connection with learning to play billiards. Some teachers have the pupil begin "playing the game around the table" with the first lesson. This is not a pure "whole" method, but it does emphasize the procedure by wholes. Other instructors adopt the "part" method, and cause the pupil to practice specific "set ups," for a considerable period before beginning the game. Which method is the more efficient has not yet been finally determined. In learning to play the piano, or any other musical instrument, the "part" method is almost universally employed.

The first experimental work designed to compare the efficiency of the two methods was done on the learning of verse and of series of nonsense syllables. If a certain assignment of verses of poetry is to be "memorized," one may learn it as a whole, reading the entire assignment through, and repeating the reading until the text has been learned according to the standard adopted. On the other hand, the assignment may be broken up into sections until memorized, before commencing the learning of the next section. The final work, of course, will be the connecting of the sections into a whole, which may require further reading.

In this experiment, it was found that the "whole" method was distinctly more efficient than the "part" method; that is, the work, as measured by the amount of reading required, was less under the first method. Similar experiments by other investigators have confirmed this result. From these conclusions, the rule has been deduced (and broadcast) that in all cases the "whole" method is superior to the "part" method of learning. If this were true, the procedure of the majority of music teachers would be wrong, and the decision between the rival methods of teaching billiards would be already made. It appears, however, that the rule is premature, settling by illicit generalization questions which are actually unsettled, and conflicting with certain indications which are probably valid. We cannot infer from the results with one type of material what results we shall get with other materials. It is possible, moreover, that differences in technique and method of learning, aside from the difference between the "part" and the "whole" methods, are important in determining the relative efficiency of these two methods. Difference in individuals also must not be ignored.

Further work with the learning of verbal material has confirmed our skeptical view and indicated clearly the real complexity of the problem. In some cases, the "part" method of learning verse is distinctly superior to the "whole" method. One factor in the determination is the length of the assignment. Assignments of certain lengths may be learned more efficiently by the "whole" than by the "part" method, while assignments of certain greater lengths, of the same type of material, may lend themselves better to the "part" method. The type of verbal material is probably important also. The effects of rhyme and of various rhythms of poetry have not been experimentally ascertained, although these may not be of the first order of importance. The sequence and interlocking of the ideas is suspected of being of major importance. In so far as there is ideational coherence throughout an assignment, it lends itself to learning by the "whole" method. In so far as it may be broken up into parts having unity of their own, it lends itself to the "part" method, the availability being always determined in part by the number and brevity of the units. In general, the total series of parts can be learned as parts more readily than the whole can be learned. The differential may be reversed by the final labor of integrating numerous parts into a whole. The whole can always be learned, if the parts are already learned, more readily than the whole can be learned, if the parts have not been learned. This has been verified in many materials, as for example, in learning nonsense syllables as compared with learning of word series. The words, being already familiar (*i.e.*, having already been learned as individual words), the word series can be learned more quickly than the series of nonsense syllables, which are unfamiliar. The exact amount of "part" learning which renders the final synthesis most efficacious cannot be predicted for any given material, but must be experimentally determined for the material, the general learning conditions, and the amount of assignment.

10. *The effects of knowledge of achievement.* How far can learning proceed, if knowledge of current achievement in the learning is withheld from the learner? Here, perhaps, we again approach the problem of "mechanical" learning, but from a specific point of view.

If that which is learned is itself knowledge, in the usual sense of the term, knowledge of achievement may be of no consequence. In learning poetry, for example, it is possible that the learning may proceed as well when the learner has no knowledge of his current success as if he had such knowledge. Suppose, for example, we should determine that a given learner, working under uniform conditions, can learn a list of twelve nonsense syllables of fourteen readings of the list, without attempting to determine how much of the series he has learned until after the fourteenth reading. Suppose he cannot learn similar twelve-syllable lists perfectly in thirteen repetitions. Let us assume, that since he does not try to find out during the learning how much he has learned, he is ignorant of his progress. If, with other twelve-syllable series, he attempts to recall after the fourth reading, and after each successive reading, he will probably be able to learn the series with fewer than fourteen repetitions. This experiment has not been carried out in the form described (it is indeed a much more difficult experiment than the description indicates), but from the results of experiments of a variety of types we might expect this result. A part of the apparently increased efficiency is really spurious, since the partial recalls may themselves be a part of the learning work.[4] Does this account for all the

[4] There is even a distinct possibility that in some types of ideational learning, the knowledge of progress inhibits the learning.

saving in readings, or is there an actual increased efficiency due to the knowledge of progress afforded by the attempts at recall? We cannot answer this question.[5] Nor can we be certain that our assumption that the learner who does not attempt to recall during the learning has no knowledge of his progress.

When we turn to the type of learning which is called the "acquisition of skill," rather than the "acquisition of knowledge," our conclusions are more definite. If, for example, we cause a learner to throw small feathered darts at a target from a distance of sixteen feet, with vision unobstructed, we find the beginner throwing wild, many of his darts not even hitting a three-foot target. If he throws two hundred and fifty darts a day for ten days, we find a steady increase in proficiency. Not only are the wilder throws discontinued, but also, on the average, the distances of the darts from the bull's-eye progressively decrease.

Suppose on the other hand, the thrower has full vision of the target before each throw, but that just as the dart leaves his hand a small screen dropped before his eyes cuts out further vision of the dart, and of the target until the dart has been extracted. He has then little knowledge of his success. He aims and throws as in the ordinary form of the experiment, but does not know where the dart strikes. Will he make any improvement? This exact experiment has not been carried out, but experiments of a similar nature, not so readily described, show an improvement (that is, learning), but not much, as compared with the normal practice with knowledge of result.

We may wonder why blind practice yields any learning at all. The reason is not far to seek. The dart thrower, in the experiment described, would not be strictly without knowledge of his success. He has thrown objects before. He has some indication, if not highly reliable, of the suc-

[5] Under some circumstances, however, attempt at recall inhibits learning.

cess of his throw from the kinesthetic perception of his arm movement, this perception being mediated by responses initiated by the receptors in the muscles and joint tissues of the arm and hand. We should, therefore, expect some learning to occur, the actual amount depending on the previous experience of the learner in throwing other objects.

No experiment has been devised in which the learner is actually devoid of information concerning his success. But with a considerable reduction in his information, learning is reduced. We can be fairly certain that there is little learning of a skill where the knowledge of results is slight. This is what we might expect, indeed, if it is true that the ideational factor in learning is the most important. We may even suspect that with complete absence of information as to success, no learning of this kind is possible. This suspicion cannot at present be verified. On the whole problem of the effects of knowledge of success in learning, our investigations so far have merely scratched the surface.

The ten problems of learning discussed above are perhaps the most important general problems in the field. Experimental psychology has accumulated a vast amount of valuable data on these problems, data, however, which must be cautiously interpreted, and which can be safely interpreted only in the light of a wide range of information from the whole field of learning. Hasty and uncritical interpretations of the data have resulted in much spurious simplification of the problems, and in fallacious principles of learning which have been imposed upon the long-suffering school teachers. There are many other major problems, and a host of minor problems, on which important data have been collected, the interpretation of which also requires great caution and extensive background. Since, however, this volume is not an encyclopedia of the learning process, we must pass on to other major topics.

CHAPTER VII

RETAINING, RECALLING AND RELEARNING

THE process of learning we conventionally call *practice.* The result of the learning process may be *knowledge*, or it may be *skill*, according to the department of learning. In many cases the practice may not go far enough to produce results to which we would ordinarily apply the terms knowledge or skill. For example, after a certain amount of study (practice) on problems of square root, I *know* the significance of the square root of a number and the root of a number; and I have the *skill* of extracting the square root of any number. After my first few minutes of study, however, my knowledge of the subject is slight and vague, and my skill may be so slight as to be undemonstrable. Yet it is evident that even a few minutes of study must have some results; else a longer period would have none, for the summation of zeros must always be zero.

The difference between the result which is knowledge or skill and the result which has not reached that level may be described as the difference between complete learning and incomplete learning. However we name it, the distinction is relative, since the point at which incomplete learning passes into complete learning is arbitrarily determined, depending on our definition of "completion." Even after I have acquired knowledge and skill in problems of square root, and may be said to have completed the learning, further study will add to my knowledge. In other words: the learning, when said to be complete, is complete only in the sense that it comes up to a certain standard, and may not be complete as judged by other standards.

In evaluating the results of learning, and hence in examining into the process of learning in any way, learning-standards or standards of learning are always necessary. Just as standards of achievement (which are learning-standards of a sort) must be applied to the child's progress in studying arithmetic, so standards are required in experimental investigations of learning; and these experimental standards must be much more precise and more accurately determinable than are the standards applied to school learning. In experimental work we select the standard which is suitable for the particular investigation which is planned, and the procedure throughout the experiment involves the learning of the material up to, and not beyond, the standard adopted. Having standardized learning in this way, we next proceed to judge its efficiency by the application of adequate criteria.

The problems of learning fall into three general classes: problems of immediate results of learning, problems of retention and problems of recall. So far we have been discussing learning with regard primarily to its immediate results, and we may therefore consider first the application of standards and criteria to the determination of such results. The standards which are available are: (a) standards of reproduction; (b) standards of work; and (c) standards of time. For use with each of these standards one or more criteria are available.

(a) Standards of reproduction. An obvious measure of the result of learning is the ability to reproduce what has been learned. This reproduction is *recall* in the case of knowledge, and *performance*, in the case of skill. If the learner practices on an assignment of verse for a sufficient length of time, he will then be able to recall the complete assignment fluently and accurately. This is perfect recall, or perfect reproduction. If his practice has fallen short of the requisite amount, his recall will be imperfect or incomplete. He may recall with hesitation, or may recall only a part of the assignment. We have therefore two sorts of recall standards available (I) standards of perfect recall, (II) standards of partial recall.

(I) Standards of perfect reproduction. These are the standards most generally employed. With such a standard, the learner is required to practice until perfect reproduction is possible, and no further. If we are comparing the learning of simple poetry learned, by the "part" method and by the "whole" method, assignments may be learned up to this recall-standard by the "whole" method; and assignments of equal length and character learned by the "part" method up to the same standard. The results are then the same in the two cases. By application of the proper criteria, we may then determine the efficiency of the two methods for this sort and amount of material. The criterion commonly employed is number of readings, each reading being so controlled that the time of the different readings is constant. The method which requires the fewer readings to accomplish the same results (that is, to learn up to the same standard) is designated the more "efficient." Or we may in some cases allow the learner any number of readings at any rate, and use as the criterion the time spent in learning. This is a less exact criterion.

In the case of skill, the criterion corresponding to recall is *performance.* For the rat learning the maze we may adopt as the standard a perfect run; that is, a run from the starting box to the food box without delay and without error. If we are comparing young rats and old rats in this learning, each rat will be given the number of trials required to produce this standard performance. The results being equal, we may determine the relative efficiency by the criterion of the number of trials required by each animal, or by the criterion of the total number of errors (entrance of blind alleys, retrackings and hesitations), or by the criterion of the total time in hours and minutes consumed in practice up to the point determined by the standard. In the case of the dart-throwing experiment, each learner may be required to throw the darts in sets of twenty-five, ten sets per day, until he attains a standard score for a set, or for a daily total. The criteria of efficiency in learning will then be the number of sets required to produce this result; that is, to learn up to the standard adopted.

The standard of "perfect" performance is always more or less arbitrary, standards of skill-performance being more arbitrary then standards of recall. In recalling verse it is necessary to adopt arbitrary standards of perfection. How much hesitation in recall, for example, is consistent with "fluency"? This must be decided by the experimenter in accordance with his observation of the learner's normal fluency of speech. In the case of the rat in the maze: What constitutes "no error"? This must be decided from a knowledge of rat behavior in general, but sometimes the determination is fixed in terms of a minimal time, in seconds, for the run from starting box to food box. In the case of dart throwing, the score which is perfect is determined by the known possibilities of performance of throwers in general and the adoption of corresponding methods of scoring. If the maximal score per dart is assigned to each dart which sticks in the bull's-eye, the size of the bull's-eye, and the distance from which darts are thrown determine the standard.

Another difference between standards of knowledge and standards of skill is in the consistency of final performance. If the learner succeeds in a perfect reproduction of an assignment of verse, he can in almost all cases make a second complete reproduction immediately. For sake of greater precision, however, two perfect reproductions are sometimes required. The variability of performance of skill is more serious. A perfect run in the maze is no guarantee that the next run will be perfect. A perfect score in dart throwing is no guarantee that the next score will be perfect. It is the exceptional case in which the first perfect run or score is followed by another perfect performance. On this account, three perfect runs, or three perfect scores, are the standards usually adopted. This indeed does not assure absolutely constancy of standard, but it may be a sufficiently close approximation for some work.

(II) Standards of incomplete recall and performance. In some cases it is not practicable to require "perfect" performance. If, for example, we are comparing the learning of children with that of adults in dart throwing, a bull's-eye large enough to permit the children to attain a score of one hundred out of a possible one hundred is far too large to permit an adequate measure of adult learning. We can, however, employ a target in which the attainment of a perfect score is impossible for the children, and adopt as the standard a score of fifty out of a possible one hundred. In working with adults alone, in fact, it is better to use a target and a method of scoring which make perfection impossible, and to adopt as the standard a score of ninety, or even less.

In the case of knowledge, while perfect recall is usually adopted as the standard, a certain percentage of accuracy in recall may be used, although the percentage of success is usually not capable of sufficiently accurate determination.

(b) Standards of work. It is sometimes practicable to assign a definite amount of work to be done in learning, in terms of trials (a definite number of runs in the maze; a fixed number of darts to be thrown; a fixed number of readings of verse allowed, etc.). Each task is then learned up to this standard. The criteria employed in these cases may be the percentage of perfection attained on the last run, the score of the last set or group of darts, etc. Or, the criteria may be the total number of errors in the practice or the sum of the magnitudes of the errors.

In the well-known *substitution test*, or *code test*, a work standard (or work limit, as it is sometimes called) is commonly employed where one individual is tested at a time. In one form of this test the individual is given a sheet on which are printed six different geometrical designs, each design appearing twenty-five times, with irregular alternation of the six designs, making a total of one hundred and fifty designs. The learner is required to mark the designs in the order in which they are printed, marking each of the six designs in a way indicated by a key at the top of the sheet. The standard is either the marking of the whole number of designs in the sheet, or a specified smaller number. The criterion is either the time consumed in the work, or the number of errors made. Since, in general, the learning of the marks assigned to the several designs is a factor in the time required,[1] the criterion may be taken as an index of learning.

(c) Time standards. The standard adopted may be one of elapsed time alone. For example, each rat may be allowed a fixed time, in hours and minutes, in the maze. The learner of verse or of arithmetic may be allowed a specified study time, regardless of the number of repetitions. The results may be judged then by criteria of percentage of perfect performance, as in the procedure with the work standard or the amount of practice work accomplished. In applying the substitution test, a group may be tested at one time by this method (sometimes called the time-limit method). Each member of the group being given a sheet, all commence work at the same instant, and work for a specified time. The number of designs marked is then the criterion of total efficiency.

No one standard is the most adequate for all purposes with all materials and with all methods of learning, or even

[1] Although the results of work with substitution tests is frequently reported and interpreted in text-books as if it were primarily dependent on learning accomplished during the working of the test, this learning factor is probably a minor factor in the usual test results. The substitution test, however, furnishes a convenient illustration of the work standard.

with learners of all ages. No one criterion is the most adequate for use with a given standard under all circumstances. The determination of the most adequate standard and the most adequate criterion for a particular experimental purpose is a matter of psychological expertness, requiring extensive background and intensive training in experimental techniques. In many cases, it is necessary to plan the experiment so that several standards may be applied, and several criteria used with a given standard.

In the substitution test, for example, it is necessary, for adequate interpretation of results, to assign a work standard, and also to determine the number of designs checked during certain definite time standards (as, for example, during the first thirty seconds, first minutes, first ninety seconds, and so on). This can be done, even in a group test, by an elaboration of instrumentation and procedure not necessary to be described here. In the maze experiments, if perfect performance is used as the primary standard, the details of each trial of the rat must be currently recorded, so that both number of trials and number of errors may be used as criteria with this standard, and that work standards and time standards may also be applied with the appropriate criteria. In the dart-throwing experiment, errors are, of course, not counted except as they are summarized in scores, but in addition to the performance standard, work standards are also applied, by selecting from the final data numbers of throws which all throwers have made. If all throwers have thrown more than a hundred sets of twenty-five darts each, standards of one hundred sets, seventy-five sets and fifty sets, etc., may be applied, with criteria of performance (score) in the last set of the standard or in the set next following the standard. In learning series of nonsense syllables or paralogs,[2] the primary standard of perfect recall is preferably em-

[2] A paralog is a combination of letters readily pronounceable as if it were a word, but having no meaning.

ployed; but since for adequate control of the learning work, an attempt to recall must be made after each reading of the series following a certain minimal number of initial readings, it is possible to apply also work standards, the criterion being the percentage of recall after specified numbers of readings.

Where time standards or work standards must be employed alone, and where the determination of percentage of success in recall is difficult, a particular method of recall is sometimes employed, in which the experimenter gives the learner definite assistance in recall, and enables him to make a complete (but not perfect) recall which would otherwise be impossible. The application of the recall criterion in this way is called the *method of "assists."* In learning series of paralogs, for example, the learner may be given a standard number of readings insufficient to produce perfect recall. In attempting to recall after completing the standard readings, the learner may be able to reproduce the first two words, and may then "stick," whereupon the experimenter supplies the next word, and the learner may then reproduce one or two more before sticking again, whereupon another "assist" is made. The criterion in this case is the number of "assists" required to complete the series. The more "assists," the less efficient the learning has been. This criterion, as will be readily supposed, has a limited range of applications.

Reproduction, that is, recall, or performance, gives definite evidence of effects of learning. If the recall follows immediately on the learning procedure (practice) the evidence bears on the primary learning process. Recall or performance, perfect or imperfect, may, however, be possible after the lapse of a considerable time between learning and attempt to recall or performance. This enduring possibility of reproduction is conventionally called *retention.* We say that the effects of learning (the knowledge or the skill) have been *retained.* Another phrase commonly employed is that *retention is the possibility of reproduction.* All we mean by the term retention as thus defined is the *abstract possibility of recall.* We may determine the conditions under which retention to varying degrees occurs; in fact this determination is a major problem of the psychology of learning, since the values of useful learning on the one hand, and the damage of undesirable learning on the other hand, are determined by the endurance of the effects. If the effects of learning disappeared immediately, there would be neither use nor harm in learning. As to the "nature" of retention itself, there is no question. It is an abstraction. As to its physiological conditions, we know that it depends in part on the characteristics of neural function; but the various entertaining theories of the storing up of "traces" in brain cells, and the establishment of "bonds" between brain cells, in which certain psychologizing physiologists and physiologizing psychologists delight are merely allegorical constructions, harmless if understood as allegorical but damaging to the understanding of the learning process if taken seriously.

Measurement of retention (for abstractions can be measured) may be secured by applying, after the lapse of time, the same criteria as are used to measure primary learning results, and certain other procedures are also available.

Having learned up to a certain standard of performance, we may, after an hour a day, a week, a month, or any other period of time, require reproduction. If the criterion of percentage of success can be applied, this gives an index of retention. If "assists" are made the number of "assists" required for complete reproduction is inversely an index of retention. Similar methods can be applied to the measurement of retention of skills. Having learned a performance up to a certain standard, the percentage of success in performance after a given time interval is the index of retention for that interval.

Still another method of retention measurement is the *relearning method.* What has once been learned can be relearned, in most instances, with less work than was required for the primary learning. If, for example, the standard of perfect reproduction with the work criterion has been employed in the primary learning of an assignment of verse, the ratio of the number of readings in the primary learning to the number required for relearning is one index of retention. The difference between the number of learnings in readings in the two cases is another index of retention. If no relearning is required; that is, if perfect reproduction is possible after the given time interval, with no additional practice, the ratio is infinity, and the difference is maximal. The retention is perfect for that period. If the ratio is unity, or the difference zero, there is zero retention, there being presumably no retention at all. An awkward feature of this method, however, is that the ratio index is sometimes less than unity, and the difference index is negative in sign. If the foregoing statements are taken with naïve literalness, as unfortunately they are in many texts, the assumption would be that less than nothing is retained. This absurdity emphasizes the need of more adequate statement of the facts, based on analysis of the features of retention. Obviously, the "index of retention" is actually an index of retention only after certain corrections have been made to it. If these corrections cannot be made, the index is an approximation which is so erratic that it is (to put it mildly) not highly reliable.

Let us consider some of the facts of retention, as implied in the accepted definition, and as presented through common knowledge as well as the results of experimental work. In the first place, the possibility of recall is known, in many cases, to diminish as the time increases between the primary learning and the attempt to recall. In colloquial language, what you learn may be "remembered" for a brief time very well. But with the passage of time, the "remembering" becomes less complete, and may eventually become impossible. Skills suffer in the same way. You may be proficient in operations in calculus, or in tennis; but with the lapse of time, you become "rusty." In the case of tennis, a part of the loss may be due to actual muscular changes; but in many cases, the major loss is due to the subsidence of the effects of the primary learning. In either case, the retention, considered as the possibility of reproduction, has decreased.

Experimental work, begun by Ebinghaus, and carried on by a number of investigators since, has had bearing upon this progressive decrease in retention with the passage of time. By the relearning method, *retention curves* (and their inverse form which are *curves of forgetting*) have been worked out for different materials and different methods over considerable periods of time. For a given individual, with uniform material, and uniform method of practice, the index of retention can be determined for periods of an hour, six months and a year. By laying off the time periods on the horizontal axis of a chart, and the different indexes vertically above the corresponding period measures, and then connecting the points so charted, a curve of retention is produced. These curves are interesting, and indeed useful. Their significance, however, is badly injured by the awkward fact of negative retention, and by several other facts which are often overlooked.

While it is true that the possibility of recall, for many results of learning, decreases with the passage of time, there are many exceptions. Many bits of knowledge acquired in early life "stick in memory" for long periods of time during which they are not recalled, with little apparent loss of retention. Other bits of information, acquired much later in life, suffer serious depreciation, most information being eventually completely forgotten. Skills are retained or not retained in the same variable way. If you learned to swim in your youth, and have not been in deep water for twenty years, you find, on again entering the pool, that a surprising amount has been retained. What you have lost, in fact, is mainly in wind, endurance and strength. The swimming movements, considered aside from strength and length of continuance, have suffered but little. Other skills may fail in retention to a much greater extent. That any skills are completely lost is not certain, except in so far as the skills, at their maximum of efficiency, were not pure skills, but operated in definite dependence on knowledge. Mathematical skill is of this type. The routine operations of calculus are not so much lost in the course of time as is the ideational knowledge in which the skills were bound up, and without which they are impotent.

We must then admit that the loss of retention with the passage of time is an exceedingly variable matter, varying from zero to total loss; and that this variation depends, in part on the kind of learning, the method of learning, the age at which learning occurs and probably many other conditions. Still, the fact of negative retention is not explained. We must inquire, therefore, what the loss in retention, when it occurs, probably is. In the first case, we have seen that our current definition of retention covers more than one factor. I may have lost the ability to play tennis or to swim, because I have acquired rheumatism. The loss is a reduction of retention if retention is "the possibility of reproducing what has been learned." The possibility of reproducing, in other words, depends not simply upon the effects of learning, but also upon a number of other factors, and we cannot always discriminate retention as the persistence of the essential effects of the learning process from retention as the occurrence and maintenance of quite different factors essential to reproduction. We may speak of "retention properly so called," or "psychological retention," and avoid some confusions; but we can in few cases, if in any, separate psychological retention from the other factors involved in the ability to recall or perform.

If we should nevertheless ask what determines the loss of psychological retention, we should have to expect the answer: "Several things, probably." For all we know, there is a "natural" loss of retention, a process of "decay" in some integration, construction, or what not, which is effected by learning. The cases in which retention is perfect over a long period of time would then be explained as due to some factor or process unknown which prevents this decay. Progressive decline in retention would then be the rule for every result of learning, unless suspended by definite prophylactic factors. But this conception, vague and *ad hoc* as it is, does not explain the occurrence of negative retention, except by relegating it to the limbo of non-psychological factors, and we are not at all certain that this is the proper solution of the difficulty. Where a non-psychological factor (such as rheumatism) is responsible for loss of retention, no amount of relearning will bring the results back to the former high level, unless the factor at fault (rheumatism) be removed. A non-psychological factor, we suspect, cannot be abolished by a learning process. Clarification of the problem is attained if we revert to the negative aspect of learning. We can learn, and we also unlearn. A large part of the progressive loss of retention in any case may be not a "natural" process of decay, but the result of actual negative learning. We do not learn solely by practice of the responses which are learned, either negatively or positively. Learning, as we have earlier shown, is a vastly more comprehensive process than the mere fixing and unfixing of habits. By learning one thing we may be learning other things, or we may be unlearning other things already learned. The obvious importance of non-retention or forgetting, in practical life, is too great to permit us incontinently to suppose that it could be efficiently left to a "natural" and comprehensive process of "decay." Many things learned need to be unlearned speedily. Others need to be retained well.

If unlearning is responsible for a considerable part of the loss of retention, we have no difficulty in conceiving of the unlearning process as not merely reducing the results of a particular learning practice to zero. The unlearning may quite conceivably go so far as to establish a barrier to later relearning. Negative retention, on this basis, is a complicating phenomenon, but is quite consonant with our general understanding of the learning process. But these considerations oblige us to admit that the significance of the index of retention is much more complicated than it superficially appears to be.

There are, however, still further complications. Retention, as measured by the criterion of recall without relearning, is not the same sort of retention as that which is measured by the relearning method. This is truly a disconcerting state of affairs, but there is only weakness in ignoring it. Retention, as measured by the recall method, is the persistence to a greater or less degree of a definitely established habit, and is measured by the portion of the habit which persists. By the relearning method, however, we can measure learning effects which have not proceeded to the formation of a habit. We can stop the primary learning in the stage in which the response which is to be formed has not yet been formed, the stage, that is, in which the responses employed in practice are progressively abolishing themselves, and are laying the ground for the formation of other and better responses. Then, after the lapse of a week or a month, the learning, up to whatever standard of performance is set, will require less practice than it would have required if the incomplete practice work had not preceded. This is not true in all cases; neither is this method of procedure as efficient for further retention as if the performance had been completely learned prior to the interruption. Yet there is a retention in such cases. The retention, however, is not retention of the same kind of results in the two cases. When we admit that the relearning method measures a kind of retention in all the cases in which it is capable of application, we have extended our concept of retention beyond the limits of the definition of retention as "the possibility of reproduction." That is retention of one type; but obviously there are still other types.

A further extension of the concept of retention is made when we apply a still different criterion of retention, namely, *recognition*. We find that what has been learned, even if forgotten to the extent of complete inability to recall, may have left results which can be demonstrated by recognition. Any item of knowledge may be forgotten, and yet, if presented again, the item may be recognized. So with skills. We may have forgotten a certain procedure, and upon relearning it, may recognize it as something we learned before. So with faces, voices and a host of other things. Even when recall or performance is impossible, and the relearning shows no saving over the previous learning, the recognition of the materials as something learned before clearly indicates retention; effects of the previous learning still persist, even if not useful for the particular purpose of an experiment.[8] But if this persistence is retention, then there are more kinds of retention than our initial definition included.

Even with the difficulties in the interpretation of indexes

[8] The factor of recognition is apparently involved to a certain extent in the "method of assists." Although the missing word, for example, supplied by the experimenter as an "assist" to the learner's attempt to recall the series may not be recognized, it seems probable that it does not actually assist unless it is recognized. Recognition is sometimes employed as a criterion for minimal learning in experimental work, but its applications are necessarily limited.

of retention which we have indicated, the study of retention is an important task of the psychology of learning. As we have already noted, the importance of learning lies in the retention of the results, which means simply their being available, for good or ill, at a later time. The more important differences in methods of practice or study, therefore, are not in the efficiency of these methods in primary learning, but in their efficiency for one or more kinds of retention. Unfortunately, there is no general correlation between these sorts of efficiency. We could not predict, in practical situations, that the method of study of arithmetic which gives the best results as measured immediately, by recitation or other tests, will give the best results in the way of a useful knowledge of the subject after one or two years. Even though after extensive experimental work, we should find this relation indicated actually to hold, we could not predict that the same relationship would obtain in the study of geography, even if the same methods used in arithmetic are applicable to geography. Yet, it is the long-time effect, or retention, which is the important educational consideration.

To take a particular case: Is the method of learning which is the most rapid (that is, the one which, when carried to a set standard of recall or performance, rates highest by the time criterion) productive of the highest retention? No general answer can be given to the question. Very probably it depends on the kind of material to be learned, and perhaps on other considerations. On the other hand, we have reasonable proof that as between two individuals who learn the same materials, by the same method, to the same standard, the quicker learner is also the better retainer. Unfortunately, hasty generalizations have been made from this particular experimental result to the more general situation; but it is clear that the comparison of the results of two individuals working by the same method, and the comparison of the results of a given individual working by two different methods, are different comparisons. Perhaps, the method which provides more rapid learning for both may also provide better retention for both. On the other hand, it is possible that a method which attains the standard more slowly in both cases will produce better retention for both. All problems of this kind require direct experimental solution. We cannot infer from the results in one case what the results will be in an entirely different case.

The problems of retention include problems as to the different kinds of retention: retention for recall, retention for relearning, retention as a basis for further and different learning, even retention as a basis for recognition. These problems are of maximal importance for school and college work. Theoretically, psychology should have the solutions of these problems ready for the educators. Unfortunately, these solutions are not ready. Alleged solutions are offered in some texts on educational psychology; but these solutions are guesses which do not stand psychological scrutiny. We can give advice as the best thing to do in the circumstances, but this advice must be labeled as tentative. In general, the best thing to do when we don't know what to do is to do nothing. But if we are forced to do something, we do the thing which has been found by group experience to work somewhat well, rather than to adopt the variant practice prescribed by the theorist. The collegiate experiments with novel methods of instruction, proposed by theorists as means of lessening the routine work of the student, should warn us that the probability of accuracy of a mere guess is always small. We have no evidence that lessening the routine work is a benefit. The premise that the less work the student has to do, the better off he is, which underlies most of these experiments, is an arbitrary assumption.

