CONDITIONS AND CONSEQUENCES
OF
HUMAN VARIABILITY

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CONDITIONS AND CONSEQUENCES OF HUMAN VARIABILITY

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TO

H. C. D.

PREFACE

THE aim of this study of the conditions and consequences of human variability is neither to develop a systematic theory of human behavior, nor to write an inventory of facts. Both of these tasks are important, but may better be left to others. As G. E. Müller once said to me, "In the service of science each of us must exploit the intellectual capital that he possesses." I am primarily an experimentalist with a bias toward exploration. But it probably serves both exploration and experiment occasionally to gather the scattered fragments of one's work into coherent form and to discuss their implications. The consequences in this case are some seemingly important conclusions concerning the significance of variability in mental development and in the integration called consciousness.

Techniques are not generally described in detail in this volume. Such details would have diluted and confused the argument. In those cases where the reader is interested in a critical evaluation or a duplication of apparatus, the original papers are always available.

Some of my readers will miss discussions of related traditions and the acknowledgment of many scientific debts. The inclusion of these features would not only have greatly increased the bulk of the present volume, but it would have detracted from its coherence. So I will not apologize. More nearly adequate acknowledgments appear in the several studies which preceded this report. My most important obligations are first to my great teachers, Erdmann in psychology and Verworn in physiology, and less directly to Exner and the schools of Sherrington and Pavlov. Obviously, I owe much to a large group of colleagues, assistants, and pupils. Quite incalculable are my obligations to the various institutions that have sheltered and encouraged my experimental work, especially the University of Halle, the Prussian Academy of Science, Wesleyan University, the Nutrition Laboratory of the Carnegie Institution of Washington, the Committee on Migration of the National Research Council, the American Association for Advancement of Science, the Institute of Psychology and Institute of Human Relations at Yale. I am under peculiar obligations to the E. K. Adams Fellowship of Columbia University for its assistance, at a critical time, in my experimental program.

R. D.

New Haven, Connecticut,
September, 1930.

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HUMAN VARIABILITY

INTRODUCTION

The Personal Equation

SHORTLY before his death my great teacher and friend, Erdmann, published a small treatise,[1] setting forth some theoretical implications of our experimental work in the psychology of reading. Among the extraordinary features of that book was the effort to acquaint the reader with the personal bias of the writer, his "Idols of the Den," the philosophic foundations of his thinking. If this were common practice it might make psychological papers more difficult to read, but it would probably make the scientific differences of the several writers more comprehensible and facilitate the eventual unification of their conclusions. The assumption that one makes no assumptions would be, of course, infantile. The point is not to avoid but to recognize them. Just as in all his thinking it is an obligation of the scientist to distinguish carefully between assumptions, experimental observations, and the hypotheses that eventuate from them, so doubtless in writing that aims to be scientific, it should be a care and solicitude to make these factors explicit. In this sense the fundamental assumptions of our scientific thinking ought to be on record for each of us, at least in outline. The following outline is presented in this spirit. It is intended neither as a justification nor as an effort to find support. That would require much greater elaboration. It is not a philosophical system,

[1] Benno Erdmann, *Grundzüge der reproduktions Psychologie* (Berlin, 1920).

though it probably implies one. It is merely an effort to present the basis for an understanding of what I have tried to do.

My fundamental assumption is that phenomenal reality is capable of presentation in many intellectual systems. Even in such apparently simple matters as the scientific description of a single event, like the movement of a train of cars, there is an indefinite number of possible points of view and an indefinite number of possible systems of coördinates on which the facts may be expressed. I assume that our only real scientific knowledge is a knowledge of relationships, and since there seem to be many possible relational systems, of which the several sciences are typical forms, I am perhaps a pluralist. Not, however, in the ontological sense. The ultimate stuff of unintellectualized reality seems to me to be an unsolvable or at least an unscientific question.

I assume further that the intellectualization of a phenomenon which we commonly call "understanding" should include an account of its conditions as well as of its configuration and its constituent parts. Emphasis on any one of these three phases of description in psychology seems to be an accident in the history of its development. According as the interests of different investigators vary, it sometimes seems more profitable for a scientist to analyze an event, sometimes to view it as a whole and sometimes to view it in relation to its conditions. The most difficult phase of the task of complete description is probably the last of the three. It seems to me, however, that it is not only the most interesting, but also the most profitable. In view of this scientific bias, I suppose for want of a better name, I qualify as a dynamic psychologist. Since many of the conditions of mental events are also commonly presented in physiological systems, the particular systems with which I am largely concerned probably class me as a physiological psychologist.

The subject matter of human psychology I assume to consist of human experience, personality, and behavior. Whatever may be true in the infrahuman sciences, it seems to me that in humans, at least, the understanding of any one of these three aspects of our life involves a more or less complete understanding of the other two. Knowledge of all three combined tends to make a reasonably coherent intellectual system of relationships. Emphasis on any one of the three is another accident of development. Since experimental investigation is naturally limited by available resources of time, energy, and technique, the personal research of any one scientist can cover only a very limited field. My own probably classes me as a behaviorist in the sense of a student of human behavior. I believe, however, that behavior is only a fragment of the psychophysiological total, and I regularly endeavor to see that fragment in suitable perspective.

In the traditions of physiological psychology there are various more or less useful working hypotheses with respect to the relationship between psychological and neural processes. They range from Pflüger's assumption that all neural processes have elementary psychic character to the Cartesian assumption that mind is related only to a given place in the nervous system. Perhaps the most common assumption in our present tradition is that of psychophysical parallelism, the doctrine that psychological and neural processes run a parallel course. This was adopted primarily as a speculative theory to avoid other philosophic difficulties. I shall outline in a later chapter the grounds for my belief that it is an unsatisfactory working hypothesis. In my opinion it has proven sterile, inhibiting, and misleading. Its very inclusiveness inhibits exploration as to the precise nature of these particular neural processes that are related to each phase of our mental life as we know it. The major problems of that relationship, namely, "What are the specific neural conditions of experience, personality and behavior?" are still unanswered. With a deep distrust of the traditional concept of cause and effect, my working hypothesis with respect to these problems follows Verworn's framework. I believe that a description of its conditions is part of the adequate description of any event, and that the sum total of its conditions *is* the event just as truly as is the sum total of its parts or its configuration.

Finally, I assume as a working hypothesis that the differentiating peculiarity of the neural conditions of consciousness is a peculiar integration or systematization rather than any specific character of the unintegrated fragments. This seems to me to be probably true of all three phases of mental life. The nerve currents, as far as we know them, appear to be similar in all parts of the neural system. Neither their nature nor their locus guarantees experience, personality, or behavior patterns, but only their organic systematizations. In this possibly unwarranted extension of the term I like to feel myself a part of the Gestalt movement, a behavioristic Gestalt, notwithstanding Köhler's argument against behaviorism.

In the following pages I shall try to indicate the main grounds of my conviction that a study of human variability is not only as important as a study of hypothetical constancy, but also brings us closer to the facts of mental life as an important condition of its development, and opens the way to an understanding of its nature.

CHAPTER I

THE VARIABILITY OF HUMAN REACTIONS

ONE of the discouraging handicaps of psychology in its effort to systematize the phenomena of experience, personality, and behavior seems to be its poverty in what often pass in other sciences as stable or invariant facts. Probably the difference is one of degree rather than of kind. It may be doubted if any science really deals with invariants, although for systematizing purposes it may be more convenient sometimes to regard them as such. To regard mental reality as a complex of invariants, however, would obscure one of its most important characteristics.

Various conditions contribute to this state of affairs. The psychophysical organism is in a perpetual state of flux. It changes not only from infancy to old age, from decade to decade and from year to year, but also from day to day, from hour to hour, and from moment to moment. Moreover, the neuromuscular consequences of two successive instances of stimulation with physically similar stimuli vary not only according to the momentary conditions of the organism and its psychophysical set, but also according to inner reactions and inhibitions. As is well known, the repetition of identical stimuli may not evoke the same reaction in successive instances. Among the possible consequences are: no overt reaction at all, the quantitative and qualitative modifications of reaction patterns, and the inhibition of reaction already begun. Moreover, no two human beings react in exactly the same way to the same physical stimulus. From the standpoint of a scientist who seeks reaction invariants on a supposed analogy to the physical sciences, psychology must seem quite hopeless. It may be, however, that the supposed analogy with physics and chemistry is a misleading residuum of a discredited scientific attitude. It begins to seem that the theoretically most significant facts even in the physical sciences are facts of change and development rather than of invariants. However that may be, psychologists have never found mental analogues of hypothetically stable physical atoms and molecules, but only changing processes.

Individual variations delayed the development of a science of human nature for a long time. It was a fruitful methodological advance when psychology rescued individual differences from the scrap heap of scientific anomalies and began to study them. At the present time surfaces of frequency and the relative positions of individuals in continuous series express our knowledge of human nature vastly better than the hypothetical "average man" of a few years ago. This has revolutionized the scientific account of personalities; and much of the recent service of psychology in the fields of education, mental hygiene, and personnel has been due to the shift in scientific emphasis from the effort to describe human nature to the effort to deal systematically with human natures. Treating each individual as a special combination of capacities, accomplishments, and tendencies has been far more productive than treating individuals as though they were all alike or as though they belonged to mutually exclusive types. Unfortunately, however, residua of this discredited hypothesis are still evident in politics, in criminology, in psychopathology, and even in psychology.

There is some basis for the belief that an analogous shift of scientific interest has been taking place from a description of the individual as a relatively stable aggregate of sensations and ideas, reflexes, instincts, habits, drives, and behavior patterns to a study of the processes that make his mental and somatic reactions what they are at any moment, and will presently make them something different. Laws of human behavior are conspicuously few and conspicuously unreliable in the prediction of any specific human reaction. Study of the laws by which human reactions change from time to time seems to be a necessary complement. It is significant that whenever the conditions of individual variability have been investigated, as, for example, in so-called "fatigue," the acquisition of skill, learning, the curve of forgetting, the effects of drugs, climate, and emotion, the interaction between motivation and reaction, and the modification of consciousness by the "unconscious," our working knowledge of human behavior has been notably increased, although often at the cost of simplicity. It is possible that the variability of human reaction is not only its most conspicuous characteristic, but also a fundamental condition of intellective consciousness as we know it. Without prejudice with respect to the later discussion, one might point out certain theoretical probabilities in this direction.

(1) If stimuli are to be understood biologically as "changes in external vital conditions" and the phenomena of consciousness and behavior as adaptive reactions to such changes, there seems to be no place for mental atoms or molecules.

(2) One of the chief characteristics of mental reaction as distinguished from mechanical is the fact of learning by experience; but unless the residua of the earlier reactions modify the subsequent reactions in a

specific way, there would be no learning. Without a specific kind of variability, no peculiarly mental phenomena!

(3) The concept of development also includes the concept of variability. Unless our lives are to be spent in the shallows of stereotypy, reactions to similar physical changes must vary according to their adequacy. This is represented in psychological tradition by the concepts of trial and error, and of purpose. On the basis of precise records of such relatively simple adjustments as the eye-movements, as well as on the basis of the history of science, I have argued that the concepts of approximation and correction are necessary complements of trial and error in the description of human behavior.

The importance of mutations is clear in the evolution of species. The importance of individual variability for the evolution of society and the ontogenetic development of the individual has never been thought through as far as I am aware. It seems a reasonable presumption, however, that in organisms of complete invariance, that is to say, if every external physical change were responded to by an invariant reaction, there would develop neither a mental individual nor a society. The opposite limit would be organisms without systematized reactions. However closely our reaction processes may on occasion approach these limits, to reach either one would be disastrous. Probably in the mental and social as in other organic development, variation and relative stability of organization must coexist and integrate with mutual limitations. Emphasis on either one misses an important aspect of reality and must be regarded as an accident in the development of scientific knowledge. To understand each in its interplay with the other as part of integrated reality is a desideratum at present remote. Our task may be best interpreted as a contribution to this end.

Nonscientific observations in everyday life establish a presumption of prevalence and importance of variability of human response to similar situations. There seems to be a widespread disinclination in healthy individuals to repeat the same behavior pattern in close succession. New walks, new music, new stories, new work, new ideas, all seem to satisfy a deep and widespread craving. Even within the same general behavior pattern we may observe great variability of reaction. Nothing that a healthy individual does seems to be exactly stereotyped. Careful inspection of such well-practiced behavior as is represented by one's signature shows no two identical performances. There may be, on the contrary, wide variations in the size, spacing, and form of letters within the same general formula. This seems to be true even in the cases of great virtuosity. Apparently even the most celebrated musicians never give two identical renditions. They may vary in many ways. Some one performance commonly stands out as supreme and the others are grouped about it in various degrees of satisfactoriness. This seems to be even more conspicuous in less-practiced and in primitive behavior.

Many of life's tragedies result from the common illusion that we ourselves and others are stable reactors. Modification of the relatively stable instincts and habits is as significant in human behavior as their stability. Even the slowly changing emotions and sentiments cannot be safely regarded as stable. Emotional reactions to persons and things do change. Scientific knowledge of the conditions of that great desideratum, called human happiness, has been delayed, in part, at least, because psychologists have failed to study scientifically the laws of emotional change. The crude facts seem to be that what was offensive often becomes delightful and what was delightful commonly tends to become a matter of indifference and may even become repugnant. Such facts are rough approximations which call for more exact investigation, a matter in which I hope to help. Whenever it is desirable to maintain a given emotional attitude or sentiment as a factor in behavior, as in morals, religion, friendship, and work, it would seem advisable to know not only the emotional profile at any given moment, but also something of the conditions under which emotions change.

The more accurately observations are made the more conspicuous human variability becomes. With sufficiently fine units of measurement no two instances of human reaction have been proved to be identical. They follow the rule of all biological phenomena laid down by Leibnitz. Precise records of human reaction show an indefinite number of combinations of latency, amplitude, acceleration, and recovery, but never have two been discovered exactly alike. This holds for such relatively simple neuromuscular events as the knee-jerk and such practiced reactions as the eye-movements in looking between two fixed points. The scientific question is not the existence of variability, but how much, under what conditions, and with what consequences.

Psychophysiological tradition includes a long list of known conditions of variability of reaction to similar stimuli. The most conspicuous may be enumerated as follows: changes in set, incentive, and emotional reinforcement; suggestion and expectation, anticipatory reaction; learning and forgetting; adaptation and resystematization; conditioning and extinction; the refractory phase and the rebound; inhibition and reinforcement; Bahnung and facilitation; relative fatigue and exhaustion; warming up and recovery; cocontraction and reciprocal relaxation of antagonistic muscle groups; drugs and internal secretions; sleep and somatic rhythms; the interaction of "delay paths"; age and physical condition.

The list is so formidable that it would seem to require some courage to generalize about habit and behavior patterns as though they possessed an inherent stability. It is, however, neither exhaustive nor are the several items mutually exclusive. It merely serves as a convenient point of departure for our discussion of experimental data.

The several items of the list are presumably of various degrees of fundamentality. We shall presently give evidence that connects at least one variety of inhibition with the refractory phase. Learning is as yet only partially analyzed into its fundamental processes, but it probably includes refractory phase and inhibition, the supersensitive phase or rebound and facilitation in various configurations, together with resystematization, of which one of the most conspicuous indicators is anticipatory reaction.