We have found a considerable amount of success in schools in measuring the progress in learning of students immediately. We have not found any satisfactory way of measuring the retention of school learning over long periods. The strong suggestion would seem to be that we should not only improve our method of measure of primary learning, but should increase the immediacy of the measurements. Term and final examinations are halfway measures, and the general low esteem in which these examinations are held today by the majority of practical educators is very probably a valid group opinion. The proposal to eliminate these, and depend on examinations held at the end of the high school career and the end of the college career, would be humorous if it were not unfortunately seriously advocated. That, for the present, improvement lies in increasing the frequency and immediacy of measurement of learning would seem obvious. We can, in this way, hold students up to certain standards of primary learning. It is too bad that we have to let retention take its chances, but there is no way out at present.

CHAPTER VIII

REMEMBERING AND FORGETTING

THE word *memory*, like many other words which have had long usage, has several meanings which must be kept distinct in orderly discourse. The term is used for the capacity or power of remembering, and also for what is remembered. These two meanings are seldom confusing, but in psychological discourse the second or objective meaning is never used. By "memory" we mean always the capacity, or else the fact, of remembering. In certain philosophical discourses, however, a more confusing usage of the term as equivalent to simple *retention* occurs, and sometimes psychologists slip into this usage inadvertently. Along with this usage goes sometimes a variant usage of the word "remember," which sometimes is made equivalent to "retain," sometimes to "recall." In order to avoid confusion, we must define or point out the exact significance of these terms in psychology, in which, in the main, the usage agrees with that of every-day speech.

In the preceding chapter we have defined retention in phraseology which may now be reformulated in the statement: *Retention is the persistence of the effects or results of learning.* These effects, we have seen, are of various kinds, and the conditions of persistence in the different sorts of retention are probably different. No mere retention, however, is strictly to be called memory, although some form of retention is involved in memory. There can be no memory without retention; but there is much retention without memory. *Recall*, we have already noted, is the reproduction of what has been learned, or the recurrence, in thought, of what has been learned previously. In other words, recall is the revival of knowledge. Now recall covers a wide range of reproductive processes, among which is *remembering*. All remembering is recalling; but not all recalling is remembering. It is highly important to distinguish the more limited fact from the more general, by different names; and the use of these two names in this way is the usage which has been fairly well established in English.

By *remembering* we mean *recalling with recognition*. If there is no recognition of what is recalled, the recalling is not remembering. In the preceding chapter we have dealt with recall, which in many cases did involve recognition; but since the recognition was not an essential factor for the discussion and may or may not have been of importance in the various cases, all these cases were dealt with under the wider name of recall.

Recognition involves a time factor, of reference to past experience. The knowledge that is remembered is not merely revived; it is consciously past-dated. I may reproduce "twinkle, twinkle, little star" without any reference to the past. This is mere recall. On the other hand, I may be conscious, in recalling the verses of them having been earlier learned, or I may be conscious of some past occasion or event connected with my actual learning of them. If, in any such way, this "pastness" or "againness" enters into the recall, then we may properly designate the recalling as *remembering*.

In most of our uses of knowledge, remembering is not necessary. Remembering is by no means the characteristic form of recall in practical life. It is possible that in the lower animals there is no remembering at all, but merely retention and recall. We cannot be certain of this, but it does seem as if the mental plane of the rat's life, and the dog's life, is that which human life would occupy if there were no remembering. The dog appears to recognize his master; but in a vast range of human conduct, the same superficial appearance of recognition occurs, without any actual recognition at all. On the other hand, it may be that the animal does have perceptual recognition, but not the recognition of what is recalled. In other words (and this is the most plausible supposition at present), it may be that remembering is restricted to human beings, and that the only sort of recognition of which the animals are capable is perceptual recognition.

By perceptual recognition we mean the recognition of things or materials presented anew in perception. For example: I have seen some person one or more times, and, on seeing him again, I recognize him. I have heard the voice of another person a certain number of times, and now, even over the telephone, I recognize the voice. The previous perceptions have left their effects; there has been a form of learning during the perceptions; and the retention of these effects furnish the basis of the recognition when the materials are again presented. We have seen in the preceding chapter that perceptual recognition may furnish evidence of the retention of preceding learning, even when evidence through recall is lacking. Perceptual recognition may occur; that is, even though remembering is impossible.

We have solicitously used the term *remembering*, and not *memory* in the immediately preceding paragraphs. "Memory" is a somewhat ambiguous term, even in the restricted psychological usage. It means not only the abstract fact of remembering; it means also the power or the abstract possibility of remembering. Since, however, remembering is not possible except on the basis of retention, the possibility of remembering includes retention. Obviously there should be two terms: one for the abstract fact or remembering (just as "recall" is the abstract term for recalling) and another term for the total possibility of remembering; that is, for the specific form of retention which makes remembering possible. This memoric[1] retention, it is clear, is something different from retention in general. Unfortunately, we do not have two unambiguous terms, and hence we use the term "memory" for the sort of retention which makes remembering possible, but carefully avoid using it for the numerous cases of retention which do not furnish that possibility, and for the cases in which there is doubt of the possibility. We should not, for example, speak of memory in animals, since we have no reason to assume, at present, that animals remember.

We have restricted our discussion of the relationships of the terms "remembering" and "memory" to recall, ignoring the reproduction of skills. This we have done deliberately. It seems probable that remembering is not involved in the simple reproduction of a skill except as knowledge is involved. Remembering can enter into my performance of handwriting; but in that case, I have, in addition to the reproduction of the writing process, ideas of the past occasions on which I wrote, or of the circumstances in which I practiced writing. Without these ideas, there is no remembering. In short, remembering seems to be exclusively an ideational process. This point is perhaps a matter for further analysis; but, in the meantime, it seems expedient to discuss the topic in what is certainly its major aspect: the remembering of knowledge.

In discussing the adequacy of memory and faults of memory, and the question how improvement in memory is possible, if it is possible, we are discussing, as the preceding section indicates, a particular field or phase of learning. We are not concerned here with learning for retention of

[1] We deliberately introduce the new word *memoric* here, because there is in English no established word which signifies *pertaining or related to memory*. The pedantic term *mnemonic* is sometimes used as we here use "memoric," but the usage is inaccurate, since "mnemonic" strictly means "assisting the memory," or more generally, pertaining to methods or devices for improving or aiding remembering.

all sorts, but solely with the learning for the specific sort of retention involved in memory, the sort of learning which makes remembering possible.

A man may have what would be called a "very poor memory" and yet may have excellent learning ability, retention and recall, of other sorts. He may, for example, by study and observation acquire a wealth of knowledge about various topics, which he may retain in form available for use in the construction of dramas, novels or even scientific treatises. He runs the danger, however, of putting out as new something which is really old and familiar. The novelist may have read the works of other men, and may retain the formulations as well as the plots of their novels, but not remembering them, he may reproduce them so literally as to be judged guilty of plagiarism. This, in fact, frequently occurs through lapses of memory in authors who have memories which are mostly "good." Similar dangers beset the scientific man who may have excellent retention of certain useful types, while having poor memory.

We are assuming that there is such a thing as "good memory" and that there is "poor memory." Certain individuals do seem to be less efficient, in respect to memory, than are other individuals. Whether the memory of a given individual may be capable of improvement, or of deterioration, is a question of some moment.

To this question contradictory answers have been given. Psychologists have tended to doubt the possibility of improvement or deterioration of memory, except in so far as changes in general health and in methods of learning may affect retention. Popularly, however, the belief persists that there is a possibility of improvement in memory, aside from these factors. This popular view has been fostered by certain individuals who make a profit from the sale of instructions and the administering of training designed to "improve the memory." Which answer is correct, or whether (as would seem possible) both answers are correct, can be decided only after we have determined exactly how a "good" memory differs from a "poor" memory.

It might be assumed that the adequacy of memory is proportional to the quantity of materials which can be memorically retained. The better the memory, if that be true, the more it holds, or the more it can hold available for remembering. The "well-stored" memory in this case would be merely the memory in which a large amount is "stored." Improving the memory would then be analogous to increasing the storage capacity of a reservoir. A consideration of the requirements of practical life, however, convinces us that this merely quantitative aspect of memory is not its "goodness" in any marked degree.

It is possible that the inadequacy of memory is due to its quantitative limitations. Some persons, it is possible, do not remember enough things or do not retain them long enough. These individuals would be mental defectives; and it is possible that some actual mental defectives are of this type. On the other hand, it is possible that some individuals are hampered by too inclusive or too retentive memories. They may remember too much or may retain (memorically) on the average too long.

The majority of persons, exclusive of those who are really pathological cases, do not, so far as we know, suffer from quantitative characteristics of memory. There are very probably considerable differences among individuals whom we would consider as "normal," but we do not know that these differences are of practical importance. We have no final evidence on this point, it is true; but in the absence of evidence we may well assume that the quantitative difference is not of great consequence for the majority of individuals. The person whose memory should be quantitatively improved, if such improvement be possible, would undoubtedly benefit by the addition of some things to his store of memory, or by their longer retention; but he would just as surely suffer from the addition or longer retention of disturbing or bothersome things. The balance in any particular case would determine the actual benefit. Since, however, we have no reason to believe that an individual's memory can be quantitatively improved, except in so far as the general health factor is concerned, and since, even if we should assume the contrary, we have not the least information as to how improvement in memory might be brought about, speculations may as well be laid aside.

The actual problem of improving memory is quantitative only as regards the retention for particular items of knowledge which are known to be important. It may be important, for example, for an individual to remember the license number of his car for a year. His street address he needs to retain, accurately recallable, for a longer time. Methods of increasing the retention of particular things are important means of improving the memory in a real sense. These methods, as we have shown in preceding chapters, are methods of learning. On the other hand, too long retention of particular items is a weakness of memory. Often, one retains the number of his car license too well; and after the annual new assignment of numbers under our stupid license system, one may waste valuable time looking for the wrong number. Much of our information requires to be retained efficiently for a short time, and then must be eliminated. In respect to particular items to be retained for a limited time, as well as in the general case of materials which should not be retained at all, forgetting is at least as important as remembering.

The problem of adequacy of memory is complicated also by the fact that other forms of retention are of vital importance in life. Memoric retention is wasteful, if mere recall-retention will satisfactorily handle the item. Even for the historian, who must retain for recall a vast number of dates, it is needless and perhaps disadvantageous to retain them memorically. Similar considerations are valid for other topics and for various phases of life. Some items need to be committed to recall-retention.

A distinction of still greater importance is that between recall-retention and retention of the more general sort, which is merely an adequate basis for further learning, and which may therefore be denoted as basic retention. The importance of this distinction is strikingly evident in the case of learning of disadvantageous sorts. Many stammerers, for example, have learned to stammer through the formation, during youth, of thought processes which cannot be recalled later. The results of the learning involved in the formation of these thought processes are retained, nevertheless, and maintain the stammering habit. There is in such a case no recall-retention, but there is basic retention. In the therapy of such cases, it is sometimes possible to aid recovery by relearning the original thought processes, the more complete retention being then more readily abolished as a whole than is the basic-retention alone.

In respect to the useful products of learning, the same distinction can be of importance which is particularly evident in the case of skill. Much knowledge is acquired in the process of learning even the simpler skills; but the most useful end of the process is that in which the knowledge is eventually abolished, and the skill, which is, in part, the result of the knowledge, is retained. During the learning of swimming, for example, recall of the instructions and purposes, and of the detailed ideas helpful in forming particular strokes, is advantageous. This recall, in connection with the learning practice (or rather as a part of the learning practice), produces results which are retained for a long period of time. But the bits of knowledge themselves may be usefully forgotten after the skill is established.

The problem of adequacy of memory, together with the larger problem of the adequacy of retention of wider sorts, is therefore primarily a problem of selection, secondarily a problem of technique of learning. If there are other phases of the problem, they are not yet apparent.

What particular items of knowledge should be selected for memorization? To this question no general answer is possible, except this: That one should memorize the things which are important for one's particular life purposes. The vocational demands of life necessarily come first, for everyone has to have a vocation, through which the necessities of life are provided. Even the social parasite has a distinct vocation through the exercise of which he robs society. The selective demands on learning made by the vocation of the bank clerk, the seamstress, the air pilot, the baker and the physician are of course different. In particular, the memory demands are different. The first thing one must decide upon, from the point of view of vocational learning, is the vocation which is to be followed. Second, one must decide what skills, what recall-retentions and what memoric retentions are required by that vocation. As one proceeds in the selected acquisition, it is well to know the periods of retention which are required for the various skills and items of knowledge. Finally, the most efficient method of learning these various things must be determined.

In seeking to increase the efficiency of memory for those things which we are apt to be in need of remembering, we may find it necessary to cut down memoric retention. If one does not remember the things which are essential, or does not retain them long enough, one should note the nonessentials which are remembered, and search for ways of cutting down this useless expenditure. Memory may not be a fixed quantum for the individual; nevertheless, the quantitative expenditure in learning one topic may, and usually does, limit the expenditure in other directions. All learning requires energy.

The relations between knowledge and skills are somewhat peculiar. Expenditure of energy in acquiring skills may actually benefit the acquisition of knowledge. This is, of course, apparent where what is being learned is a composite matter of skill and knowledge. In this case the rule works both ways as we have earlier seen. The acquisition of knowledge may be an actual condition of the acquisition of certain skills. In cases where the individual's task is primarily the acquisition of knowledge, and little acquisition of skill is involved, as in the study of history, there is a benefit in the acquisition of skills quite unrelated to the knowledge. Hence the value of games and sports for the student or the "mental laborer" generally. Obviously the main benefits are the results of the muscular exercise, the effects of the circulatory organs, etc.; in a word, the effects on general health. However, mere muscular exercise does not benefit one as sports do; and the relationship of the sports to the general health is more complicated than appears at first view.

On the other hand, there is no evidence that the acquisition of knowledge unrelated to a particular skill is of any benefit to the acquisition and retention of that skill. That the mechanic, the prize-fighter or the air pilot, gains anything whatever in learning or retention in his particular field through the study of history, mathematics, literature or any other topic not directly contributory to the practice of his art is highly improbable. It is indeed possible that such study is a detriment, until the individual has reached the maximal skill necessary in his art. For this reason, measurements of "general intelligence" are of no value for the determination of the degree of ability to acquire an art, except for the sorting out of those of the very lowest "general" intelligence, who may be, and in some cases are, inefficient in the learning of almost everything. Among individuals of the medium grades in average learning ability we find the most surprising things. Some "morons" make excellent mechanics, chauffeurs and air pilots. Others do not. In some of the fine arts, persons of low "general" intelligence (as measured by the common "intelligence tests") seem to succeed very well; but success is apparently dependent on their expending the major part of their efforts on the art, and avoiding serious learning in other directions. Many who succeed well in professions and in scholarship of various kinds would turn out to be morons if they were rated by "intelligence tests" which should give proper weight to learning ability such as required in the mechanical arts or the fine arts. The variability in aptitude for most arts is perhaps as great among morons as among those of higher "general" intelligence.

Conflict in learning is found not merely in matters of knowledge but in the learning of skills as well. Rats in the maze do best when learning one maze at a time. College students preferably study one language at a time; and this fact is especially noted in the acquisition of the elements of the languages. It is possible that if, instead of pursuing four college subjects throughout a year, a quarter of the year were devoted to each subject, the efficiency of learning would be greater. This, however, cannot be predicted in advance of experimental determination, since, as we have seen, the distribution of learning periods may in itself promote efficiency of learning. What the proper distribution is for collegiate subjects, and how far a loss in this respect would offset a gain due to elimination of conflict in learning, can be determined only through actual experiments in this direction which have never been attempted; and we cannot infer the results from simple laboratory experiments, although these laboratory experiments have furnished the bases on which the further determinations are possible.

It has been popularly assumed that a change of work is beneficial for learning. This is at present an unfounded assumption. No evidence on this point has been secured. There is no doubt that relief from particular forms of learning work is important, and that there are other factors involved in the efficiency secured by proper distribution of practice periods. The particular use made of the free time intervening between practice periods is another matter. We suspect that any use made of it for learning of any kind interferes with the full efficiency of the practice periods, although we must make the exception that practice in learning knowledge may not be injured, and may even be benefited, by the use of the free periods for the acquisition of certain skills.

The practical problem of learning, however, does not involve the learning of one particular topic of knowledge only. Other things have to be learned, and life is not only short, but the period of life during which learning is most efficacious is still more limited. It may be true that for the learning of arithmetic alone, with properly distributed practice periods, the use of the free time for rest and sports only would be the most efficacious method of procedure, so far as the arithmetic is concerned. There is, however, pressing need for the learning of other things. By using the intervening periods for learning these, the study of arithmetic may be made less productive; but the gain in the other things more than offsets this loss. What we have to find are the most advantageous combinations of learning during particular years of life.

The determination of the exact topics of learning which are essential for a given vocation, and the exact way in which the learning of these may best proceed, is a difficult problem. The problem as to what other forms of learning, beyond the vocational, are important is a still more vexing one. This, however, is the problem of what forms of culture are most worth while, a problem which lies beyond the scope of the present volume.

There is a still further problem of selection which is of considerable importance; namely, the selection from a given specific topic of those details which are to be memorized or otherwise retained and those which are to be eliminated. In regard to any coherent but complex item of knowledge, this is a vital consideration.

Let us assume that it is important, for some purpose, that a motion picture which has been seen shall be memorically retained. It may be said that if complete forgetting of a film is possible, this is desirable, since a film can be seen again with as great satisfaction as on the first view, if in the meantime its retention is so completely abolished that it is not even recognized. The results of reading certain literature would doubtless be more satisfactory if retention could be completely prevented. Further advantages would be derived from the release of memory from a mass of junk. Such abolition is not possible in all cases, however; and for certain purposes individuals often value the memoric retention of motion pictures. This memoric retention is, of course, maximally important for the scenario writer. The question is (aside from the recognition retention, which cannot be easily controlled): What details should be memorically retained and which non-memorized?

The answer for each particular case must depend on the particular materials and the particular purposes determining the usefulness of retention. That there are important limits in any case is obvious. From a purely social consideration, the person who cannot report on a motion picture without going into minute details of incidents involved is a bore. A useful purpose for every person, of course, is to avoid being a bore.

The foregoing considerations of utility are of wide applicability to knowledge, quite aside from the question of personality traits. The abstracting of the essential points for memory, and the elimination of the non-essential points, are important points in the acquisition of knowledge. The technique of this process is one of the major features of instruction and learning in any province of knowledge, and cannot be simply imparted.

It will be objected that the adequate reporting or the recalling or the remembering of details is not the whole objective in analyzing, abstracting and summarizing. A person who reports acceptably on a motion picture, for example, may very well be able to give a detailed account, but judiciously refrains therefrom, and reports only the features which are important at the moment. This is true. The retention which is necessary for one purpose is more than that which is essential for other purposes. Persons who report too much do not always retain too much. There is, however, a constant tendency for the one who retains too much to report or remember too much. One who finds himself constantly remembering too much should always suspect that he is memorically retaining too much, and should consider what can be done about it.

We may consider separately the problem of fixing in memory items which should be retained, and of eliminating those items which should not be retained. Obviously, there should be procedures which are applicable to mere recall-retention as well as to memory-retention. We may suspect, moreover, that there are certain differential procedures through which we may secure recall-retention without memory-retention; but our psychological analysis up to the present time has not revealed these procedures. We may consider, therefore, the general procedures which are apparently applicable to memory-retention and to recall-retention.

1. One procedure important for memorizing is the going beyond the mere *thinking of* the item, and thinking *about* it. This thinking about it involves the thought of the relations of the item of knowledge to other items. Some "memory systems" emphasize this procedure by setting up an arbi-

trary system of knowledge to which all further items are related. Such systematization involves a considerable amount of retention beyond that which is essential, and its utility, while perhaps demonstrable in particular instances, is low. The adequate procedure is the integration of the item with items already known in the same topic. Memorizing the technical name of a certain ant which is enslaved by other ants may be difficult as a simple act of association. Relating this variety to the variety which enslaves it, and to other varieties which are enslaved in different ways, by actually thinking these relationships, is the proper procedure. If it is important actually to remember the name of the variety described (which it is not, for most persons), the proper procedure is the analysis of the different ways in which dominating species of ants are related to subservient species. In this framework of knowledge the names of the several varieties may eventually be caught. On the other hand, the morphological relationships, or other life relationships of the various kinds of ants, may serve as an equally good framework. The best framework is in the synthesis of all the facts of the various relationships of different species which the student of ants eventually acquires. In other words: to best retain details in any unified field of learning, we must study that field, not as a collection of isolated items, but as items involved in specific and complicated relationships. Only in this way can adequate memory for the important details be secured.

2. In this process of study, desire to learn is an important factor. Learning may occur without desire; but for efficiency the desire is indispensable. The desire awakens interest and motivation of complex sorts, which are the means of keeping the learning process up to an effective pitch. The desire may be aroused in various ways. In some cases, knowledge already acquired makes the topic initially interesting, and the interest arouses the desire which still

further heightens interest. In other cases, the regressive spreading of desire is the important factor in its attachment to the topic. One may, for example, desire to make a living, and may believe that learning a certain topic offers plausible hopes of attaining that end. From the object of making a living, the desire spreads to the topic as a means of making a living. The desire to be effective must always attach eventually to the topic itself, and, in the optimal cases, becomes independent of the original desire. For this reason people keep on working long after the necessity has vanished. For this reason also, persons who take up a vocation temporarily do not make the progress in it that is made by those who consider the vocation as permanent. Whether it eventually turns out to be permanent does not matter. On this account men are preferred to women in all lines of work for which men can be secured. The actual ability of the women may be no less than that of the men; but the women, in too many cases, consider the vocation a temporary one, hoping to escape from it earlier or later by marriage. Men, on the other hand, know that there is no such escape.

3. The third important thing in learning a topic is to keep everlastingly at it. Persistence cannot be taught, but it is essential. Discouragement is fatal. This applies to retention of all sorts, of course, not merely to memoric retention.

4. Constant evaluation of progress is to be avoided. This is destructive in many cases through producing discouragement; but it seems to produce disastrous effects in other ways not easily charted. The merchant who takes stock daily cannot do business efficiently. Neither can the student make adequate progress if hampered by constant stock-taking. Many college students and persons in other walks of life who come to the psychologist for help, having "gone to pieces" in their work, are suffering from the keeping of too keen a check upon their progress; by which they

not only entail upon themselves worry and discouragement, but are actually unable to make the progress of which otherwise they are capable. Progress should be checked; otherwise the student may be pursuing a hopeless trail; but these checks should be made at infrequent intervals, at times when there is a breathing spell from actual work. The student of any type, at these stock-taking periods, should consider his whole situation. His objectives should be reviewed; his progress toward the objectives estimated; and in the light of the review and estimation, the methods which have been employed may be evaluated. Then the ideals or objectives should be newly emphasized in thought, and plans made for work for the next period, which may be a certain time-period, but preferably the period of a course of study, or a definite epoch of life, circumstantially determined. In metaphorical terms, we may point out that it is useful to the traveler at times to look forward to his goal, and look back over the progress made. Most of the time, however, he should be giving attention to the business of traveling the road.

Ideals, purposes and plans are the indispensable conditions of effective learning. These, however, do their work best when let alone. Constant purposing, constant planning, constant determining of ideals are damaging. These important operations should be undertaken at specific times, with reference to definite succeeding periods, and then dropped from thought. This is a sovereign prescription, often hard to follow. The aid of the psychologist is extended in particular cases through making the prescription, and then helping the patient to follow it by means adapted to the specific conditions. The prescription can be formulated; the method of enabling the learner to take it cannot.

5. In spite of all these methods, there is often need of memorizing, at a particular time a particular item, which may not be completely provided for by the progress the

student has made. For these emergencies, there are certain methods available. The chief among which is the fullest attention at the time to the item to be memorized; second in importance to which is *negative practice.*

That an item which is perceived or thought with full attention for an adequate length of time is better retained than is an item which receives only partial attention is a commonplace of popular knowledge. The difference in the two cases is the difference between integration in which the fact to be memorized is dominant in the total thought or the total perception; and the integration in which something else is dominant. This difference has been picturesquely described as the difference between the item occupying the "focus of attention," and its occupying the "fringe of attention." This is an important matter. An equally important matter, not sufficiently recognized until recently, is the fact that there is a time-limit of effective attention.

Holding the attention on the item by the employment of subsidiary means is often useful. In fact, the actual benefit of the various artificial memory systems employed is not altogether due to the mere integration of the item with other items; the mere prolongation of attention to the item, which the application of the system involves, is also effective. This prolongation beyond a certain point, however, is actually destructive, making later remembering more difficult.

The proof of this inhibitory effect of over-prolonged attention is found in the efficacy of negative procedure. If the name of a place, or of a person, needs to be retained, success is promoted by giving full attention to the name for a moment, and then ignoring it as quickly as possible. In the case of a name which one has been persistently unable to remember, in spite of positive practice, a single negative effort will sometimes do the work which long positive practice has not accomplished. At a time when the name is perceived, or brought into thought by some accidental means, it should be attended to for the briefest possible time, and then a strong effort should be made to forget it. It is true that in many cases this effort prolongs briefly the attention to the item, and it might be objected that the memoric value of this procedure is due simply to this additional attention. That this is not the true explanation is demonstrated by cases in which there have been previously repeated attempts to fix the name in memory, by positive practice, without success. In such cases, one or two applications of negative practice (effort to forget) will succeed, although the prolongation of attention involved is slight, as compared with the attention which has been given in the previous and unsuccessful positive attempts.

There are, therefore, two "fixing" factors available: one, the effects of full attention, briefly given; the other, the effects of effort to forget. In cases of new learning, the two may be effectively combined. Where there has been previous prolonged positive practice, without success, the further attention given to the item by the negative procedure does not matter much one way or the other, but the effort to forget matters much.

In applying the negative method, it should be borne in mind that the involuntary lapsing of the item from attention is not efficacious. The item must not be *allowed* to lapse; it must be voluntarily *put* out. Probably in this volitional process is the real secret of the effect.

The effect of negative practice would be unintelligible from the orthodox point of view which regards learning as the mere increase in the tendency to a given response that is assumed to be produced through the response itself. As has been pointed out, however, a response may increase or decrease the tendency to its reproduction according to the ideational and affective features included in response. The effects of negative practice, as described above, are in thorough accord with the fundamental principles of learning as we now understand them.

The nature of "effort" is complicated; and it is not necessary to enter into a psychological discussion of that topic here. It is evident, however, that positive effort is in general detrimental to learning. In other words, the effort to learn interferes with learning. The effort to forget, on the other hand, is equally detrimental to forgetting, that is, it assists learning. From this another practical rule, of especial importance, is deduced. In case of failure to remember, never make an effort to remember, unless the remembering of the particular item is of more importance than the damage the effort can produce. In the vast majority of cases, the adequate procedure, when some item cannot easily be recalled, is to dismiss at once the attempt to remember. In this way, remembering at a later time will not be prejudiced, whereas the effort to remember not only reduces the probability of remembering the item later, but also may make its effective retention impossible even after further learning. In a great many cases, the persistently non-retainable items are non-retainable because at some time, when the learning had been insufficient to produce adequate retention, a prolonged (and perhaps successful) effort to remember the item had been made.

The application of the negative method may be made in a variety of items other than names. Whether similar application can be made to the retention of skills remains to be determined. The avoidance of effort to remember is an important matter in every province in which memory and remembering occur.

Neglect of the fact that adequate memory for practical purposes is selected memory for pertinent topics and items has led to much confusion of the public in regard to the whole topic and laid many persons open to exploitation by promoters of artificial memory systems. Striking demonstrations of "freak" memory ability presumed to be attained by a certain system have passed as proofs that the system is a useful aid to memory. The ability to read a column of a page in a telephone directory, and then recall the names and numbers correctly, is an amazing stunt; in some respects it is as amazing as that of the juggler who keeps three china plates and seven oranges in the air at once. But who wants to remember a telephone directory? For most of us, the cluttering up of our memories with such items would be disastrous. If there is any vocation in which memory of such topics is necessary, the individual engaging in that vocation might do well to investigate the system on which the stunt is supposed to be based, and determine whether the stunt really depended on an exceptional ability for that stupid type of memory or whether the system is actually a help. The probability is that any artificial system is a hindrance in most of the lines of learning which are practically important.

A vocation which might seem to require learning of a type similar to that of memorizing lists of names is that of the bank teller, who really does perform astonishing feats of memory which are essential for his business. You may be one of thousands of the bank's clients. You may go to the teller's window at rare intervals; but the teller recognizes you when you do go, and remembers your name. If, after being introduced at a new bank, you do not return for some months the teller may not recognize you. If you go several times within a short interval, the teller may not recognize you the second or third time until he has looked at your signature. After a few visits, however, he will be able to recognize you, and remember your name after an interval of several years.

This sort of accomplishment is important for the bank teller and for men following certain other vocations, such as hotel clerks and politicians. It would not be useful for men in many other vocations. How does the bank teller acquire the facility? He not only learns people's faces and names quickly; he has learned to learn them quickly and accurately. Does he use an artificial memory system? I have never heard of a teller who proceeded in that way. Some tellers do make use of integrative aids. One has reported, for example, that he makes a point of engaging a newly introduced client in conversation, thus knitting the name and face into a system with other information about the client. This procedure is capable of being scientifically elaborated and made more effective.

Actually, there are great differences in ability of different tellers in this sort of memory. Each teller improves with experience; in other words, there is a learning of learning. This might be taken as supporting the popular notion of a "faculty of memory" as a mysterious "mental organ" which can be trained as such. The objection to this formulation is that we would need, in that case, to assume an innumerable number of "faculties of memory," since the increase in the ability for this sort of memory does not bring with it any increase in other sorts of memory. The probability is that the acquisition of ability in this particular sort of memory actually brings with it a loss in memory for other sorts of materials.

The teller, of course, does profit by the common procedures of memorizing, other than integration (2, 3, 4; above). He selects this particular sort of memory as essential; he strongly desires, in his early training, at least, to succeed in memorizing names and faces accurately and speedily; and he keeps everlastingly at it. If he worries about it, the worry interferes with his progress. He eliminates, doubtless, a great deal of memoric work that men in other lines carry on. Further than this, we have little definite information as to his technique.

Memory for isolated items under quite different conditions was required of street-car conductors before the pay-as-you-enter system was adopted, and is still required of railroad conductors on certain local runs. The conductor, while taking fares, must memorize each face, else he may either ask passengers for fares a second time or omit to take the fares from others, and either error is apt to get him into trouble. This memorization, however, is a short-time affair, for one trip only, and must be eliminated for any longer period, or serious confusions will occur. Differences in ability have been noted. Some old-time railroad conductors were noted for their astonishing ability in this line. The interesting point is that practically every man who had the other abilities, required for success as a conductor, has been able to acquire this particular learning ability to a sufficient degree. As in the case of the bank teller, selection of the task, incentive to master it and sticking to the job are the main determining factors.

Psychology has indeed a great deal to offer toward the solution of the practical problem of memorizing the items which it is important to retain. On the converse problem, the deleting from memory of the irrelevant and the unnecessary, psychology has had but little to contribute. Yet this is undoubtedly as important as the other problem. We have now reason to suspect that deletion or de-memorizing is, in fact, the most important condition of the improving of memory. The reason why we are not able to supply applicable principles is that until very recently the importance of de-memorizing has not been appreciated. We have indeed just begun to develop techniques for the abolition of bad habits, mostly of the skill type, and it is probable that with further time and the progress of experimental work, we shall discover a technique for the prevention of learning, as distinguished from the undoing of its effects, and that specific methods of preventing memorization will be made applicable. In the meantime, it may be pointed out that the general factors which produce learning should be avoided as far as possible where prevention of learning is desirable. In so far as we can prevent integration of items with existing knowledge, in so far as the desire to learn can be replaced by aversion, in so far as the ideal of not retaining materials of certain types can be installed, non-learning is assisted. Adequate selection is, after all, the basal procedure for both facilitation and inhibition of learning. While selecting the topics and operations which are to be memorized, we may also select the topics not to be memorized. Identification of topics and classes of items which are not to be a part of one's "stock-in-trade" is the essential preliminary step; and this in itself is a considerable step toward avoiding their memorizing. The attitude expressed by: "These are not the things I remember"; not to be constantly maintained in thought, but to be clearly consciously taken at critical times, is the key-note to success in avoiding memorization.

We should not leave the topic of memory without calling attention to the progress which is continuously made in the providing of substitutes for memory and for recall-retention, which more and more relieve us of the necessity of remembering, or even recalling, considerable classes of items. Among these mechanisms of relief are telephone directories, card indexes and innumerable works of reference. In so far as any items are not essential parts of our stock-in-trade, and are needed only at rare intervals, we turn them over to these devices. Instead of memorizing vast numbers of items, some of which may be urgently needed at any moment, but of which the need cannot be predicted, we learn to use these reference mechanisms. With the increasing complexity of knowledge and the limitations of human retention, the development of these substitutes for memory and retention becomes more and more pressing, and learning adequately to employ them becomes more and more essential.

CHAPTER IX

PERSONAL AND SOCIAL ADJUSTMENT

If an individual's responses to his environment produce results which on the whole are beneficial to him and to his social group, we say of him that he is *well adjusted.* If, on the other hand, the individual's responses are harmful in their results, or if they are adverse to his welfare or to that of the group, we say that he is *maladjusted.* The responses of the well-adjusted person are adequate to his environment while those of the maladjusted person are inadequate. Maladjusted persons are out of harmony with their environments. *Readjustment* is the process of establishing harmony between the maladjusted person and his environment. The problems which are involved in the recognition of maladjustments and in readjustments are especially important and especially difficult, and their solutions require solicitous application of the facts and principles of habit making and habit breaking.

Maladjustments are so numerous in type and in sub-type that it is impossible to list them. Many attempts have been made to classify them under a few headings, but the classifications are full of difficulties. In dealing with problems in any field, classification is a useful preliminary procedure, if we do not lose sight of the fact that any classification is arbitrary, determined by the purposes of the classifier, and receives its importance, if any, from the further use made of it. However useful a given classification may be for a given purpose, we must always be prepared to reclassify if we need to deal with the problems in a new way.

For present purposes, we may classify maladjustments under six headings, as follows:

1. Physiological maladjustment. Incapacity adequately to digest food or to maintain at the proper level any other bodily functions which are requisites to the maintenance of life.

2. Structural maladjustment. Blindness, deafness, lameness and muscular weakness of certain types come under this heading.

3. Primary mental maladjustment. This is feeble-mindedness, or amentia.

4. Pathological mental maladjustment. This includes insanity, epilepsy, etc.

5. Educational maladjustment, involving faulty knowledge, or lack of knowledge, of the environment and its features, and of the results of simple responses. Faulty *praxis*; that is, faulty method of practical life is the result. Under educational maladjustment, we may class also many defects in skill.

6. Emotional maladjustment. Failure to correlate desires, feelings and ideas with perception and automatic action in an efficient way.

Looking over these categories (which are not logically complete, but which may serve our preliminary needs), we note that they can be summarized under two heads: Non-social and Social. The first class includes the adjustment of response to the physical environment. This may include all of (1) and much of the other classes, but does not exhaust them. The club-footed man and the blind man are unable to respond properly to their physical environments, but they are also hampered in responding to other persons. The insane man is incompetent to utilize the objects which surround him, but he is also handicapped in dealing with his fellows. Similar considerations apply to the feeble-minded, the ignorant, the unskilled and the emotionally aberrant.

Readjustment of the person who is physiologically inadequate is primarily *medical treatment*. Pathological mental conditions fall in the sphere of work of *psychiatry*. The other types of maladjustment are basally matters of habit and learning, and hence require psychological treatment. Yet it is true that, in the strictly medical cases, the modification of the patient's responses by psychological treatment is often the vital feature of the therapy, and it is probable that, even with the insane, something could be done by psychological methods. The structurally defective person may be primarily in need of surgical treatment or mechanical aid. His readjustment may be effected mainly through fitting him with spectacles or a wooden leg. Nevertheless, his adjustment to his environment by means of these aids may be in part psychological. Mental conditions which cannot be cured, or even alleviated by any means at present known, may be rendered less troublesome by readjustments which are possible.

In the case of the man suffering from digestive trouble, the basic part of the treatment may be medical. He may need "medicine" or he may need surgical treatment. He may need a radical change in his diet or rest in bed or one of many other forms of treatment which may be arranged for him. In these cases, he is modified by non-learning methods, or else the environment is changed. On the other hand, he may need, above all, a change in his responses to food and to other features of his environment. He may need to learn to select his food better. He may need to learn to eat at regular hours, to avoid work or excitement immediately after eating. These are educational learning factors.

The consideration of the sick man brings us at once to a problem of readjustment which is vastly more important than that of classification. Is adjustment to be secured by changing the individual, so that he now makes adequate adjustments to patterns to which he formerly made inadequate? Or is the environment to be changed, so that the habits of response which do not fit the present environment will fit the new one?

Medicine now recognizes the importance of both of these factors, but in the past the importance of changes in the environment has been insufficiently recognized. In fact, medicine began in the assumption that the environment is fatally fixed. The sick man cannot cope with it; hence we must do something to him which will make him able to cope with it. From this point of view were developed the long list of magical procedures and instruments beginning with ceremonies and incantations and proceeding through blood-letting and the nauseous armament of the pharmacopeia.

Gradually, however, a distrust in medicine developed, assisted by the disquieting success of rival systems, and of "schools" which administered no drugs and let no blood. The present notion seems to be that we must first adjust the environment to the sick man. Later, we may possibly re-adjust the sick man to his old environment, or we may find him permanently to require a different environment. With the passing of magic medicine, has come, however (partly as cause, partly as effect), a greater ability to change the man himself, either by direct action as in surgery, or by a list of drugs and antitoxins, or by a learning process, so that he may be better adjusted to an old environment.

The situation throughout the whole range of maladjustment is the same as in the field of medicine. First, the emphasis is on the changing of the individual. The world is considered as a fatal arrangement, a Procrustean bed, to which man must be fitted. Then, after the horrors of this method of treatment have become appalling, we begin to consider changing the environment, instead of the man, as being the best procedure in at least some cases.

It may be pointed out, of course, that man acquires control of the environment very slowly, and that a high degree of social organization must be achieved before any considerable change in the environment of a sick man or an otherwise maladjusted man can be made to fit the case. This is true. In a crude state of society, any seriously maladjusted person must merely perish, and speedily. Readjustment, medical or psychological, is possible only in a highly socialized group. We must not conclude, however, that readjustment is the optimal procedure, even when it is practicable.

We recognize, then, two requirements in personal and social readjustment: (1) We must determine in any case the extent to which the environment must be changed, and the extent to which the individual himself must be modified. This involves the determination of the precise relation of the two changes to each other. (2) We must discover means for the actual production of the desirable changes in the environment or the actual production of the changes in the individual, or both. Neither of these is a simple matter.

Before we consider the technique of psychotherapy, however, we must deal with the problem of the recognition of maladjustment. Here we run against the major problem of readjustment, and the one which is in far too many cases ignored. It is a commonplace fact that the physician must first recognize a disease before he begins to treat it, although the principle has been abundantly honored in the breach in the past. It is not sufficient, however, that the physician recognize a *condition.* He must know whether or not the condition is unhealthy, disadvantageous or not, and also whether it is unhealthy in itself, or merely a symptom. For example: A fever cannot be treated merely as an unusually high temperature, simply to be reduced. A yellowish skin cannot be incontinently assumed to be an unhealthy one. A blood pressure of 160 may be a symptom of an undesirable condition, or it may be a sign of health.

In social and emotional readjustment, the same situation is found. A child who, on being opposed, has a tantrum is as unlovely an object as is a stevedore covered with dust and dripping with sweat. But is either of them maladjusted? In the one case, is it a simple matter of modifying the child's response so that the tantrums disappear? In the other case, is it a matter of coating the stevedore with a preparation which will prevent perspiration? By such treatments you might indeed ruin both patients!

In medical practice it is frequently difficult to determine whether or not a given condition is "normal" or "abnormal," and if "abnormal," whether it is in itself "abnormal" or merely a "normal" symptom or result of an underlying unhealthy condition. It is even more difficult to determine the "normality" or "abnormality" of mental conditions.

Let us consider against the physiological conditions. The patient has a fever. His temperature is, let us say, 101°, while the "normal" temperature of a human being is 98.6°. From this point of view, the febrile condition is "abnormal." But let us consider the fever, not as a condition *per se* but as a symptom. Suppose the patient is found to be invaded by active typhoid bacilli. At once we recognize this as the "abnormal," the unhealthy, condition. The fever itself is the response which the otherwise "normal" individual makes to the typhoid invasion. From that point of view, the fever reaction is a "normal" response. So "normal" is it that the absence of the febrile reaction would indicate a condition so "abnormal" that the patient might have no chance of recovery. What is the adequate treatment? To render the patient incompetent to make the adequate febrile response? This would be fatal, unless, it were done by making the patient immune to the other effects which the typhoid toxins have on his system. This may perhaps be done, but we must be certain that the means adopted do not merely suppress the symptomatic response, while leaving the primary damage unchecked. We must remember, too, that the febrile result may be beneficial; and hence its suppression, except by removing the cause, is doubly dangerous. Moreover, the removal of the cause is actually a change in the environment, rather than in the reacting machinery, although the distinction between organism and environment almost vanishes where we are concerned with the response of the organism to stimulus-patterns which are included within the organism itself.

Let us go over now to the more difficult matter of the mental responses; that is, to the responses of the organism to stimulus-patterns currently impressed from without (perceptual responses) and its ideational and emotional responses.

The tantrums of the maladjusted child are indications that something is wrong, either with the environment in which the child is growing or with the child's neuro-muscular organism. But is the fact that the child has tantrums under these conditions an indication of a fundamentally unhealthy condition? If the child, under the conditions in which this child is placed, did not show the tantrum symptom, would it be an indication of a more healthy or less healthy condition? We have no difficulty in answering this question. The child who has tantrums is a hopeful case. The child who *under similar conditions*, of present and past handling, has no tantrums is a distinctly less hopeful case. However much may be wrong with the child who is subject to tantrums, the other child is in a more serious condition.

Let us look at the problem from a slightly different angle. Does the child who has tantrums behave in the same way as would a normal child if subjected to the same conditions? If so, the child is normal, the environment is abnormal. This solution is not so simple as we might think. What do we mean by a "normal" child? Here, for example, is one child, who, in charge of its regular nursemaid, has frequent tantrums. Suppose we take another child of same sex, age and general growth, who has never shown tantrums, and put this nurse in charge of it. It will, under the care of the nurse, either have tantrums or it will not. If it does not have tantrums, the same environment (presumably) which evokes tantrums from the first child does not from the second. *Ergo*, the children are different. *Ergo*, the trouble is with the first child; it is "abnormal."

But suppose a "normal" child, at an earlier age, were subjected to the same treatment as has been accorded the "abnormal" child. Would it become "abnormal" under this treatment? Would it show the same tantrums after a sufficient course of treatment? If so, the becoming "abnormal" may reasonably be said to be a "normal" response to a faulty environment. We are left in the paradoxical position of holding that the child that develops tantrums is "abnormal," but that its abnormality is the expression of, or sign of, its "normality."

All of which brings out the fact that, in considering the normality of any person's behavior, there is a dual point of view. (1) With regard to the behavior itself, abstracted from the stimulus-patterns: If fever, that is, temperature above 98.6°, is the usual thing, or if the majority of people, in all sorts of conditions, have this temperature, then fever is normal. If, on the other hand, the majority do not have fever, then fever is not the rule or norm; it is abnormal.

(2) From the other point of view, the question is more complicated. Do the majority of persons, when subjected to conditions of particular sorts (*e.g.*, typhoid infection), have fever? If so, fever is the rule for such conditions. Fever then is normal for these conditions. The specification of conditions "of particular sorts," however, involves the distinct assumptions that, under other conditions, fever may not be normal.

Are "normality" and "abnormality," in the literal senses we have indicated, important? Yes, in a certain way, but only when our knowledge of conditions is incomplete. What we finally want to know about the "normal" and the "abnormal" is a quite different matter. Even if the majority of the population were "running temperature" we should not consider fever a normal condition, except in the second sense indicated above: as normal to the state of infection of the population. We should still consider it as abnormal in the more general sense. This means that we select one set of environmental factors and one organic condition, both or either as the case may be, and we designate the behavior or the physiological results which occur in these circumstances as normal.

Any other type of behavior or of physiological processes in these circumstances is abnormal. It is against the rule, but (this is the important point) the rule is not a rule of *numbers*. It is a rule of what is, or is supposed to be, *health*, *i.e.*, the adequate or beneficial response of physiological process. In other circumstances, the normality or abnormality is similarly judged by the rule of health in accordance with our conception of health.

There may be, in medicine, difficulty in determining norms; that is, maximally healthy conditions. There is, however, a clear line of procedure in the determination. There is no doubt as to the benefits to be considered. The physiological condition which is the most healthful is that which is most beneficial to the individual himself; no other person need be considered, nor can any other person be considered in these determinations.

Moreover, there are certain accepted standards of benefit. That which is beneficial to the individual must (1) Prolong his life; (2) Increase his productivity; or (3) Increase his happiness. These standards are definite and there are no others. We may have difficulty in applying the standards, as is the case of "moderate" drinking of alcoholic liquids. We may not be able to determine whether tippling or total abstinence increases longevity or productivity or happiness;

but if we can determine these, the conclusion is certain. Tippling is hygienic or not, in so far as its effects measure up to these standards.

That the standards are not directly correlated is possible, even probable. Alcohol may shorten life but make it more happy and productive. It may possibly lengthen life and increase happiness, while decreasing productivity. In such events, we would be in difficulty, since there is no way of evaluating one standard against another.

When we consider "normality" or adequacy of response or conduct, our difficulties are far greater. In the first place, it is by no means agreed that the "normal" type of response (in the sense of that which is most beneficial) is that which is best for the individual *per se*. On the contrary, complex codes of norms are set up which have reference to the interests of other persons or of the group, as opposed to the interests of the individual himself.

Murder and theft are "abnormal" behavior, but we make no pretense that they might not be most advantageous to the murderer or the thief. They may increase his longevity, his productivity and his happiness. They often do. It is true, we seek to remedy this condition (for others, not for ourselves) by introducing penalties of an arbitrary nature. We scourge, imprison and execute, in the hope of making anti-social behavior really disadvantageous to the criminal. But we do not succeed; and the vast majority of real criminals flourish like the green bay tree.

The criminal is maladjusted. We may try to substitute readjustment for penal procedure. But what is the nature of the readjustment? Is it the making of the criminal's response more adequate to his own interests? We may tell him this is so, but we do not believe it. What we are really attempting is a readjustment which will make the criminal's behavior more adequate to our own interests.

Readjustment of the criminal can take only one course;

namely, that of making crime disadvantageous to the criminal, and then adjusting the criminal to the altered situation. This involves the introduction of ethical ideals by which the interests of the group are substituted for the interests of the individual. Then his responses are to be adjusted in accordance with these ideals, so that his responses actually are, in many respects, to his decided disadvantage. This is the exact opposite of the medical procedure, and is a type of readjustment which is quite remote from our main idea of increasing the "advantageousness" of the response. For the advantage of the individual, we substitute the advantage of the group, in opposition to which the advantage of the individual is an evil.

This substitution is, however, a source of danger. Instead of the actual advantage of this group, which is the moral standard, we may substitute the advantage of a particular faction or class; and this is immoral. Or, we may, in our ignorance, substitute an entirely fictitious advantage, taking, therefore, from the individual, but giving to no one. Much current social regulation is of the first type. Unfortunately, also, much is of the second type.

Constructive criminology is a difficult subject. The readjustment of criminals requires not merely a knowledge of how to readjust to given conditions; it requires also the assurance of the adequate conditions to which criminals should be readjusted. Neither of these requirements has been met so far as scientific knowledge is concerned. The adjustment of the individual is important, and the adjustment of the social environment is still more important, but criminology is still in the dark on both points.

Aside from criminology, there is, however, a vast field of maladjustment. Large numbers of children and adults are socially troublesome, although not in the way that is usually designated as criminal. These individuals are obviously maladjusted. They do not respond in ways which are adequate for the purposes of other people; that is, our purposes. This inadequacy may be, to a slight extent, a matter of the longevity or the happiness or the productivity of these persons. To a far greater extent it is a matter of their interfering with our plans and projects. In most cases, it is exclusively such a matter, for the low productivity of these persons concerns us only when we desire the products which they fail to supply, or when we have to contribute to their support.

In this troublesome class are the insane and the feeble-minded. It is true that certain of the insane are physically diseased, and their lives are shortened. This class may be classed as sick, along with the tubercular, the leprous and the malarial. For the greater part of the insane, it is not evident that they are shorter lived or less happy than the "normal" person. But they are non-productive; they require our support; and in other ways they annoy us and interfere with our plans. This is the essence of insanity. An insane man who is self-supporting, who interferes with no one else and who annoys no one else does not exist. The type of thought processes does not matter. The "mind" of the average lunatic is probably as orderly as that of the average "sane" man. Certainly, many lunatics are far sounder in their mental processes than many successful "men of affairs."

The situation with the feeble-minded is much the same. The feeble-minded man who can earn his own living, who does not get into the toils of the law, who does not require assistance from "social agencies," and who does not in other ways upset our plans and projects is not a problem case. He is not maladjusted. He may be low in intelligence, but what of it? Large numbers of such are citizens of great financial success, socially well esteemed.

As for longevity and happiness, the feeble-minded are well off. In a ruder culture, the feeble-minded might be killed off more readily than the intelligent; their lesser intelligence might make them more easy prey to wild beasts or to human enemies, less skillful hunters, less provident. Curiously enough, we do not know that this has been true. Except for the idiots and imbeciles (who contribute no very large part of our feeble-minded problem), the feeble-minded may have been just as "fit" for the life of the new stone age as for the present day; they may even have been more "fit" than the more intelligent type. At any rate, they seem to survive excellently today. They are also happy. Their responses seem admirably adapted to the requirements of the environment, except to certain requisites of the social environment. They are a nuisance to the rest of us, and their lack of adjustment is a failure to adjust to our purposes.

Neither the insane nor the feeble-minded can be readjusted to any considerable extent in their ordinary environments. Some of the insane, if well cared for, recover, just as typhoid patients recover. Psychiatrists of reputation do not speak of "curing" the insane. But in the cases of those who recover (usually the recovery is temporary), there is no process of readjustment. There is no learning to react differently to definite circumstances. Something has gone wrong: the machinery was temporarily (or permanently) impaired and functioned in an erratic way. Then in certain cases, the machinery is cleared, and again begins its "normal" functioning. No learning is involved.

For the feeble-minded, there is no possibility of recovery. That is what we mean theoretically by the feeble-minded: those persons in a permanent, not a temporary, low condition of intelligence. In certain types of feeble-mindedness a certain amount of readjustment may be brought about. Although the low intelligence is specifically low learning ability, still the moron can learn. By abandoning the endeavors which are most difficult, and concentrating on those things which are easy to learn, and which are most certain to be of use in earning a living, the moron may succeed financially. He may support himself and his family. He may even become a millionaire. Few have become successful in professions, such as law or medicine, but doubtless many have attained fame in art and literature by concentrating their efforts on a few simple techniques.

For the classes of feeble-minded for whom this minor adjustment is not possible, and for the insane in general, the solution which is usually adopted, where practicable, is a change of the environment. In institutions, properly adapted, the insane are in an environment in which they are "normal." Their responses to this environment do not interfere with, or distress, those of us who are outside; and since the attendants are paid for the work, and therefore dependent upon the insane, the reactions of the patients are adequate for the attendants' purposes.

In the early "mad houses," and apparently in some of the private "sanitariums," this is as far as the adjustment of the environment goes. In modern hospitals for the insane, however, a still further achievement is made. The environment is made such that the patient's responses thereto are contributory to his happiness. The responses to this artificial environment are therefore adequate in every sense except in that of being productive. The patient is adjusted as well as may be because the environment is made suitable for the patient's type of reactions.

Under these circumstances, recovery of the patient is made more probable than under other conditions. Some adjustment of the patient to the new environment is of course brought about; the new environment, however adequate it may be ultimately, cannot be at once adequate for the patient brought in from an environment in which he was responding in a quite different way. This slight and possible readjustment may contribute to the recovery. In some cases, the habits acquired in the institution, habits of responding to the environment in such a way as to avoid trouble, may carry over to the outer world when the patient is released. In other words, the readjustment to the more suitable environment of the institution may involve a re-adjustment to the outer world. In such a case, the patient, without any "recovery" at all, so far as disorderly thinking is concerned, may be "sane" when released. In other cases, the patient, "adjusted" (and therefore sane) in the institution, is again maladjusted (insane) when released as "recovered" or "cured."

Such readjustment as may occur in cases of insanity is not to be confused with recovery in the proper sense, nor with a change in the orderliness of thinking or perceiving. Mental disorder is not the basis of insanity, nor clear and orderly thinking that of sanity. Sanity and insanity are matters primarily of social expression; that is, of responses as judged by their results. A man who insists that he is Julius Cæsar or a fried egg is insane. Many sane people estimate themselves in as absurd ways, but either express their estimates in different terms or keep them to themselves. You may admit to yourself a desire to kill someone because you can't endure his face or his laugh. If you ever attempt to kill him, you will be *ipso facto* insane.

Again, your thinking may agree with the thinking of others; you then are not insane, since your thinking does not annoy other people. You think that a bit of metal tied around your neck will preserve you from drowning. You are sane; millions of people hold the same belief. On the other hand, if you think there is a snake or a mouse inhabiting your body, you are insane; other people do not think that, and your idea annoys them. Yet, in another part of the world, you would be perfectly sane today.

Institutions provide adequate environments for the feeble-minded, in which the responses of the feeble-minded are quite adequate. No one else is annoyed or disturbed. In this environment the feeble-minded may become "normal," but such readjustment does not in itself imply that they would be "normal" in the world outside. On the other hand, it is possible in the cases of certain types of feeble-minded to make certain adjustments to the institutional life which will carry over to the outside life. For this the institutional environment has to be specifically designed, and this is not in any case a major institutional service. Institutions for the feeble-minded, apparently, are useful, and will be useful for the permanent segregation, in an environment which is suitable to their types of responses, of individuals it is impracticable to handle outside of such institutions. In these situations there is no concealing the fact that the segregation is done primarily for the benefit of the larger group, the non-feeble-minded (*i.e.*, "ourselves") and not for the benefit of the segregated feeble-minded. That the feeble-minded may also benefit, as they may in properly conducted institutions, is an interesting, but additional, consideration.

Certain changes of a non-learning type in the feeble-minded themselves may assist in fitting them to the institutional environment or to the outside world. Let us consider some of the typical aspects of the responses of the feeble-minded.

Many feeble-minded persons are improvident. They are either unable to forecast the results of their immediate responses, or else the forecasted results are less effective through motivation in determining action than is the case with more intelligent persons. In many cases it is difficult to ascertain which of these weaknesses is involved, but apparently both enter into many cases. For example, it is evident to the intelligent person that, given a sum of money, the spending of a large part of it for luxuries (expensive food, drink, phonographs, etc.) reduces the amount which will be available for necessities (food, clothing, rent, fuel, etc.). The desire for these necessities operates as a motive to restraint of action along the line of desires for luxuries. The desire for luxuries may remain, but is counteracted. The desire for necessities "spreads" to the desire for economy. To the feeble-minded the situation may be logically less clear, but sometimes it is sufficiently clear, but the motivation does not occur. The immediate desire produces action uninhibited by adequate motivation. This type of response, in the feeble-minded, is incurable. "Recovery" is not possible, and readjustment, if possible, is possible only on a minor and insufficient scale. The feeble-minded must therefore be put in an environment in which providence is not necessary. This is possible in an institution. Under certain conditions it is possible outside of institutions, but it is difficult. In so far as this type of environment is secured, the feeble-minded may be "normal" in their responses.

In another direction, providence may be rendered unnecessary by a surgical operation which changes the individual immediately, without immediate effect on the environment, although there are subsequent or indirect effects. The sexual appetites of the feeble-minded are much like those of more intelligent persons. The sexual activities of the more intelligent persons, however, are usually controlled by motives drawn from sensitivity to public opinion, esthetic or moral scruples, fear of venereal disease and consideration of the probability of conception in heterosexual relations. These motives modify sexual conduct in a variety of ways, producing secretive or clandestine relations, refraining from sexual activity, economic efforts to obtain conditions (marriage) for uninhibited sexual expression, refraining from certain types of sexual intercourse, such as seduction, prostitution and adultery, prophylactic measures against conception and vice of various kinds, such as masturbation and homosexuality, which may be more easily concealed or which offer less danger of conception or infection than does normal copulation.

Such motivations are very much less in the cases of the feeble-minded, and their sex activity, accordingly, is far less restrained and modified. As a result, the feeble-minded are no less happy than the more intelligent persons, since public opinion and moral scruples do not operate to depress them. They are perhaps more exposed to venereal infection. They do (and this is the important consideration for the larger group) propagate children more copiously.

The activity tendencies cannot be modified. Restraint (segregation in institutions) cuts down the propagation. But this is not possible for the greater number of the feeble-minded, and (although this is a secondary consideration) it reduces very materially the happiness of the patients. Sterilization, however, makes propagation impossible. An important adjustment of the individual is secured, so that without modifying his action tendencies, and without change in his environment, his responses are more adequate. Not only are his sex activities rendered harmless to the larger group (ourselves), but also less harmful to himself. For the feeble-minded, therefore, an enormously important adjustment to his environment may be secured by very simple means.

We have digressed at length from our main line of investigation in considering the maladjustment and readjustment of the criminal, feeble-minded and the insane. From this digression, however, we should have obtained illumination of our residual problem. We have shown the distinction between medical readjustment and psychological readjustment, and indicated that each may have an important rôle. We have shown that in readjustment both the modification of the individual and the modification of the environment are important, and that sometimes one, sometimes the other, must be stressed. We have further shown that the concept of "normality," of "adequate" response, is a complicated one, involving sometimes the interests of the malajusted individual, sometimes the interests of the larger group as opposed to the individual. We have disclosed the possibility that the determination of what is maladjustment, what is adequate response, from either point of view, is largely arbitrary. The reader probably does not need to be told that the standards of "normality" which are practically adopted in this, that or the other case may often be grossly erroneous; that in readjusting an apparently maladjusted individual we are really in many cases bringing about a maladjustment of an initially well-adjusted person. Let us see if we can apply these results to the cases in which the term psychological adjustment is most commonly applied.

These are cases of (1) Neuroses, and "neuro-psychoses"; (2) Unhappy individuals not included in preceding categories; and (3) Inefficient individuals. These classes overlap, but no one of them includes all the cases which the others cover.

Here again we find that personal and social standards are involved. The inefficient individual may not be unhappy. But he may be both inefficient and unhappy, and yet may not annoy other people, or constitute a burden to them. If, however, he is either inefficient or unhappy, and is a problem for other people (without being insane, feeble-minded, or a criminal), he is *neurotic* (or in more fashionable terminology "phychopathic"). The distinction here is strikingly like that between the insane and the sane. So long as he does not make other people unhappy, or irritate them, and so long as he does not make a drain on the finances or personal service of other people, the maladjusted person is *not* psychopathic. On the other hand, if his responses are not adequate to the purposes of other people, and he is neither insane, feeble-minded, criminal nor physically "sick," he falls at once into the class (a waste-basket class, obviously) of the psychopathic. His inadequacy consists, in most cases, in his inefficiency; in some cases, however, merely in his unhappiness.

The main problem of psychological adjustment, accordingly, is the modifying of the individual, or of his environment, as the case may be, so that his responses shall be more adequate in the sense of rendering him happier or more efficient, or both. The field of operation is not confined to the neurotic. On the contrary, the larger part of the field is outside the confines of the neuroses. Obviously, however, the social demands make it unavoidable that the most pressing cases which the psychologist will have to treat will be neurotic. If, eventually, the scope of operation is more largely outside the psychopathic field, this will be because of the necessity of preventing neuroses, by catching and readjusting the patients before they become neurotic.