The process of conditioning can probably be reinterpreted, in part at least, in terms of more familiar psychophysiological principles as anticipatory reaction in which systematically related stimuli take the place of more adequate stimuli as conditions of reactions.

Set, bias, expectation, and intention may be regarded in many instances as the remoter consequences of earlier reaction or of the elaboration of antecedent sensory data through "cortical delay paths." Analogous subsequent action of the "cortical delay paths" is probably represented in every complex reaction as the "epicritic" refinement or modification of such "protopractic" beginning reactions as the reflex, habit, and instinct; according to the concepts of approximation and correction.

The effects of emotion on reaction are both direct and indirect. They are more direct in those neuromuscular processes which are commonly called expressions of emotion and more indirect in the complex effects of emotions on the rest of the neural system. These latter may include an emotional facilitation of—or, in extreme cases, the inhibition of—cortical controls, together with the reinforcement of primitive response. Many of these special cases may probably be reduced to such simple and fundamental processes as inhibition and reinforcement, with related autonomic and glandular activities. All these analyses, however, are still largely conjectural and represent working hypotheses developed from exploratory data. My experimental explorations are fragments of this more general process.

CHAPTER II

THE INFLUENCE OF REFRACTORY PHASE ON BEHAVIOR

PSYCHOLOGY has long recognized in learning one of the important effects of repeated stimuli, namely, an increased sensitivity of particular systems to restimulation. Properly spaced successive instances of the same kind of stimulation tend to facilitate repetition of the same kind of reaction. This has been verified by many different observers in a long series of memory experiments and finds quantitative expression in the curves of learning. While this positive effect of repetition has been generalized in the laws of learning, the gradual disappearance of a tendency to repetition has been analogously expressed as the laws of forgetting.

It has been less obvious that the repetition of stimuli may have the opposite effect to that which is commonly referred to as learning. Antecedent experience may raise a barrier to repetition which at present is designated as refractory phase. The first experimental evidence of the barrier in psychology was given by Münsterberg. It has been verified recently by Thorndike and Dunlap in quite different situations. In simpler tissues these barriers are easily verified. If the adequate stimulus for the reaction of a nerve or muscle is repeated within a sufficiently short interval, the response to the second stimulus shows a decrement. That decrement is commonly regarded as an indication that the reacting tissue is for a measurable time relatively insensitive to that particular stimulus. That is to say that the tissue is in a refractory phase.

Refractory phase is commonly called *absolute* if the tissue is entirely unresponsive to a repetition of any given stimulus, however intense it may be. It is called *relative* when the tissue is in some way less responsive after reaction. This may be shown either by a decrement in the magnitude of response to the same intensity of the stimulus or by requiring more intense stimulus to evoke a response of the same magnitude. In complex systems such as are involved in most human behavior the absolute refractory phase is difficult or impossible to demonstrate. Consequently when we use the term we regularly refer only to the relative refractory phase.

The concept of refractory phase is not regarded by the oriented as ultimate, but rather as a convenient general term for a measurable decrement of response to a second stimulus of the same order. It is probably analyzable into more fundamental metabolic or biological processes, such as the exhaustion of available oxygen, the persistent concentration of ions, or something else. The possibility of some common denominator for this biological manifold may not be overlooked. Such a common denominator, however, need not be any one of the several items in the manifold. The probability is rather that it may be found in some such general biological process as the restitution of the biological equilibrium which was disturbed by the change in the external vital conditions of the tissue. While the precise biological process may vary in the several tissues, there seems to be no reasonable doubt that the more or less protracted disturbance of biological equilibrium consequent to stimulation and a gradual recovery of equilibrium after reaction to a momentary stimulus is a general characteristic of living substance.

A period of relative inexcitability—or better, a decrement of irritability immediately after reaction—was first found in heart muscle by Kronecker and Stirling, and was graphically recorded by Marey, who christened it "*phase réfractaire.*" The discovery of a similar phenomenon in the nervous system was made by Broca and Richet. Refractory phase was found in a variety of neuromuscular systems by Verworn and his school; in deglutition by Zwaardemaker; in nerve fibers by Gotch and Burch, and by Lucas; in various reflexes by Sherrington; in the knee-jerk by Dodge; and in the lid-reflex by Zwaardemaker and Lans, and by Dodge. Far reaching influence of the refractory phase in the integration of behavior is indicated in the Verworn-Lucas theory of inhibition, which has been experimentally shown by Dodge to be applicable to at least one of the forms of inhibition found in human reaction systems. The shortest known refractory phase is that of nerve fibers. It is of the order of a few thousandths of a second. Refractory phase of the reflexes varies with the neuromuscular mechanism. It lasts approximately one-half second in the knee-jerk and three seconds or more in the lid-reflex. Complex neuromuscular systems seem regularly to have a longer refractory phase than simple ones like a reflex or a nerve fiber. In some cortical systems reduced responsiveness after reaction may last a long time. This is not altogether mysterious. Any neuromuscular system may reasonably be expected to have a refractory phase at least as long as the longest in any of its parts. Moreover, if recovery of irritability is an oscillating process as now seems probable, a relative refractory phase may be expected to last until the several phases of complete recovery coincide in all the various links in the chain. Under these circumstances a barrier to repetition might last for years. In some cases one conjectures that the original equilibrium is never reconstituted. Whether or not these barriers in complex systems may be related to their specific metabolic conditions is not our problem. Neither is it our problem to discover whether the barrier is situated in nerve fiber, nerve cell, synapse, or some unknown point. At present there is no proof that the barrier to repetition is identical even in nerve and muscle fibers. The probability is against it. In view of all this ignorance and until the fundamental physiology is clearer we use the term "refractory phase" merely as a convenient descriptive term without definite physiological implications as to its nature.

The first comparative study of refractory phase at various neural levels was undertaken by me when I held the Ernest Kempton Adams Fellowship. The extensive data of that study will not be repeated here. It seems sufficient to recapitulate the main results. In that study some barrier to repetition analogous to refractory phase was discovered in all neuromuscular systems where reactions to recurrent stimuli were accurately recorded.

In heart, the duration of the cardiac cycles, and the consequent pulse rate, is probably determined in part at least by the length of the refractory phase of heart muscle, in the original sense of that term. In the continuous interplay of refractory phase and intensity of heart stimulation it is difficult to determine, in the intact human, how far changes in the pulse rate depend on either factor. The regular daily rhythms of pulse rate in our records consequent to the same conditions of the ingestion of food, periods of relative quiet, light muscular work, and time of day, suggested among other causes some rhythmic changes in refractory phase. The problem lay outside our facilities for experimental inquiry.

The regular appearance of a refractory phase in the knee-jerk and lid-reflex when otherwise adequate stimuli were repeated after an interval of 0.5″ was entirely predictable from available data. The laws of its relation to the amplitude of antecedent contraction were, however, less clear in the tradition. Our records indicate that the relationship is not simple. Throughout our long series of experiments there was a rough average proportionality in both the lid and patellar reflexes between the amplitude of reactions to the paired stimuli. That is to say, the rhythmic or arrhythmic conditions that depressed the first of the paired reflexes depressed also the second. It seems as though there might be something like a physiological quantum of reflex irritability, which changes in available amount from time to time and partially recovers in the 0.5″ intervals between stimuli covered by our records. The proportionality between the two responses to the paired stimuli was, however, not constant in successive instances. Important irregularities appeared in responses to faint stimuli. In unpublished records of the knee-jerk, it was found that if reaction to the first faint stimulus was excessive then reaction to the second was of less amplitude than usual. The converse was also true. If reaction to the first stimulus was unusually small, reaction to the second was unusually large in amplitude. This seems to support the physiological quantum of reflex irritability hypothesis. If the quantum of irritability was unusually exhausted by the first stimulus, there was less available for the second. If, on the contrary, the first stimulus found the system refractory for some reason, the second found greater irritability.

Our records show an important difference after 0.5″ between the refractoriness of the knee-jerk and the lid-reflex. The average amplitude of the second reaction in the knee-jerk is of the order of two-thirds that of the first. In the lid-reflex it is approximately a quarter. This indicates a difference in the duration of the refractory phases of the two processes. That is to say, the refractory phase of the knee-jerk seems to be almost over in 0.5″ while that of the lid-reflex is in an earlier and more nearly absolute stage. This hypothesis is confirmed by unpublished records of the knee-jerk which were taken with increasing interval between stimuli. These show that 0.5″ lies near its "critical period," when refractoriness passes over into supernormal excitability or the "rebound." We have reported data showing that refractoriness of the lid-reflex lasts from two to three seconds or possibly even longer. Its refractoriness is still high after 0.5″. From the available evidence one may tentatively generalize that the amplitude of reaction at any given moment within the relative refractory phase of a reflex will be inversely proportional to the probable duration of the remaining refractory phase.

Direct experimental evidence for refractory phase in neural systematizations higher than the reflexes, has hitherto been conspicuously inconclusive. This is especially true of cerebral systems. There is, however, a strong theoretical presumption that some barrier to repetition analogous to refractory phase is a post-stimulation phenomenon of all neural tissue. Such a presumption is congruent with many observed facts in behavior and consciousness, but there are few experimental techniques for demonstrating it.

Whether or not motor phenomena like the oscillatory movement of the fingers show a refractory phase is also debatable. We might suppose on theoretical grounds that the shortest time between two flexor innervations is determined by a barrier to repetition in some part of the neuromuscular system. One such barrier occurs in the mechanics of the joint. After one complete flexion there cannot be another until the former muscular equilibrium is partially restored. Such an equilibrium must have central as well as peripheral factors, since it includes the relaxation of the flexors as well as the contraction of the antagonistic extensors as Sherrington has shown. It is, consequently, a fair assumption that the total barrier to repetition of the neuromuscular system involved in oscillation of the fingers would be measured with reasonable accuracy by the maximum speed of oscillatory movement. In finger-movements this is of the order of one-eighth of a second.

Similarly, eye-movements in the same direction usually succeed one another only at a relatively slow rhythm if successive movements are interrupted by true fixation pause, but the duration of the barrier is much less in optic nystagmus and the vestibular reflex, suggesting a simpler neural mechanism for the slow phases in these types of eye-movements. The existence of characteristic refractoriness for other motor systems is a reasonable conjecture, but it is based at present on theoretical presumption rather than experimental evidence.

Whatever the barriers to repetition may be, they differ at various neural levels and these differences may be of considerable importance in the development of overt behavior.

For example, in the experimental exploration of the reflex responses of the guinea pig by Dodge and Louttit the records show that the reflex pattern changes in

learning ?
[illegible],
adjustment

response to a rapid succession of noise stimuli. Those factors with a long refractory phase disappear from the reaction pattern, leaving only those factors whose refractory phase is shorter than the interval between stimuli. When the stimulation frequency is still further increased, the pattern again changes as the separate impulses summate. This will presently be discussed in a little more detail.

For the influence of a relative refractory phase on serial-word reactions our records held quite definite data. Analogous to the action of refractory phase in the guinea pig's reflex behavior, it operated as a condition of changing behavior.

The experimental data justify us in regarding refractory phase, or the reëstablishment of biological equilibrium after reaction, as an important condition of important variations in human reactions and reaction systems. It limits the frequency of responses when the repetition of stimuli is more rapid than a certain critical interval. It operates to modify the pattern of response to repeated stimuli in humans as well as in animals. It facilitates the consolidation of isolated conscious events into series.

Theoretical considerations. One conjecture growing out of the theory of refractory phase concerns the probable effects of different durations of refractoriness within the central neural system, and the corresponding consistency of mental reactions. For example, if the neural systematizations corresponding to different parts of a system of ideas have different refractory phases, then, quite apart from the matter of reinforcement and inhibition, those ideas with short refractory phases should tend to persist in consciousness as a consequence of recurrent or persistent stimuli, while the others would tend to disappear. The analogy demands a little more detailed description of the relevant experimental facts. In the start reflex of the guinea pig it was found as we have said that bodily jerks disappeared before the twitches of the ear and it was conjectured that the bodily jerk had a longer refractory phase than the ear twitch, though it might seem superficially to indicate a kind of learning process. Adequate experimental technique showed that the start reflex to sudden noise begins with a slight shove of the front feet downward, occurring generally about 30σ after the stimulus. This is followed by a retraction of the forelegs. The start reflex decreases in amplitude on repeated stimulations. Its relative refractory phase is over 0.5″. The guinea pig's ear reflex to sudden noise consists of a clonic reaction, usually composed in our records of two successive movements of the ear backward and downward. The first of these movements begins about 20σ after stimulation and the second about 60σ to 70σ after the first. The refractory phase is relatively short, certainly less than 0.1″. If a second stimulus is given within from 60σ to 90σ after the first, the first wave of the second reaction seems to coincide with the second wave of the reaction to S′, and reinforce it, giving a sequence of three waves. There is evidence that the amplitude of the ear movements becomes less with repeated stimulation.

The theoretical implications of the experiments may be summarized in the hypothesis that the refractory phase may be a factor in determining the survival of any element of a composite reflex response to rapidly recurring stimuli. It is apparently combined in some unknown way with a longer process of negative adaptation.

Familiar experience seems to indicate that some motor processes have shorter barriers to repetition than do intellectual processes. There is a relatively small barrier to repetition in telling of the same story, singing or whistling the same melody, or recounting the same personal experience. The effect on the listener is, however, quite different. Repetitions in the receptor systems easily become annoying or unbearable.

Many other experiences seem to raise more or less protracted barriers against repetition. By preference we drive in directions where we have not been for some time. We eat the kinds of food we have not recently eaten. Repetitions of words or phrases do not occur too frequently in good writing. The repetition of paragraphs not at all.

All these barriers, however, are relative, not absolute, and are less evident in some kinds of experience than in others. For example, perceptual experience that may be classified as artistic is less disturbing on repetition (has a shorter refractory phase) than ordinary experience. This is shown in part by the endurance of classical literature and music, and the pleasure that still accompanies their repetition. This aspect of art and artistic products is presumably not simple, but there seems to be a factor in it which is analogous to the short refractory phase. A closer approximation to the nature of this factor awaits more exact experimental analysis, but enough seems to be clear to add weight to the Thorndike theory of some intimate connection between readiness to act and satisfaction.

& the physiological theory refractory phase really applicable here?

With a little speculative license one might conjecture what effect differential barriers to repetition would have on ideational contents consequent to repeated experiences. One might ask what would be the result if there were greater barriers to the repetition of specific concrete ideas than to the repetition of general ideas. The latter would obviously tend to survive and dominate consciousness while the former would tend to disappear. It is conceivable that even without direct reinforcement a single idea of very short refractory phase might survive indefinitely in consciousness. It seems probable that some analogous selective process is at work participating in the control of both intellectual content and habitual acts. Not only is the barrier against repetition of the beautiful and the general apparently less than that against the ugly and the specific, but certain moral and ethical ideas with both these factors in their favor might persist for long periods of time.