It is obviously impossible to detail and illustrate all the types of unhappiness and inefficiency. It is also impossible, within the limits of this discussion, to analyze the total field of neuroses, determine the various types and relate them to one another. As a matter of fact, the efforts of hundreds of workers in this field have not accomplished this objective, but have merely produced a mass of conflicting, and, for the most part, puerile theories and interminable pages of confused discussion. We may reasonably assume that no such attempt at analysis is useful, or will be useful, until much further work of a less pretentious type shall have been done.

The traits which are the results of learning or habit formation, and which are definitely detrimental to the individual personally, or are socially unfortunate, may not actually be infinite in number, but practically they cannot be enumerated. We may however make up an illustrative list, which covers an important part of the range of such traits, as follows:

Depression, self-pity, self-appraisement and introspection, recurrent mental confusion, insomnia, odd sensations or "queer feelings," inappropriate emotional responses, interference of emotional expression with expression of thought, excessive recall, pathological cleanliness, superstitious impulses, associating with a wrong age-group, cruelty, obscenity, self-centered attitudes, seeking of notoriety, arrogance, callousness to the feelings of others, rudeness and insolence, bad temper and irritability, graspingness, inability to bear pain, inefficiency in work, lack of initiative and of ability to plan, dependence on others, excitability, apathy and listlessness, indolence, shyness and timidity, inability to concentrate, loss of memory, giving way to discouragement, speech defects (stammering, etc.), tics, faulty posture and movement, offensive table manners, sexual vices (masturbation, homosexuality, etc.), polygamous excitability, and sexual motor defects.

The foregoing list is merely illustrative. It is a list, moreover, of symptoms, many of which are found also in cases of organic disease and in psychoses. We are concerned here only with the traits found in individuals in whom competent medical examination shows no organic basis for the traits, and in which there is no indication of "mental disease."

Although strictly speaking, the occurrence of any of the traits which have been listed, or any one of a vast number which might be added to the list, marks the person in whom it occurs as maladjusted, no individual is described as a *maladjusted* person unless he exhibits these traits in a way which seriously interferes with his personal enjoyment of life or with his social relations. There probably is no individual who is perfectly adjusted. Almost all persons are maladjusted in certain ways which are evident to the one who studies such a person carefully. It is, of course, a benefit to any individual to overcome his detrimental traits, however minor they may be relative to his general equipment of well-adapted traits. It is the function of the psychologist to assist in readjustments of this kind. Certain persons, however, are especially in need of assistance, because they possess maladjustment to such a degree, or in such complexity, that their total adjustment is conspicuously lowered, and they have become maladjusted persons.

Neurotic individuals are those whose maladjustment is of serious import to others. There are no fixed limits, however, to the class of "neurotics," on the one hand, and "normal" persons on the other, and the "maladjusted" in various degrees who fall between.

Neurotics may be classified according to the type of maladjustments they possess, but it is doubtful whether any such classification is useful because neuroses are of infinite variability. Even if, for certain purposes, we should describe types, these could not be recognized from the descriptions except by persons who have had copious and intelligent experience with a wide range of neurotic patients. The cases are recognized by the expert, not from their enumerated details of maladjustment, but from the patterns they present.

The fact that an enumeration of details, however accurate, is not a basis for recognition of a human pattern may be illustrated from the pattern of facial expression which we call a *smile*. A smile involves certain facial changes and "sets" which we may describe; but no one attends to those details as such in recognizing a smile. Nor could anyone who had not learned to recognize smiles, and to distinguish them from other facial expressions, identify a smile from the mere description.

Psychological analysis of patterns of traits is an indispensable method of the psychologist, but he must first recognize the patterns. For the layman it is important to recognize the fact of maladjustment, through the practical results, and to refer the cure to the psychologist for identification, analysis and treatment. Further, particular maladjustments should be noted even where they occur in "normal" persons, for they can be remedied.

The acquisition of undesirable traits has been in most cases a process of learning. They can be removed, corrected, only by a process of learning. The maladjustments, in short, are bad habits. The process of readjustment is the process of unmaking these habits.

The causes which lie behind the habits are of various sorts. By "causes" here we mean the conditions or situations which have conduced to the learning or acquisition of the unfortunate habits. If the causes persist, it is, of course, difficult, if not impossible, to unlearn the habit. The function of the psychologist is to ascertain the causes of the bad habits, to remedy the causes if they still persist, and to assist the patient to break the habit. These functions can be fulfilled only on the basis of an adequate knowledge of the principles of learning.

From time to time it has seemed possible to persons unfamiliar with the psychology of learning to find a few simple causes for neuroses or less serious maladjustments, or even a single cause for all maladjustments, and by dealing with such supposed causes, to "cure" the maladjusted person. Hence we have had a succession of psychotherapies, from Christian Science to psychoanalysis.

As a matter of fact, some cases are "cured" by every such system. Where the original actual causes no longer persist, methods of practice which are arbitrarily constructed, on no scientific basis, may accidentally fit the need of various cases. One particular unlearning method which is employed in many systems is to associate, by the operation of the patient's thought processes, the patient's traits (or habits) with a "complex" constructed for the patient, or with some other arbitrary thought object. Then the patient eventually learns to think about himself and his traits in a different way, or not to think about them at all. Since thinking is the essential feature of living, the overt behavior of the patient may be then modified. Against such accidental cures, however, we must set the cases in which the patient treated by such a mental nostrum becomes much worse. Psychologists are familiar today with the numerous cases "treated" by psychoanalysis, or other arbitrary systems, and left in a condition in which the maladjustments are not only more serious, but more difficult to remove than in the patient's original condition. Suicide following psychoanalytic treatment has been frequent.

The salvation of the neurotic lies in the application to him of the results of scientific psychology. Of these results, the principles of learning are by far the most important, but the application must be based in a broad psychological understanding of the patient's situation and processes.

The process of readjustment, for any of the traits in the list we have given, requires well-directed unlearning, and in many cases, further positive habit formation of specific sort. Not only does each trait require a different treatment, a different learning process; the same trait when based on a different original cause may require a different treatment. There are no set formulas, no simple forms of treatment, applicable to a variety of cases. The treatment of each faulty adjustment must be developed for the particular nature of the trait, from the complex principles governing habit making and habit breaking.

Obviously, it is impossible in a brief treatise to discuss the method of treatment of innumerable bad habits. We shall, therefore, for illustrative purposes only, describe in the next chapter the general forms of treatment of certain habits which are more uniform in their manifestations in different persons than are most of the habits in the list above given. From these illustrations, the reader may perhaps gain an understanding of the complexity of the psychological problem and the general features of attack

CHAPTER X

THE BREAKING OF SPECIFIC BAD HABITS

ONE of the results of learning is the fixation of habits; the formation of new but relatively fixed ways of response. When a particular habit has been formed, the particular learning process is finished and gives place to other learning processes. As children we learn to lace and unlace our shoes, to button and unbutton our clothing. Through this learning we eventually form habits of response to shoes and clothing which enable us to manage our dressing and undressing with the minimum of energy, and the maximal efficiency. It is true that these habits require slight modification from time to time, but in the main, they are fixed methods of response.

In the same way, we learn to speak. This involves a long process, which may continue well into adult life. From our early childhood, this learning proceeds by the formation of minor habits, which are gradually combined into larger habit systems, to which progressive additions are made. By the age of twenty, speech is predominantly a matter of habits which have been established. In a similar way, we learn the various processes of arithmetic, forming habits of adding, multiplying, dividing, etc., which are then available for our later practical use with little modification. We put our energy and time into the learning of these things in order that they may become habits, thus releasing our time and energy for further learning of new responses. The constellation of habits which the adult has formed, and which constitute the framework of his life, is enormous in its complexity and range. It includes habits of social behavior, habits of work, habits of play, habits of emotional response, habits of judgment, inference and generalization, habits of eating and drinking, habits of sexual behavior, habits of sleep; but these are merely illustrations of the list.

The practical aspect of the formation of habit presents two questions: What habits shall be formed? How shall we form them most efficiently? The second problem is the general problem of learning. The first is the problem of adjustment. There is, however, a third problem, which arises because of the frequent failure to solve the first two, namely: How shall we break or destroy a habit which has become firmly fixed, but which is a habit of inadequate response? While this is actually a problem of the modification of response, and so a part of the general problem of learning which we have discussed at length in the preceding chapters, it is a form of learning which has special applications in mental readjustment, and which offers special difficulties, and therefore requires special technique in many cases.

Three classes of "bad habits," selected from the list given in the preceding chapter, will serve to illustrate the procedure required for the "breaking" of such habits. These are:

(1) Stammering (including stuttering), which is an inadequate speech habit.

(2) Tics; that is, obsessive motor performances such as thumb sucking, and finger-nail biting, and recurrent movements of an annoying sort, such as jerking the head, twisting the shoulders, making facial grimaces, etc.

(3) Bad sexual habits or vices, such as masturbation and homosexuality.

The assistance given to an individual handicapped by one of these habits, and attempting to "break" it, is fundamentally through the supervision of a course of training, carried out in accordance with the principles of learning which have been presented in the preceding chapters. The central features and conditions of this training are as follows:

(1) The patient must understand the situation in which he is. He must be aware of the actual inadequacy of the habit, and the nature of the handicap which it contributes.

(2) He must form, or must accept, the proper ideals, conceiving as clearly as possible of the results which will accrue from the elimination of the habits.

(3 He must have an actual desire to realize these ideals, to modify the habitual responses in the direction of the more adequate ones.

(4) He must engage seriously and persistently in the practice which is outlined for him as a means to the desired end.

The problem for the psychologist is to plan adequate practice for the patient, in accordance with sound psychological principles. Under the influence of the old theories regarding habit formation, it was formerly supposed that the sufficient procedure, even the only procedure, competent to break a habit, was to cease the old response and commence the new and adequate response. In other words, the way to break the habit of doing a certain thing is to *stop doing it*. This, however, is the end of practice, its goal, not its technique. In the more important cases, this goal is impossible of attainment without the intervention of practice technique of a different sort.

In some cases, it is true, the mere recognition of the inadequacy of the habit, the idealization of the adequate habit, and the desire to make the change, *appear* to be sufficient to accomplish the result. It may be doubted, however, if this is really ever the case; and with the more important bad habits, such simple accomplishments are clearly impossible. The stammerer may realize his condition. He may clearly conceive of the type of speech which he should employ, and he may be intensely desirous of making the change.

But he cannot stop stammering except by stopping speaking. He cannot speak fluently. In some cases he does speak fluently at times, but this does not prevent him from stammering at other times. The more earnestly he determines to speak fluently, the more he makes effort, the more he stammers. The stammering habit is an excellent illustration of the difficulty of breaking a well-established habit. It also affords an illustration of the methods of practice which are available.

By stammering we usually indicate any defect of speech which is not due to anatomical faults, but is an actual speech habit. *Stuttering* is a particular form of stammering in which the patient repeats syllables or words, or initial sounds of syllables. "I bu-bu-bu-believe I have mu-mu-met you bu-bu-before" is typical stuttering. There are, however, many types of stammering. Some patients merely pause for a long time between words, with the facial and throat muscles in a fixed position. Others make extraneous sounds, not involved in the words intended to be said. Some have specific breathing gasps. Many subjects combine several of these types of stammering, and add facial contortions, tensing of chest and arm muscles, tic movements of the hands, etc.

Faulty pitch of the voice, and some other defects of speech, may be due to the nature of the vocal organs, cleft palate, etc. Persons having these handicaps are not stammerers, and the types of response are not to be treated as habits. On the other hand, certain cases of unpleasant pitch or timbre of the voice are habitual, and are to be treated as stammering.

The causes of stammering are undoubtedly different in detail in different cases. In some patients, a general "nervous" condition; that is, a condition of tension, perhaps dependent on malnutrition, may be a cause.

We have reason to believe that faulty diet has been an important causal factor in the stammering of many small children, contributory to the formation of a habit which persists into adult life. In such cases, it is not probable that the nutritional factor is the sole cause, but rather that it predisposes the child to the formation of bad speech habits, when he is subjected to adverse psychological conditions. Obviously, however, the predisposing causes are highly important and a better nourished child may successfully withstand environmental conditions which will produce stammering in a less nourished child.

There has been popular a theory that left-handed children, who are trained to use their right hand properly, are especially liable to acquire stammering. This popular notion was at one time bolstered up by explanations based on an old theory of brain centers, and has been recently made the foundation of one of the numerous "systems" of curing stammering. So far as studies show, there is no basis for the old theory, and parents of "left-handed" children may not fear to have the children's right hands trained, provided the training is correctly done. There is no doubt that the use of brutal and incompetent methods of training children, not only in respect to use of their hands, but in any way, may contribute to the production of stammering, or any other neurotic trait.

Whatever predisposing causes, such as malnutrition, may exist, the stammering habit is actually induced by psychological causes, and these causes, in practically all cases, emanate from the adults who form a large part of the child's social environment, and whose actions and attitudes may have a powerful inhibiting and disturbing effect on the child's speech expression.

Sometimes the child is restrained from speech unduly; sometimes he is forced to speak when he strongly desires to be silent. Sometimes his mode of speech is constantly subject to criticism. Sometimes he is in constant fear lest he inadvertently reveal something he would rather his elders did not hear. Often his general emotional attitude is constantly upset by family or school situations, and the disturbance is reflected in disturbed speech expression. There are innumerable other family and school situations which may lead to similar results.

Very often a stammering child does not need treatment at all, but will resume correct speech if parents, grandparents, uncles, aunts, teachers, or whoever may be at fault, are corrected. Sometimes it is merely required that the adults shall "lay off" the child and allow him greater freedom. In many cases, however, a more positive extension of sympathy, interest and attention is required.

There is seldom difficulty in determining the cause of a child's stammering, if the type of stammering is carefully observed, and correlated with home and school conditions. No specific instructions applicable to all cases can be given for analyzing the conditions and breaking the habit, and procedure by "rules" or simple principles is always bad. Treatment is a clinical matter; each case must be analyzed as an individual one by a psychologist skilled in speech defects and child psychology, and treated as an individual case. Psychoanalytic treatment or any other application of arbitrary theories is especially to be avoided, since much damage may be done.

In a large number of cases, the original causes of stammering have ceased to operate, and the defect is merely a habit. In diagnosing any case, even of an adult, it is essential to get back if possible to the original causes, since they *may* be found to be operating even when they appear to have subsided, and it is futile to attempt to "cure" the stammering if the causes are still effective. In the cases of adolescents, the deadly environmental factors (the parents' actions, for example) as such may have disappeared; but an unfortunate attitude engendered in the child toward his parents may persist, and must be removed. The young child does not need to be "adjusted"; his environment (parents) need changing. For the adolescent, there may be need of adjustment of the individual himself, in regard to his family relations or his general social relations.

In a large number of cases it is a problem simply of breaking a habit. In many more, where adjustment of the environment or adjustment of the individual are required, the habit still remains to be broken. It is with this aspect of treatment that we are here concerned.

It is obvious that the practice in any learning must consist of doing something which the patient can do, and of doing it under the proper conditions. The stammerer can sometimes speak fluently; hence it might be supposed that practice in correct speaking would constitute adequate treatment. A certain stammerer, for example, can read fluently from a book, can sing without stammering, and can converse with intimate friends, when under conditions involving no essential stress. But with a little excitement or when speaking to strangers, he stammers.

Practice in speaking correctly, under conditions which make fluency readily possible, does not, in these cases, alleviate the stammering. If it did, the particular stammerer just described would already have been cured, since he has had years of practice in correct speaking.

Theoretically, we might begin with the patient speaking correctly under the most favorable conditions, for his particular case, set up a routine of practice under these conditions, and gradually make the conditions less favorable, approaching the adverse conditions, under which he was formerly unable to speak correctly. If the practice were skillfully planned, and the conditions changed slowly enough, it is probable that we might carry the correct speech over finally to the formerly prohibitive conditions. Practically, however, such a procedure is so laborious and lengthy that it can be carried out only on few patients, and on those at prohibitive expense. In the cases of other patients, who stammer under *all* conditions, the technique is of course impossible.

We must search, therefore, for something the patient can do other than to speak correctly, which may be made a "carrier" for the conditions of thought and desire which are essential to learning. We may give him breathing practice, for example, since every stammerer breathes. Perhaps through breathing exercises (connected in the patient's thoughts and desires with the stammering defect, with the objective of correct speech, and with the desire to speak correctly) we may modify his speech habit.

Again, there are certain speech sounds the subject can make correctly, although they do not constitute speech. He can repeat the vowels a, e, i, o, u. Or he may even be able to "speak" in a squeaking falsetto. These speech functions he has not previously knit into his stammering habit, and so can carry them on correctly, or can quickly learn to do so. We might be able to utilize this capability of the stammerers, beginning a practice series with these vocal performances and progressively modifying the practice conditions in the direction of actual speech.

Still another basis for practice might be suggested by the fact that the stammerer, under conditions which otherwise induce stammering, is able to enunciate words in a slow sequence which he punctuates with rhythmic hand movements. For example, he may say "The—dog—ran—down—the—street," accompanying each word by a beat of his hand at a favorable rate.

Still other procedures, utilizing performances the stammerer can make, are possible. A stammerer can learn to play the piano; he can learn to play tennis; there are a large number of non-language performances which stammerers can learn as readily as can non-stammerers, and which, therefore, might be made the basis of practice. If we find that a stammerer can speak fairly well under conditions such that an electric shock is administered to him every time he makes a verbal slip, we might base a practice course on this.

All of the suggestions made have been utilized by various "systems" designed to "cure" stammering, and all of the procedures have been "successful" with some stammerers. But, in general, they are not successful. In most cases of "success," the process is laborious, lengthy and expensive; since an enormous amount of the trainer's time must be put on each case, if a satisfactory end is reached. For the majority of cases, these, and similar methods, are not available, and they are employed at all (aside from the financial returns to the trainer) only because of the desperate lack of better methods.

There is, however, one thing the stammerer can do, and which has been strangely overlooked in the past. *He can stammer!* This is a performance which obviously should be considered as the possible basis for an adequate system of practice.

The function of practice is to modify response. The particular responses which are employed in practice may be fixed by the practice, or they may be modified by the practice. There is no inherent tendency of a response to make its own recurrence more probable in the future. A given response may be more probable, or less probable, in the future, according to the conditions actually involved in the response. These determining conditions, as we have earlier shown, are thoughts, desires and ideals.

What we are to do, therefore, is to teach the patient to stammer voluntarily, as nearly as possible in the way in which he now stammers involuntarily. Then we must cause him to practice stammering in this way, under the conditions of thought and desire appropriate to the destruction of the habit which we are using as the basis for the practice. This is simple in theory. In application it is difficult, requiring an expert psychologist for its direction; but *it seems to be effective!*

We have said above that there is one thing the patient can do; namely, *stammer.* There is a catch in this. The patient stammers involuntarily. The repetition of the equivalent vocal performance voluntarily has to be acquired. The first thing the patient must do is to learn to stammer. This, however, is not a learning process which precedes the practice which destroys the habit, but is a part of it. The learning to stammer is the initial part of the practice which breaks up the stammering habit.

The psychologist, therefore, must carefully study the patient's stammering. This is important for the diagnosis of the case. It is also essential for the purpose of teaching the patient to stammer. Having analyzed the stammering, the psychologist assists the patient, by criticism and by illustration, to make the speech of voluntary stammering as nearly like that of the involuntary as possible. No fixed rules can be laid down for this procedure. The psychologist must possess the knack of teaching, or he would better not undertake the treatment of stammering at all. By constant effort and labor the psychologist can improve his technique.

Before commencing the training of a stammerer, the practice conditions must be prepared. The patient must understand the nature of his defect and its serious disadvantage, and must be desirous of learning to speak fluently. In most cases, the patient already has the proper understanding and desire. Nevertheless, the matter must be discussed with him, and the total situation made crystal clear. The patient must understand that the desire is the fundamental curative agency, that following the psychologist's instructions meticulously is the secondary factor, essential to the operation of the desire.

Further, the assurance must be conveyed that the method will succeed, but will take time and effort, and that all efforts not prescribed by the psychologist are detrimental, and must be avoided. Included in these detrimental effects are efforts to avoid stammering, except in so far as, and at the times at which, such avoidance may be prescribed by the psychologist. The patient must understand that in his daily life it is important not to avoid stammering, and that he must let his speech proceed as it happens. He must understand that stammering under ordinary conditions is of no consequence, that it will not interfere with the breaking of the habit of stammering, but that, on the contrary, the effort to avoid stammering tends to fix the stammering habit. In this way only can the patient be put in the proper emotional attitude, decreasing the worry and effort which is an actual detriment, and be in a condition to be readjusted.

It is essential also that parents, guardians, uncles, aunts and other environmental adults should be warned not to notice the patient's speech; neither to comment on it, nor in any other way to indicate that the stammering is of any consequence or importance.

The next procedure is to explain to the patient that an important part of his cure is daily practice in stammering. He should understand that this stammering, *when prescribed* and *as prescribed,* is the method of cure. He should therefore be ready to practice stammering, under the psychologist's direction, eagerly, and enthusiastically. He should be led to understand that stammering, when practiced per instructions, not only is not defective speech and is not deplorable, but that it is a necessary accomplishment. He should be led to engage in it without inhibition, doubt or qualms. This is especially important.

Before beginning practice, and before each practice period during the first few weeks, the patient should be led to think in detail: "I am going to stammer; I am going to perfect my stammering (*i.e.*, make it as near as possible like my usual stammering). I am to do this now, when I want to, because so doing will make it possible later for me not to stammer when I don't want to. The better I stammer now, the sooner I will break the habit of stammering."

The formulations above are the contents which must be conveyed to the patient. In few cases will the words or phrases of the formulation be such as can be employed in instructing the patient. The skill of the psychologist must be exercised to find wordings which will convey to the patient the meanings embodied in the above formulation, and to arouse in him the attitudes which are required. This will not be accomplished in one session, nor in two or three. For a long time (but not too long) the psychologist must bring the essential points daily to the attention of the patient, but the practice may be begun as soon as the patient understands the situation reasonably well.

The periods of practice will vary from case to case. In some cases half an hour a day will be adequate. Fifteen minutes twice a day is probably better. In some cases less time will suffice after the first few weeks. In other cases more time will be required.

At first, nothing should be done in the daily practice except the practice of stammering. After three or four weeks, a part of the time may be occupied in trying to speak correctly. The *modus operandi* of this is to have the patient practice stammering a phrase until he stammers it well, and then immediately speak it correctly. If he succeeds on first trial, he may repeat it once or twice. If he fails, he should be stopped at the first appearance of stammering, and stammering practice should be immediately renewed. Avoid, above all, during the practice period, having the patient trying to speak incorrectly and failing.

The practice material should be varied. Sentences as nearly as possible in the style of the patient's language should be prepared, and these should be used exclusively in the early weeks. Later, while still employing the patient's language, other types should be included. At first only repetitions of sentences read by the psychologist should be employed. These are most useful for the correction of the stammering; that is, the attaining, voluntarily, of stammering closely similar to the patient's involuntary stammering. After the patient has mastered the voluntary stammering fairly well, conversation should be employed. The psychologist should ask questions, to be answered in the patient's own language. These questions should, of course, be prepared in advance, with special reference to the topics of knowledge and interest of the patient, and with regard to the opportunities for stammering which the probable answer will offer. Impromptu questions in most cases will be incompetent, and will disorganize the practice period.

Before beginning the practice period, during the analysis of the case, the psychologist will have obtained basal information concerning the patient. This information will not be complete, however, and from time to time further information will be obtained through conversation; and advice as to home and school procedure can be given to the patient from time to time as the case becomes more clear. Conversations with the patient (except those prepared for practice) should always occur at the beginning of the period, preceding practice, and the patient, in conversation, should make no special effort not to stammer. Conversation at the end of the period should be avoided, especially in the early weeks of practice.

At certain stages in the progress, positive practice in correct speech may be useful for some patients. This positive practice should always avoid effort; hence for this purpose unison reading is especially good. In unison reading, the stammerer must, first of all, synchronize his speaking of a word with that of the trainer. The attempt to read as many of the words as possible is important but is secondary. Any word the stammerer cannot pronounce early and without delay he must skip, and go on to the next one. Failure to read a word is minor. Getting behind the trainer is a major failure. Struggling unsuccessfully with a word is especially pernicious, and makes the unison reading not merely a failure, but a damage.

In some cases daily practice in stammering may not be possible, and fewer periods may be necessary. The progress will be slower under these conditions, and less than three periods a week is probably not worth undertaking in any case. After a few weeks, if the patient keeps his stammering up to the mark without continual coaching, he may be trusted to practice alone certain days of the week, using text prepared for him. In most cases, however, the patient when left to practice by himself will either omit the practice, or will let down in his stammering so that it does not resemble his involuntary stammering, and hence is useless.

The total time required may be as short as three months or may be six months. Certain adolescents, who were bad stammerers in the beginning, have been able, after three months' treatment, to speak so well that persons who had not known them earlier have not suspected that the patient ever stammered. In some cases, after four or six months of faithful practice, the patient may have a residue of difficulty, which will clear up later with no further treatment. In any case, the patient, after treatment is suspended, should report at weekly or bi-weekly periods for observation, and reports should be secured from parents and teachers, in order that, if a relapse seems imminent, treatment may be resumed promptly.

It is probable that when more cases have been treated a system of treatment may be devised by which, instead of stopping treatment abruptly, when the patient is readjusted or nearly readjusted, the periods of treatment will be made less frequent after the first two months, and the frequency gradually lessened, so that the treatment will be extended over a longer total time, while involving fewer practice periods. This method may be more economical and also more efficacious. Nothing can be predicted on this point at present.

From the foregoing description of method and procedure, the trained psychologist will realize that the actual treatment of stammering by this method is by no means simple. The method is not one which can be applied by a layman with much probability of success. The mere laboratory psychologist, untrained in the handling of adjustmental problems, will probably have as little success, and the "clinical" psychologist, with no more training in psychological application than the usual "clinical" psychologist possesses (*i.e.*, technique in administering mental tests), is at least as unprepared for the use of this method as the mere laboratory psychologist. The medical practitioner or psychiatrist without technical psychological training and background cannot expect to be able to apply the method.

The psychologist of any branch, provided he has had a thorough training in the general principles of scientific psychology, and is free from psychoanalytic, behavioristic or other "school" theories, and provided he is thoroughly versed in the problems and principles of learning, may well acquire the technique of applying the method, if he is willing to put in a year or more of solid work in making the acquisition, provided he has the ability to make adequate contacts with patients and the wisdom to analyze cases and profit by his work. It is to be suspected, however, that there are many psychologists who may be of high proficiency in experimental or clinical work, with excellent backgrounds in general psychology, and good scientific understanding of the learning problems, who do not possess the personal qualifications requisite for success in this difficult field of readjustment. It goes without saying, that no one, whatever his general psychological training, should undertake this work unless well trained in laboratory psychology of the modern type.

The person who suspects that he has the background and personal ability may well commence with a small group of patients, preferably between the ages of twelve and twenty, and proceed to "practice" on them. If no benefits are conferred on the first patients, at least no harm will be done, if the general scheme of the method is rigorously followed. Thousands of stammerers are being "treated" today in clinics, special classes and commercial schools, with no benefit to the patients; and the patients of these beginners in this method are no worse off than they would be under any of the various methods in use, and as a matter of fact are in a more favorable condition, since there is a strong chance of their being benefited, even by the tyro. Of course, the beginner will not make any charge for his labor!

In a few years the situation will change. There will be enough psychologists, skilled in the application of readjustment methods, so that it will be a deprivation to patients to be treated by the independent beginner. Then there will be needed clinics in which psychologists will be trained in the methods under the direct personal supervision of experts.

To those who look on psychological methods as simple procedures which can be applied by anyone who reads the rules carefully, the actual situation may seem very discouraging. They should remember that psychological applications are like the more recondite medical specialities. No one would expect to be able, by reading a description of methods of cardiac diagnosis, even with the mastering of the technique of the stethoscope, electro-cardiograph and other instruments, to be able to make sound diagnoses, much less to supervise therapeutic treatment for heart conditions. A sound background in physiology, pathology and general medical subjects is recognized as indispensable, but this must be built upon thoroughly long clinical experience with heart cases under the guidance of experts in this field.

The same conditions obtain for psychological application. A thorough grounding in learning and habit formation and in general psychological principles and data, with laboratory experience, must be built upon through lengthy practical training in the particular speciality of application. It goes without saying that expert guidance in this training is a great asset.

The treatment of tics is, in its general aspect, parallel to the treatment of stammering. Tics are generally symptoms of basal maladjustments, the sources of which are various. Obviously, the causal factors should be sought and remedied, if possible.

In every case, the patient who shows a tic should have a competent medical examination. No psychologist will attempt a diagnosis until assured that all possible medical attention has been given. A competent medical diagnostician will not only determine the general bodily state, the presence or absence or specific infection, etc., but will also indicate the probable need of a neurological examination or examination by any other medical specialist. If there are no medical defects discernible, or if those which are discerned (and of course given adequate attention) are not causal factors, so far as the tic is concerned, the psychologist may then proceed with further examination of the case, including an examination of the family, social and sexual conditions. If the case is clearly a psychological one, treatment may be commenced. If there is evidence of mental disease, the case should be referred to a psychiatrist. If the case is a doubtful one, it should be referred to a psychiatrist who is able to coöperate with the psychologist in the further handling of the case. Only the competent psychologist, however, can decide in most cases whether the case is a psychological one or not.

Our present concern is not with the general adjustment problem, but with the particular point of abolishing a tic where such abolition is the important matter. The procedure here is parallel to the procedure with the stammerer. There must b on the part of the patient the understanding of the habit and its detrimental effects, the ideal of abolishing and the desire to be free from the habit, and faithful carrying out of the practice prescribed. The details of application and the results are, however, somewhat different from the corresponding features of the treatment of stammering.

The treatment of tics by the beginner, of adequate psychological grounding, is more successful than in the case of stammering. Perhaps a more accurate statement is that the well-grounded beginner learns the successful technique more rapidly than he learns the technique for treating stammering.

With adequate technique, tics are much more rapidly eliminated than is stammering, although relapses are more apt to occur after a "cure" than in the case of stammering. This may be due to the shorter period of treatment required, or it may be otherwise conditioned. Nothing definite can be said on this point at present. Let us commence with the case of a boy between twelve and thirteen who shows a tic consisting of a spasmodic twisting of the head to the left, a tic which occurs at irregular periods, but persistently. Tics of this type are sometimes due to the inadequate outlet for certain normal desires of the boy, sometimes to failure to repress a desire for which there is no outlet possible at that age. In some cases, the inadequately provided desire is the activity desire. In some cases, it is one of the "social" desires (for conformity or preëminence). In some cases it is "sex" desire.

Let us assume that the case has been analyzed (not psychoanalyzed) and by adequate modifications of parents' attitudes and by provision of proper general conditions of life —for sports, for group membership, or whatever is necessary—all is done that can be done to remove the causal conditions.[1] The tic may still persist as a well-entrenched habit, and by its persistence may inhibit the personal adjustment of the boy to his improved environment. The habit must be broken.

Practice for a case of this type involves the producing of the tic voluntarily. This the patient quickly learns to do. After a dozen or so of such movements in the first practice periods, executed on signals from the psychologist, the tic may be absent for an hour or more, although ordinarily occurring every few minutes. In other cases, such immediate results are not obtained. A relatively few practice periods, however, may abolish the tic for long periods of time, but it will return after practice is abandoned. Some-

[1] It would be too much of a digression to discuss the adjustmental features of such cases. It should be pointed out, however, that the actual repression of the desire involved is in no case the cause of the symptomatic tic. The desire is in such cases not repressed. In the case of sexual desire, the adequate treatment is repression. If the desire can be actually "thrust down into the unconscious" it is no longer existent. It is its actual existence, consciously, in the full psychological sense of the term, which works the damage. Unfortunately such unrepressed desires are treated by the psychoanalyst as if "unconscious" or "repressed" in the theoretical psychoanalytic sense, although from the case histories given by psychoanalysts it is usually apparent that the desire (if not a fictitious one) is actually unrepressed. The only treatment for sexual desires possible for the young is more complete repression. Activity desire and social desire might be treated in the same way. If the child no longer has the desire, the causal basis of his symptom is destroyed. Unfortunately, the social and activity desires and their satisfaction are essential in childhood, and the repression, while it may prevent tics, produces disastrous results in the child's later life. The repression of the sexual desires in childhood, on the other hand, is a distinct advantage.

times the tic is permanently abolished, but a tic of a different sort takes its place. There is an enormous variation of result in different cases.

The proper procedure in any case is to abolish the tic by daily practice. When it is abolished, the practice should be continued, nevertheless, with lengthened intervals. In a particular case, daily practice for a week may produce the primary abolition, and practice continued on alternate days for six or eight days keeps the tic abolished. A few further practice periods every third day, followed by weekly practice for two or three weeks, finishes the job. In other cases, different time relations must be employed. Careful check on the patient must be kept up for weeks after practice is finally discontinued.

If other tics develop as substitutes for the original one, these must be treated concurrently. It may be that the basal maladjustment of the patient is little modified by the abolition of the tic. Yet the abolition is of great value socially, and assists materially in the ultimate readjustment of the patient.

Finger-nail biting and thumb-sucking are habits which for practical purposes are to be classed with tics, although not generally so designated. Thumb-sucking is an infant habit which is especially pernicious, for although it disappears in later childhood, it is a difficult habit to break during the first few years of life, and many of the methods and appliances which have been applied in the attempt to "cure" the habit produce other bad effects. The thumb-sucking is indeed a symptom of a basal condition which is theoretically easy to adjust, but which, for economic reasons, is often practically difficult.

Thumb-sucking is in almost every case the result of bad social treatment of the child during the first year of life. It is a sign that the child has been not adequately socially stimulated. The child which is handled in the way which has been recommended by some influential physicians is especially apt to become a thumb-sucker. This method, which has sometimes been called "scientific" is systematic neglect of the child, camouflaged under the guise of "science" to relieve the parent's feeling of guilt. It is much easier for the mother (provided her feelings are assuaged by the supposition that her procedure is "scientific") to let the child "cry itself out" if it is unhappy, to let the child amuse itself for long periods and to train it to go to sleep without rocking, petting or any of the little time- and energy-consuming procedures which have been used by our grandmothers for soothing babies. Many mothers, on account of the demands of labor, are forced to neglect their babies in this way; many others find it exceedingly difficult to keep their babies normally happy without spoiling them. But it should be distinctly understood that neglecting the infant has no shred of actual scientific backing.

The baby which is neglected does in the course of time adjust itself to its unfortunate environment. Such babies become "good" babies, and progressively easier to neglect. Such procedure is no more justified by these results than is the method of keeping the baby mildly drugged. The neglected baby may not suck its thumb; but nevertheless, as it grows older, it shows social maladjustment which handicaps it seriously in contacts with other children, and later in contacts with adults. The boy reared in this way may be an easy one to manage, and a boon to a mother who is economically limited or lazy or incompetent. The price is paid in later childhood and adolescence, and often in adult life.

Here again space forbids us to enter into a full discussion of the adjustment problem. It is important to note, however, what the best treatment of the baby requires: (1) The baby should not be allowed to cry, or rather that crying be minimized, and never allowed to continue long.

When the baby cries, it either is sick or something is seriously wrong with its environment. (2) The baby should be allowed to amuse itself only for short and carefully controlled periods. It should be amused; that is, socially stimulated during practically all of its waking time. (3) So far as possible, a baby, or a child of any age, should be prevented from lying in bed awake.

Fortunate is the infant whose mother has time to entertain it, and whose efforts in this line are supplemented by those of grandparents, uncles and aunts. Over-stimulation is to be avoided and sufficient periods of sleep assured. Of course, keeping the baby happily stimulated during its waking periods, and preventing crying, while not "spoiling" the child, is a difficult task, too difficult, perhaps, for the intelligence of many parents. Spoiling a child, however, is a minor evil; neglecting it is a major one.

Thumb-sucking, and the more serious maladjustment of which it is the symptom, may be prevented by adequate treatment of the child from birth. If thumb-sucking appears, the treatment of the child must be improved at once. Unfortunately, although the fundamental maladjustment may be readily removed, the habit of sucking the thumb, once commenced, is difficult to break. It may be lessened and ameliorated by proper care, and, if not deeply imbedded, it may disappear. Direct treatment is, however, not possible until the age of four or five, and is even then extremely difficult. Experiments on thumb-suckers have already shown that at five children may be made to understand the disadvantages of the habit and actually to desire to break it. Under these conditions daily practice in voluntarily sucking the thumb markedly reduces the habit rather quickly. The difficulty is in keeping the attention of the child on the problem and process during the "practice"; but this may be overcome by skillful treatment. As the habit lessens, however, the motivation lessens, and the completion of the process becomes difficult. Since (under present conditions) the treatment must be entrusted to the mother, it is not easy to bring about a complete break. Progress made, however, indicates the possibility. Even a lessening is worth while, for although it is alleged by some that thumb-sucking does not deform the jaws, there seems to be considerable evidence that it does, and a heavy orthodontist's bill may be the later penalty of allowing the child to form and continue the habit.

Finger-nail biting is a vice which characteristically afflicts individuals in adolescence, and may be continued into adult life. This habit is always a symptom of maladjustment, minor or more serious, and the family, social and personal life of the finger-nail biter should be carefully analyzed. The sources of the maladjustment are extremely diverse, but like the tics, properly so-called, frustration of or inadequate outlet for normal desires are the most frequent causes. In spite of the need of fundamental readjustment of the patient or his environment, it remains true that in the majority of cases the habit itself needs to be broken. Here the application of the negative practice is fairly simple, and if the essential conditions of thought and understanding are secured, as in the outlined stammering treatment, the practice results are obtained with surprising quickness.

The practice which is effectively employed in this treatment is of a type concerning the efficacy of which there was, at first, considerable doubt. Here we might prescribe voluntary action, as in the treatment of simple tics and stammering. In the cases of patients whose nails are already gnawed down to the quick, or rather far beyond the quick, as is usual in confirmed cases, we hesitate to prescribe practice in further actual gnawing. Hence the form of gnawing was prescribed, without stressing the need of making it actually as savage as is the involuntary habit. Somewhat to our surprise, this practice carried out in two daily ten-minute periods succeeded with every member of a group of college students. The nails of the members of this group had been gnawed down to a point which might hardly be believed possible, being on the average about a third of the way from the finger top to the base of the nail. In less than three weeks, the nails had grown out to normal length, and the biting habit was broken in every case. Practice was then suspended permanently. Complete check on the group later was impossible, on account of the graduation of some of the members, but in several cases there were later relapses. Obviously, a thorough follow-up in all cases is necessary, and immediate resumption of practice is indicated at sign of a relapse. It is probable, however, that if practice had been continued on the above group for several weeks, with lengthening intervals between practice, relapsing would have been prevented. In some cases, the further practice would have been superfluous (since these cases did not relapse) but would have done no harm. Since patients of this type can be given practice in a group, the labor and time of the psychologist is economized, and there would have been no loss from that point of view in extending the practice of the whole group, even if it were possible to guess, at the time of the primary result, which cases would need follow-up work and which would not. The time of the patient is actually a minor consideration in these cases.

Where cases are treated singly, the problem of determining when to lessen the practice, and how long to continue it, is an important one, and the psychologist must in most cases proceed empirically. It is best in any case not to cease practice abruptly, but to taper it off as the particular case seems to require. Then the condition of the patient must be checked at periods varying from a week at first to a month finally.

It is quite possible that the total practice is most efficient if it is discontinued in such a way that slight relapses occur, practice being resumed promptly upon such relapse. On the other hand, this procedure may be slightly less efficient than the tapering off which permits no relapse. No decision can be made on this point at present, and its determination is obviously difficult.

The range of personal habits of the tic type which are detrimental to the patient, socially or otherwise, is extensive. It is probable that all such cases can be brought under the treatment successfully of negative practice. Even guzzling soup noisily and laughing while telling a story can undoubtedly be conquered economically by this method. We have not yet given the method its ultimate extension in this field. Habits of day dreaming and scattering of attention have, however, been successfully abolished.

Especial difficulty is encountered in the breaking of sexual bad habits, but the method of negative practice has proved applicable in this field. The range of sexual vices is long and complicated. Here we may reasonably confine our attention mainly to masturbation and homosexuality.

The causes of masturbation are various, and in general are persistent. Prevention by adequate hygiene and inculcation of adequate thinking is, of course, the most important line of activity. Nevertheless, we must recognize the fact that most boys, and many girls, do form the habit; and that a vast number find great difficulty in breaking the habit.

While there is no reasonable doubt that the evil effects of masturbation have been much exaggerated in the past, and that the fear and brooding, not to say agonizing, which has resulted from this over-emphasis has been a damaging thing to many patients, there is equally little doubt that the recent propaganda of the theory that masturbation is harmless, even beneficial, is as seriously pernicious. Various definite detrimental results do accrue from masturbation in a great many cases. Social maladjustment and marital difficulties are the most conspicuous pernicious results. Whether or not prostatic troubles in the male are commonly prepared for is a subject of dispute at present. Other personal damage is certainly done in many cases. The situation, emotionally, perceptually and ideationally in masturbation, is distinctly abnormal, and the possibility of permanent or prolonged damage is one to which young people should be subjected as little as possible. That regulated promiscuity between the sexes in youth would be a far less serious matter than masturbation, there can be no doubt, so far as the hygienic results are concerned, however unadvisable this may be from the social point of view.

It is possible that the damage done by masturbation, commenced in the adolescent period and abandoned shortly, is neither serious nor permanent. The habit commenced in childhood or continued into late adolescence or early adult life is another matter. The problem of breaking the habit in a young child is one which we may avoid for the present as a problem involving threats of damage from the treatment itself, whatever method of treatment is employed. The situation of the masturbator between twelve and twenty is less precarious, and intelligent treatment, carefully applied, has no possibility of damage, however deplorable ignorant or unskillful meddling may be.

Negative practice in the treatment of masturbation cannot be safely undertaken except by the mature psychologist already skilled in the handling of the problem of youth, and able to secure the entire confidence of the patient. The least indication of either modesty or immodesty on the part of the psychologist or the least indication of what is called popularly a "moral" attitude is fatal. The boy must first be led to understand that masturbation is exactly on the plane of a physical ailment, so far as its "immorality" or its importance is concerned. He must understand that the habit is, on the whole, an undesirable one, like the habit of sniffling or the habit of stammering and that, if persisted in, it may possibly injure him. On the other hand, he should not be frightened or led to look on the habit as loathsome or scandalous. He should not be led to assume that it has already damaged him or that its continuation in the immediate future will seriously damage him. He should understand that the continuation may eventually be damaging, that its continuation for any length of time involves risk, and that it is far better to break it now than to postpone the abolition.

The wise psychologist, although he may well prepare the boy for treatment by explanation given confidentially, perhaps in several sessions, will not proceed to treatment without the assistance of a third party, carefully selected. This is for the protection of the psychologist who may otherwise be accused of "immorality" himself, if the fact of his administering treatment becomes known, as it inevitably will be. The third party should not, in most cases, be the boy's parent. A physician, known to the boy as such, is the best safeguard. A mature man, whose character is known to the psychologist and whose selection is approved by the boy as well as by his father, is the next best. In any case, the treatment must be approved by the parent, and the value of the treatment must be clearly understood by the parent. It is not necessary that the boy know that his parent is privy to the details; and in any case, the parent must be discreet enough to avoid all reference, direct or indirect, to the matter. Unless the psychologist is certain of the discretion of the parent, he will do well not to undertake the case at all. In no case must the boy be given instructions, and left to carry out the practice privately, without supervision. Such procedure may be effective with patients over eighteen, but it is not to be risked with adolescents. The technical ordering of the treatment is the problem which the psychologist must solve in each case. For the benefit of the critical public, it may here be stated definitely that the psychologist does not in any case touch the patient during the treatment and that the necessary third person takes no part, even verbally, in the treatment.

The treatment of girls offers especial difficulty, for the present insuperable. A girl can be treated, of course, only by a woman psychologist; and at present there is lack of women psychologists who are both prepared and willing to enter this field. This lack may be remedied in the future.

The problem of homosexuality suffers at present from the emotional horror with which the general public regard the topic and the homosexual patient. It suffers also from the pernicious propaganda emanating apparently from psychoanalysis, which has succeeded in convincing considerable numbers of young people that homosexuality is perfectly "normal." This doctrine is the result of a vicious confusion between homosexuality, as verbally defined, and the specific vice of homosexuality, which is quite a different thing. "Homosexual" means literally *pertaining to the same sex*. In this literal sense, a friendship between two women is a "homosexual" friendship. Is it not indeed a friendship between two persons of the same sex? Two men, or two women, lunching together are literally engaged in a homosexual luncheon. In this literal (but spurious) sense of the term, homosexuality is indeed normal. It is, in fact, one of the finest features of human life. To become confused by verbal juggling and conclude, consequently, that homosexuality in every sense of the term is fine and normal is an appalling procedure, but it is a procedure which, camouflaged in vague phraseology, has become a social blight.

The homosexuality which constitutes the serious problem, with which those responsible for the care of youth are today wrestling, is far different from the verbal homosexuality. It is a form of perversion in which a person deliberately stimulates sexual, or is stimulated sexually by, a person of the same sex, by direct contact, proceeding usually to arouse the maximal excitation of the sex organs, and to the culmination of the sexual process as if in normal sexual intercourse.

That the results of homosexuality (the real homosexuality) are pernicious is clearly known to those who deal with homosexual patients. Social maladjustment is the most obvious result. The confirmed homosexual is precluded from entering into normal social relationships in most cases. Personal damage is also apparent, and marital adjustments are rendered impossible. There is no question about these facts.

The causes of homosexuality have been popularly misconceived. It is true that some cases become homosexual as a result of "normal" (*i.e.*, heterosexual) excesses, as a result or end condition of other forms of perversion, and sometimes through economic causes. It is not true, or at least there is no evidence that homosexual tendencies are "inherited," except in the general sense in which everything is inherited. Homosexuality is certainly in the great majority of cases learned. One homosexual teaches the vice to another person.[2]

Every homosexual is a point of social contagion, a real vampire, whose "bite" makes a vampire of his or her victim. That large numbers of confessed homosexuals are at large, constantly teaching it to youths, is a fact which the public should understand. They are present in college faculties

[2] As an illustration, however, it may be noted that there are other sources of learning homosexuality besides personal instruction. The practice of males impersonating women in college dramatics, for example, where it does not make the plays ridiculous, has direct connection with homosexual problems. The really adequate female impersonators are youths who already have homosexual tendencies, which are expanded by the indulgence in female impersonation. In a surprising number of cases, the successful impersonators are actually homosexual in practice already. This, of course, does not apply to other types of impersonators, who impersonate only ludicrously.

and student bodies, department stores and school systems, Y.M.C.A.'s and army camps, and in every other walk of life. A form of "education" is active of which we must take account. The experienced guardian of youth early recognizes the homosexuals with whom he comes in contact. The general public, of course, cannot identify them until they become involved in scandal. It should be noted, however, that every man who shows a tendency to caress or needlessly handle boys, every woman who "courts" young girls, should be watched.

The instruction of youth in regard to the facts of homosexuality is the best prevention. On the other hand, considerable damage has been done in certain groups by well-intended but ignorant lecturers, who have been unable to distinguish between actual homosexuality and adequate and useful friendships and associations between man and man and between woman and woman.

In addition to the repression of instruction in homosexuality, there is a considerable range of social preventive measures that should be supported, but this is a topic into which the scope of this volume does not permit us to enter.

Many men, and many women, are drawn into casual homosexual practices, but do not become confirmed homosexuals. If the habit has not really been fixed, a change in the environment, the occurrence of normal sexual relations or an intelligent understanding of the situation permit a recovery or end the serious infection. The homosexual situation is however always dangerous, for in many cases a brief learning process fixes the habit.

Where the habit is fixed, the cure is a matter of difficulty. In many cases, the patient does not want to be cured. Like the dope fiend, he is satisfied with his condition. This is especially characteristic of the female pervert. In such cases, there is nothing to be done. But even where the patient is fully awakened to his condition, and urgently desirous of readjustment, the readjustment is no simple matter. How generally the treatment by negative practice may be applied we do not yet know; but it has succeeded in some cases, and there is a reasonable hope of its wider application.

It must be emphasized that, for the homosexual, positive treatment is seldom possible. He cannot break the homosexual habit by simply forming heterosexual habits. In the past, this method has often been recommended to, and attempted by, patients, with uniform failure. Marriage makes a bad matter worse. No homosexual should under any circumstances marry until the homosexual habit is broken. The number of ghastly families (often, unfortunately, with children) in which husband or wife is a homosexual cannot be estimated; but a considerable number are known to every psychologist who deals with family adjustment problems, and the familiar story in the *Decameron* is really lifelike. Recourse to prostitutes, for the male homosexual, is as futile as marriage as a therapeutic measure, although not entailing the suffering of an innocent party.

One reason for this situation is that although seemingly "normal" sexual intercourse is sometimes possible (not in every case, however) to the homosexual, the ideational and emotional factors involved in his sexual acts with persons of the opposite sex are decidedly not those which really occur in normal coitus. The practice, therefore, does not act as a positive learning process, but rather as a negative learning process, turning the pervert more strongly against heterosexual intercourse.

Homosexual acts are, however, possible to the confessed homosexual, and they may be made the basis of effective negative learning against the homosexual processes themselves. We do not, however, feel inclined to prescribe complete homosexual relations, even to the homosexual. From an unbiased point of view, such prescription can be justified. The homosexual is already engaging in such activities. He will continue to do so, if not treated. The treatment would not involve any overt action which will not occur anyhow. Nevertheless, the psychologist is cautious in these matters, and the caution is not to be decried. We have, therefore, considered, in contemplating such applications, whether an approximation, including the form but not the actuality of homosexual acts, may not serve as practice material. We have already seen that an approximation suffices in the case of finger-nail biting. As a matter of fact, it is found that an approximation is serviceable in the case of homosexual negative practice. Whether it will be sufficient in all cases remains to be seen. By using the approximation, the form rather than the substance, adverse criticism (however unfair) is avoided.

The general procedure in these cases is parallel to that in the situations already described. The desire to break the habit, based on an understanding of the actual nature and effects, must be assured. For this purpose, all "moral" (in the popular sense of the term) ideas and attitudes must be avoided by the psychologist. Homosexuality must by no means be viewed as a "sin" or as "scandalous," but as a mere detrimental condition, as appendicitis or a running nose would be viewed. The attitude toward the prescribed practice, as a common-sense procedure, quite commendable for the purpose of getting rid of the habit, must be built up as in the case of stammering. Confidence that the habit will be broken by this procedure is highly important.

In making voluntary performance the practice material, it is obvious that the act must be voluntarily initiated, and that the involuntary impulse will not do. Yielding to the impulse to stammer, to bite the finger nails, cannot be made the basis of negative practice. The patient must always "beat the impulse"; he must initiate the act voluntarily, at a time when there is no involuntary impulse. In the treatment of stammering, tics and masturbation, there is no especial difficulty in securing this condition, even without emphasizing its importance to the patient. In the treatment of homosexuality, this point requires especial emphasis, because the patient will not be directly supervised in his practice, but will be given instructions to carry out in the absence of the psychologist.

Self-treatment is not the best method, from the abstract point of view. In the treatment of most habits it is inadvisable, both because of its inefficiency and probability of failure and because supervision is readily arranged. In the case of homosexuality, the inefficiency of self-treatment (*i.e.*, practice in the absence of the psychologist) is not so marked (partly on account of the age of the usual patient) and there are definite reasons why supervision is not desirable. Among these reasons is the obvious requirement of another person in the homosexual act, even if merely formal.

The approximation to the homosexual act is determined by the features of the particular case treated. The patient is instructed, in particular (1) to initiate the activity voluntarily at a time when the involuntary tendency or urge is not present; (2) to carry the procedure to the point specifically prescribed by the psychologist, *and no further*. Strange as it may seem, the homosexual, if of average intelligence, and properly prepared in accordance with the conditions we have outlined for negative practice, can actually carry out this instruction.

The procedure just outlined may not be applicable to certain homosexual cases. Further work will have to determine the modifications, supplementations and extensions which may be necessary and which may be approvable, for various types of patients. Relapses occur, as in the treatment of other habits. The same procedure of check and re-treatment when necessary, which has been indicated for other habits, is even more important in the case of sexual perversions.

While emphasizing the importance of negative practice in the breaking of habits, and its indispensable function in many cases, we must not overlook the value of positive practice, where applicable, in these cases. We have seen in earlier chapters the large rôle which positive practice plays in learning in general. We have seen there that the relation of positive practice to negative practice is that negative practice begins the learning process and positive practice finishes it. We commence, in the procedure of learning a response, with practice of other responses under the typical negative conditions. We modify, through practice, the practice response itself, away from its actual nature, toward the ideal response which is desired. If the ideal is sufficiently approximated, further practice must be of a positive nature, disposed to fix the response employed, rather than to modify it. That is, the formation of a habit, making the practice response habitual, is the final process of learning.

Efficient learning, then, depends on four primary factors: (1) The conception of the ideal result to which the learning tends. In colloquial terms, knowing what it is that is to be learned. (2) Desire to learn, to achieve the ideal that is contemplated. (3) Learning capacity. There are, of course, differences between individuals in this respect, and for different sorts of learning the same individual differs in abilities. One may, for example, clearly understand the nature of certain difficult billiard shots, be strongly desirous of learning to make them, but finally be unable to "make the grade." (4) The approximation to the ideals which the learner accepts as "satisfactory." The achievement of a response or of knowledge which falls short of the ideal originally concerned, but which is accepted as a sufficient approximation, is always the end of learning, temporarily at least. At this point in the learning process, negative practice is superseded by positive practice, which tends to make permanent the level of accomplishment reached. This happens even if the individual is actually capable of further modification of the response pattern in the ideal direction.

The determination of the point at which, in a learning process, positive practice should be begun is of fundamental importance. In some simple learning processes, such as dart throwing, card sorting, etc., this transition may be determined automatically in the course of the practice without deliberate planning and control. In more practical learning processes, as in school work, learning trades and profession techniques, etc., the determination is usually left to chance, but it is apparent that by scientific determination and control greater efficiency in the learning can be attained. In learning to typewrite, for example, positive learning and negative learning are mixed together in chance order, according as the tyro now succeeds, now fails, to make the precise consideration which is ideally contemplated. The disadvantage of such procedure is that the best conditions for either positive or negative learning are not secured. (This is also true of the learning of the laboratory types above referred to.) The ideo-affective conditions which most forcibly fix the "correct" response, or modify the "incorrect" response, are not established before the response occurs, but occur afterward. Or rather, in the total response, the motor discharge to the muscles which produce the "action" precede the ideo-effective feature of the response which are determinations of the learning, whereas for the maximal efficiency the time order should be the reverse.

In the general process of learning this relative inadequacy of the practice process cannot be avoided. Where, however, a better learning condition can be set up, even as a minor part of the series of practice, it is extremely advantageous to do so. In typewriting, for example, we have seen that at the stage when "errors" of a persistent kind have demonstrated themselves, there is pronounced benefit derived from negative practice on these errors. Such procedure is advantageous in many learning processes, although perhaps not applicable to all.

In the breaking of bad personal and social habits, the ordering of the relationship of negative to positive practice is extremely important. In many cases, as we have seen, the primary process of treatment is through negative practice, because negative practice is possible, and positive practice is not. In some cases, the treatment succeeds with negative practice alone, so far as the formal treatment is concerned. The procedure does not really exclude positive practice. The treatment may not include it, but it is furnished by the responses of the patient in life outside of the treatment periods. In many cases, however, it is essential that positive practice be included in the treatment. It must be remembered that unsuccessful attempts at positive practice (like unsuccessful attempts at negative practice) may be definitely detrimental to the patient. Hence, the supervision of positive practice during the treatment, in order that it may proceed usefully outside the treatment, is an important matter. Equally important is the prohibition of attempts at positive practice in the early stages of the treatment.

In treating stammering, for example, we have noted the importance of having the patient, at the proper time, commence to avoid stammering, through the ordering of the material and practice in such a way that success is possible at the time. In the case of tics, formal positive practice is usually unnecessary, but in certain cases it may be necessary. It may be advisable, for example, for the patient to refrain from biting his nails, in spite of an impulse to do so. It may be important for the value of a head tic to practice holding his head in a normal, balanced position, and the making of head movements of a normal type. The possibility of such measures as an aid to the learning process must be kept in mind by the psychologist.

In the case of homosexual persons, positive practice is an essential part of the therapy. The homosexual young man has no interest in girls. He does not seek their society. It is futile for him, in his homosexual condition, to attempt normal procedure in this direction. His behavior will be as absurd as his actual interests, and a fiasco will be prejudicial. After the homosexual condition is broken, however, even before it is completely abolished, normal interest in the other sex becomes possible. This condition should be utilized as quickly as possible, and association with girls should be commenced under conditions which will make success probable. Dancing, for example, is an activity of high value, social dancing and the technique of the ball having been developed to the point where it furnishes the maximal chances of association with the minimal chance of humiliation or disappointment (provided the relatively simple technique of the dance steps is acquired). The brevity of the dance, the continual change of partners, and the framework of associations supplied by the dance itself are extremely valuable assets, so that the dance provides the best available means of learning social adjustments to the opposite sex. Marriage should be undertaken as the final positive feature of the treatment, but being a permanent condition, it should be entered upon only when certainty of the outcome of the total treatment is assured.

In various types of learning, it may be possible that alternating of positive and negative practice may be a useful procedure. Having attained by negative practice a certain approximation to the ideal, recognized as not final, it may be useful to accept this approximation temporarily, and by positive practice fix it as a stage in the learning from which shortly a further progress by renewed negative learning may take place.

In swimming, such procedure is commonly employed, perhaps wisely, perhaps not. Having mastered a clumsy stroke, which makes it possible to keep afloat, the learner uses this stroke for a period of time, not attempting to improve it, but merely to make it habitual. If he stays at this stage too long, he certainly prejudices his further progress. On the other hand, it is possible that a brief period of positive practice at this stage makes further progress easier, and ultimate accomplishment higher. Even in this simple case, it is impossible to decide whether these alternations are beneficial or prejudicial at best, and in other forms of learning the situation is still more obscure.[3] The question involved is, however, an exceedingly important one for the general problem of learning, and its experimental answer would be a worthy objective.

[3] Such procedure would perhaps account for the "plateaus" in learning curves, but we do not know whether learning with "plateaus" is more or less efficient than learning without "plateaus."

CHAPTER XI

HABITS OF EMOTIONAL RESPONSE

Some of the undesirable traits named in the illustrative list in Chapter IX are currently described as emotional habits. They are habits of responding emotionally to current patterns of stimulation of certain types or they are habitual emotional attitudes which the individual takes to the environment more or less generally. Bad temper, irritability, apathy and timidity are readily recognizable as traits of this sort. The individual who is not normally interested in the objects and persons with which he comes in contact, and in the affairs of his daily life, leads a dull existence and is apt to annoy other persons and to be unproductive in his work. As the habit becomes more pronounced, the individual becomes sluggish physically as well as mentally. Such persons are seldom entirely apathetic. Certain situations and events may arouse them to keener emotional states, but the greater part of the stream of events by which they are surrounded bores them. In this they differ from the normal individual who, apathetic or emotionally unresponsive to many things, as we all must be, finds the greater part of his life interesting and enlivening.

Another person may be over-excitable, stirred emotionally by events which leave the normal person calm, and violently excited by that which only moderately stirs the normal one. Such an individual weeps and sobs or is violently depressed, laughs hysterically, flies into rages, becomes tense of muscles and loud or shrill of voice, fidgets and fusses, and in other ways displays an excess of emotional response under conditions which produce only moderate emotional changes in normal persons.

The timid person has emotions of embarrassment, dismay, apprehension or fear in situations where normal persons show little emotional effect, or where quite different emotions would be more appropriate. As a result, apparently, the timid one lacks courage, assurance and effectiveness in action. Approaching his employer except in routine matters or approaching a difficult and important customer are for him undertakings comparable to a parachute leap for another. In social relations as well as in business or professional relations he is handicapped and weak.