CHAPTER III

THE INFLUENCE OF RELATIVE FATIGUE

In physiological nerve-muscle fatigue experiments, the stimulus is usually momentary, relatively simple, constant in intensity, and repeated at regular time intervals. For a variety of reasons the stimulus that is most used is the faradic current. It is capable of fine adjustment, may be held at approximately constant intensity over long periods, and is exceedingly effective in quantities that do not damage the tissue. No physiologist would start a fatigue experiment with stimuli of unknown or variable form and intensity. Unfortunately, that seems to be common practice, and perhaps the only practicable procedure in so-called "mental fatigue" experiments. Nobody knows the relative stimulus value of two different mathematical sums. But what is vastly more embarrassing, nobody knows how to follow or to evaluate the ever changing inner factors in the total stimulus situation, such as the instructions, the personal interest of the experimental subject in the scientific aspect of his task, its bearing on the particular exigencies of his academic career, and so forth. It was one of the great services of Kraepelin in his analysis of the work curve to show how these inner factors may change during an experimental period. The real significance of that analysis, as I apprehend it, is not given in the precise number of variables or spurts that he found, not in the assumption that they are always present, but rather in the demonstration that variables in the inner stimuli may occur and must be reckoned with. It would not take us long to add to his objectively defined list many others taken from our experimental experience, such as competition and personal pride, repetition of the instructions, encouragement and persuasion, the presence of the instructor, rewards and penalties of various sorts, and the unanalyzed mass of obligations.

I am not unaware that this subject of the inner stimuli to mental work is packed with problems that we have no adequate technique to investigate. But that is no excuse for ignoring them. It is our business as scientists to try to see things as they are, even if they are complex. There is at least some ground for the suspicion that most if not all our real mental fatigue of the work decrement type is really a fatigue of the inner stimuli rather than any specific reaction system. This would account for the extraordinary correlations in the fatigue of the most diverse functions. In many so-called mental fatigue experiments the only common factor discernible to introspective analysis is the *intent* to keep working as fast as possible and the inhibition of competing interests.

In physiological experiments fatigue may be shown in two ways, either by a rising threshold or by decreased response to a constant suprathreshold stimulus. Only in the latter case is there an obvious work decrement. The former case permits a constant work output with a gradually increasing stimulus intensity. In mental work we are often distinctly aware of such changes in the inner stimuli that keep us at a disagreeable or monotonous task. Mere interest in the task may lose its force comparatively early. Then the task is continued from stubbornness, the dislike of failure, sense of obligation, honor, fear of ridicule, hope of reward, or some other motive. These may operate in succession. In the end, all of them may lose their force and we say, "I do not care what happens, I cannot go on with this thing any longer tonight.'' There may have been no measurable work decrement until the ''break,'' as Yoakum called it. But the process is none the less real fatigue when the continuation of work depends on a change of motivation.

All of this emphasis on the importance of the neglected factor of changing stimuli in the fatigue concept is probably sufficient to justify the formal statement of a necessary correction in the traditional definition of mental fatigue. We may call it the *First Law of Relative Fatigue,* neglected rather than new. Without pretending to give it final formulation we may express it as follows: *Within physiological limits, all fatigue decrement in the results of work is relative to the intensity of the total stimulus.*

Education and mental hygiene have a very practical interest in this phase of the fatigue problem. They make use of a large number of incentives in which, as Thorndike wisely points out, the changes in satisfyingness may be a real cause of work decrement. The adequate adjustment of inner and outer stimuli to the development of the individual and the needs of the case would seem to be a very real problem in the training of backward and gifted as well as of normal children. It seems strange that we have so little experimental knowledge of the relative value of available reinforcements. Inner reinforcement by increased motivation is, I believe, at least one factor in the underlying psychophysics of James's ''reservoirs of power'' which may be quite as significant for psychology as the action of adrenalin to which Cannon has introduced us. That long-continued mental and bodily activity under the reinforcement of emotion, or even the educational use of play, may be a source of serious exhaustion we have been warned by Kraepelin. Some other reinforcements are conspicuous for their insistence. Such a one is worry, with its endless repetition of disturbing ideas. It would seem to be no accident that this is so closely connected with exhaustion psychoses.

I believe that the relative value of the various inner stimuli would repay the closest study. Just now it seems to be interesting the abnormal rather than the normal psychologist. Everyday experience is full of approximations. Their refinement by experimental techniques would not seem to be an impossible task.

Relative fatigue and the refractory phase. It is possible that we can study relative fatigue not merely by the changes that occur during long series of repetitions but more expeditiously in the relative refractory phase which the genius of Verworn proved to be identical with the fatigue process. Since the relative refractory phase is common to all nervous tissue, I asked the question long ago, whether we can find in mental processes a similar phenomenon. Probably every mental process shows something analogous. We have already seen how repetitions of many sorts are avoided for some time whenever practicable. The repetition of questions, courses, lectures, phrases, and even words is possible enough, but, except for special reinforcing circumstances, it is postponed until the effect of the initial case is somewhat worn off. The routine is regularly less alluring than the unusual. Possibly the decreased effectiveness of overmemorization is a case in point. Possibly even the lack of attention to frequently repeated processes, which is commonly regarded teleologically as a freeing of consciousness for new adjustments, may be caused by the longer refractory phase of the more complex systematizations of attention, so that the rapidly repeated factor is dynamically excluded from conscious emphasis. It is not impossible that Aristotle's catharsis by dramatic representation of suffering and evil really operates by developing a refractory phase, and a kind of relative fatigue. How far this principle operates in habituation to environment, indifference to shocking conditions of poverty and morals, to suffering, and to the horrors of war, as well as to luxuries "when the novelty has worn off," I am not prepared to estimate.

It would seem that some of these or analogous phenomena ought to yield data for a scientific study of the intensity of the inner stimuli in connection with fatigue if we only knew how to use them. But the very difficulties of technique emphasize how far we are from a real knowledge of mental fatigue.

Relative fatigue and competition. The simplicity of the nerve-muscle paradigm of mental fatigue is further misleading in that it gives no indication of certain important complications which are characteristic of higher systematizations, and which Sherrington called their competition. In a nerve-muscle preparation the neural impulse has only one possible path. In the higher nervous systems on the contrary any afferent impulse may theoretically activate every efferent path. Just which motor process it finally initiates is determined by a kind of competition. Competition appears even in the spinal reflexes, though less conspicuously than in cortically conditioned action, where it is the rule. Unfortunately, however, just where it is most significant it can seldom be followed objectively by our present means of investigation. But there are evidences of its operation in associative thinking, in attention, and in perception as well as in conduct.

The relatively fixed tendencies of competition in the cord are probably determined relatively simply by neural growth and maturation. In higher systematizations the outcome of competition tends to follow habitual patterns which have originated in the varying life history of the several competitors. At any given moment in a developed system of this sort, the outcome may be modified by a variety of reinforcing and inhibiting factors. Among the latter we must count fatigue. In closely balanced competition the absolute degree of fatigue need not be high to make it a deciding factor. Indeed, it is conceivable that if the balance of the other factors is close enough, an infinitesimal fatigue, or the slightest trace of a refractory phase may totally change the character of the response, just as intrinsically trivial reinforcements or accidental inhibitions may be the arbiters.

The relation of fatigue to balanced competition gives us a second type of fatigue relativity. Fatigue is relative, not only in the relation of apparent work decrement to stimulus, as expressed in our first law; it is also relative in the sense of a differential fatigue of the various factors in a competing group. We may tentatively express this second type of fatigue relativity in the form of a law which for want of a better name we may call the *Second Law of Relative Fatigue,* because it implies a higher systematization than the first law. *In any complex of competing neural tendencies the relatively greater fatigue of one tendency may eliminate it from the competition in favor of the less-fatigued tendencies.*

Unfortunately, the mechanism of competition cannot be studied at all in simple preparations and only imperfectly in the reflexes. The most characteristic systems are the least accessible. In the search for accessible human systems of greater complexity than the reflexes, it occurred to me, over twenty years ago, that the motor apparatus of the eyes offered some unique advantages. There we may study various intimately related and delicately adjusted final paths which are directly connected with reactions of considerable biological importance. Furthermore, every variation of their interaction is capable of being recorded on a single plane, without complicated mechanical devices, and without the distortions incident to the moving of heavy masses, like the limbs. Since that time the eye-movements have proved to be an unusually valuable indicator of neural conditions in some forms of insanity, under the action of alcohol, and in the daily rhythm. In the hope of using them for an analysis of fatigue phenomena, I took a considerable number of binocular records of rapid successions of eye-movements after the model of the ergograph. Though reported on informally from time to time, publication of these records was delayed a long time because of my inability to account for some of their most conspicuous peculiarities. As these difficulties were gradually cleared, the records have been seen to illustrate in a remarkable way some of the characteristic phenomena of mental fatigue, and pseudo-fatigue. In particular they admirably schematize the second law of relative fatigue and the "breaks" that it conditions.

Let me assume the reader's familiarity with the technique of photographically recording the eye-movements from the corneal reflection. For the records under discussion the eyes moved horizontally through an arc of sixty degrees, fixating successively two knitting needles which were situated thirty degrees on either side of the primary position of the line of regard. Each dot or dash on the records represents one phase of the alternating current, and a time interval of about eight thousandths of a second.

The succession of eye-movements in these records was as rapid as practicable with subjectively adequate successive fixation of the two fixation marks. Some of the more characteristic fatigue phenomena which they show are: (1) The speed of movement becomes less toward the end of the series; (2) the fixations become less accurate; (3) and, finally, the line of movement itself becomes quite irregular. Our records show the climax of these processes in a "break" after the gradual decrease in angle velocity corresponds to the work decrement of extirpated muscle. But in this case, in view of Sherrington's demonstration of the reciprocal inhibition of antagonistic eye muscles, it doubtless involves something more. The greatest angular velocity of eye-movement could only occur when the relaxation of the antagonistic was perfectly coördinated with the contraction of the agonistic muscle. The pseudo-work-decrement in this case then is not purely muscular, but is, in part, a matter of defective neural integration and muscular coördination. The increasing errors of fixation probably have a similar origin. That is, the total elaboration of the contraction impulse and the corresponding relaxation of the antagonistic becomes less exact in successive repetitions of the act of fixation. But the coördination is not limited to the internal and external recti as one might expect them to be in horizontal movements of the eyes. All the records of sixty-degree eye-movements, which I have ever seen, show a vertical factor. In all my records this vertical factor results in an elevation of the line of regard. But it varies from movement to movement. That these vertical components are not accidents of purely muscular origin is shown by binocular records. Since the disturbances are homologous for both eyes, their origin must lie in the central nervous system. While occasional gross disturbances occur early in the series of movements, they become more and more conspicuous as the series progresses. The vertical components represent the intercurrent action of related and competing, but this is a case of non-inhibiting systems. When they become extreme they tend to interrupt for a moment the main rhythm of horizontal movements. In some cases these various disturbances produce a moment of confusion and a break in the process which in ordinary mental fatigue experiments would be interpreted as complete fatigue or exhaustion.

Our eye-movement schema for the relative fatigue of competing systems is particularly free from complications through extrinsic facilitations and inhibitions. Retinal fatigue or adaptation is reduced to a minimum by the eye-movement itself, and the consequent shift of the area of retinal stimulation. The homologous fixation marks, under constant illumination present the same stimulus for each reaction in the same direction. Central conditions of the successive reactions, such as interest, attention, and motives to continue at work, cannot of course be guaranteed to remain constant. But the experiment itself introduces no obvious distractions like the physical discomfort of the ergograph. Moreover, all the relative fatigue phenomena appear during short experimental periods.

In order to protect our conclusions from the dangers inherent in a single line of experimental evidence, I sought other similarly complicated coördination systems. While thus far no other has been found with all the advantages of the horizontal eye-movements, those movements of the index finger which Bergström recommended for ergographic work show a similar complication. Undoubtedly, the strongest and best-practiced oscillatory movements of the index finger are the flexion-extension movements. Considerably less easy for most of us are movements of the finger sideways in the plane of the hand. In any event, rapid oscillation of the finger in this latter direction is always disturbed by intercurrent action of the flexors and extensors. Their action prevents rectilinear movement, decreases the angular velocity, and finally may so confuse the process as to produce a break in the sequence of oscillations, quite like the disturbances of the eye-movements.

It was the phenomena of these relatively accessible complex systematizations that encouraged me to reanalyze the mental-fatigue concept. The eye-movement paradigm gives us the clue not only for a more intelligent experimental investigation of mental fatigue, but also for the interpretation of previous investigations. Probably much of the irregularity of the tradition may be understood as a consequence of the neglect of the laws of relative fatigue. The work decrement can no longer be regarded a satisfactory measure of fatigue, nor the break as a consequence of exhaustion. Perhaps the least expected change that the new paradigm will make in our tradition is the function of the sensations of weariness. These may, after all, turn out to be important factors in relative fatigue. Their effect in any concrete case, however, will be determined by their relative importance in the group of competing tendencies.

It will be noted that the eye-movement paradigm is still much too simple to apply directly to our mental processes. In place of its anatomically restricted competition to the nuclei of the third, fourth, and sixth cranial nerves, we have reason to believe that cortical competitions are as indefinitely complicated as the various active association tendencies. That a variety of tendencies to associative reproduction are normally aroused as the effect of a mental stimulus is indicated by many facts, notably the free-association experiment. This normal spread of excitation, coupled with the effect of psychophysiological rhythms, and the complication of simultaneous stimuli from the different receptor fields, gives the competition in mental operations an almost chaotic complexity. In addition to all that, we must extend our notion of competition and relative fatigue to those more slowly changing inner determinants of action that we call motives, controls, and the like. As we have already noted, it seems probable that these, since they are often the only continuously acting factors in mental work, are more apt to be the locus of absolute fatigue than the several discrete association tendencies which are involved in the mental task.

But, notwithstanding obvious limitations in complexity, our paradigm probably represents something quite fundamental in the fatigue process. However long the experimental mental operation may be continued, and however insignificant its work decrement, there comes a moment when it stops. It may be interrupted by demands for food, for sleep, or by some competing task. It may be interrupted by the gradually increasing insistence of inhibiting sensations like thirst, eyestrain, muscle pains, or pressure pains from sitting still. In any case, the work decrement of the consequent break can never be fully understood if we regard it as a direct product of fatigue, but only in connection with intercurrent competing tendencies. Real fatigue may be a contributing factor, but the apparent decrement of the break may bear no regular relation to the degree of absolute fatigue in the tissues which performed the discontinued task.

This enables us to understand why in pathologic nervous exhaustion the physician in search of a therapeutic measure may seek to strengthen some competing interest. He may even try to develop some fad like philanthropy, golf, the calculation of food calories, or what not, to compete with the old system and its emotional, business, or religious reinforcements. Some normal lives seem too full of competing tendencies. In my own case I have been interested in observing how every prolonged period of monotonous work like correcting examination papers, for example, finds before its close some insistent demand for interruption. If I successfully suppress one demand, more insistent ones arise, until, finally, effective voluntary reinforcement of the main task suddenly ends. The voluntary reinforcements may have developed such sensations of strain that the surrender to a competing impulse brings great relief. I know that the interruption is not permanent. I consent to it to get the competing matter off my mind, expecting to return presently to the main task, freed from the incubus of that particular competitor. In very much the same way, after lying awake for a time on one side we turn over, not because we could not lie on that side longer, not because we expect any great improvement in comfort from the change, certainly not because we expect to lie on the other side forever. The displacement of the body mass is scarcely the product of fatigue. But in the complex of competing tendencies a little relative fatigue becomes the occasion for an entirely disproportionate reaction. Possibly social unrest follows a similar course. The body politic seeks a change in government, or in social and economic conditions, not because the present is really unendurable, not because it expects a permanent betterment. In some cases at least, it acts from relative fatigue, to shift the pressure. One conjectures that relative fatigue is intimately related to the dynamics of social revolution. I suppose all the phenomena of restlessness and the corresponding attractiveness of change finally reduce to competition and the relative refractory phase. They operate in work and play, in social and economic activities, in politics and in religion. Without their interference in our lives, unwelcome as it sometimes is, we must have continued indefinitely in the direction of our first activity, with the consequent loss of that vital equilibrium of various capacities on which the organism as a unit of different parts depends for its continued existence. Without their interference the initial process would work itself out to the final collapse of complete exhaustion.