An irritable person may "boil over" in an offensive way or express himself snappishly under stimulation which should be only mildly annoying. On the other hand, he may, for politic reasons, somewhat restrain his outward expressions, but still feel the irritation he does not flagrantly display. The bad-tempered person, however, seldom restrains his expressions of irritation completely. Commonly, such a one, however urbane to persons whose good opinions are indispensable to him, "takes it out" on his employees, on his family or on strangers.

Another type of emotionally maladjusted individual is popularly described as "soft." His sympathy is too easily aroused, and he is an easy prey for clever swindlers. He pities not merely the unfortunate person, but also the deliberate miscreant, and so is an impediment to the maintenance of social order and justice. At the other extreme is the "hard-boiled" man, who is callous to the suffering and misfortune of others and who spares the feelings of no one. He may, because of his callousness, be personally successful in the ways he most values, but he is nevertheless an inimical agent to the social order, and is therefore rightly classed as maladjusted.

Self-pity, unlike the emotional defects above described, is not an exaggeration of a normal habit, but is a trait which is undesirable throughout. In any degree, it is a weakness which tends to make defect permanent and turns handicaps into millstones about the neck. However deserving a man may be of pity from others, he cannot afford to pity himself. The neurotic, from whatever complex of disadvantageous traits he may suffer, is especially prone to self-pity, which confirms and strengthens his neurosis. The one general feature of the remedial treatment of neurotics, which is essential for almost all cases, is the breaking of the habit of self-pity.

Self-satisfaction or emotional egotism, on the other hand, is a social maladjustment, rather than a personal one, except where it is developed to a high point. The smug egotist may succeed in business or in a profession or in politics. He is offensive, however, to his associates; and the degree to which they resent his selfishness determines the effect which his egotism has on his personal fortunes.

Repentance is always a weakness, and sometimes becomes a habit. The emotionally sound individual does not repent. He estimates his mistakes and his misdeeds intellectually, and then dismisses them as matters of the dead past, concerning which he need take no emotional attitude whatever. He is in a position, accordingly, to profit in the future by the errors of the past, to avoid doing thereafter that which he has judged morally wrong or practically disadvantageous on the basis of his experience. Repentance, on the other hand, is an emotional living in the past which makes improvement less probable. For neurotics who are not by themselves able to rise above repentance, absolution is frequently a necessary aid. We are not here using the term "absolution" in the technical sense of the forgiving of sins, but in a broader psychological sense, which, however, includes the theological significance. From the theological point of view, the effects of past acts or omissions threaten the sinner in the form of the vengeance of an affronted divinity. From a psychological point of view, past actions rankle in the memory in settings of disturbing emotion. The result is as deadly as the divine displeasure is theologically supposed to be, and the most easily demonstrable punishment which sin brings is really through the emotional contemplation of the assumed divine threat. The burdens laid through repentance upon the one who has committed errors of any sort are distinct from other consequences of the misdeed; and these burdens can be abolished. The emotionally freighted remembrance of past responses is frequently a trait of maladjusted persons, and is in fact itself a serious maladjustment. The psychologist who persuades the neurotic that the events of the past can be crossed off the account, except in so far as their demonstrable effects persist; and that they are, therefore, not matters for emotional concern is in effect granting absolution to the patient. This is especially important for the neurotic whose complex of maladjustments is bound up with events of his past life which are criminal, vicious or otherwise matters of adverse public opinion or conventional reprobation. Some of the cases, in which the granting of absolution, with the necessary complex steps leading up to it, have sufficed to effect a cure, are striking.

A typical case is that of the "ruined" girl, whose repentance prevents her reëstablishing her social relations on a sound basis, and brings about other maladjustments of varying sorts. Although "ruined" girls are not as frequent as they were a generation ago, since the breaking of social conventions by the female is today considered a remediable fault, characteristic cases are still occasionally found. A girl who had led a "virtuous" life up to a certain time, defining "virtue" as stopping short of copulation, slips over the arbitrary line. She may, thereafter, be haunted by the emotional thought that the act is irrevocable and has removed her from the class of "respectable" girls. By repenting; that is, by brooding emotionally over the past behavior, the girl may become neurotic with a combination of any of a considerable number of maladjustments. Failure in school work or in a commercial vocation may be the symptoms which attract the attention of other persons. Usually there are other symptoms which the layman is less apt to note.

Absolution is ordinarily granted to such girls by explaining to them that no girl needs to be permanently damaged or "ruined" by any such episode; that while certain consequences of the behavior may be inescapable, these need have no serious effect on her success and happiness, nor even on her ability to lead a chaste life if she elects to do so. She may be brought to see that she has made a blunder, involving unjustifiable risks. Perhaps her experience is presented as analogous to needless exposure to infection by typhoid fever or to carelessly falling off a horse. She comes to see that she may cross off the past events, so far as emotional consideration is concerned. The girl learns (sometimes with astonishing rapidity) a new attitude; the repentance habit is broken; the patient resumes her normal habits of life; and so far as can be ascertained, the probability of a future lapse from conventional behavior is smaller than before. Absolution has been given and has been efficacious.

Repentance is not so frequent among women who have lapsed from conventional sex behavior as it is among men. The girl or married woman who has engaged in illicit sex relations usually does not regret or repent of her conduct at all.[1] She may, however, become neurotic through constant fear of exposure and social punishment. Men are the conspicuous repenters of sexual conduct which infracts the peculiar male standards, and through such repentance they frequently establish serious maladjustments, although the

[1] We are here not discussing the prostitute or the frankly loose woman. It should be noted, however, that the "repentant Magdalen" is almost exclusively a character in pious fiction.

repentant attitude does not at all inhibit the formation of the habit of infracting the masculine code.

We need not multiply cases. An emotional state is a response. All responses, including emotional responses, are capable of becoming habitual. We learn to respond emotionally in certain ways, and some of these ways are advantageous or adequate while some are inadequate. The formation of adequate emotional habits is a process into which the features of learning we have described in the foregoing chapters enter as definitely as into the acquisition of skills or knowledge of the less emotional types. The proper emotional development of the individual depends on expert guidance, the detailed analysis of which fills many of the topics of child psychology and the psychology of adolescence. The breaking of bad emotional habits is work for the direction of the psychologist expert in mental adjustment. The present discussion must be confined to fundamental principles.

Clearly to understand the problems presented by emotional habits, we must view the emotions in the proper light. We must know, if possible, what an emotion really is. If we cannot, at present, discover the real nature of emotions, we must approximate the facts as closely as possible.

In our first chapter, we have set feeling over against thinking and perceiving. Several of the illustrations given of the process of feeling might be otherwise described as "having an emotion." If, in accordance with the conventions of every-day speech, I say that I "feel glad" I mean the same thing as when I say I "have an emotion of gladness." Moreover, we use the noun "feeling" as equivalent to the noun "emotion" in many cases, so that I could also say: "I have a feeling of gladness." There is obviously some verbal confusion here which may be resolved without difficulty. Let us admit that we *feel*, and then when we feel, we feel *something*. This "something felt," we happen to call now a "feeling," now an "emotion." In strict psychological analysis, the two terms are not really synonyms. We restrict the term "feeling" to the apparently simpler "some things felt," such as pleasure, pain, excitement and strain; and we apply the term "emotion" to the more complex things felt, such as anger, fear and sorrow. The distinction is obviously of a tentative sort, but it is worth while to observe it.

The view of these feelings and emotions which is now prevalent, and which is well founded, is known as the "James-Lange theory of the emotions," from William James and Carl G. Lange, who, among others, formulated the essential points of the theory independently. According to this theory, an "emotion" is a bodily state, which is the result of a response to some stimulation. This bodily state, in turn, is an effective stimulation to nerve endings in the body, through which a new response is initiated, and this second response is that in which, or through which, we "feel" the "emotion." In modern formulations, we state that the action-pattern in which the first response ends, or a certain part of that action-pattern, *is the emotion.* The second response, the stimulus for which is the action-pattern of the first response, is required in order to *feel* the emotion.

We may illustrate this theory by reference to a hypothetical operation which might be performed on a dog. If we should cut all the dorsal (afferent or sensory) roots of the spinal cord, leaving intact all of the ventral (efferent or motor) roots, responses initiated in the cranial sense organs could discharge to the muscles and glands of the body as in the normal animal, but no responses could be initiated except through the cranial senses. The dog's muscles would lose their tonus as a result of the operation; that is, they would become relaxed and flabby. If we assume, however, that after a sufficient recovery time, the muscular tonus would be sufficiently restored, the dog would then show the same "emotional" expressions upon visual, auditory or olfactory stimulation as he showed before the operation. He would cower, growl, wag his tail, etc., much as usual. In terms of the modernized James-Lange theory he would *have* the emotions, but would be unable to feel them.

This experiment, apparently, has not been performed, and perhaps would not be operatively a success. Sherrington, however, has performed a more drastic operation, severing the dog's spinal cord in the neck region, and severing the vagus nerve. This completely cuts out all afferent and efferent connections between the brain and the body, leaving only the connections between the brain and the head and part of the neck. The only expressions which could occur, therefore, would be "facial," with head movements. According to Sherrington's report, these "facial" expressions after recovery from the operation were much the same as before the operation. Reports on a dog's "facial" expressions may not be convincing, but these at least agree with the James-Lange theory.

In simple statement, the emotion includes the bodily processes which are popularly called the "expression of the emotion," and the "effects of the emotion." The "expression" is the muscular action-pattern which is the immediate result of the stimulus, including such actions as tensing or relaxing of muscles, postural changes, facial expressions, exclamations, etc. The "effects" are the changes in the visceral organs, which may be as directly produced as the outward muscular movements, but which are slightly more delayed. Some of these inner features of the emotion, in fact, may be delayed for several seconds after the initial stimulus and are suspected to be indirectly aroused through intermediate glandular activity. This serial order of arrival of different features of the emotional pattern are readily noted by one who is practiced in observing his bodily states under emotional stimulation.

We might adopt another terminology, which James seemed to favor, and apply the term "emotion" to the inner action-pattern only, excluding the pattern of the action of the external or skeletal muscles, which might then still be called the "expression." This separation is of questionable validity, however, and is not material to our present discussion.

James assumed that the first response, which causes the emotion, is a perceptual one. The normal dog first perceives his master and then feels the emotion which results from the perceptual response. This would imply that the first response, producing the emotion, involves the cerebrum (cerebral hemispheres), since we have always assumed (without any definite proof, to be sure) that responses do not involve consciousness unless they include activity of the cerebrum. Cannon, however, has lately produced evidence that responses producing emotion do not necessarily involve the cerebrum, but that the nerve current entering from the sense organs may be shunted out to the muscles and glands from the thalamus (a part of the brain lower than the cerebrum). This, however, does not change the essential features of the James-Lange theory, the emotion still remaining as a complex of bodily conditions produced as the end result of a normal response, this response being certainly sometimes perceptual, although we may admit that sometimes it is not.

We see then that the emotional responses are responses of the same type as others, and should be subject to the laws of learning and habit formation. We see also how emotional habits may affect the physiological functions profoundly, as they seem to do. We are not surprised at finding, for example, the habit of mental apathy may produce bodily sluggishness, and the habit of alertness and interest may improve the general bodily functioning. The well-known fact that sudden laughter may produce non-voluntary urination, that fear may produce defecation and that depression may upset the digestive process—these are as intelligible as the facts that each of these involves changes in the external muscles.

In popular discourse and in the psychology founded on language habits, we have a long list of emotions. Attempts have been made to reduce these to a fewer number of "primary emotions" from varying proportions of which the others are compounded. These attempts have seemed generally unsuccessful, and the failure is probably due to overlooking the fact that the popular nomenclature is not based upon distinction between different feelings, but upon differences in the situations which arouse feelings and emotions, and differences in the thoughts which enter into various emotions which are popularly distinguished.

An emotion is really a complex affair, and feelings constitute only a part of its complexity. A feeling, in any useful meaning of the term, is a bodily state, existing in muscular and connective tissue, and capable of stimulating nerve endings. By the neural stimulation from such a stimulus-pattern, a response may be initiated; and through this response the individual may be conscious, more or less vaguely, of the bodily state which initiates it. We call the response a feeling response, and say that through it one "feels the feeling," or that he "has the feeling."

In "having an emotion," we are not merely "having feeling." The actual response is complicated by thinking, or rather by what would be thinking if it occurred alone, instead of being complicated by the feeling factor. The thinking which occurs in emotional response is by far its most important feature. Popular usage is well founded in distinguishing emotions by the sort of thinking which is involved in them, for without the thought-feature, there would be no emotions, only feelings.

The valid objection to the James-Lange theory in its original form is that it is not sufficiently analytic in its applications. If we designate it as a theory of the *feelings*, instead of a theory of *emotions*, we shall be doing violence to James' expressed prejudices, but we shall be retaining the important points in the theory.

We distinguish fear from anger. We say that these are distinctly different emotions. In what way are they different? They may differ in the feelings involved. From the point of view of introspective analysis, however, fears under various circumstances differ from one another very much, and angers under different circumstances also differ markedly from one another. In fact, it is not clear but that the feeling component in a certain anger may be more like that in a certain fear than it is like that in a certain other anger. Conversely, it seems possible that one emotion of fear may differ from another, as regards the feeling components, more than it differs from a particular emotion of anger. Introspection settles nothing, but it raises interesting problems. When we consider the actual reasons why certain emotional conditions are classed together as "fear" and certain others are classed together as "anger," we find the reasons in the external situations which stimulate or arouse the responses and in the thoughts which combine with the feeling process.

Fear and anger both normally arise in circumstances where the individual is threatened, where something or someone threatens to injure him, or to deprive him of something. Not all emotion arising under such circumstances is fear or anger, however. There must be, in addition, the recognition of the threat as actual and serious. The thought-component is the actual determinant. The distinction between anger or any other emotion and fear depends on the thoughts involved. If the threatened individual thinks that the injury or deprivation is not to be averted by his actions, the emotion is fear. If he thinks it can be averted, the emotion is something else. This distinction is valid, regardless of the actual feelings involved.

We class an emotion as anger only when the threat comes from the action or the expected action of another person or from some object or source which is personified. Further, there is necessary the thought of resisting such adverse acts, or of preventing the enemy from performing the act, or of inflicting punishment for the act. The mere thought of injuring another person does not constitute anger, whatever the feelings associated in the response. Such a response may be cruel; but without the thought of a threat or completed injury from the other person, it is not anger. The particular feelings involved in anger are apparently various, and the external expressions are far from uniform.

We are familiar with the fact that in fear, almost any external expression may occur. The individual who is afraid may be struck immobile; he may tremble; he may run; he may be muscularly relaxed; he may fight; he may laugh; he may weep. All of these variant expressions occur also in anger. We might reasonably expect, therefore, that the visceral changes would be as various, and that the feelings which are felt in these emotions are correspondingly diverse. The diversity does not matter, so far as our naming the emotions is concerned. Fear is any feeling, or complex of feelings, whatever which is integrated with certain thought processes. Anger is similarly a type of thinking in given circumstances, combined with any feelings which may arise in these circumstances.

The analysis of fear and rage gives us the key to other emotions. Jealousy, shame, pride, grief—these and all other emotions which might be named, together with the vastly larger number which have no conventional names, are determined by the thinking involved in them, and the quality of the feeling involved is not material. Each of these emotions is essentially a certain thought-complex, integrated with any feeling process whatever. Although aroused in conspicuous cases by definite external stimulations, the thought-complex characterizing any emotion may be aroused in some other way; and if feeling occurs, the result is the specific emotion. Feelings, it is clear, are not always aroused perceptually or by sub-perceptual reflexes, but may be aroused by thought processes, in sequences of responses which have been made habitual by associative learning as described in earlier chapters.

The extreme variability of feelings entering into emotions such as fear is not characteristic of all emotions. In grief, for example, the range of feelings which actually occurs is apparently more limited. This does not confuse the analysis, for even in grief, it is the thinking which determines the emotion, as is indicated by the fact that not only the external expressions, but also the feelings characteristic of grief, may occur in quite different emotions also.

We must distinguish between emotions, properly so-called, in which the feelings and the ideas are integrated in a temporal pattern, and *emotional moods*, in which feelings persist or repeatedly recur, with little qualitative variation, through successive changes in thought topics. Apathy and callousness, for example, are not emotions, but emotional moods. Whether the bad habits which are matters of mood rather than habits of emotional response in the strict sense require different forms of treatment in their correction remains to be seen.

We must distinguish emotions also from the systematized habits of feeling which are properly designated as *sentiments*. Love and patriotism are not emotions, and not feelings, but are typical sentiments which involve in definite patterns of habit many emotions and many feelings. The mother experiences pleasure or tender emotion when she caresses her child. She has fear when it is in danger, and anger when some person threatens to injure it, or speaks slightingly of it. She has pride when the child does well, and shame when it does ill. Under still other circumstances, disgust, grief, and almost the whole gamut of distinguishable feelings are appropriate to love. Maternal love, in short, is a complex habit of having emotions in specific circumstances, and is defined or described through the specific circumstances in which the specific emotions occur. Patriotism also includes almost the full list of emotions of which the human being is capable. The patriot is angry when his country is insulted, fears when it is seriously threatened, rejoices in its successes, is depressed when it is in evil circumstances, is disgusted at its bad management. He experiences pride, shame and a host of other emotions in thinking of his country in various ways and in various circumstances. Sexual love differs from other sentiments in including amorous feeling, being perhaps the most inclusive of all sentiments. The difference between the selfish man and the benevolent man is a difference in sentiments, both experiencing the same emotions (as usually described) but in different habitual patterns.

The variations in sentiments are unlimited, as variations in complex habits in general are unlimited. Among the infinite variety we have names for those only which, for some cause, it is practically useful to label.

What we call emotional maladjustments are in many cases bad habits of the sentimental sort. Bad emotional habits usually do not stand alone, but are parts of complex habit systems, and must be diagnosed and treated as such.

The foregoing analysis of emotions should show that in forming "good" emotional habits, and in breaking "bad" ones, we have to deal primarily with thought and with thought habits. "As a man thinketh, so is he" certainly is true for emotional traits, if we understand the saying in both its positive and its negative implications. Feelings may be in themselves important for practical life. The importance of feeling in emotional habits is slight, however, as compared with the importance of the thought-factor.

If abnormal habits of feeling have a foundation in disease or other abnormal organic conditions, they need remedial attention; otherwise, they may be given minor consideration in therapeutic work. It is of great practical importance to impress on the neurotic patient that his feelings, as reported by him, may be of use to the psychologist in diagnosing his case, but so far as he himself is concerned, *it doesn't matter how he feels.* Often, when the patient really grasps and accepts this, his cure is in great part accomplished.

The interpretation of an individual's statement as to how he feels is no simple task. So accustomed are we to the confusing of feelings with emotions, and to the labeling of our emotions in accordance with their important thought-features and in accordance with circumstances in which the emotions arise that our attempts to identify or to analyze our fundamental feeling contents are seldom successful. When an individual reports that he feels "discouraged," "enthusiastic," "depressed" or "cheerful" or "excited," it means little, except that he is thinking of his circumstances, his problems, his successes or his failures in certain characteristic ways. The actual feelings, apart from the thoughts, cannot be inferred simply from such statements. A man, for example, may report that he "feels cheerful," when he means merely that his circumstances, as he evaluates them, are such as should, in his estimation, make a man cheerful.

Expressions of emotion or feeling by external actions other than speech are often no more trustworthy than are words. A man who is whistling "cheerfully" at his work may have feelings which do not differ much from those he has later when he sighs dolefully. A smile is no guarantee of pleasure or of approval, although some men may smile only when pleased.

The most reliable verbal reports, and the most indicative expressions other than those of speech, occur when feelings are fairly well localized. Aches and pains, abdominal discomforts, pleasant feelings resulting from food or from the stroking of the head—these are illustrations of localized feelings. The feeling of those bodily conditions, in fact, approaches measurably to the process we call perception, and these "felt" conditions are on this account sometimes distinguished as "bodily sensation." Fatigue occupies a place intermediate to the vague, unanalyzable and unlocalized bodily state which we call "feelings proper," and the more definitely localizable, and more clearly attended-to feelings. The reports on fatigue conditions are correspondingly intermediate in reliability. Yet no statement as to relative degrees of fatigue is to be trusted. If one man says that he feels "very tired" and another says he does not feel "tired," there is no way in which we can tell how the feelings of the two men actually compare. In the same individual, we frequently find sudden changes in report and expression which cannot be ascribed to bodily changes in any process which might reasonably be designated as a feeling of fatigue, but which are obviously due to a change in attention and in ideational processes. One feels exceedingly fatigued; then several entertaining persons arrive on the scene, and the fatigue vanishes. Is this due to the actual change in a feeling? Perhaps it may be, in part. In large part, the situation is comparable to a "trick" picture, which at one time is the picture of a rabbit and at another time the picture of a duck. The picture has not changed, but the thought processes of the one looking at it have changed.

Attempts have been made by several psychologists to devise simple tests for emotional attitudes and habits. Some of these tests involve long lists of questions as to how the individual feels under definite conditions of treatment by associates or parents, whether he was happy or unhappy in certain periods of past life, etc.; the questions all being formulated to require a simple "yes" or "no" answer. That in checking the results of experimental use of these tests we find that the same individuals will give opposite answers to many of the questions at different times occasions no surprise. The individual is no more able to report reliably his habit of feeling than he is to report the particular feeling at a particular time. In the majority of such questions, the person tested can truthfully answer both "yes" and "no" to the same question, according to the particular way he generalizes. Reporting on feelings experienced in the past is even less accurate than reporting on present feelings.

Tests which attempt, on the other hand, to obtain reports on immediate feelings aroused by presenting certain words which are names of objects, situations, processes or functions (bath-tub, larceny, lascivious, kissing, etc.), which might be assumed to have unpleasant associations for some and pleasant associations for others, achieve no more success. We cannot tell, from the reports, whether the words really arouse the feeling reported or not, and if we should assume the actual occurrence of the feeling reported, we would have no data on the causes which have led to the association of the feeling and the word.

If some short and easy method were available for determining the emotional habits of individuals, the work of mental adjustment would be much simplified. Unfortunately, no such method exists. Each maladjusted person must be studied as an individual case. His performances, his symptoms and his reports must all be carefully weighed. His reports on his feelings and emotions will be in the main not representative facts. They may, however, give the necessary clues to his habits of thinking.

There can be no doubt that in many semi-popular discussions, including many which are supposed to be technical, the emphasis placed on the emotional aspect of behavior is misunderstood both by authors and speakers and by their audiences and readers. Emotional factors in response are important; their main importance lies in the ideational processes which characterize them. The psychologist, in studying the maladjusted individual, will necessarily pay careful attention to his descriptions of his emotional attitudes and his feelings. He will interpret these descriptions, however, as indicating primarily directions and habits of thinking, together with habits of perceptual response. He will take especial care to require as little introspective work from the subject as possible, since introspection, on the part of the man who does not himself understand the significance or the non-significance of his self-observations, increases maladjustment through the fixing of the habit of self-observation and self-emphasis and the development of self-pity, and through the strengthening of self-deceptive faith in the literal significance of his own verbally formulated report.

CHAPTER XII

LEARNING ABILITY AND INTELLIGENCE

THE fact that an individual can learn we state more abstractly by saying that he has *learning ability*. When two individuals, subjected to closely similar conditions for learning, show different results or when they have to be subjected to different conditions to attain the same results, we say that the two individuals differ in learning ability. "Learning ability" is an abstraction, and like all abstractions it is apt to be misused. We may forget, for example, that it is an abstraction and assume that it is something which explains learning. Used with due caution, however, the concept is a useful one.

Since "learning ability" is an abstraction, we may define it in a variety of ways, each definition determining a different abstraction. It may be defined as a fixed factor for each individual, constant through life. It may be defined as variable, in any of several ways. We may conceive it as increasing or decreasing with age, or with the accumulation of knowledge and skill. We may define it as a single "general" learning ability, abstracted from all learning in every learning situation, or we may define it as multiple, involving different specific learning abilities, abstracted from each situation in which learning occurs or may occur. No one of these definitions is more valid than any other, except in so far as we may find one or another more practically useful. All such definitions are "true," and the abstractions they determine may each be useful for certain scientific purposes. Most of them, however, are unwieldy when we attempt to fit them to the results of experimental work.

In experimental work on learning, we set definite standards of achievement of knowledge or of skill and adopt definite criteria for the measurements of the results. The practice is regulated by methods, time allotments and materials which are as definite as we are able to make them. Our measurements of the results give us data which are valid for the conditions under which the learning occurs, with no intimation as to further extensions of validity. From the results of experiments under several different conditions we may generalize in statements which cover all the conditions and may hazard predictions as to what will be found under still other conditions. These generalizations and these predictions are not data. Our data are obtained by measurements under specific conditions, and the more definitely these conditions are known, the more valid the data.

Any conception of a "general" learning ability, therefore, must rest on the primary conception of diverse learning abilities, specific to the conditions under which the learning occurs. For all psychological purposes, the conception of a particular learning ability for each different learning situation is fundamental. When we rate one person as higher or as lower in learning ability than another person, such a rating is for the ability to learn under the specific conditions under which the measurements are made.

We find that John learns more arithmetic than Thomas when employing the same method of study for the same length of time. We say then that John has the greater learning ability—for arithmetic, when studied under the set conditions. John may have greater learning ability for arithmetic than Thomas when other conditions are used; he may have greater learning ability for Greek or tennis or homosexual vice, when the two boys are subjected to similar conditions for learning or habit formation in these topics. Our experimental data, however, do not tell us this. Generalization can be useful, only when backed by comprehensive experiments covering the range for which we wish to generalize.

We may measure the learning abilities of the two boys, under conditions identical for each, for arithmetic, Greek, tennis and geography. We may then average the measures of ability of each boy for all four subjects, and find that John has the higher average. He has, then, the higher average learning ability—for these four subjects. We might then form the abstraction of a single learning ability for the four subjects, but this abstraction merely represents the average of the performances represented by the specific abilities.

"Learning ability" has in recent years become for many psychologists synonomous with "intelligence." If this identification of terms were complete, and consistently adhered to, there would perhaps be no objection to the usage. Unfortunately, however, few who accept the identification actually adhere to it, but by employing the term "intelligence" sometimes to signify learning ability, and sometimes in a quite different sense, arrive at a confusion of conclusions and deductions.

The word "intelligence" has had a variety of meanings, and one of these meanings, perhaps the most common, has become so intrenched in our speech that its implications are almost unavoidable. We speak of a division or bureau which has the function of collecting or disseminating information as an "intelligence division" or "intelligence bureau." We also speak of bits of information as "intelligence." Intelligence, moreover, in the philosophy which was formerly widely accepted, and is still embedded in current language, is contrasted with "instinct." These are illustrative manifestations of an ancient and persistent use of the term "intelligence" to signify what is otherwise called *knowledge* and *skill*, and which is admitted in part at least to be a result of learning (that is, of *acquisition*). Where the word "intelligence" is used to designate knowledge or skill, we shall, in the discussion which follows, call it *intelligence I.* Where, on the other hand, "intelligence" is employed to designate learning ability, we shall call it *intelligence II.* A still different use of the word, to indicate a vague collection of assumed abilities, including the ability to learn, we may call *intelligence III*, although there are a considerable number of variant usages included under this particular designation.

The originators of "intelligence tests" seem to have assumed that the tests measured intelligence III, and this assumption is still maintained by some educational psychologists, while others have seemed to think that the tests measure intelligence II. Hence we had for some years the remarkable spectacle in America of the most prominent exploiters of intelligence tests claiming publicly and teaching their students that "intelligence tests measure capacity directly, uninfluenced by acquisition." One of these educators, probably the most influential in the Eastern States, proclaimed to a gathering of college professors that there was no possibility of improving a man's score on an intelligence test (aside from cribbing, of course) except by raising the man's intelligence, which (he said) is impossible, since intelligence is an innate capacity!

Experimental psychologists, on the other hand, pointed out that the tests measure directly only intelligence I; there being no question or problem in any of the tests which could be answered or solved except from knowledge or skill previously acquired. This saner view has gradually prevailed, and the most pernicious phase of intelligence testing has passed, although in wide usage today, intelligence tests are applied and interpreted, and the victims disposed of as if the tests measured intelligence II directly.

The confusion probably arose through the use of the ambiguous word "ability." Intelligence tests do measure "abilities" of certain orders. A child cannot point to his nose as required in the Binet-Simon Test unless he has the "ability" to do so. No one could solve the arithmetic problems which are important features of many intelligence tests unless he had the "ability" to solve the problems. These "abilities," however, are all learned or acquired, on the basis of the more fundamental "ability" to acquire them. They are matters of knowledge and skill. By adopting the term "intelligence" as indicating "abilities" of some sort, it has apparently been easy for the promoters of tests to confuse the acquired "abilities" they were really measuring with the presumably innate "abilities" they longed to measure. The objective was to measure intelligence II, or some form of intelligence III. The tests did measure "intelligence," and the fact that they measured intelligence I only was overlooked because the several meanings of the term "intelligence" were not logically discriminated.

If, indeed, learning capacity could be measured directly, it would be a grand thing. We could arrive by simple tests at the comparative abilities of different individuals, without worrying about their "instincts," or the history of their learning processes. The score of each individual on an adequate test, properly administered, would be a relative measure of his intelligence II. Unfortunately, we have no tests of this kind. The only tests we have measure only intelligence I directly.

The knowledge which an individual has depends upon his actual opportunities to learn, including not merely school conditions and instruction, but social and economic circumstances also, and upon the desires or incentives to learn. If we can assume that opportunities and incentives have been the same for all the individuals in a group, the results (that is, the knowledge or skill) may be assumed to vary with the learning abilities of the individuals, defined as excluding opportunities to learn and incentives to learn. Learning ability so defined may be called *basic learning ability.* If the basic abilities are the same, and opportunities are the same, the results (knowledge or skill) may be expected to vary with the incentives to learn. If incentives and basic abilities are the same, then the results may be expected to vary with the opportunities.

In order to infer to basic learning ability (intelligence II) from the results of an "intelligence test," we must then be assured that opportunities and incentives are identical for all the individuals tested, or rather that difference in these respects are below the fineness of measurement of the test. Obviously, we can never be certain that this essential condition is secured, and we must hence proceed to make corrections for the differences as far as possible.