Relative fatigue, then, is not a mere limitation of human efficiency. It is not exhaustion, but prevents it. It is a conservator of organic equilibrium, as well as a condition of organic development. The incapacity of the young child for long-continued monotonous tasks may be a symptom of an actively developing mind. Lack of competition would result in stereotypy, in mental deformity, or serious exhaustion, just as truly as the lack of stable reinforcing systems in the adult would mean perpetual infantilism. Thus it seems to me that the principles of relative fatigue have a direct bearing on the practical problems of education which the traditional doctrine of fatigue as apparent work decrement entirely missed. The development of the capacity to sit still, to continue long at routine work, the adequate response to recurrent formal requirements, all demand the weakening or elimination of competing tendencies. At least one of the perils of routine education arises from this depression of spontaneity, but I disclaim any right or intent to discuss the educational side of the problem.

I cannot quite resist the temptation, however, to point a methodological moral. There has seemed to me to be something almost humiliating in the eagerness with which tests of mental fatigue have been sought, while there is still so much that is uncertain in our knowledge of the fundamental nature of the process that we would test. In this connection, I sound a note of warning, that in my opinion the tendency to supplant psychological investigations by tests contains a serious menace to psychology. Both have their proper place. But it can only lead to confusion and work to the discredit of our science if the search for practical tests blinds us to the necessity for studying the dynamics of the processes that we hope to test. We cannot afford to develop a new phrenology.

Thus far this discussion of relative fatigue was written substantially as it was printed in 1917. The intervening years have partially justified my fears. They have seen test psychology reach an absurd climax and recede in relative prominence to reach a more critical phase, as a useful method of psychological investigation.

The experimental data on competition and the "break" have substantially increased, but have not seriously changed, the picture. One of the most notable additions, within my personal experience, to the psychology of "break" was a consequence of a study of the tenability of gas masks. By the courtesy of Dr. Benedict, Director of the Nutrition Laboratory of the Carnegie Institution, it was possible, during the war, to measure the decrement in a group of related neuromuscular processes consequent to protracted wearing of various gas masks. The group of measurements was substantially the same as that used in our collaborative study of the effects of moderate doses of alcohol. The decrement in response varied with the physical characteristics of the several masks. But all the earlier forms of mask had one factor in common, they were uncomfortable. Protracted tenancy of the masks produced decrements in most of the measured neuromuscular processes, but these effects were insignificant compared to the total results. With uncomfortable masks the degree and importance of discomfort steadily increased until it reached a point where no available reinforcements sufficed to hold me to the task. Finally, neither patriotic duty nor professional pride was able to make me continue the experiments. I could readily understand how one might rather face probable death than continue to wear those masks. It is credibly reported that our soldiers sometimes reacted in just that way. As recompense for my minor discomforts, I had the satisfaction of helping along the design of masks that less flagrantly abused human endurance. The phenomenon of the "break" due to increasing discomfort is doubtless a familiar one in the history of human torture and the third degree. I had enough personal experience with it to develop a certain amount of insight and to confirm my previous conviction of the importance of rival stimuli.

Less spectacular but rather more illuminating are the records of eye-movements when visual stimuli are in rivalry with those from the vestibule and also when oscillating visual fixation objects have too high a frequency and acceleration.

In the former case the picture was as follows. When vision of an abnormally moving environment was interrupted the vestibular reflex snapped back into dominance with a latency which was too short to measure on our records. When vision was reinstated the corresponding pursuit reaction broke through the reflex with a relatively long latency of the order of 0.2″. The reinstatement of normal pursuit was sometimes still further delayed by a period of hesitation and confusion between the two systems of reaction. With still greater velocity of the visual stimuli this confusion was protracted as the main form of reaction.

In records of uncomplicated ocular pursuit of oscillating objects of regard, the extent of pure pursuit varied inversely with the frequency of oscillation and the acceleration of the object of pursuit. At certain speeds of the object, pursuit broke down into approximately still fixation. The latter may be regarded as a rival mode of reaction of simpler constitution.

All these records of ''break'' seem to emphasize the fundamentality of what we have called the second law of relative fatigue. I conjecture that it is as important in abnormal as in normal psychology.

CHAPTER IV

INHIBITION AND VARIABILITY

WITH the discovery of the effect of the vagus on heart, the inhibitory influence of higher centers on reflex activity, and the interference of various stimuli with the reflexes of the frog, inhibition became one of the notable problems of the operation of the nervous system a long time ago. The researches of Sherrington gave us an entirely new idea of its importance in gross behavior. His experiments demonstrated that it functions in the fine grading of the strength of reactions and in rhythmic alternating movements; such as stepping, flying, swimming, and the movements of respiration. It also plays a major rôle in the conditioned reflexes of Pavlov and in what we ordinarily call training. But notwithstanding its scientific significance and an almost overwhelming literature, its physiological nature still remains obscure.

Many instances which more or less closely resemble the phenomena of inhibition as described by the physiologists have been noted in mental life. The earliest observation that a more intense pain overcame a lesser one is credited to Hippocrates. Probably the most extensive use of the concept of inhibition in psychology is found in Herbart's attempt to develop a mechanics of ideas. Consistent attempts to exploit inhibition as a fundamental neuropsychological process were made by Exner and Verworn, but it has also been more or less conspicuous in every systematic psychophysiological description of human consciousness and behavior. The most frequent outcrops of the concept of inhibition in experimental psychology have been in connection with investigations of attention, memory, choice and the conflict of impulses, the rivalry of sensory stimuli, and the effects of practice. In Wundtian psychology it became the physiological substratum of the fundamental process of apperception. In the Freudian school it plays a major rôle in several psychological dramas.

It is a scientific tragedy that in all this exploitation of the concept of inhibition, notwithstanding many unproven hypotheses of far-reaching psychophysical import, there has been no satisfactory analysis of the process and no conclusive proof that what the physiologist means by neural inhibition has any real relation to mental phenomena. In view of the physiological uncertainty with respect to the nature of inhibition, it is not altogether the fault of psychology that the physiological concept has come to be used somewhat uncritically. But without further argument I venture the thesis that if we continue to use the concept of inhibition, psychologists have a real stake in the task to help investigate, with such resources as may be available, not only its apparent outcrops, but also the nature of the processes which are involved. It is of little or no scientific value to construct hypothetical schemes of neural action to account for the observed facts of mental life unless the hypotheses can somehow be submitted to experimental test. Facts without hypotheses are dead, but hypotheses which cannot be verified might as well be.

Restricted in the method of direct experimentation to techniques that may be applied to intact and complex nervous systems, the experimental psychologist is peculiarly dependent on the results of physiological experiments on isolated tissues and simple systems both for his working hypotheses and his elementary facts. But he must take scientific responsibility for testing the extension of these hypotheses and facts to the complex conditions of human behavior, experience, and personality. The complexity of his subject matter, his limitations in techniques, and his responsibility, all impose special obligations to scientific caution. While it is often regarded as unfortunate that nerve fibers, nerve-muscle preparations, and reflexes cannot be functionally isolated on demand, in the intact human, it should not be forgotten that it is exactly in the mutual interrelationships of the various neural strata in human behavior and the type of integration which is called consciousness that there lies a group of problems of the utmost practical importance.

Fortunately, alongside of the highly complex processes which are characteristic of the cerebrum, simpler systems are accessible even in humans which are more or less comparable with the reflex preparation of the physiologists. While these simpler structures may never be completely separated from the action of superior systems in normal human life, their low latency, specific excitability, and course of development often give them a position in neural organization which permits differential experimental study. Complete separation from the rest of the nervous system would rob them of their peculiar human significance, but they should serve as a connecting link between the nerve-muscle preparation and the more elaborate processes with which the psychologist is chiefly concerned.

Our uncertainty as to the nature of inhibition is not unique. Even the most fundamental questions in physiology of the neural system, such as how stimuli produce neural action, how excitation is propagated within the axone, how it is transmitted from neurone to neurone, and how neural action produces muscle contraction, are not without their scientific difficulties. While these questions remain incompletely answered, familiarity with the phenomena, and apparent, but often illusory, correspondence with common interpretations of the law of the conservation of energy, give the positive effects of stimulation a relatively simple appearance. The increase of neural action by increased stimulation seems almost axiomatic. Decreased neural action by increased stimulation, on the contrary, seems strange and unreasonable, opposed to familiar physical experience and to the fundamental laws of dynamics. Doubtless, in part at least, on account of these pseudo-logical barriers, the decrements which are effected by stimulation have received relatively less attention in psychology than the positive effects of stimulation. Even in physiology there are impatient voices that would ''have done with inhibition.''

Notwithstanding all this, there is a widespread conviction that in both simple and complex systems the inhibitory effects of stimulation are just as real and fully as important as the excitatory effects. In a paper on ''Habituation to Rotation,'' I pointed out evidence of certain analogies between the process of learning and the inhibition of afternystagmus in a series of rotation experiments. At that time I expressed the hope for some direct experimental evidence as to the nature of inhibition in human neural processes. This and the succeeding chapter are steps toward the realization of that hope.

One may well bear in mind the physiological as well as the psychological warning that possibly there are several different kinds of inhibitory processes. Certainly the arrest, decrement, or discontinuance of human action consequent to changes in the external vital conditions are of various appearances. One of the simplest behavioristic aspects of arrest is the mechanical inhibition exercised by antagonistic muscles. The extension of the leg in the knee-jerk, for instance, may be voluntarily reinforced by additional arbitrary contraction of the leg extensors. This reaction may even become, by a little practice, the habitual response to the same blows on the patellar tendon that produce the reflex. It is in nature a purely mechanical reinforcement due to a secondary contraction of the quadriceps muscles consequent to a delayed conduction from the proprioceptors, probably through the cortex of the cerebrum. In an analogous manner the extension of the leg may be stopped before it has reached its maximum amplitude in the knee-jerk by an arbitrary contraction of the flexors. With adequate recording technique (where the record is made by the thickening of the quadriceps on the one hand and of the antagonists on the other) this decrease in the amplitude of leg movement may be shown to be due to the mechanical interference of contracting antagonistic muscles. Holt reported a similar inhibition of vestibular nystagmus by suitable lateral fixation. It is possible to produce in this way a purely mechanical decrement in the amplitude of extension of the leg, quite apart from all question of central inhibition and neural conduction with decrement.

Analogous peripheral "terminal" inhibition of the movement of a limb by the action of antagonistic muscles is common enough in human behavior. Initial contraction of antagonistic muscles was found by Langfeld as the effect of negative instructions. When we stretch, muscle pulls against muscle without gross movement of the limbs; we shut our jaws to keep back speech. We open the eyes to prevent winking and grip the dentist's chair, in part at least, to keep our hands from interfering with his particular modes of stimulation. There may be some question whether this inhibition of action in typical forms of behavior falls within the proper definition of inhibition, even though the usual definitions do not specifically exclude it. Certainly "terminal" inhibition is different from either central or reflex inhibition. Its conditions and course are relatively simple. Its nature is entirely clear. This may be regarded as an extreme case, but in the group of phenomena that are more specifically neural inhibitions there may be anomalies that are just as real, if not as obvious.

Hesitation and block not infrequently appear in normal human behavior consequent to the interplay of opposed motives or mutually contradictory perceptual data. While this central conflict bears a superficial resemblance to the simultaneous enervation of antagonistic muscles and is not infrequently hypothetically identified with it, there is no direct evidence in physiology or psychology of any real antagonistic neural action.

Two other varieties of inhibition are sharply differentiated in my experimental experience from terminal mechanical inhibition. When a second blow on the patellar tendon follows its antecedent within an interval of less than half a second, if the stimulus blows remain the same, the second thickening of the quadriceps is regularly less extensive than the first. In this case the second stimulus is said to fall within the "relative refractory phase" of the first reaction. There are many psychological events that appear to fit the general framework of refractory phase. Some of them we have already discussed. Experimental evidence is not wanting in the intact human that stimuli of a given intensity may be too frequent for normal reaction and that stimuli introduced during the decreased excitability subsequent to initial stimulation may fail to evoke the usual response. Nevertheless, it is certainly untrue that all inhibition of neural arcs depends on and follows only the normal reactions of these arcs. Before the concept of the refractory phase can be applied to any of the usual varieties of decrement by additional stimuli, one must ask the plain experimental question, "What are the neural effects of subthreshold stimuli?" That they occasion some change in the neural systems on which they impinge is seen in the phenomena of summation of subthreshold stimuli as well as in the perpetuation of the refractory phase by stimuli that are too weak to produce a regular reaction. The whole field of subthreshold stimulation has scarcely been touched by experimentation either in physiology or in psychology though its problems and presumptive importance date back at least to Leibnitz.

In addition to the decrement of refractory phase, it was also possible to decrease the muscular response to a blow on the patellar tendon by a voluntary and entirely arbitrary deadening of excitability, the exact nature of which I have as yet been unable to discover, either in the tradition or by experimental investigation. This deadening was initiated by a complete relaxation of the thigh muscles, but it seems to consist of something more which could not be recorded mechanically from the muscle. It appears in introspection to resemble a detachment, deadening, or going to sleep of the limb. Similar phenomena in the mental life appear in relaxation as well as in sleep and drowsiness. Pavlov in a recent summary of his work regards inner inhibition and sleep as fundamentally the same.

The first phenomenon in voluntarily deadening a reflex is a real decrease in muscle tonus. This is evidenced by records from the muscle and more simply by observing a decreasing resistance to gravity. If it is free to move, the limb glides to a more stable equilibrium. It falls or swings against supports or hangs limp from its center of rotation.

Subsequent to this initial relaxation further depression of tonus appears to be exceedingly small or entirely wanting, while the deadening process reaches wider areas and increased depth. The relaxed limb comes to have a numbness and a feeling of detachment as though it never could move again and didn't belong to the rest of the body. The spread of deadening is irregular, following voluntary attention to the several parts of a limb or to various limbs. Each in succession may be felt to press more firmly against its support or to hang more limply from its hinges. In extreme cases the eyes close. In some way the depth of the process also seems to be related to depression of inspiration. There may be for a time an almost complete disinclination to breathe. It seems for a few seconds as though one never would breathe again. This commonly ends in a disturbing spasm of inspiration. I conjecture that with some non-exciting artificial respiration, relaxation of this sort might eventuate in normal sleep, perhaps even in something more serious.

Something similar appears to occur in the intermittence of pain which is conditioned by excessive pressure. In contrast to inhibition effected by attention there may be no apparent rivalry and displacement by other conscious contents, but rather an intermittent breaking through of the pain sensations. How far these spasmodic exacerbations are controllable and capable of delay would make an interesting experiment with many practical leads, and the possibility of a better understanding of some abnormal phenomena. The phenomenon is not simple adaptation. It is directly affected by intent. Analogous phenomena without conscious intent are found in the depressing effects of some drugs, such as ether, chloroform, alcohol, and morphine; and by the partial products of decomposition, such as CO_2 and the fatigue toxins. To these could probably be added some of the depressant glandular secretions and the chemical conditions or products of auto-intoxication. All these are true stimuli in the sense that they are changes in the external vital conditions of neural tissue.

Objectively quite different still is the total inhibition of a reflex that occurs as the consequence of rivalry and competition for a final common path. This variety of inhibition has been frequently described by Sherrington. Only one case, so far as I know, is graphically recorded in the complex conditions of the intact human nervous system. As we have already noted in connection with the second law of relative fatigue, when visual stimuli compete with opposed vestibular stimuli each tends to produce slow movements of the eyes in opposite directions (respectively, the opposed slow phases of pursuit and of reflex compensatory eye-movements). Under certain circumstances the visual stimuli come into exclusive control of the final common path to the eyes and completely inhibit the opposed vestibular reflex. This inhibition may appear after a more or less protracted period of rivalry and apparent confusion. It regularly has a relatively long latency of the order of a fifth of a second. But when the dominance of the visual pursuit is once established, and as long as adequate visual stimuli persist, the vestibular reflex may be as completely inhibited as though it had not existed. If the visual stimuli are suddenly withdrawn while the vestibular stimuli persist, the reflex emerges suddenly and completely. The latent time of the reflex emergence is very short, too short to be measured in our photographs. The subsequent course of the vestibular nystagmus is as though it had never been interrupted. That is to say, the amplitude and frequency of the reinstated vestibular nystagmus correspond to the stage of adaptation to rotation that would have been reached if the reflex had not been temporarily interrupted. This variety of inhibition seems to have many analogies in mental life, especially in attention and memory. As we have elsewhere pointed out, our records show that it is incompatible with the Drainage Theory.

One of the most regular varieties of sensory inhibition is represented by Heymans' Law. In a number of sensory fields, Heymans measured the inhibitory power of a stimulus by the intensity of the stimuli that it is capable of rendering subthreshold. In a series of strikingly consistent experiments on a single subject, he found the general law that the inhibitory power of a stimulus increased in direct proportion to its intensity. Heymans' Law has been confirmed and generalized by various experimenters, notably by Spencer's experiments on the white rat and a group of human subjects. He found further evidence of its central origin in the inhibitory effects of a stimulus which was given to one retina on threshold stimulation of the other retina.

A unique form of inhibition which was discovered early in the history of response of nerve to electrical stimulation by the constant current is the anode block. While this has no known analogies in normal physiology or psychology, there are several phenomena of systematic or persistent inhibition that superficially resemble this form of inhibition such as prejudice, set, and some of the associative inhibitions. There seems to be no inherent impossibility in the hypothesis that persistent stimuli in complex neural systems as in simple ones may artificially produce a region of decrement somewhere in the system that they affect which operates to extinguish subsequent excitations.

One of the most notable instances of systematic inhibition was that which was found by Sherrington in the reciprocal innervation of antagonistic skeletal muscles. The convincing experimental evidence for this form of inhibition led to its almost immediate adoption in the psychological tradition and to its more or less uncritical extension to many phenomena where real antagonism has been difficult or impossible to prove.

In contrast to these relatively ephemeral forms of inhibition is that form of central inhibition by which excitability is gradually diminished through a long series of stimulations. A common name for such phenomena is "negative adaptation." For example, in successive instances of rotation it has been found by various experimenters and confirmed by photographic records of the eye-movements that there is a gradual change in the character of the reflex vestibular nystagmus. With closed eyes the nystagmus of acceleration as well as of deceleration gradually decreases in amplitude and frequency to mere beginnings of normal reflexes. The slow phase of the nystagmus which is the true reflex "compensatory" phase gradually decreases in angular velocity and becomes progressively less and less adequate as a mechanism for maintaining a fixed position of the line of regard. The more or less adequate initial angular velocity of the eye reactions to the beginning of the first instance of rotation is gradually inhibited in subsequent instances by some neural mechanism of which we are profoundly ignorant. Similar decreases in reflex responses were found in a protracted series of measurements extending over a year and a half in both the lid-reflex and the knee-jerk. I have pointed out elsewhere some significant practical implications of these experimental results.

Whether there are still other types of inhibition which correspond to such mental processes as attention and will is an open question. There may be, in fact, many mechanisms of inhibition, of which we now know nothing whatever. It is conceivable, on the contrary, that all the various forms may be reduced by more adequate analysis to a few fundamental types, or even a single fundamental process.

The moral is obvious. There are in the literature many theories of inhibition. Evidence that one is true does not disprove the others. Each must stand or fall by the specific evidence that may be found to prove or disprove it. Perhaps in the end we shall not be content to refer to the rôle of inhibition in our mental life but shall inquire rather as to the rôle of a variety of fundamental conditions of decrement.

Whatever its condition may prove to be, neuromuscular inhibition plays many more or less obviously important rôles in the development of personality—though in the latter case they are largely unexplored. While less obvious than overt action, the time has passed when the negative effects of stimuli may be ignored in the description of mental phenomena. They may be just as important as the more obvious positive effects of stimulation. In any event their systematic investigation is a clear obligation of psychology, calling not for speculation but for experimentally controlled facts.

CHAPTER V

INHIBITION AND SUMMATION AS CONSEQUENCES OF REFRACTORY PHASE

THE hypothesis that inhibition is due to the development of a more or less protracted refractory phase in the relevant tissues originated with Verworn. Forced to abandon the Gaskell-Hering hypothesis of assimilative inhibition because of lack of experimental proof, he inquired whether the dissimilative process itself did not carry the conditions of its own depression. This inquiry led to the refractory-phase hypothesis. It involved a reinterpretation of the available data and resulted in a long series of brilliant experimental investigations which reached their climax in the work of Fröhlich in Germany, of Keith Lucas and Adrian in England, and of Forbes in America.

The hypothesis that inhibition is due to refractory phase is not without its difficulties. Some critics find apparently insuperable objections to it. Many doubt that it accounts for all the phenomena of inhibition. The evidence, however, for some intimate connection between refractory phase and inhibition cannot be lightly dismissed.

This evidence is partly theoretical and partly experimental. It began with the theoretical probability that reduced excitability after reaction is a general phenomenon of all living tissues, and the experimental fact that it may be prolonged indefinitely in simple neural paths by stimuli which succeed one another so rapidly that the path fails to recover normal excitability. What was first found experimentally in the strychninized frog by Verworn, Tiedemann, Fröhlich, Vészi, and others proved to be true of a considerable variety of processes under various experimental conditions.

Historically, the first bit of experimental evidence for the refractory-phase hypothesis was the ''apparent inhibition'' of Wedensky. In connection with a study of the fatigue of nerve fibers he discovered in 1884 the interesting and suggestive ''paradox'' that if the nerve of a nerve-muscle preparation is excited by an interrupted induced current, apparent inhibition takes place when the rapidity of interruption is increased beyond a critical point. This critical point he found to be lowered by narcosis (1903). Hofmann (1902) held that the phenomenon was due to fatigue of nerve end-organs in the muscle.

One of the most frequently cited supports of the refractory-phase hypothesis is the extremely condensed paper by Vészi (1910). His main question was entirely foreign to the problem of inhibition. He sought evidence that the reflex arc in the frog was composed of three neural links. To this end he stimulated the seventh, eighth, ninth, and tenth roots of the ischiadicus separately and in various pairs, using the prepared gastrocnemius as indicator. Only those consequences bearing on the theory of inhibition need now concern us. He found that muscular response was most vigorous from stimulation of the ninth root; least, from the seventh. Stimulation of the latter, however, inhibited reaction from the former, even at intensities that were too weak to evoke muscle contractions. Both the seventh and eighth roots also inhibited the tenth. He interpreted this in terms of decrement. That is to say, stimuli reach the final common path through some neural avenues so weakened that they regularly or exclusively evoke inhibition instead of excitation. The mechanism of this decrement was not discussed; neither, as Howell points out, was the possibility considered of specific inhibitory fibers in the seventh and eighth roots.

More direct evidence for the refractory-phase hypothesis of inhibition was furnished by Fröhlich in a series of papers beginning in 1907. Fröhlich identified the Wedensky paradox with the increased fatigability of reacting tissues when the stimuli were weak. The experimental evidence was obtained from the reactions of crab claws and the reflex frog. In 1908 he systematized the available data into a general theory of tonus, facilitation, and inhibition.

According to Fröhlich relative refractory-phase inhibition showed itself in the closing muscles of crabs' claws by a beginning tetanus when the muscle stimulated was without tonus. This beginning tetanus was explained as follows: During the first moment of stimulation the refractory phase was shorter than the interval between stimuli. As stimulation continued, fatigue set in; the vital processes became progressively slower until the duration of the relative refractory phase came to exceed the interval between stimuli, when inhibition ensued. The same strength and frequency of stimuli which produced beginning tetanus and subsequent inhibition in muscle without tonus, produced either inhibition exclusively, or momentary contraction followed by inhibition, when the muscle was tonically contracted. Analogous phenomena occurred in the reflex frog. Low frequency and high intensity of stimulation tended to evoke reflex beginning tetanus and subsequent inhibition. Stimuli which were too faint to produce reflex contraction or only slight contraction tended to inhibit an existing excitation.

Fröhlich regarded these instances of inhibition as a kind of dissimilative paralysis depending on the easy fatigability of the respective centers. This explained the propagation of inhibitive impulses without assuming special inhibitory fibers. The existence of nerve fibers which usually evoke inhibition was, however, not excluded. They may be neurones of special threshold and fatigability. The general tendency of weak stimuli to produce inhibition and the corresponding long duration of the relative refractory phase after faint stimuli, Fröhlich referred to the characteristics of the curve of the restitution of equilibrium as determined by Nernst. Excitability for a given intensity of stimulation should theoretically be reëstablished more slowly the nearer the logarithmic restitution-curve approaches its limit.

The general relationship between inhibition and the frequency of stimulation was confirmed by Beritoff. He found that the semitendinous muscle contracted reflexly to a stimulation of the ipsilateral peroneal at a rate of two hundred per second. During reflex contraction it was inhibited by the additional stimulation of the contralateral sciatic at a rate of twenty-five per second. Action current records showed a series of gaps in the effects of rapid stimulation at the same rate as the slower stimulation. This phenomenon was confirmed by Adrian.

While the experimental study of the relationship between refractory phase and peripheral inhibition began in the Verworn School, it achieved its highest technical refinement in the work of Keith Lucas, Adrian, and Forbes. The Verworn-Fröhlich doctrine was notably modified by the researches of Keith Lucas. In his investigation Lucas set out from the following critical consideration, "The refractory phase is a consequence of the nervous impulse and if the second of two stimuli falls in the refractory phase of the first and consequently is ineffective in the sense of setting up no nervous stimulus, then it will also set up no refractory phase and a third impulse will again set up a nervous impulse.'' This was experimentally confirmed by test on the sartorius muscle. ''A second stimulus so timed as to fall just within the refractory phase of its predecessor, and consequently to cause no second electrical response in the muscle, does not reduce the electrical response which falls just outside the same refractory phase.'' The results proved to be quite different on stimulation of the motor nerve in a nerve-muscle preparation. A stimulus ''interpolated at such a time that it does set up a secondary impulse in the nerve but is too early to cause response of the muscle'' prevents a response from the following impulse. His explanation is that the second impulse falls at such a time that it is propagated in the nerve in a reduced condition as though it had passed through a region of decrement.

Keith Lucas' explanation of refractory-phase inhibition consequently differs from that of Fröhlich and Hofmann. It makes inhibition due not to the refractory phase of junctional tissue as Hofmann held nor to the prolonged refractory phase of a narcotized or fatigued region as Fröhlich held, but to the combined refractory phase of normal nerve and the decrement of junctional tissue.

As Adrian put it:

> The whole mechanism of peripheral summation and inhibition may be summed up in a very few words. If there is an obstacle to conduction in the course of the nerve fiber, this obstacle may be surmounted by timing a series of impulses, so that all but the first are greater than normal, and it may

become insuperable if the impulses are timed so that all but the first are smaller than normal.

The regular locus of decrement and the normal obstacle to conduction was regarded as the synapse.

The evidence that the synapse is always a region of decrement seems to be far from complete. If the synapses are to be regarded as semipermeable membranes interposed as resistances between conductors of neural energy, it is difficult to understand the fact that neural responses may increase in magnitude within neural tissue as well as decrease. Such increase has been repeatedly demonstrated as in the case of the strychninized spinal preparation. It seems to be peculiarly prevalent in mental phenomena. The phenomena of summation would probably appear much the same if neural action across the synapse should prove to be, as many neurologists believe, not conduction, in any proper sense of the word, but the stimulation of new links in a chain of irritable neurones. In the latter case the response of any link would, however, depend rather more on the characteristics of the link. It might be either more or less in magnitude than that of its stimulator.

Adrian distinguished two types of summation of stimuli, namely, summation of local disturbances in the receptor and a summation of nervous impulses. In the former, a stimulus which is too weak to produce a nervous impulse may produce local disturbances which take a short time to subside. "And if another stimulus of the same strength is applied before it has subsided, the additional disturbance may be great enough to reach the intensity required to start the nervous impulse." This effect is "purely local" and is generally known as the "summation of inadequate stimuli."

Summation of nervous impulses is quite a different matter. It depends on a second stimulation of a nerve fiber which occurs during a phase of heightened neural excitability, such as appears as the end effect of previous stimulation. The consequent excitation is then stronger than normal, as is indicated by its ability to travel farther through a region of decrement. When synapses are reached which would ordinarily be regions of decrement, or of extinction, this increased intensity of excitation will be able to pass, even though the synapses would otherwise be impassable. In this way a larger number of motor neurones might be stimulated, thus increasing the magnitude of response.

The converse would probably hold true if the secondary stimulation reached the nerve fiber in a phase of depressed neural activity, i.e., during its relative refractory phase. In that case synapses which were just permeable to normal excitation would become nonpermeable and effective blocks to further conduction of excitation. Normal action would then be inhibited.

Both of Adrian's two kinds of summation might combine to produce increased response, since summated conductions in the stimulated neurones would not only excite those neurones which were normally susceptible, but the repetition of subthreshold local disturbances in other receptor neurones might also summate, provided each stimulus fell within the phase of increased irritability produced by the preceding one. Conversely, if the stimulating impulses were so timed that each succeeding stimulus fell within a phase of depressed local irritability, no response would be elicited in receptor neurones with a high threshold.

Whether the synapse is merely a point of resistance to neural currents or a point of restimulation of neural links, any normal series of excitatory impulses may, and probably will, develop local summation in some receptor neurones and local inhibition in others, which have a slightly higher threshold, or a longer refractory phase. Total neural response may consequently be regarded as configurated response with both summated and inhibited factors.