For one thing, we can deal only with groups which have had comparable training. The tests must then be so constructed that they bear heavily on the common feature of the training and avoid effects of training in which the individuals differ decidedly. These conditions may be met, in a measure, in the devising and application of intelligence tests for college entrants. All candidates have been subjected to somewhat similar training in primary and secondary schools. If we assume, for a moment, that their trainings have been the same, then differences in results will be due to (a) differences in home and social training; (b) differences in basic learning ability; and (c) differences in incentive, which indeed may in part be due to differences in the a and b factors, but are not necessarily entirely so conditioned.

We are further assisted in this work by the fact that many individuals who, through basic incapacity or faulty incentives, have not learned adequately, have been eliminated at the end of primary education or during high school. We have then, in candidates for college entrance, a selected, relatively uniform group. Intelligence tests designed to bear exclusively on the results of the school work of these individuals, avoiding other factors, give some basis for inference as to basic learning ability, although complicated by differences in motivation, and from these inferences we may, within certain limits, predict the success or failure the candidate will have in college.

We come here on two interesting facts. First, in this case, the best intelligence test is the test which best shows what the student has acquired from his previous studies. The specious distinction between an "intelligence test" and and an examination of the usual type vanishes, and the only excuse for an "intelligence test" applied to candidates for college matriculation is the abject failure of the high schools to determine what students have actually learned in their courses. Second, we have slightly altered our definition of "intelligence." As applied to the inference from the test results, "intelligence" is a complex of learning ability and incentive to learn. But for practical purposes, this complex is exactly the basis for inferences which we need. If we understand by "learning ability" (intelligence II) a basic ability to learn, distinguishable from the incentives which may effectuate the learning, we shall have to designate the intelligence to which, for practical purposes, we wish to infer from the results of intelligence tests, as intelligence IV.

It is obvious that the actual school trainings of candidates for matriculation are really not identical, and hence that there is a latitude of error in the ratings given these individuals. This defect can be partially avoided by a skillful making up of the tests, based on a thorough study of the various schools from which the candidates come. Into the technical requisites for a satisfactory test of this limited scope, and the details of manufacture we cannot digress. It should be noted, however, that no such test is simply put together, but requires, at the least, many years of work on the part of persons of high technical training,[1] and that no

[1] This is not in conflict with the fact that a number of spurious tests, hastily concocted by incompetent persons, are on the market and are highly profitable to their owners.

test, however good, covers a wide field. A test, for example, which adequately picks out the poorest one-tenth of the candidates in a total group, is not adequate to select the highest or best one-tenth, and *vice versa.* Any test is adequate in proportion as it is devised for a particular limited purpose, and for use on a narrowly selected group.

A good intelligence test for entering freshman and other similarly limited trade tests (for the college entrance test is strictly a trade test, and all trade tests, whether for plumbers or motormen, must be devised in accordance with the same stringent conditions) stand at the extreme of the range of intelligence tests. At the other extreme is the "general intelligence" test, of which the Binet-Simon scale is the most worthy example. The Binet-Simon test is intended for use with groups in which selection is low; that is, which includes individuals from various economic and social levels and subjected to school conditions of wide variation. Hence, the plan of the tests avoids as far as possible the results of special training, seeking to measure only knowledge and skill for which the opportunity of acquisition is offered to everyone. Such an objective, of course, cannot actually be attained. The nearest approximation to it is found in the cases of children up to ten years of age. Beyond that age no generally applicable test seems possible.

Even within the narrow age limits, the tests are only roughly applicable to groups differing in social and economic station and in family and school training. By nearly twenty years of labor, Binet succeeded in putting together tests which bear on knowledge which the great majority of French children had had opportunity to acquire. This series of tests, of course, is not applicable to American children in general, whose home, school and social training differ on the whole from that of the French; hence for American use extensive revisions were made by Goddard, Kuhlman and Huey, who introduced the Binet-Simon test into America;

and still further revisions were made by Terman and his assistants. For children of other nationalities, still more drastic revisions are required. When the test adopted to the particular nationality (*e.g.*, "The Stanford Revision" for American children) is applied, the individual scores can in no case be interpreted as indicative of relative intelligence II or relative intelligence IV. Even in the same school, the training and opportunities of the children have been various, and until these are equated, the significance of the scores is uncertain.

For statistical purposes, this is a matter of no consequence. In statistical studies one does not need to know the significance of the individual scores at all. All one needs is the happy assurance that if the score ratings are interpreted as indicating the relative intelligences II or IV, there will be as many children rated ten, twenty, and thirty per cent too high as are rated ten, twenty and thirty per cent too low, and so on. If thousands of children are tested by experts in administering the tests, and if the test has previously been standardized on a group whose average actual condition is the average actual condition of the group now measured, this requirement is reasonably well satisfied. While the actual standardization of the tests in America has not been made on a sound statistical basis, and the rough standardization seldom is adequate to the groups to which the tests are later applied, certain statistical corrections can be made and the results may be statistically important. The interpretations for individual cases, however, are quite different matters, and no amount of statistical manipulation can soothe the wrongs of the child who is rated low in intelligence II or IV when he is not really low, or the corresponding child who is rated high in intelligence when he is really low.

The competent clinical psychologist, of course, takes the test score merely as indicating knowledge or skill (intelligence I) and makes no assumption as to learning capacity (intelligence II or IV) until the environment, training and general history of the child have been investigated, and fair allowances made for their deviation from "standard" conditions. Unfortunately, however, intelligence tests are widely used (or abused) by persons who are not acquainted with the significance of the tests; and although many of the errors made by certain clinics in which psychological verdicts are determined by non-psychologists are subsequently rectified by competent psychologists, a vast number escape check, to the grave misfortune of the children and their parents.

The Binet-Simon tests are peculiar in that, through their sagacious construction, useful measurements are actually obtained in groups where it would seem almost impossible. The essential conditions for such success are definite. (1) The test must be adapted to, and standardized for, the most common characteristics of knowledge and skill of the group to be measured. Binet-Simon tests adapted to English children are inadequate for children of other nationalities because the things which these other children have commonly learned are not the same things which American children have learned. The tests have been devised for children and have only a limited and vague significance for adults or children over ten. Furthermore, the scores for children of the upper social and economic classes are not directly comparable with the scores for children of the tenement district, and so on. (2) No significant results are obtained unless the tests are administered with technical adequacy. This proficiency is attained, in most cases, by personally qualified individuals with solid background in psychology, through two or three years' work in actually administering the tests under the supervision of an expert psychologist. Test scores obtained by a layman, or one with superficial psychological training, are sometimes interesting, but seldom significant. (3) Reliable scores having been obtained, they may be said to represent certain phases of the knowledge of the individual tested, but the learning capacity cannot be inferred except on the basis of the previous opportunities for learning and the conditions under which learning has occurred. The significance of a child's failure to tell "in what way wood and coal are alike (as required in the eight-year-old test, for example), cannot be determined until it has been found out what opportunities the child has had to learn something about coal. (4) Since the norms for each age-group have been determined for that particular age group, the intelligence (II, III, or IV), as determined for children of any given age, has no assignable relation to the intelligence of children of other ages. If the tests have been properly standardized, therefore, and large numbers of unselected children of all chronological ages from four to ten are tested; and if the scores made on each age-test by children of that age are averaged, then the average for each age should be the same. This does not indicate an average constancy of any kind of intelligence from year to year, but is merely a predictable result of the adequate construction and standardization of the tests.

Sufficient has been said to indicate the definite relation of "intelligence tests" to the general learning problem. In between the Binet-Simon type and the trade type, there are a multiplicity of test types; and beyond the trade tests, there are the "tests of special ability," by which the results of learning more restricted topics, such as typewriting, etc., may be estimated. These are all "intelligence tests" as the term is conventionally used. They are direct measures of the results of learning, *i.e.*, of knowledge or acquisition, in short, of intelligence I, and from them may be inferred the basic learning capacity (intelligence II) or at least the complex of learning ability and motivation (intelligence IV). If any reader has the least doubt that intelligence tests are designed directly to measure knowledge and skill only, he may resolve his doubts by examining the Binet test in any of its forms. He will find no question and no task in any part of the test which can be answered or performed unless the child has learned the answer or the method of performance. That this fact is implicitly recognized, even by the belated mental testers who do not explicitly admit it, may be certified by comparing so-called general achievement tests with so-called general intelligence tests of the best type. They are tests of the same kind.

Learning ability, we have earlier pointed out, is an abstraction which may be formed in different ways, and the particular abstraction which is most useful is the one which should be employed. For example, we may define learning capacity as (1) highest in infancy, and decreasing steadily, although not necessarily uniformly, with age; (2) as constant throughout life; or (3) as increasing during early life and decreasing in later life. Each of these three conceptions has a certain applicability to problems of learning, but there is at present no way of determining which is the most applicable. Each of these is, of course, a different learning capacity, but none can be said to be a "true" capacity as distinguished from the others. We are at perfect liberty to adopt any one of the three conceptions, provided we adhere to it. Each of these conceptions offers serious difficulties in fitting it to the phenomena of learning, and, in any case, an adequate fitting will require experimental work. No amount of experimental work or of statistical work can decide which theory should be adopted, except on the basis of the simplicity of application.

The tests of knowledge and skill, which, like the Binet-Simon, use an age-scale, explicitly and intentionally avoid the problem of age-differences in learning capacity as we have seen above. Each age-test is standardized for children of that age, and there is no assumption whatever as to the learning capacity at different ages. The only assumption is that the child of a given age who passes the test for children of that age has at least the knowledge and skill of the "normal" child of that age. Under proper conditions, as above explained, we can infer from these results that he has the intelligence (II or IV) of the average child of that age. Whether this is lower or higher than the capacity of a normal child of another age is a matter of indifference to the test. Conversely, if a child of eight passes the test for age five but flunks the test for six and higher ages, we cannot under any condition infer that the child has the intelligence of the child of five. We can say (if the conditions of the test and interpretation earlier emphasized have been fulfilled) that the child does not have the learning capacity of the normal eight-year-old child; if he had, he would have learned as much as the normal eight-year-old child has. He has learned less; by hypothesis he has had as good a chance to learn. Hence his learning ability must be lower. But the capacity of the child in point may be as much lower than that of the normal five-year-old child as it is below that of the normal eight-year-old child, for the normal capacity of these two ages may be the same. Our conclusion on this point depends on the definition of learning capacity which we adopt. The Binet-Simon Scale, however, is so designed as to be entirely independent of this problem. Whatever normal gradient of learning capacity we assume, the Binet-Simon results are equally valid, for they are made independent of the assumptions through the standardizing of the age tests separately.[2]

[2] It is somewhat amazing to the psychologist to find investigators naïvely attempting to determine whether or not *intelligence* actually remains constant from year to year. Such investigators, obviously, have failed to understand the plan of construction of the tests, and the method of standardization. Failure to find constancy of the proper numerical index of intelligence from year to year (for the averages of large groups, not for individual children) would merely show that the lists were imperfectly constructed or inadequately standardized. It is doubtful, however, whether the I.Q. is even roughly an adequate index of any sort of intelligence. The I.Q. or *intelligence quotient* derived by Ebbinghaus (who called it the *intelli-*

It is true that popular statements of Binet-Simon results are in language which implies an ascending gradient of learning capacity in the normal child from four to twelve. If the eight-year-old child's test shows a "pass" for the five-year test, and "failure" beyond, it is said that he "has the intelligence of a five-year-old child." This, however, is easily explained if we recall the three kinds of intelligence above distinguished. The child has the intelligence I (knowledge and skill) of the five-year-old child. No assumption is made as to the "age" of his intelligence II (capacity). The intelligence ratio, therefore, is a knowledge-ratio, and not a capacity ratio. Although it may show that the capacity is subnormal or supernormal, it gives us no indication as to its relation to the normal capacities of other ages.

Intelligence tests which do not employ the age-scale system, but use one test for all purposes, scoring on points which represent the total number of questions answered correctly, or tasks acceptably completed, may also be independent of the problem of capacity age-gradients. In the Army Alpha test (to take a familiar illustration) the man of twenty-five who makes the score which is made by the average boy of twelve may be of lower learning capacity than the average man of twenty-five, which would account for his having learned less. But there is no assumption involved in the test which would permit us to infer that his learning capacity is the same as that of the boy of twelve. It may be as far below that of the normal twelve-year-old as it is below that of the normal twenty-five-year-old. If we should make such an assumption, it would amount to a definition of learning ability as identical with knowledge and

gence ratio) is the ratio of the highest age-test the individual passes to his chronological age. Thus, if an eight-year-old child passes the five-year test, and not the six-year test, his I.Q. is computed as 5/8 or .425. This is, of course, an illicit process, for which the Binet tests have never been designed or standardized.

skill, irrespective of training. If a boy has not the "ability" to solve a problem in calculus without having studied the subject, his learning ability would be rated as less than that of another boy who can solve the problem, but has studied calculus. If the first boy, after studying calculus, could solve the problem, we would have to say that his learning ability has been increased!

We have spoken above of "general intelligence tests," meaning tests which are generally applicable to wide ranges of individuals, instead of to selected classes. We have indicated that (with possibly a certain exception in the case of children of ten or below) the significance of a test is inversely proportional to its generality of application. There is, however, another interpretation of the term "general intelligence test," which is somewhat confusing. The term "general" is sometimes taken to indicate (such is the power of words) that there is "general intelligence" (as contrasted with "special intelligence").

Doubtless there are differences between different individuals in respect to kinds of learning ability as there are manifestly different kinds of knowledge and skill. One may "know" arithmetic, grammar and history to a fairly high degree. Another knows little of these, but has an extensive knowledge of baseball scores, movie actors and comic strips. One man is highly skilled in the law, another skilled in cabinet making, and so on. Are there corresponding differences in the underlying capacities? Is it possible that one man has a high capacity in learning law, with less capacity for acquiring manual skills; and another has the capacities in reverse relationship? Or is the whole result a matter of environment, opportunity, incentive and different levels of "general" capacity? On the whole, the evidence tends to show that there is an indefinitely large variety of capacity patterns. That there is a correlation between certain learning capacities (as empirically determined) is also probable.

In other words, capacities to learn certain things, as carpentry and plumbing, are possibly closely related, and may vary concomitantly; but the correlation between certain other abilities to learn, or rather between abilities to learn certain other things, may be low or zero. In all mental testing, we are striving to find and use the positive relationships, that we may, by measuring accomplishments in lines which have been undertaken, be able to predict success in lines which may be undertaken in the future. We are indeed never certain of the value of a correlation until it has been experimentally established, and the establishment can be only approximate. Hence an essential condition of adequacy of any intelligence test is that the *knowledge which is measured by the test must be as closely as possible allied to the knowledge for which it is desired, on the basis of the test, to predict ability to learn or acquire.*

Again we are brought up against the fact that high specificity is the characteristic of a valid test or measure. But what can we do in cases where specificity is impossible? (1) Where we do not know in detail the past training of the individual to be tested, or where we know the training has been diverse. (2) Where we do not know the specific line of prediction as to future success which will be needed. (3) Where the persons to be so indefinitely rated are beyond the years of childhood. This is the broadest case of general application, and requires a broad test, one which measures nothing in particular, for no particular purpose. This is what the determining of "general intelligence" really means.

The best that can be done is to select tests of sufficient variety, to cover a wide range of training and a wide range of possible predictive purposes. We thus measure "general knowledge" in the sense that we test for widely scattered bits of information, and score by the number of correct answers. We thus determine the extent to which the individual knows the kind of things we ask him about. But we do not determine his information on topics which are not in the test. In so far as the topics are important for the future work of the individual, the scores are significant. We are measuring "general intelligence," however, only in so far as "general" means "unspecified." The more "general" the intelligence test, the less its value. By increasing the specificity, in regard to the known past training or opportunities of the individuals, and in regard to the lines of learning, for which prediction is desired, and by lessening the generalization of the test, we add to its value. Charles Dudley Warner once shot a bear by "aiming at it generally," but it is a poor method.

For applied psychology, as in mental testing, a considerable element of "generality" may be permitted, and yet the results of application be valuable. For experimental work on problems of learning capacity, on the other hand, the requirements are more stringent. The highest specificity is required, and it is not probable that any work with intelligence tests of the sorts which are useful for rating groups of individuals will contribute to our psychological knowledge directly. Properly designed, properly applied and properly interpreted, such tests have demonstrated their large practical value in educational and social problems. As instruments for research, except on problems of technique and procedure, in the construction and application of such tests, they are sometimes of subsidiary use, but not of primary importance.

APPENDIX

APPENDIX

A. HISTORICAL NOTE

HABIT formation is a central topic in psychology, closely knit with the other psychological topics, and with the physiology of response and behavior. The first treatise on psychology, at least, the earliest extant, was Aristotle's περὶ ψυχή (*Peri psyche:* in Latin, *De Anima*), which has been rendered into English as *Psychology* (Hammond's translation, 1902, Scribner's, N. Y.), and as *On the Vital Principle*. The word "psyche," unfortunately, has conventionally been translated as "soul," which has in English implications sharply different from those which "psyche" had for the Greeks. Aristotle's psychology, like the psychology of today, was the study of the process and conditions of living; and living, as we have seen, is fundamentally learning and unlearning.

The development of theories of learning, and of experimental research on learning, should be the most interesting and important parts of the history of psychology. These histories, however, have not been written except in fragmentary ways. G. Murphy's *History of Psychology*, 1929, Harcourt, Brace, N. Y., contains a brief history of experimental work on memorizing and on the learning of skills in chapters XI and XV, but nothing on the development of the theories of learning from Descartes to James which dominated the conceptions of psychology until very recently. Boring's *History of Experimental Psychology*, 1929, Century, N. Y., is an excellent collection of biographical sketches of men who have contributed to the development of psychology, experimental and theoretical; with some historical materials on the philosophical theories which have shaped the subject, but none on the especially important theories of learning. On the development of the problems, methods and techniques of experimental psychology, which the title of the book might lead us to expect would be fully treated, there is very little.

The particular phase of learning in which Aristotle and the philosophers from Descartes to Herbert Spencer were interested is the "association of ideas." H. C. Warren's *History of the Association Psychology*, 1921, Scribner's, N. Y., covers this in outline, with references to sources. The equally important development of the "brain-path" theory of learning which has its roots in Descartes' writings has not been systematically worked out by any one, although the materials are abundant. Whether or not there were important foundations of the later work in the centuries before Descartes will not be revealed until the Latin writings of those centuries are adequately analyzed.

Experimental work on learning was initiated by Hermann Ebbinghaus, shortly after 1880. Ebbinghaus published his results in *Ueber das Gedächtniss*, 1885, Leipzig, and presented them in a more critical way as a chapter in his *Grundzüge der Psychologie*, 1813, Leipzig. The early monograph has been translated by H. A. Ruger and C. E. Bussenius, *Memory*, 1913, Columbia University; the chapter is now being translated and will appear in *Psychology Classics*.

Ebbinghaus worked only on serial learning, using as materials the "nonsense syllables" which he invented. His problems included the effects of magnitude of task, distribution of practice periods, amount of learning, conditions affecting retention and mediate associations. These problems, and Ebbinghaus' methods have remained since as important features in experimental psychology.

Further pioneer contributions from Germany were made by G. E. Müller and F. Schumann, Experimentelle Beiträge zur Untersuchungen des Gedächtnisses, 1893, *Zsch. f. Psychol.*, 6: 81-190, 257-339; G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, 1900, *ibid.*, Ergänzungsband I.; and Lottie Steffens, Experimentelle Beiträge zur Lehre von oekonomisches Lernen, *ibid.*, 22: 331-382, 465. Müller began work in 1887, and introduced paired association material and a mechanically actuated exposure device to replace the simple reading from typed sheets employed by Ebbinghaus. Steffens, working in Müller's laboratory, performed the first experiment in comparison of the "part" and "whole" methods of learning.

The pioneer contributions from America were notable. W. L. Bryan and N. Harter, Studies in the physiology and psychology of the telegraphic language, 1897, *Psychol. Rev.*, 4: 26-53, performed the first experiments on the learning of skill, through practice in sending and receiving the Morse code. They introduced the learning-curve, or practice-curve, suggested by Ebbinghaus' graphic representation of retention, and they noted the "plateaus" in the curve which indicate periods of cessation of learning-progress. Bryan's views as to the significance of these plateaus are given in a second paper by Bryan and Harter, Studies on the telegraphic language: the acquisition of hierarchies of habits, 1899, *Psychol. Rev.*, 6: 346-375. These plateaus were a topic of much speculation at the time, but their importance was much overestimated.

Experimental work on animal learning was begun independently by E. L. Thorndike with cats and chicks, and by Willard Small with white rats. Thorndike's investigation, commenced at Harvard in 1896 and completed at Columbia in 1898, is reported in Animal intelligence, 1898 (June), *Psychol. Monog.*, no. 8 (Vol. 2, no. 4). Small carried out his investigations at Clark University, in 1898 and 1899, with the backing of Professor E. C. Sanford against Stanley Hall's disapproval, and with practical coöperation from Small's fellow student, L. W. Kline. Small read Thorndike's monograph before publishing his own work, but apparently was not influenced by Thorndike. Thorndike had priority both in the commencing of his work and in publication, but Small's work was apparently the more influential on the development of the researches on animal learning which have since flourished.

Small's Notes on the psychic development of the white rat, 1899, *Amer. Jour. Psychol.*, 11: 80-100, was merely a detailed study of the development and behavior of the rodents from the first to the twenty-eighth day after birth, plainly modeled after Wesley Mill's observations on puppies. His second and third papers, An experimental study of the mental processes of the white rat, 1899, *Amer. Jour. Psychol.*, 11: 135-165; and 12: 206-239, contain the accounts of his learning experiments.

Small used, apparently, the first problem boxes of the entrant type; that is, boxes into which the animal had to find his way, and at Sanford's suggestion, a modified form of the Hampton Court maze which is diagrammed in the article on Labyrinths in the *Encyclopedia Britannica.* These apparatus have been made the bases of standard apparatus for animal learning since. Small employed food motivation for the learning of the problem boxes and the maze, procedure which has been followed in the majority of animal learning experiments by Small's successors.

In addition to demonstrating the measurability of animal learning, Small profoundly influenced later investigations by emphasizing the importance of individual differences, age differences and sex differences in rat learning. From observation of rats which become blind through disease, Small tentatively inferred the primacy of kinesthetic processes in maze learning, and suggested the operative destruction of the cranial senses as a step towards the investigation of the kinesthetic factor. He did not carry out this suggestion himself, although he did remove the vibrissæ from one rat.

One of the immediate effects of Small's work was that of interesting John B. Watson at the University of Chicago. Watson's first research reported in *Animal Education: an experimental study of the white rat, correlated with the growth of its nervous system,* 1903, Univ. of Chicago Press, was begun with problem boxes expressly modeled after Small's and with methods branching out from Small's. Watson's second research, *Kinesthetic and organic sensations: their rôle in the reactions of the white rat to the maze,* was carried out with a copy of Small's modified Hampton Court maze, and upon Small's suggestion of the importance of kinesthesis, and the operative technique of destroying the cranial senses. In both publications, Watson made it clear that he was following out Small's suggestions and checking his results, but he expanded the scope of the investigations.

Thorndike used mainly the escape motivation, confining his animals in cages and enclosures from which they had to learn the method of escape. This motivation and type of problem box have not been much used in animal work. Thorndike's work, however, undoubtedly added its effect to those of a number of other workers in stirring up interest in animal learning as an experimental field. It was Watson's work, however, which caught the popular interest, and had the major effect in drawing many young psychologists into this new field.

Between the beginnings of the work of Small and Thorndike, and the publication of Watson's second report, a number of papers on animal learning experiments appeared. In 1901: Thorndike, *Mental Life of the Monkey,* Macmillan, N. Y.; R. M. Yerkes, Habit formation in the turtle, *Pop. Sci. Mo.,* 58: 519-25; Norman Triplett, The educability of the perch, *Amer. Jour. Psychol.,* 12: 354-60. In 1902: A. J. Kinnaman, Mental life of two macacus rhesus monkeys in captivity, *ibid.,* 13: 98-148, 173-218; R. M. Yerkes, Habit formation in the green crab, *Biol. Bull.,* 3: 241-44. In 1903: R. M. Yerkes and G. E. Huggins, Habit formation in the crawfish, *Harvard Psychol. Stud.,* I: 576; R. M. Yerkes, The associative processes in the green frog, *ibid.,* 1: 579-638. In 1904: J. P. Porter, A preliminary study of the psychology of the English sparrow, *Amer. Jour. Psychol.,* 15: 313-46; E. G. Spalding, An establishment of association in hermit crabs, *Jour. Comp. Neur. and Psychol.,* 14: 49-61; and Allen, The associative processes of the guinea pig, *ibid.*, 14: 293-359.

Of the men just mentioned, Yerkes deserves particular recognition, and should be bracketed with Small among the pioneers of animal learning. Yerkes commenced his animal research at Harvard, and contributed the punishment motivation to animal research. He is the only one of the pioneers who has remained continuously in animal research. His investigations were extended from the animals named above to the earth-worm, crow, ring-dove, dancing mouse and pig, but since 1915 has confined his scientific attention to apes and monkeys.

All of these publications were influential in promoting further work on animal learning. For the influences which put this whole group of investigators at work, we must look to the writers who were interested in the general field of animal psychology. Among these were G. J. Romanes and Lloyd Morgan, whose books however were mainly of the popular, anecdotal type; Wesley Mills, Professor of Physiology in McGill University, who collected his articles published from 1888 on in *The Nature and Development of Animal Intelligence*, 1898, Macmillan, N. Y.; C. O. Whitman, *Animal Behavior*, 1898, Ginn, Boston; Jacques Loeb, *Einleitung in die vergleichende Gehirnphysiologie und vergleichende Psychologie*, 1899, Leipzig (*Introduction to Comparative Psychology of the Brain and Comparative Psychology*, 1900, Putnam, N. Y.); Pierre Hachet-Souplet, *Examen psychologique des animaux*, 1900, Paris; L. W. Kline, Methods in animal psychology, 1899, *Amer. Jour. Psychol.*, 10: 257-279, and Suggestions towards a laboratory course in comparative psychology, *ibid.*, 10: 399-430. The publications of Fabre, Wasmann, Forel, Pieron, Jennings, D. A. Spaulding and Preyer are not to be overlooked.

In connection with the development of the animal work in America, the personal influence of several men not conspicuous as direct contributors to the field, including H. H. Donaldson at Chicago, C. B. Davenport at Harvard, and C. F. Hodge and E. C. Sanford at Clark, is plainly indicated.

B. REFERENCES AND NOTES ON TOPICS DISCUSSED IN THE SEVERAL CHAPTERS

The references in the lists given below are intended to furnish starting points from which the topics may be pursued through the mazes of the literature. In some cases they bear only indirectly on the topic, direct bearing references being few for some of the topics to which special emphasis is given in the text. Many of the books and articles contain bibliographies from which further references may be drawn. The student will need to supplement these by use of the *Psychological Index,* in the volumes of which the books and articles appearing each year are grouped under appropriate topical headings. For the more recent literature, the General Reviews and Summaries in the *Psychological Bulletin;* Memory 1928 and 1930, and The acquisition of skill, 1927, 1929, 1931, will be found especially useful.

In citing periodicals the following preferred abbreviations are used. Variant forms appear in other bibliographies, but can be readily identified from this list.

Abnor.: abnormal. *Acad.:* academy. *Advan.:* advancement. *Amer.:* American. *Anat.:* anatomical, anatomy. *Anim.:* animal. *Arch.:* archives. *Appl.:* applied. *Asso.:* association. *Behav.:* behavior. *Bibliog.:* bibliographical. *Biol.:* biological, biology. *Brit.:* British. *Clin.:* clinical, clinic. *Coll.:* college. *Comp.:* comparative. *Cont.:* contributions. *Delinq.:* delinquency. *Dis.:* diseases. *Diss.:* dissertations. *Educ.:* educational. *Embryol.:* embryology. *Exper.:* experimental. *Gen.:* general. *Genet.:* genetic. *Hdbk.:* hand book. *Inst.:* institute. *Internat.:* international. *Jour.:* journal. *Med.:* medical. *Ment.:* mental. *Mo.:* monthly. *Monog.:* monographs. *Nat.:* national. *Neurol.:* neurology, neurological. *Ophth.:* ophthalmological, ophthalmology. *Otolar.:* otolaryngology. *Ped.:* pedagogical. *Person.:* personnel. *Philos.:* philosophical, philosophy. *Pop.:* popular. *Proc.:* proceedings. *Psychiat.:* psychiatric, psychiatry. *Psychoanal.:* psychoanalytic, psychoanalysis. *Psychobiol.:* psychobiology. *Psychol.:* psychological, psychology. *Pub.:* publications. *Quar.:* quarterly. *Rec.:* record. *Rep.:* reports. *Res.:* research. *Rev.:* review. *Sci.:* scientific, sciences. *Sem.:* seminary. *Soc.:* society, social. *Ser.:* series. *Suppl.:* supplements. *Surg.:* surgical, surgery. *Teach.:* teachers. *Univ.:* university. *Zsch.:* zeitschrift. *Yrbk.:* yearbook.

The prepositions *of, in* and *for* are regularly omitted from English names of periodicals. *De* and the forms of *der* are represented by *d.,* and *für* by *f.* The monograph supplements of the *Psychological Review* are variously cited in bibliographies, but are here designated as *Psychol. Monog.*

It will be noted that in all citations the year is placed directly after the title. This is an improvement on the conventional order, having the advantage, in the case of journal articles, of separating the year from the numbers indicating volume and pages.

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CHAPTER I

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Dorcus, R. M., and Gray, W. L., The rôle of kinæsthesis in retention by rats, 1932, *Jour. Comp. Psychol.*, 13: 3.

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Yoshioka, J. G., Is vision useless for the rat in a familiar maze?, 1929, *Jour. Genet. Psychol.*, 36: 342 ff.

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Angell, J. R., The doctrine of formal discipline in the light of the principles of general psychology, 1908, *Educ. Rev.*, 36: 4-8.

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Orata, P. T., The theory of identical elements, 1928, *Ohio State Univ., Contrib. in Princ. of Educ.*, No. 3.

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Whipple, G. M., The transfer of training, 1928, *27th Yearbook, Nat. Soc. Educ.*, 179-209.