If this construction is true in all parts of the human cerebrospinal system, the spread of neural impulses seems to follow a complex but understandable schema. At any given moment spread would depend on four factors: (1) The actual threshold of neural links at the physiological periphery of each active neural systematization as modified by refractory phase and recovery; (2) the magnitude of neural excitation in antecedent links, as modified by refractory phase and recovery; (3) decrements due to conditions met at the neural junctions; and (4) the presence of reinforcing or inhibiting collateral stimulation.

Since each neurone in a given chain of neural reactions is supposed to react maximally throughout to every superthreshold stimulus that reaches it, the limits to spread of neural reaction at any given moment would be given when the stimuli reached areas of high threshold. Modified spread would occur when any one of the four determinants is changed by any cause whatsoever. Changes in intensity of overt reaction as well as changes in configuration are probably to be regarded as due to changes in spread of neural excitation.

Such systems might be intrinsically stable or intrinsically unstable. Neural systems in the cord approximate stability, but with sufficiently fine recording technique, as we have shown, even they are not invariant. Systems of notable instability, on the contrary, occur in the cortex with others of intermediate degrees of stability. There is abundant evidence in the phenomena of learning that this is the case. The main anatomical difference between cord and cortex seems to be the number and complexity of association paths with consequent limitless possibilities for inhibiting, reinforcing, or sustaining collateral excitation in the latter organ.

If this construction is true, it should have some important consequences in the psychophysiological understanding of the variability of mental events and especially of the sudden shifts between excitation and inhibition which seem to occur in movements of attention. The depression of irritability at any point in the chain of neural events by any means whatsoever should theoretically be found to change the balance between excitation and inhibition which characterizes any given neural systematization. For example, whenever the accumulation of fatigue-stuffs or of depressing glandular secretions reaches sufficient concentration, the inhibitory effects of stimulation would tend to overbalance the summation effects with consequent more or less profound modification of behavior patterns. Changes in experience and even in personality may follow a similar formula. In extreme cases of accumulation of depressing stuffs, total unconsciousness would appear to be a normal consequence. Widespread depressed excitation under such circumstances would appear as a spread of inhibition. Stable conditions of irritability, on the contrary, would tend to foster stable neural systematizations of reciprocal inhibition and stimulation. Resystematization or relatively persistent changes in the spread of neural action would be expected when recurrent stimulation found any part of the system in persistent refractory phase or the rebound. Such an interplay of summation and inhibition as we have pictured consequent to inner changes in the irritability of neural tissue might easily occasion the illusion of lawlessness that seems to characterize so much of our mental life.

Verworn showed very clearly certain discrepancies between the all-or-nothing law and his formulation of the refractory phase hypothesis. According to the law any element of the nervous system responds to stimulation maximally or not at all. The experimental evidence for this law is commonly held to be conclusive for motor neurones with a strong presumption for sensory ones. The fundamental discrepancies between the law and the refractory phase hypothesis of inhibition is that inhibition is a more or less protracted process while refractory phase is transitory. If inhibition is really conditioned by the refractory phase, then the latter must be continually renewed to produce prolonged inhibition. But refractory phase is a consequence of activity; if the all-or-nothing law holds it is the result of maximal activity. Consequently, the paradox seems to follow that the protracted inhibition of a tissue is coincident with its maximum activity. This paradox appears to be a real difficulty in the Verworn formulation of the hypothesis and constitutes one of the main grounds for the revision by Lucas and Adrian. Even if, as Hofmann held, the synapse is also the seat of a refractory phase, one still faces the theoretical discrepancy between the maintenance of a refractory phase and inhibition, provided the former is always a consequence of action.

The discrepancy becomes specific in the reciprocal inhibition of antagonistics on the one hand and the inhibitions which are involved in preventing overt behavior on the other. The reciprocal inhibition of extensor muscle fibers on stimulation of a flexor should be possible according to the hypothesis only after their previous contraction. While this condition of inhibition has never been reported experimentally in physiological literature as far as I am aware, both Dodge and Dodge and Bott have published records that make it probable. In rapid oscillatory movements of the finger, the first movements are slower and of less amplitude than the succeeding ones. Records of such finger-movements which were taken with our precise microscope recorder in pneumatic connection with a sphygmographic cuff around the wrist showed four contraction-relaxation episodes for every complete oscillation except the first. While these episodes come to be exceedingly regular after the first few, the record of the first oscillation is uniquely long and smooth. That of the next one or two still shows unusual relationships between the several episodes. Records of muscle thickening by Dodge and Bott unequivocally show a cocontraction of antagonistic muscle at the beginning of oscillation, with reciprocal relaxation only after the muscle had first contracted.

Considered in terms of overt behavior and consciousness the requirement that positive action must precede inhibition would appear to mean that the inhibition of a reaction could only occur after that reaction was an accomplished fact, i.e., reactions could never be blocked before they occurred. This seems to be contrary to fact. The orderly progress of controlled behavior and mental events seems to require a form of inhibition which may on occasion operate without preliminary reaction. Either refractory-phase inhibition must on occasion be elicited by subthreshold stimulation or there must be some other mechanism of inhibition. Consciousness of inhibited residua is apparently necessary neither in attention nor in attempts at recall. It is certainly not always necessary in voluntary reactions as is demonstrated by the delay or block of anticipated reaction before it actually occurs. As Howell puts it, "It would seem inadequate to define inhibition simply as the quelling or blocking of action in progress. It is something more positive and characteristic."

A second difficulty in the refractory phase hypothesis as pointed out by Forbes concerns the alternation of inhibition and excitation which he located in the "premotor" neurones producing inhibition with stimulation frequencies above a critical frequency and excitation below it. After the critical frequency has been reached, or rather after the onset of inhibition, he found it difficult to explain on the basis of the refractory-phase hypothesis how any additional stimulation whatsoever can reëstablish excitation. Experimental facts prove that it does as Sherrington showed. It is possible that this discrepancy between the facts and the theory is not as great as at first it seems to be. The facts would not be unexplainable from the standpoint of the hypothesis, if it were taken into consideration that refractory phase is absolute only momentarily and does not necessarily involve all the elements of a neuromuscular system to the same extent. We have already discussed the possibility that in harmony with the all-or-nothing law, local summation and inhibition may depend on the strength of total stimulation that reaches the relevant tissues as well as on their frequency.

A third difficulty is mentioned by Howell in his searching criticism of the refractory-phase hypothesis of inhibition. He points out that according to the all-or-nothing law the *graduation of response* subsequent to the absolute refractory phase, as the latter gives place to relative refractory phase, is difficult to understand. This seems to be peculiarly true if we accept the Lucas and Adrian theory of extinction of conduction in a region of decrement. It seems to me, however, that this is no more incomprehensible than the graduation of response in general. The latter is quite as well founded, however, as the all-or-nothing law. Even in the reflexes it must be accepted as demonstrated. Perhaps it is not, however, a question of accepting one or the other. Both may be found to be consistent if due weight is given to differences of threshold in the several elements of the same neural link and the differences in the extent and duration of the relative refractory phase.

Howell points out still another difficulty of the refractory-phase hypothesis in that it furnishes no mechanism for the conduction of inhibition from one part of the nervous system to another. In this it seems to be in direct opposition to the conclusions of Pavlov.

Since it lacks both the information and the techniques to acquire information, psychology should be very cautious in taking sides on the purely physiological questions which are raised by the refractory-phase hypothesis. The ultimate nature of refractory-phase inhibition, its relationship to the all-or-nothing law, and the locus of its incidence must be settled by methods over which the psychologist has no mastery. Quite aside from these physiological questions, however, there is a legitimate psychological problem in the experimental question whether or not there is a kind of inhibition in human behavior which corresponds to refractory phase, and how far refractory phase can explain the outcrops of inhibition with which we are familiar as psychologists. These questions are not to be decided by the congruence or lack of congruence of the hypothesis with more or less probable physiological laws. The real test of the refractory-phase hypothesis in human behavior and experience is its congruence with experimental facts.

CHAPTER VI

THE EFFECTS OF FAINT STIMULI ON HUMAN SENSORY AND MOTOR REACTION SYSTEMS

THE fundamental question at issue in this chapter may be stated as follows: Is there any experimental evidence that refractory phase actually operates to inhibit any form of human behavior or experience? In the human reflexes at least this must be answered in the affirmative. Not only has refractory phase been experimentally demonstrated in all human reflexes in which it has been sought by reliable techniques, but, in one case at least, it has been shown to operate in a protracted series of stimulations to inhibit all reactions after the first. Quite apart from the question of mechanism by which a refractory phase can be continued without overt acts, Dodge has shown that it may be so continued in the knee-jerk. In this case, the sequence of stimuli was about ten per second, too slow for an effective refractory phase in nerve fibers. Consequently, the experimental results cannot be referred directly to Keith Lucas' formula of extinction at the synapse of reduced nerve conduction. One conjectures rather a local condition of depressed irritability at some point of restimulation in the neural chain, as was suggested by Lucas' discrimination between local and nerve summation. While detailed experimental investigation of the locus of the conditions of this decrement seems to be outside the present resources of the psychologist, the fact remains that under certain experimental conditions refractory-phase inhibition is a real phenomenon of human behavior. The general extension of the principle to the more familiar outcrops of inhibition faces the difficulty which we have already noted, that in experience and behavior there is often no noticeable antecedent positive reaction which is commonly supposed to be necessary to produce a refractory phase. This seems to be a critical point in the application of the refractory-phase hypothesis to human experience and behavior.

The crucial experimental question consequently seems to take the form, not whether a refractory-phase inhibition is possible in human life, but whether refractory-phase inhibition can be induced on occasion without antecedent reaction. This question might be expressed as follows: Can stimuli which are too weak to produce reaction depress irritability in a neuromuscular system, so as to inhibit reaction to stimuli that would otherwise be adequate?

While there is no direct answer to this question in the tradition aside from my own experiments, there are some indications in the literature of the subject that might lead us to expect an affirmative answer. In the first place, there seems to be no doubt that stimuli which are too weak to evoke muscular contraction may on occasion increase irritability. This is apparently the case in summation and "Bahnung." Where readiness to react can be produced by stimuli which are normally too faint to evoke positive reaction, there is a presumption in favor of the possibility of evoking unreadiness to react under appropriate conditions.

Moreover, some tendency of weak stimuli to delay or inhibit reaction has been noted in various physiological and psychological experiments. Wundt, reporting on the excitation and inhibition produced by the constant current at the cathode and anode, respectively, found that very faint stimuli caused anode inhibition alone, while stronger stimuli produced both inhibition and excitation at the respective poles. Verworn, reporting on the movements of the cilia of paramecia, found that the movements of the cilia are inhibited when that organism meets a faint mechanical resistance. Its movements are reversed into a negative thygmotaxis when it meets a strong resistance. An account of analogous phenomena consequent to weak stimulation was given by Miss Buchanan. "The electrical reflex responses of muscles which serve as extensors, especially those which serve mainly as such, may begin with a transient positive variation, usually lasting about 0.01 sec. when the stimulus is a single instantaneous one." It occurs in more than 50 per cent of the cases, "when there is no reason to suspect any abnormal condition of tonus," in response to stimulation of both the crossed brachial and that of the same side, when any one of three drugs, strychnine, phenol, or caffein, has been used to increase the excitability of the cord. "With two preparations in which the positive variation was found to last over 0.04 sec. the mechanical responses [of the gastrocnemius] began with, or was mainly relaxation instead of contraction." Interruptions of the action current in antagonistic muscles indicated that reciprocal innervation was in force even when this could not be detected in the mechanical response of muscle.

Fröhlich found that with gradually increasing strength of stimuli there was a zone in which they failed to evoke a muscular response. They might be strong enough to inhibit claw openers while they were too weak to excite them. This seemed to confirm Biedermann's contention for a neutral zone which can be produced by variations in intensity as well as by variations in frequency. Higher frequency at a strength which was just sufficient to stimulate the openers and inhibit closers operated to inhibit the openers. After that frequency was reached the stimuli could be observably increased before the closers were stimulated to action. By increasing the frequency the limiting strength of inhibiting stimuli was pushed upward. Sherrington found that strychnine, which notably increased the irritability of tissue, transformed inhibition into excitation. Apropos to this, Fröhlich remarks, "Inhibition is nothing but weak excitation."

The relaxing effect of weak stimuli is obvious in many of the human sensory fields. Moderate warmth is relaxing and enervating, while hot things and cold things may lead to vigorous reaction. Lights, contacts, smells, and tastes apparently follow the same rule. Faint persistent stimuli tend to put one to sleep, while stronger stimuli of the same order may keep one awake.

While strychnine increases the effectiveness of stimuli, changing inhibition to excitation, chloroform converts exciting into inhibiting stimuli. Bayliss reported that when electrodes were placed on the floor of the fourth ventricle, at the so-called "vasomotor center" of the rabbit, chloroform converted the usual pressor effect into a depressor one.

Direct experimental attack on the question whether refractory phase can be developed in humans without antecedent overt act seemed entirely feasible. In terms of the knee-jerk and the lid-reflex the question took the form: "Can refractoriness be produced by a stimulus which is too weak to evoke a recordable reflex, so that subsequent normally superthreshold stimuli become ineffective?" The main requirements seemed to be a series of graduated stimuli from subthreshold to normally effective, and adequate recording techniques.

Adequate recording techniques were already available for both the knee-jerk and the lid-reflex.

In a crucial experimental test of this question the knee-jerk was recorded from isometric muscle by the registration of muscle thickening. The quadriceps-proprioceptors were stimulated by two weak blows and one stronger blow on the patellar tendon. The blows were delivered by three pendulums 20 cm. in length and of appropriate weights. The aim was to make the first two stimuli at or near the threshold and the last clearly superthreshold. The pendulums all fell from the same height. By interposing a light horizontal transmitting rod between the pendulums and the tendon the force of the blows was transmitted to the tendon at the same point. The pendulum weights were respectively, 2, 2, and 3.5 ounces. Each fell thirty degrees to the vertical and the interval between stimuli was 0.3 of a second. All records of this series were preceded and succeeded at an interval well outside the known refractory phase by stimuli from the heavy pendulum. The recorded amplitude of these control reflexes ranged from 3 to 18 mm. It was never zero. As was to be expected, the weaker stimuli of the series were not always subthreshold and the reactions to the superthreshold stimulus vary within wide limits. Both were subject to reinforcements and inhibitions of which we had no adequate control. In the case of one record there was disturbing conversation in the room. This was noted on the record as the probable cause of an enormous exaggeration of the reflex. The other records were clear. When a weak stimulus evoked a response the third or stronger stimulus regularly found the reflex system in a refractory condition and the response was regularly smaller than the average excursion evoked by isolated stimuli from the same pendulum. This was entirely predictable. But even when the weak stimuli evoked no response, the response to the strong stimulus was notably decreased or entirely lacking.

Similar results were found for the lid-reflex to sound stimuli. Unfortunately, for both available subjects, no noise that our instrument could produce was so faint as not to be followed by a beginning wink more frequently than not. One series of my lid reactions are given in the following table:

TABLE I

MILLIMETERS OF LID-MOVEMENT IN RESPONSE TO SOUND STIMULI, AS RECORDED BY AN ARTIFICIAL EYELASH. SUBJECT D.