Winch, W. H., The faculty doctrine: correlations and educational theory, 1911, *Jour. Philos. Psychol. & Sci. Meth.*, 8: 337-348; 372-384.

7. *Experiments on the rate of tapping and simple reaction time:*

Baety, A. M., Some observations upon practice as they affect rate of tapping, 1909, *Amer. Jour. Psychol.*, 20: 449-455.

Dodge, R., The reaction time of the eye, 1899, *Psychol. Rev.*, 6: 477-483.

Külpe, O., *Outlines of Psychology*, 1895, Trans. by E. B. Titchener, Swan Sonnenschein, London. (See pp. 406-411 for a discussion of simple reaction-time experiments.)

Lemmon, V. W., Discriminative reaction time, 1927, *Arch. Psychol.*, 94: 38 ff.

Macht, D. I., and Isaacs, S., Action of some opium alkaloids on the psychological reaction time, 1917, *Psychobiol.*, 1: 19-32.

Wells, F. L., Normal performance in the tapping test, 1908, *Amer. Jour. Psychol.*, 19: 437-483.

Woodrow, H., The measurement of attention, 1914, *Psychol. Monog.*, 17: 16-65. (The influence of varying the preparatory interval on reaction time.)

8. *The influence of knowledge of results on learning:*

Arps, G. F., Work with knowledge of results versus work without knowledge of results, 1920, *Psychol. Rev. Monog.*, 28: 41.

Boswell, F. P., and Foster, W. S., On memorizing with the intention permanently to retain, 1916, *Amer. Jour. Psychol.*, 17: 420 ff.

Gilliland, A. R., The effect of practice with and without knowledge of results in grading handwriting, 1925, *Jour. Educ. Psychol.*, 16: 532-536.

Judd, C. H., Practice without knowledge of results, 1905, *Psychol. Rev. Monog.*, 8: 185-198.

Rich, G. J., Directed attention and learning, 1917, *Jour. Educ. Psychol.*, 8: 239 ff.

Wang, C. H., The influence of tuition in the acquisition of skill, 1925, *Psychol. Monog.*, 34: 5.

Waters, R. H., The influence of tuition upon ideational learning, 1928, *Jour. Genet. Psychol.*, 1: 534-549.

CHAPTERS VII AND VIII

RETAINING, RECALLING, RELEARNING, REMEMBERING AND FORGETTING

Discussions and data on these topics are found for the most part as details in articles on various phases of learning, and an adequate bibliography would include references to almost the whole experimental and theoretical literature of learning. In most cases, unfortunately, authors make no distinction between the several sorts of retention, and are apt to apply the results of measurements of one sort to conclusions concerning other sorts. "Remembering" and "recalling" are far too often used as interchangeable terms, obscuring the vital difference. In many discussions and experimental reports "memory" is used to designate any effects of knowledge-learning whatever, and still more confusingly in biological discussions, to cover all learning effects. The references below should be useful to the reader, but some of the articles will be found confusing and inaccurate on the above points. Effective progress lies in the direction of the interpretation of data in terms of the concepts distinguished in the text of Chapters VII and VIII.

In our present ignorance of what goes on in the brain in learning, retention can be defined only in terms of the effects which are probable after a lapse of time. The delayed effects as we have pointed out are of several sorts, namely: (1) Skill or knowledge, previously learned, may be reproduced. (2) What has been learned and forgotten may be relearned more easily than it was previously learned. (3) What has been learned, forgotten, and not relearned may be recognized when again presented. (4) What has been learned may be effective later in the modification of other responses. The common assumption that the retention in these different cases is the same in qualitative respects, differing only quantitatively, is obviously self-contradictory. There may be a common element in these different retentions, but we have no information about it. We have at present no warrant for excluding the possibility that the actual brain effects of learning are complicated, and that the different delayed effects may depend on different features of this complicated process.

1. ***The nature and measurement of recognition and recall, and relevant topics.***

Anderson, J. P., and Jordon, A. M., Learning and retention of Latin words and phrases, 1928, *Jour. Educ. Psychol.*, 19: 485-496.

Dodge, R., *Elementary Conditions of Human Variability*, 1927, Columbia Univ. Press, N. Y.

Hausing, O., The doctrine of recollection in Plato's dialogues, 1928, *Monist*, 38: 231-262. (Shows that this doctrine is really an hypothesis to explain the learning process.)

Harden, L. M., The effect of emotional reactions upon retention, 1930, *Jour. Gen. Psychol.*, 3: 197-221.

Koch, H. L., The influence of some affective factors upon recall, 1930, *Jour. Gen. Psychol.*, 4: 171-190.

Krueger, W. C. F., The effect of overlearning on retention, 1929, *Jour. Exp. Psychol.*, 12: 71-78.

MacDougall, R., Recognition and recall, 1904, *Jour. Philos.*, 1: 229-233.

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Munsterberg, H., and Bigham, J., Memory, 1894, *Psychol. Rev.*, 1: 34.

Myers, G. C., Recall in relation to retention, 1914, *Jour. Educ. Psychol.*, 5: 119.

—— A comparative study of recognition and recall, 1914, *Psychol. Rev.*, 21: 442.

Pan, S., The influence of context upon learning and recall, 1926, *Jour. Exper. Psychol.*, 9: 468-491.

Stoke, S. M., Memory for onomatopes, 1929, *Jour. Genet. Psychol.*, 36: 594-596.

Symonds, P. M., Size of recognition and recall vocabularies, 1926, *School and Soc.*, 24: 559-560.

Trow, W. C., Recall *vs.* Repetition in the learning of rote and meaningful material, 1928, *Amer. Jour. Psychol.*, 40: 112-116.

Whitely, P. L., The dependence of learning and recall upon prior intellectual activities, 1927, *Jour. Exp. Psychol.*, 10: 489-508.

Whitely, P. L., and McGeoch, J. A., The curve of retention for poetry, 1928, *Jour. Educ. Psychol.*, 19: 471-479.

2. *Memory devices for quick learning, and methods of efficient study.* (See also the references to Chapter VI; The conditions of efficient learning.)

Book, W. F., *How to Succeed in College*, 1927, Warwick and York, Baltimore.

Dashiell, J. F., *Fundamentals of Objective Psychology*, 1928, Houghton Mifflin, N. Y. (Cf. pp. 380-385, discussion of improving memory.)

Martin, P. R., and Fernberger, S. W., Improvement in memory span, 1929, *Amer. Jour. Psychol.*, 41: 91-94.

The following present the more deplorable aspects of popular notions concerning the improvement of memory:

Feinaigle, Gregor (von), *The New Art of Memory*, 1912, Sherwood, London.

Grey, R., *Memoria Technica: A New Method of Artificial Memory*, 1730, London.

Pick, E., *Memory and Its Doctors*, 1888, Trubner, London.

Renshaw, S., An experiment on the learning of "paired associates," 1927, *Jour. Appl. Psychol.*, 11: 226-233. (Shows the comparatively short time and small amount of practice required for undergraduates in the laboratory to duplicate one of the exhibitional feats of a memory expert.)

Strong, R., Another attempt to teach how to study, 1928, *School and Soc.*, 28: 461-466.

Whipple, G. M., Experiments in teaching students how to study, 1929, *Jour. Educ. Res.*, 19: 1-12.

Wiley, J. A., *Forty Choice Ways of Learning and Recalling*, 1928, Author, Cedar Falls.

3. *Retention and reproduction in lower animals.*

MacBride, E. W., *The Idea of Memory in Biology*, 1928, New York, Oxford Univ. Press, London.

Maier, W. R. F., Reasoning in white rats, 1929, *Comp. Psychol. Monog.*, 6: No. 3.

Tinklepaugh, O. L., An experimental study of representative factors in monkeys, 1928, *Jour. Comp. Psychol.*, 8: 197-236.

Tolman, E. C., Habit formation and higher mental processes in animals, 1928, *Psychol. Bull.*, 25: 24-53.

Warden, C. J., The sensory capacities and intelligence of dogs, with a report on the ability of the noted dog "Fellow" to respond to verbal stimuli, (Proc. Galton Soc.), 1928, *Eng. News*, 13: 2-6.

Wundt, W., *Lectures on Human and Animal Psychology*, 1912, N. Y., Macmillan. See especially Lecture 23, Problems of animal psychology, pp. 340-352; Lecture 24, Mentality of the higher animals, pp. 353-366.

Yerkes, R. M., A new method of studying ideational and allied forms of behavior in man and other animals, 1916, *Proc. Nat. Acad. Sci.*, 2: 631.

—— Ideational behavior of monkeys and apes, 1916, *Proc. Nat. Acad. Sci.*, 2: 639.

—— and Yerkes, D. N., Concerning memory in the Chimpanzee, 1928, *Jour. Comp. Psychol.*, 8: 237-271.

CHAPTER IX

PERSONAL AND SOCIAL ADJUSTMENT

To give a few important references on maladjustments or a brief list typical of works of high order is an impossible task. The general reader or the beginning student may profit by S. I. Franz, The abnormal individual, Chapter 21 in *The Foundations of Experimental Psychology*, 1929, Clark University Press, and Pierre Janet's *Principles of Psychotherapy*, transl. by H. M. and E. R. Guthrie, 1924, and *Psychological Healing*, transl. by E. and C. Paul, 1925, Macmillan, N. Y. Beyond such elementary helps, useful reading is a tedious selection from clinical material in much of which, unfortunately, it is almost impossible to arrive at the facts because the descriptions include only features which can be stated in the terms of the clinician's theory. Text-books on "abnormal psychology" are of little assistance, since the materials for systematically covering the field have not yet been gotten together; hence, authors feel obliged to fill in wide gaps by naïvely falling back on one or another sort of psychoanalysis. J. J. B. Morgan's *The Psychology of Abnormal People*, 1929, Longman, N. Y., is probably the best text in this field, but the lack of sound foundation in this as in the other texts is indicated by the references the authors give, which are almost without exception to works of little scientific or practical importance.

In view of the hullabaloo which has been created in recent years about the "unconscious," the lay reader may perhaps wonder why nothing has been said in this volume about "unconscious habits."

He may wonder why in the discussion of bad habits and neurotic traits nothing was said about the function of the "unconscious mind" which has been popularized by psychoanalysis. The reason can be given in a few words. We have discussed all the facts which are attributed by non-psychologists to the unconscious mind, but we have discussed them without confusing them under a misleading term. By so doing, we avoid the mummery and the mysticism which have sprung up around the vague concept of "unconsciousness," which is merely a god drawn from the machine to cover ignorance. The discussion in terms of sober matters of fact is perhaps less thrilling and less impressive to the layman than it would be if the mythological demon which psychoanalysis supposes to inhabit or infuse man were made the magic source of human conduct. Psychologists, however, prefer to go behind these mythical entities and forces and, where possible, to get at the humdrum facts. Where the facts are not yet available, it is honest to confess ignorance, and busy ourselves with the problems in which the facts are buried.

On a low level of human knowledge it is appropriate to explain phenomena by constructing gods and demons. For the Greeks of Homer's stories, thunder and lightning were simply explained as anthropomorphic actions of Zeus. This explanation in fact is verbally valid today if we understand by Zeus the total system of causes which eventuate in thunder and lightning, and are not further interested in the phenomena of sound, light, heat and electricity. Such explanations, however, do not advance science or human living.

Psychologists may perhaps be accused of resorting to another *deus ex machina,* the brain. To a certain extent, this indictment may be just. We know little about the brain physiologically, and the tendency to construct divinities does appear at times in the naïve explanations of psychological processes by verbal reference to the unknown functions of the brain. The saving fact is that psychologists themselves recognize this danger, and the leading members of the profession are constantly warning against it. We are careful not to draw upon the brain for even descriptive help, except in those instances in which the cumulative results of neural histology and physiology make it clear that the brain is involved in psychological processes; and even in those instances we are not under the illusion that by referring to the brain we have explained the processes.

Psychology has gone a long way from the theories and speculations of the eighteenth and nineteenth centuries upon which the "unconscious mind" is founded. We no longer believe in ideas as entities which may appear in or disappear from an enclosure called the mind. We no longer believe in sensations as mental entities which can have "copies." We do not accept the notion of consciousness as a mental entity, subject to observation as a leaf or a toad may be observed. Mental objects and mental processes as transformations in a mystic mental ground substance have gone to join Zeus and phlogiston. The real difference between psychology and the popular systems such as psychoanalysis lies not merely in the avoidance of the concept of the unconscious mind; it lies deeper in the discarding of the antiquated conscious mind on which these anachronistic pseudo-psychologies are based.

We still have left the problems of perceiving, thinking and feeling; the problems of living. We still find "consciousness" a useful abstraction in the discussion of these problems. We may still use the term mind—but we use it in a matter-of-fact way, if we do not avoid it because of the confusion which the term has for many people. We have no entities, however, except physical entities and abstractions. Perceiving, thinking and feeling we look upon as reactions or responses, because we have no other scientific way of looking at them. The increased knowledge of the ways in which these are bound up with, and dependent upon, organic reactions leaves us no other point of view.

The term "conscious" is employed in psychology just as it is in the more consistent popular usage. It designates thinking, perceiving, and feeling indifferently, and has no further implication. If we assume that an individual is either thinking or perceiving or feeling we say that he is conscious. If we have reason to assume that he is doing none of these, we say that he is not conscious, or unconscious. The word unconscious has no other meaning in psychology. By this usage we avoid all assumptions except the universal assumption, denied by no one, that men do see, hear, and otherwise perceive; they do imagine, remember and otherwise think; they do have pain and excitement, and otherwise feel. The explanation as to how man is able to be conscious is another matter. These are problems for factual investigation, and are not prejudiced by the simple classificatory use of the terms conscious and unconscious.

From this point of view, an important feature of habit is always unconscious. This feature is retention. The learning process is conscious; recall or other forms of reproduction are conscious, but retention is not. This is an obvious matter of fact. There would be no harm in defining the "unconscious mind" as the abstract condition of anything of which one is not conscious. In that sense, everything in the entire universe, but of which I am not at the moment thinking, or which I am not perceiving or feeling, is in my unconscious mind. This seems to be a superfluous terminology, and we might restrict the unconscious mind to containing everything which I have ever experienced, but am not now experiencing. Probably no one would consider this concept as defined to be useful. We may therefore try a still more limited definition, altering the style of phraseology. The unconscious mind includes everything that I have experienced, and which is subject to reproduction, or which in any other way affects my conduct, but which I am not actually remembering.

This final definition actually does cover a great part of what is meant in popular literature by the terms "unconscious" and "unconscious mind." The less pretentious term for this, however, is simply *retention*. To become excited over the discovery that retention is unconscious strikes the psychologist as somewhat like the child's discovery of the moon. When we use the term retention, however, we recognize that it presents an unsolved problem instead of an explanation, and that retention is a complicated matter, and not a homogeneous entity, place or force. Those who use the more pretentious term are in constant danger of apotheosizing the concept as final and explanatory, and as having other engaging features which could not be smuggled in under the prosaic term "retention." They are also in danger of forgetting the limits of the definition, and drawing in features of the first and second definitions we have above suggested. There are in the literature innumerable instances in which these very things have been done.

The confusion in use of the term "unconscious" is however not confined to the general features. There are several particular features of learning and habit which are well known to the psychologist, who has definite terms for some of them, but which sometimes astonish the layman, and which are often pointed out as illustrations of "the unconscious" by the theorist who is unversed in psychology.

One of these is the distinction between remembering and recalling, which we have presented in earlier chapters. To the psychologist, remembering is the more puzzling of the two, but to the layman recalling without remembering often seems the more striking. On this account, doing or thinking something which one has previously learned but without remembering in any way the learning, is said to be "unconscious" although the actual process may be as conscious as any other. The discrepancy between what one recalls or reproduces, and the previous learning which has merely modified actions of other sorts is still more impressive. The neurotic is changed in his actions because of experiences which he does not recall, much less remember. This basal result of learning, characteristic not merely of the neurotic but of every person throughout his life, is called a manifestation of "the unconscious" by those who see the phenomenon in only a few types of performance and are ignorant of its basic nature. From this confused understanding have arisen the "complexes," although when mythologically described they take on horrendous features, quite comparable to the aspects of the demons which possessed our ancestors.

In still other cases, not confined to features of habit, the term unconscious is applied to that which is unintended or unpremeditated, although fully conscious in the sense in which the psychologist uses the term. When the statement is made: "I unconsciously put out my hand," the speaker does not mean that the action was unconscious. Applying the confusing term, however, relates such performances, which are the usual performances of normal life, to the mystic concept of the unconscious mind. The demon or one of the demons possessing me made me do it, is the implication. In still other cases, the fact that an act or a thought cannot be explained by the actor or the thinker is knit into the concept of the "unconscious mind." I can't discover, even when I seriously try, why I acted, spoke or thought in that way. There must be a reason, however (every psychologist admits this to be true); applying the term unconscious serves as a substitute for the reason. "I did it unconsciously; it came out of my unconscious mind."

It is no news to the psychologist that the reasons for particular responses are in most cases difficult to discover. The learning process is manifold, and its results are manifested after days and years. Even the immediate operation of associations derived from the past are difficult to follow, although we did not need Edgar Allan Poe to demonstrate that one who analyses acutely may be able to trace certain associative processes with remarkable appearance of accuracy. We are aware also of the painful fact that the analyses which seem quite reasonable are often quite off the track. The associative processes of the analyser, or his deliberate intention, supply the "reasons" in many cases.

The reason why the psychologist avoids the terminology of the "unconscious" (except in the proper meaning, of not conscious at all) should be evident from the foregoing brief presentation. We can take account of the facts and principles of human life in greater detail by the use of other and more definite terms. By so doing we avoid the cloud of mysticism and confused speculation which has grown up about the terms "the unconscious" and "the unconscious mind." The public can be impressed and patients attracted by reverting to mythology and magic, but the psychologists are not interested in either result.

CHAPTER X

THE BREAKING OF SPECIFIC BAD HABITS

The books and articles on stammering cited below illustrate the wide range of theories and methods which have been constructed to explain and cure this maladjustment. One theory in particular, that stammering is due to the training of an initially left-handed child to be right-handed, is an old theory, discredited by our increased knowledge regarding handedness and the functions of the brain and by factual surveys. It is admitted that breaking a child of left-handedness (or of any other habit) by improper methods may result in stammering or any of a number of other maladjustments; but there is no danger from the retraining by proper methods. Handedness is a relative matter, every normal person employing the right hand preferably for certain purposes, the left hand for other purposes, the range of preferences varying widely among normal persons. There are certain functions, such as writing, for which the right hand is used by the majority, and since our written language is designed for this usage, and desks and lighting systems arranged accordingly, every child should early be trained to employ the right hand for writing and for other functions for which the right hand is generally employed.

Regardless of the theories on which the diverse methods are based, some of them are useful in treating certain cases of stammering, when subordinated to an adequate method of reeducation. Breathing exercises, rhythmic exercises, electric shocks, piano practice, tennis playing, etc., may well assist in redirecting the child's energies, re-forming his thought habits and controlling his emotional responses; but the particular accessory of this sort must be selected in accordance with the particular case, and is in any case only an accessory to the treatment.

The volume by Fletcher is the only general treatise which attempts to approach the defective speech problem from a psychological point of view.

On homosexuality, an excellent presentation of the situation of the female homosexual of the less unfortunate type is in Radclyffe Hall's *The Well of Loneliness*, 1928, Cape, London. Although this book has been objected to as immoral, it presents the results of homosexuality in a far from attractive light. The attempt of the author to explain the case as a result of prenatal influence accentuated by early tomboy experience, and the insinuation that women who adopt occupations usually reserved for men are homosexual are, of course, nonsense. This sort of prenatal influence is a mere myth, and tomboys are generally the most female of girls. The real source of homosexuality is indeed plainly indicated in the book (whether by accident or design of the author) in the person of the homosexual nurse.

On the more unfortunate and repellent cases of homosexuality, there is little literature which would be significant to the general reader. There are many books on marital maladjustment, but almost all of them contain much misinformation and little of useful fact. An exception is M. J. Exner's *The Sexual Side of Marriage*, 1932, Norton, N. Y.

The "explanation" of the efficacy of the method of negative practice is not a matter of great moment. The facts can probably be speciously fitted into any theory of learning, however useless such a theory may otherwise be. Unfortunately, any "explanations" of learning at present are assumptions in the realm of the unknown, and should be sedulously avoided. The further ascertaining of facts concerning the way in which we learn various materials under various conditions and for various purposes is the all important matter. The important point about the negative method is that it is quite consonant with the facts we know about learning in its more familiar aspects, and was deduced from those facts.

1. *On the nature and therapeutic treatment of stammering from various theoretical points of view.*

Appelt, A., *Stammering and Its Permanent Cure*, 1930, New York, Dutton. (A discussion of the causes and treatment of stammering resulting from emotional stress.)

Bluemel, C. S., *Mental Aspects of Stammering*, 1930, Williams and Wilkins, Baltimore. (Considers types of stammering as related to impediments of thought and suggests thought training as the basis for speech correction.)

Coriat, I. H., Stammering: a psychoanalytic interpretation, 1927, *Neur. and Ment. Dis. Monog. Ser.*, No. 47.

—— Active therapy in the analysis of stammering, 1930, *Psychoanal. Rev.*, 17: 342-347.

Dickinson, E. D., Educational and emotional adjustments of stuttering children, *Teach. Coll. Cont. Educ.*, No. 314.

Fletcher, J. M., *The Problem of Stuttering:* a diagnosis and a plan of treatment, 1928, Longmans, N. Y.

Glassburg, J., The cause and cure of stuttering, 1927, *Arch. Otolar.*, 5: 122-155.

Johnson, W., *Because I Stutter,* 1930, Appleton, N. Y. The autobiography of a stutterer. (The effect of the speech defect on personality, attitudes and ambition, as well as therapeutic treatment, is discussed.)

Lima, M., Speech defects in children, 1927, *Ment. Hygiene,* 11: 795-803.

McAllister, A. H., Speech disabilities, 1927, *Rep. Brit. Asso. Advan. Sci.*, 372.

Meagher, J. F. W., Homosexuality: its psychobiological and psychopathological significance, 1929, *Urologic and Cutaneous Rev.*, 33: 505-518.

Orton, S. T., Studies in stuttering, 1927, *Arch. Neur. & Psychiat.*, 18: 671-672 (An Introduction).

Prince, M., The educational treatment of neurasthenia and certain hysterical states, 1898, Boston, *Med. & Surg. Jour.*, 139: 332-338.

Robbins, S. D., *Stammering and Its Treatment,* 1926, Boston, Stammerer's Inst.

Rockwell, A. J., A study of probable causal factors of masturbation in a girl of six years, 1930, *Psychol. Clin.*, 18: 236-241. (The habit was broken through the provision of more normal emotional outlets.)

Rosanoff, A. J., Constitutional psychoses ending in permanent recovery, 1924, *Arch. Neur. & Psychiat.*, 11: 70-81.

Starr, H. E., Psychological concomitants of high alveolar carbon dioxide, a psycho-biochemical study of the etiology of stammering, 1928, *Psychol. Clin.* 17: 1-12.

Taft, J., The re-education of a psychoneurotic girl, 1925, *Amer. Jour. Psychiat.*, 4: 477-487.

Truitt, R. P., Methods of preventing delinquency, 1926, *Arch. Neur. & Psychiat.*, 16: 613-619.
Tartar, G., Report of a case of stuttering as a problem of vocational readjustment, 1928, *Jour. Abnor. & Soc. Psychol.*, 23: 52-58.
Travis, L. E., Disintegration of the breathing movements during stuttering, 1927, *Arch. Neur. & Psychiat.*, 18: 673-690.
—— *Speech Pathology*, 1931, Appleton, N. Y.
—— Recent research in speech pathology, 1929, *Psychol. Bull.*, 26: 275-304. A review of 51 studies.
Wallin, J. E. W., Speech defective children in a large school system, 1926, *Miami Univ. Bull.*, 35: No. 4.
Young, E. H., *Overcoming cleft palate speech:* help for parents and trainers, 1928, Hill-Young School, Minneapolis.

2. *Studies in the therapeutic treatment of bad sexual habits.*

Berkeley-Hill, O., Habit formation, 1929, *Jour. Ment. Sci.*, 75: 298-301. A report on the breaking of various *bad* habits of 58 patients.
Bridges, J. W., A study of a group of delinquent girls, 1927, *Ped. Sem.*, 34: 187-204.
Food, C. A., Homosexual practices of institutionalized females, 1929, *Jour. Abnor. and Soc. Psychol.*, 23: 442-448.
Gordon, A., The history of a homosexual: his difficulties and triumphs, 1930, *Med. Jour. & Rec.*, 131: 152-156.
Hubner, Max, *Disorders of the Sexual Function*, 1928, Davis, Philadelphia.
Killich, V. W., Suggestions for parental administration calculated to reduce juvenile delinquency, 1927, *Jour. Delinq.*, 11: 194-205.
Moll, A., *Perversions of the Sex Instinct.* Transl. by M. Popkin, 1931, Julian Press, Newark.

CHAPTER XI

HABITS OF EMOTIONAL RESPONSE

To obtain the historical development of the present theories of emotion and sentiment, the books by Darwin, James, Lange and Shand are of primary importance. Bard's chapter is a carefully technical, but readable, account of the neural mechanisms involved in producing the muscular and glandular "expressions" of feelings and emotions, and is to be highly recommended. A synopsis of data and discussions on the rôle the "expressions" play in the further functioning of the organism is included, with a summary of the arguments of Sherrington, Cannon and others against the James-Lange theory. Bard, however, does not attempt critically to evaluate these arguments.

The points to be borne in mind in entering controversial discussion of the basis of the feelings and emotions are:

1. The James-Lange theory predicts that if (a) the efferent pathways to any group of muscles, the facial for example, be intact, while (b) other efferent pathways are blocked by paralysis or severed, then the "emotional" stimulation which normally arouses "expression" from these muscles will arouse "expression" of the (a) group alone, which will be somewhat like the normal "expression" of this limited group. How like, cannot be predicted.

2. It is predictable that if the entire group of visceral organs be disconnected from the spinal cord and brain, but the external (skeletal) muscles be left connected, emotional "expression" will be much the same as in the normal animal.

3. If the sensory or afferent pathways from the external (skeletal) muscles are blocked, but the neural connections of the viscera left undisturbed, the emotional experiences of the animal should be somewhat modified, but the change in any "emotion" should be no greater than the difference between different occurrences of the "same" emotion in the normal animal.

4. The essential improvements in the James-Lange theory indicated by physiological investigation and psychological analysis are: (a) The conception of the primary response in emotion as not necessarily perceptual. Apparently, neither James nor Lange would have considered this an important change. (b) The application of the theory to the feeling element in emotion, admitting that the differentiation of the "emotions" as popularly named is mainly by the thought processes involved. This would have been highly objectionable to James, apparently, but not to Lange.

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CHAPTER XII

INTELLIGENCE AND LEARNING

The literature on intelligence and intelligence tests, aside from the reports of routine measurements, is mostly the production either of promoters of wholesale testing as a profession, with little appreciation of the problems involved and devotion to naïve theories, or of persons who are up in arms against the flagrant abuses of applications and interpretations these others have fostered, and who appreciate the practical uses of testing as little as the others appreciate its crudity and danger. The references given below range from the scholarly general presentations and critical articles to contributions of considerably lesser value; but may be taken, as a whole, to represent the better grade of test literature.

The non-professional reader may obtain a general picture of the application of intelligence tests from the books of Freeman, Dearborn, and Pintner, although the discrepancies, difficulties and problems we have emphasized are minimized in these books, which are intended for the use of students acquiring the routine technique of testing rather than inquiring what it is all about. The College Entrance Examination Board has recently announced a monograph by Carl Brigham, apparently designed to supply a theoretical basis for intelligence testing. The monograph has not yet appeared, and although the table of contents is interesting, the suspicion that Professor Brigham does not take either his work or his prospective readers very seriously is indicated by the final sentence of the sample page circulated by the Board, in which he suggests certain techniques sometimes employed in training children and animals by advising readers of the book to "rub their noses in it."

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A SELECTED BIBLIOGRAPHY AND EVALUATION OF RECENT DEVELOPMENTS IN HABIT FORMATION, 1932-1949

A SELECTED BIBLIOGRAPHY AND EVALUATION OF RECENT DEVELOPMENTS IN HABIT FORMATION, 1932-1949

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Almost every investigator working in psychology or one of the related sciences has become interested in the field of learning or habit formation. As a result literally thousands of journal articles have appeared within the last two decades. In order to prepare a usable list of references a most rigorous selection must be made from the mass of available material. In compiling the supplementary bibliography which follows a number of criteria were used as a basis of selection. Because of their relative inaccessibility, foreign articles, except for a few originating in Canada or Great Britain, were omitted. Since our main concern is with humans in a learning situation, reports on animal experimentation have not been included. An exception has been made in the case of some animal work where the results have been applied to humans. Reports of two or three pages were usually not listed. Where several articles by the same author have appeared, an attempt has been made to select a representative contribution.

CHAPTER I

THE PROBLEMS OF HABIT AND LEARNING

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CHAPTER II

THE FUNDAMENTAL PROBLEMS OF LEARNING

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CHAPTER III

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CHAPTER IV

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CHAPTER VI

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CHAPTERS VII AND VIII

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CHAPTER IX

PERSONAL AND SOCIAL ADJUSTMENT

Within the last decade there has been a tremendous surge of interest in mental hygiene and clinical psychology. As a result the psychology of adjustment has assumed an importance that overshadows almost all other fields. A number of books have appeared which reiterate the Freudian tenets but there are also some excellent publications such as Shaffer's "The Psychology of Adjustment" and Cameron's "The Psychology of Behavior Disorders." In view of the recent development of clinical psychology as a profession, it would seem that the inclusion of a number of representative titles is warranted.

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CHAPTER X

THE BREAKING OF SPECIFIC BAD HABITS

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CHAPTER XI

HABITS OF EMOTIONAL RESPONSE

In recent years there has been a revival of interest in the field of psychosomatic medicine. In 1939 the journal, "Psychosomatic

Medicine," was established in order to provide a medium for the publication of articles specifically in this field. Psychologists have long been aware of the relationship between emotions and physiological states. The recent literature has served to emphasize the field which lies between psychology and medicine.

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CHAPTER XII

INTELLIGENCE AND LEARNING

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