	Controls *Stronger Stimuli*	*Weaker S.*	*Stronger S.*
1	7.5		
2		3.0	0.5
3		0	0
4	2.0		
5		1.5	0.2
6		0.3	0.1
7	4.0		
8	4.0		
9		1.0	1.0
10		0.2	0.5
11		3.0	1.0
12	2.0		
13		2.0	0.5
14		3.0	1.0
15	3.5		
16		3.0	0.2
17		1.0	0.2
18	3.5		
Mean	3.78 mm.	1.64 mm.	0.47 mm.

The data in Table I are reproduced from shadow records of great precision and complete freedom from instrumental friction. As the table shows the experiment was conducted so that control experiments were interspersed in the series recording the reaction to the stronger stimulus alone. The results of these control experiments are given in the column at the left. In no case either in the control experiment or in a long series of records lasting over several years did the single stronger stimulus alone fail to evoke a measurable wink. The average amplitude of normal response in this series was 3.8 mm. When a weaker stimulus preceded the stronger, the average amplitude of reaction to the latter was reduced to less than .5 mm. The responses were sometimes barely perceptible. In one case the reaction to both stimuli was zero. This single case would be sufficient to demonstrate the possibility of inhibition by faint stimuli without previous overt reaction.

It proved difficult to produce stimuli too weak to elicit perceptible reactions without gross change in the quality of the noise. With our delicate recording techniques measurable reactions were frequently registered from surprisingly weak stimuli.

The results were essentially similar in both the knee-jerk and the lid-reflex. Both reflexes suffered marked decrement in consequence of approximately threshold stimuli. Both reflexes showed evidence of a mechanism which may on occasion inhibit all response after a preceding weak stimulation which failed to evoke an overt act. The agreement of both reflexes enormously increases the reliability of the results. Workers in the Institute of Psychology have recently obtained analogous results using the guinea pig as subject. Even in those cases where the second response was not totally inhibited, it seems possible, according to the all-or-nothing law, that neuromuscular elements which did not react to the preliminary weak stimuli participated in the refractory phase that it set up. Just where in the neuromuscular reaction system, neural reaction ceased to be an adequate stimulus for the next link in the neuromuscular chain we had no means of determining.

The experiments whose results are here reported make no pretense of finality. They do not demonstrate the validity of the refractory-phase hypothesis. That seems already to have been done. Our records add little to the mass of physiological evidence already available. They do show, however, that under certain favorable circumstances stimuli which are too faint to evoke recordable muscular contractions may on occasion completely inhibit the usual or normal response. They show that the refractory phase may produce in the intact human neuromuscular system phenomena of inhibition of a kind with which psychology is familiar. That is to say, refractory phase may produce inhibition without previous overt reaction.

The further significance of these experiments and theoretical consideration, it seems to me, is to indicate the probable importance of systematic investigation of the effects of stimuli which are too faint to evoke overt response. Somehow, somewhere, they do something which does not conform to the all-or-nothing law of conduction. As to what that something is we are quite ignorant. No one questions the importance of the summation effects. It seems probable that the refractory phase may also be found to be important in the future theory of behavior as well as in the practical control of reaction. I conjecture that this principle is involved as a condition of that training by which the effect of any naturally disturbing stimulus may be moderated by the proper graduation of preliminary experience. The firing pointer in the navy is not at first assigned to guns of large caliber. His early training is graduated to inhibit disturbing reactions. It begins with the dotter and subtarget practice. The full horrors of warfare are not presented at once to a raw recruit. The enormities of style are developed by graduations that prepare the way for the extremes and prevent mob interference which not infrequently follows sudden social innovations. The same principle may also apply to the development of a fugue and the plot of a tragedy, to the presentation of a scientific thesis, the patter of a salesman, and the evolution of a social order. I should not be surprised if it is found to be involved in the theory and practice of biological immunity.

The rôle of consciousness in human behavior has often been the subject of controversy. In addition to being a special variety of integration and a convenient substitute for overt acts in the trial and error method of meeting new situations, it may also, through imagination or memory, represent an overt reaction which is aborted by the development of inhibition. I believe that this may be an important consideration also in the psychology of punishment and the elimination of undesired responses, perhaps as important in human life as conditioning by overt reaction is in other animals.

On the sensory side a recent study of the consequences of rotary and rectilinear oscillation by Travis and Dodge showed several significant facts near the 50 per cent threshold. There was a fairly regular distribution of voluntary manual compensation responses at all intensities of stimulation which we used, below as well as above the threshold. Measured in percentages of adequacy of compensation, there was a gradual displacement of total distribution areas as stimulation decreased in intensity. They did not approach a limiting zero as the traditional theory of threshold would seem to imply, but included a greater and greater number of reactions in the wrong direction as the stimuli grew less intense. Under some circumstances these negative reactions actually predominated. The inclusion of reactions in the wrong direction with reactions in the right direction in the same regular distribution seems to indicate a sensorimotor system that is complex enough for the interplay of chance. There must also be a reversal or negative factor somewhere in the system which may become dominant when the stimuli are below the supposititious threshold. The technique was too elaborate to be included in the present report, but the experimental records are quite unambiguous. I regard them as evidence for the interplay of some inhibiting process in normal sensory systems. In this case analysis of the records seemed to indicate that the inhibiting factor was of central origin.

Traditional psychology has long concerned itself with comparisons between images of peripheral and those of central origin. It has sometimes sought differential criteria and sometimes points of similarity. The rôle of such images in control of behavior has received less consideration, though rough observations are plentiful. We are familiar with those extreme cases in which hallucinatory images become stimuli for reaction and for occasional difficulties in social adjustment, due to failure to differentiate between percepts of central and peripheral origin. In some cases the hallucinations may have an equal or even more convincing reference to reality than the true perceptions, with more or less disastrous consequences. Less tragic, but probably not less important, instances of the importance of centrally aroused factors are subject to casual observation in the guidance of behavior according to inner patterns, such as standards, ideals, purposes, and the like. Relatively few instances are on record, as far as I am aware, of the changes in behavior, measured by precise experimental methods, in any of these cases. They might be quite illuminating in the theory of behavior as well as of education.

The investigation of manual compensation to rotation furnished experimental quantitative evidence of the identity of the consequences of stimuli of internal and external origin when the external stimuli were near the threshold value. These experiments raise the question: Can perceptions evoked by near threshold stimuli ever be free of subjective reinforcing and inhibiting factors? If these subjective factors do cooperate in every perception at this level it may furnish the basis for understanding the regular distributions of reactions to subthreshold stimuli that were actually found.

Another exploratory attack on the problem of the effect of faint stimuli was made by Newhall and Dodge in the field of the visual perception of chroma. Since this investigation seems to be of special and fundamental importance it will be presented in more detail.

Our specific experimental question was whether one could induce an adaptation of the visual system, resulting in colored negative afterimages, by chromatic stimuli whose onset and increments were imperceptible. An affirmative answer to the specific question would seem to furnish additional evidence of some change in receptor systems consequent to faint stimulation, that is, not subject to the all-or-nothing law.

In preliminary experimentation we used four differently colored disks and ten subjects, some practiced and sophisticated, others unpracticed and naïve. One result was the decision to employ only the Hering red

(No. 1) for stimulation in our extended series of observations. The blue (No. 13) was rejected because the illumination from the "daylight lamp" sources reflected from our grays seemed yellowish and was subject to some confusion with yellow afterimages. The Y (No. 5) was subject to a similar difficulty and both the Y and BG (No. 10) when mixed with gray and equated in brightness with the other disks, elicited only faint afterimages in our exploration, even under most favorable conditions.

Reports during the preliminary experimentation frequently indicated negative afterimages without perception of the originating color. Concerning the possibility that the experimental conditions might tend to reinforce in some measure the bluish green afterimages through the influence of the Purkinje phenomenon or through brightness more favorable to the appearance of the afterimage than to the perception of the stimulating color, as in the study of Ferree and Rand, it seemed to us that our procedure adequately controlled the brightness conditions and in some degree the possible influence of the Purkinje phenomenon which, as far as we know, has never been proved to affect afterimages in foveal vision.

Table II shows the relative frequencies of color percepts and afterimages when the rate of introduction of the color was 0.6° per second and the rate of reduction was approximately 75° per second. Starting with a gray field the chroma was gradually introduced to 7.5° of the disk when it was reduced to zero without warning. Without pause the chroma was next raised to 15° and then again reduced to zero. This procedure was continued, as is indicated in column 1 of the following table, until the maximal chroma of 75° had been introduced and withdrawn.

TABLE II

SLOW INTRODUCTION OF CHROMA (HERING RED. NO. 1) FOLLOWED BY RAPID WITHDRAWAL, WITH A UNIFORM INCREASE IN THE MAXIMAL CHROMA PRESENTED IN SUCCESSIVE TRIALS

	Subject D		*Subject N*	
Degrees of Color Presented	*Frequency of Color Percepts*	*Frequency of Afterimages*	*Frequency of Color Percepts*	*Frequency of Afterimages*
0– 7.5	0	0	0	0
0–15	0	2	0	7
0–22.5	3	8	3	10
0–30	3	10	7	10
0–37.5	7	10	10	10
0–45	9	10	10	10
0–52.5	9	10	10	10
0–60	10	10	10	10
0–67.5	10	10	10	10
0–75	10	10	10	10
	—	—	—	—
Total	61	80	70	87
Partial total[1]	31	50	10	27

Table II is based on ten series of ten trials each, each successive trial being with a different degree of color stimulation, as indicated in the first column. Reference to the table shows that afterimages were more frequently perceived than the stimulating color from 15° of chroma up to the point where both were perceived with maximal frequency. Both the several frequencies and the totals indicate that under our experimental conditions the thresholds for afterimages were lower than for the stimulus color. Furthermore, these and subsequent data indicate that the color perception of D is less effective than that of N. D's casual experience and such color equating as was done on him in Tschering's laboratory lead him to believe that his color perception is abnormal to a slight but measurable degree.

[1] The partial totals for *D* are of the first seven figures and for *N* of the first four.

TABLE III

SLOW INTRODUCTION OF CHROMA FOLLOWED BY RAPID WITHDRAWAL, WITH A RELATIVELY LOW AND CONSTANT MAXIMAL CHROMA IN SUCCESSIVE TRIALS

	Subject D		*Subject N*	
Degrees of Color Presented	*Frequency of Color Percepts*	*Frequency of Afterimages*	*Frequency of Color Percepts*	*Frequency of Afterimages*
0–22.5	2	8	3	10
throughout	2	9	4	10
	1	8	5	10
	2	9	4	10
	0	10	2	10
	0	8	5	10
	0	10	6	10
	0	10	3	9
	0	10	2	10
	1	10	4	10
Total	8	92	38	99
0–22.5	0	8	5	9
throughout	0	9	2	9
	0	8	5	9
	0	10	3	9
	0	10	5	9
	0	10	3	9
	0	8	3	8
	0	9	6	10
	0	9	4	9
	0	10	6	9
Total	0	91	42	90

Table III shows the relative frequencies of color percepts and afterimages under conditions similar to those for Table II. The chief difference was that for Table III the same relatively small and uniform amount of color was introduced on each trial. Table IV represents conditions essentially identical with those of Table III with the exception that a larger amount of chroma was introduced into the field. Examination of these tables brings out two interesting facts: The preponderance of afterimages under the experimental conditions specified is related to the magnitude of stimulus, and the results of various series are relatively consistent and correspondingly reliable.

TABLE IV

SLOW INTRODUCTION OF CHROMA FOLLOWED BY RAPID WITHDRAWAL, WITH A RELATIVELY HIGH AND CONSTANT MAXIMAL CHROMA IN SUCCESSIVE TRIALS

	Subject D		*Subject N*	
Degrees of Color Presented	*Frequency of Color Percepts*	*Frequency of Afterimages*	*Frequency of Color Percepts*	*Frequency of Afterimages*
0–37.5	7	10	10	10
throughout	10	10	10	10
	10	9	10	10
	10	10	10	10
	10	10	10	10
	10	10	10	10
	10	10	10	10
	10	10	9	10
	10	10	9	10
	9	10	10	10
	—	—	—	—
Total	96	99	98	100
0–37.5	9	10	10	10
throughout	9	9	10	10
	7	10	10	10
	10	10	10	10
	10	10	10	10
	9	10	10	10
	9	10	10	10
	9	10	10	10
	9	10	10	10
	7	10	10	10
	—	—	—	—
Total	88	99	100	100

Table V summarizes the results for five observers following the same procedure as that for Tables III and IV. The left half of the table gives the figures for the lesser stimulus and the right half for the greater. The columns headed "A.I./C.P." give the ratios of frequencies of afterimages to color percepts. Thus, when the amount of color introduced was 22.5°, subject D reported negative afterimages 22.9 times as often as perception of the stimulating color; but when the stimulus was raised to 37.5, this subject reported afterimages only 1.1 times as often as stimulus color. It will be noted that all observers reported afterimages more frequently than stimulus color when the lesser value of stimulus was employed. It seems reasonable to suppose that if the stimulus were adjusted with respect for individual limens even more striking results could be obtained.

TABLE V

SLOW INTRODUCTION OF CHROMA FOLLOWED BY RAPID WITHDRAWAL, WITH CONSTANT CHROMATIC MAXIMA IN SUCCESSIVE TRIALS

		0–22.5 Degrees			*0–37.5 Degrees*		
Subjects	*Instances*	*Frequency of Color Percepts*	*Frequency of After-images*	*A.I./C.P.*	*Frequency of Color Percepts*	*Frequency of After-images*	*A.I./C.P.*
D	200	8	183	22.9	184	198	1.1
N	200	80	189	2.4	198	200	1.0
B	100	83	100	1.2	96	100	1.0
H	100	36	99	2.8	89	97	1.1
S	100	10	88	8.8	89	96	1.1

Again the facts are unambiguous. Slow increases of chroma, up to a certain level, inhibited the perception of color, but permitted the perception of afterimages when it was withdrawn rapidly. Analogous results occurred when chroma was slowly withdrawn. Since change in external vital conditions is the real stimulus, slow change is equivalent to weak stimulation.

All these results at reflex, sensorimotor, and perceptual levels seem to agree in showing the inhibiting consequences of weak stimulation in intact humans. All seem to agree in support of the physiological presumption that faint stimuli are in the main inhibiting rather than stimulating in human reactions, and that inhibition may be produced without preliminary reactions.

We have, of course, only scratched the surface. The rôle of faint stimuli in psychophysiological and possibly in other biological processes deserves and will, in the future, undoubtedly receive more adequate investigation.

CHAPTER VII

THE OCCASIONAL DEVELOPMENT OF BEHAVIOR IN SIMPLE PATTERNS

THE simplest behavior pattern of which I have recorded the development is the voluntary rapid oscillation of fingers. It may be described roughly as the alternate contractions of the flexor and extensor muscles of the fingers with reciprocal relaxation of their antagonists. Accurate muscle records show that this pattern is not quite so simple as this description would seem to imply. It is complicated by persistent tonic contraction of both antagonistic muscle groups and by a terminal contraction of the antagonistic muscle at the end of each phase of motion.

The precise statement of this action formula does not concern us in the present discussion. The main point is that this particular pattern of relaxations and contractions does not spring into existence immediately in successive instances of this kind of behavior. In each new instance the pattern goes through a period of development, which begins with cocontraction of both antagonistic muscles, and passes into a relatively stable system of differential contractions and reciprocal inhibitions in a kind of rapid learning or warming-up process.

In the muscle records of Dodge and Bott, from various muscle groups, cocontraction of antagonistic muscles regularly appeared at the onset of either voluntary extension or flexion of a relaxed limb. Reciprocal relaxation appeared only when the antagonistic muscle was caught in a condition of contraction at the moment of new innervation of a prime mover. This rule held in voluntary rapid oscillations of a finger. Starting with cocontraction, the first phase of oscillation was produced by the preponderant pull of one of the cocontracting antagonistics. In all the later phases the pull of the agonist operated against the tension of a partially contracted antagonistic muscle, which suffered only partial reciprocal relaxation. The series of events is more or less clearly perceptible by palpation. Our muscle records show it to be a regular phenomenon of rapid oscillation of all the mobile members which we investigated.

In rapid oscillation, neither group of antagonists completely relaxed between phases, but if the oscillation was sufficiently slow, relaxation of both antagonists was commonly completed before each new contraction began. Under these latter circumstances each oscillation episode starts with cocontraction, as though it were the first of a series. If, instead of allowing it to take a natural course, the cocontraction was voluntarily prolonged throughout slow oscillation, the antagonist suffered reciprocal relaxation as each new phase of movement began. Different from either of these circumstances the voluntary contraction of either one of the antagonistic muscle groups may begin while its antagonist is in a state of protracted contraction. This happens, for example, in suddenly withdrawing the foot after protracted pressure of the limb against a stop. Such cases followed the rule of reciprocal inhibition. Whatever the origin of the overt act, relaxed antagonistic muscles cocontracted; only contracted antagonistic muscle regularly relaxed.

In connection with our main problem the development of a relatively persistent general pattern in each new episode of voluntary rapid oscillation of a finger is especially instructive. Regular reciprocal relaxation appeared first at the end of the first phase of oscillation. Consequently, the first few phases of oscillation seem confused and irregular. A persistent general pattern of thickening and thinning commonly began with the third oscillation and lasted as long as rapid oscillation continued. This pattern is not simple. In our records both extensor and flexor thickening curves are dicrotic and relatively long compared to the thinning curves. Each relaxed during the period of thickening of the other, but at different moments of that thickening. The reciprocal thinning of the extensor began during the first part of the flexor thickening curve; the thinning of the flexor, during the last half of the dicrotic wave of the extensor thickening. All this seemed to be superposed on still shorter waves and gave an arrhythmic appearance to the curves. Within the general persistent pattern there appear many variations in the temporal incidence and amplitude of the several component waves.

Slowly alternating maximum extension and flexion gave records of somewhat different appearance, but began with the same initial cocontraction which characterized the records of rapid oscillation. Starting from relaxation, the muscle curves of cocontraction, like the analogous curves in rapid oscillation, proceed in waves. These waves were apparently simultaneous in both antagonists. They cannot be explained by elastic oscillations of the recording systems, since the latter were not identical either in weight or in pressure against the respective muscles. Furthermore, these waves continued into subsequent contraction, when mechanically conditioned elastic oscillations must long since have died out. They were too short for pulse waves and lack the characteristics of known proprioceptive reflexes. Their frequency, simultaneity, and persistence suggest that they are related in some way to the neural innervation waves, but this hypothesis needs experimental proof. If it is true, it would indicate a common central origin of the cocontraction impulses, beginning at the same moment and proceeding in waves which at first, at least, are practically simultaneous. If such a spread of impulses from a common center to both antagonistic muscles is demonstrated, it will appear almost a matter of course that the contraction-relaxation rhythm should be profoundly affected when innervation waves of different origin and frequency impinge on a part of the final common path which is already stimulated in response to innervation waves of a given frequency. Something like this seems in fact to be indicated in our records of both slow and rapid oscillation subsequent to the initial cocontraction. The primary cocontraction of an antagonistic flexor is exactly coincident with the onset of the contraction of the extensor. Relaxation of the antagonistic in subsequent phases is, however, seldom strictly simultaneous with the contraction of the agonist, but may be delayed as much as .06″. Some records show that the antagonistic relaxation may even precede thickening of the prime mover. While these facts make a strong argument against any mechanical origin of these waves, they are exactly what one might expect if the inhibition of the antagonist is a product of converging impulses of different frequencies that develop a refractory-phase inhibition. Even the irregularity in time of origin corresponds to this theory, but the theory is too important to rest on the available records. For the present we would limit our conclusions to the suggestive phenomenon that cocontraction of antagonists is common when both muscles are previously relaxed, while relaxation is the rule when either one of the antagonistic pair is in sustained or residual contraction at the moment of dominant contraction of the other. The schema seems to fit many psychological phenomena of attention and recall, but experimental evidence is fragmentary.

The development of an adequate ocular pursuit pattern. Analogous to rapid finger oscillation, records of the ocular pursuit of an oscillating object present a very irregular appearance in the first phase with subsequent development of a general pattern. After the object began to move the recording eye stood approximately still for a time, corresponding to a latency of the order of 0.2″. Then it made a corrective jump in the direction of the objective movement, which passed into a pursuit glide. The corrective jump is an eye-movement of the saccadic type such as commonly corrects lapsed fixation, or the new fixation of a peripheral object. The glide is the first indication of true pursuit. The slope of the first glide does not correspond to the contemporary velocity of the object at the moment of response, but to an earlier and lesser velocity. Both the saccadic movement and the first pursuit glide show the lapse of a latent time between the movement of the object and the response of the eye. This contrasts sharply with later phases of pursuit, where latent time seems to disappear.

Some of our records show an important variant of the general picture of the first phase of pursuit. Possibly due to some more or less clear anticipation or set of which the record is the only indicator, the eye record may show a preliminary slow glide before the first saccadic refixation. In one record the preliminary glide is almost simultaneous with the beginning of objective motion in the other direction. It is obviously an anticipatory false reaction. This is followed by an approximately normal rapid or saccadic movement in the right direction which passes into a particularly inadequate pursuit glide and a second long saccadic movement to the extreme point of oscillation. The unique feature of this record is the false anticipatory glide. If the start has been made in the right direction, this initial glide might have been read by the uninitiated as a true reaction with an extraordinarily short latency. Such records should make us very conservative in measuring latencies from initial glides. They also emphasize the general formula of the first phase with its saccadic correction of lapsed fixation and its ineffectual glides, both after relatively long reaction times. To a less degree they indicate complications introduced by "set."

The irregular beginning reaction changes notably in the second phase of oscillation. In normal subjects, whenever the objective movement is not too rapid, all phases after the first show a much closer approximation of the ocular pursuit to the actual motion of the object, while latency is apparently decreased or eliminated. Occasional corrective movements, however, are found in every phase of pursuit. Eye-movement records of eight normal subjects pursuing a small visual object in harmonic oscillation indicate that 100 per cent accuracy of pursuit will never be reached at any oscillation frequency.

The theoretical implications of our records are clear enough. Like the oscillation of a finger, even such a simple and practiced act as the ocular pursuit of an object in harmonic oscillation does not start perfectly formed in successive instances, but shows a temporal development of approximation and correction in each case. As the adjustment in each new instance becomes more and more nearly adequate there is a consequent increase of true pursuit up to a limit of approximation which differs with each frequency of oscillation. Corrective saccadic movements, however, follow a quite different course. They are most extensive at the beginning of adjustment, and in later phases at moderately high frequencies. They are especially long near the threshold of the break after the true pursuit becomes notably inadequate, but disappear almost entirely before inadequate pursuit glides disappear.

Approximative and corrective eye-movements are not peculiar to ocular pursuit. They have been found in various photographic records of eye-movements. As was pointed out first by Dearborn and then by various others, objects which are first seen peripherally are commonly fixated in from two to three saccadic jumps. The first movement in the direction of the peripheral object is a more or less gross approximation which is corrected by a second and sometimes even by a third readjustment. Even the fixation of a motionless object is never maintained absolutely accurately. Photographic records of fixation by Dodge show not immobility but a more or less close approximation to fixation with corrective refixation movements at irregular intervals.

Much overt human behavior and human thinking seem to follow a pattern in which a rough approximation is followed by a phase of correction, which in turn may be only another approximation. I do not know how general this pattern is, but I conjecture that it is a common biological phenomenon and an almost universal way of meeting situations after the first crude trial and selection gives the general direction of response. Our practical adjustments often follow this same formula, from driving an automobile to hanging a picture, from lining up a lens system to scientific writing. As I understand it, even science progresses according to this formula, whether it deals with astral physics or atomic weights. In such complicated cases, however, the actual procedure may be exceedingly difficult to record and analyze. The main point is not an effort to extend the formula, but to show that a formula which appears in so much of our thinking and behavior appears also in the development of such simple acts as the eye-movements of pursuit and fixation where it may be adequately recorded and studied.

The breakdown of adequate ocular pursuit adjustment. When the objective movement evoking ocular pursuit became too rapid, the amplitude of pure pursuit decreased, while the saccadic corrective movements became longer and more numerous. As the oscillation accelerated still further, both the corrective movements and the pursuit tended to disappear and to be replaced by approximately still fixation. Where each double oscillation takes about 0.4″, photographic records show that the pure pursuit movements are relatively short and the corrective movements long. After a few pursuit oscillations at this speed, the pattern broke down, the long corrective movements disappeared and approximately still fixation supervened. In addition to indicating the limits of adequate pursuit, such records give some important information concerning the breakdown of an adequate adjustment pattern as the objective situation becomes too difficult.

Normal and abnormal psychology seem to furnish innumerable instances of analogous breaks and the substitution of other and often simpler reaction patterns. When situations entirely preclude adequate adjustment, whether in our day's work, in social contacts, or in reactions to economic pressure, inadequate behavior patterns do not persist indefinitely. In response to some unknown neural mechanism, possibly due to relative fatigue, possibly to the law of effect or to some other condition of "retroflex" action, the inadequate pattern is abandoned, while another and in many cases a simpler reaction pattern more or less analogous to still fixation of the eyes takes its place. Under such circumstances the human reactor may rest, take a vacation, leave the room, arrange for marital separation, go into bankruptcy, or take refuge in the world of fantasy. Certainly a great many human breakdowns seem to follow the general formula that is found in the relatively simple pursuit reactions of the eyes when the objective movement of the object of regard becomes too complicated.

Our second point is that in the breakdown of adjustive pursuit eye-movements we find an analogy to a wide variety of breakdowns of human adjustment. Breakdown may be regarded as an extreme form of variability.

Anticipatory reaction as a form of learning. Still another feature of our eye-movement records deserves consideration in connection with our main problem. As we have already noted, the development of approximately adequate ocular adjustment to oscillating objects involves the reduction or elimination of reaction latency. After the first phase of pursuit the slowing down and speeding up of the eye-movements does not wait for the slowing down and speeding up of the object with an intervening latent time. In many records the eyes actually start their return sweeps coincidentally with the object, or even before it. In terms of traditional psychology this constitutes what we commonly call an anticipatory reaction. The facts may be seen clearly in most of our photographic records of ocular pursuit of oscillating objects. After the first phase of oscillation, the changes in the direction of eye-movement are either coincident with or antecedent to changes in the direction of the movement of the object. The disappearance of latency is doubtless only apparent. It would be the least plausible of the various hypotheses which we have considered to assume a short-circuiting or automatizing process by which the latency was suddenly reduced to the point of disappearance. The crucial evidence against such a hypothesis would be two aspects of our records, one of which we have already noted, namely, that the reaction may antedate the original stimulus and the eye may turn before the object reaches the extreme point of its swing. This seems to preclude a short-circuiting process. The second evidence against this hypothesis is that when the object comes to a sudden stop, eye-movements may continue for an appreciable time.

Two somewhat more plausible hypotheses have been considered to account for the apparent loss of latency. It might be explained as a case of anticipatory reaction in which eye-movement followed cues which were associated with the original stimulus. In that case one would regard the pursuit eye-movements as a kind of conditioned response in which a given velocity of the object evokes a very different velocity of eye-movement.

The first argument against this hypothesis is the extreme complexity of the reaction system which it would involve, together with the rapidity with which each case of pursuit becomes adequate. The second argument against regarding adequate pursuit movements as a chain of conditioned reactions is that any specific angular velocity of the object occurs twice in each phase of oscillation, once in acceleration and once in deceleration. A conditioned eye-movement evoked by any particular speed of the object would consequently depend on some clue furnished by the general configuration of the oscillation before it could respond in smooth pursuit to the two different meanings of any angular velocity of the object. That is to say, no speed clue would be adequate to determine a conditioned pursuit reaction, independent of its setting in the oscillation pattern.

This naturally leads us to another hypothesis, namely, that the objective motion is apprehended as a unitary configuration of accelerations to which the eye reacts in a unitary configuration of response. This hypothesis lays especial emphasis on the approximately smooth pursuit movements of the eyes of a normal subject after the first phase. Such smooth pursuit seems irreconcilable with a chain of conditioned reactions. Its peculiar sine-wave character suggests some kind of integration into a unitary response pattern. The sensory conditions for that integration would seem to be given in the character of the stimulus situation as apprehended during the first half-oscillation. At any rate, the difference between the first and successive phases of pursuit forced us to regard the latter as the product of a learning process.

Further evidence in support of this hypothesis is furnished by regularity of some of the less adequate pursuit patterns and especially by the character of the post-excitation pattern after the sudden cessation of objective motion.

That the integration of response is only partially adequate, however, is indicated by the small corrective movements that occur even in the smoothest pursuit. These are of various amplitudes, sometimes positive and sometimes negative in direction. They are best understood as specific reactions to lapsed fixation.

Moreover, pursuit may falter at eye-movement speeds well below that of the saccadic corrections, and be followed by occasional moments of adequate pursuit. These facts would seem to be inconsistent with the closed integration of behavior patterns.

It is conceivable that the process is not so simple as any of the preceding hypotheses regard it and that the corrections represent specific reactions to lapsed fixations which are superposed on a more or less adequately integrated pattern. Certainly the second and third hypotheses are not mutually exclusive. At present the easiest reconstruction to account for the recorded facts of ocular pursuit seems to be the operation of specific sensory cues superposed on total situation patterns. This falls in line with various casual and experimental observations of the interrelation between configuration and detail, as was shown in the psychology of reading.

Whether we regard an ocular pursuit as a chain of conditioned responses, an integrated response to a configurated situation, or a combination of both processes, it remains clear that we have to do with the occasional development of anticipatory reaction, which is represented by analogies in many everyday responses. Intelligent persons do not wait for events to occur before they adjust to them, whether they are collisions with approaching automobiles, economic issues, social functions, political responsibilities, old age, or death. The systematic sequences leading to all these events often furnish preliminary cues for anticipatory reactions. Here again we would not generalize from the eye-movement. The main point is that the reactions of ocular pursuit involve anticipatory adaptive responses which may be accurately recorded and analyzed and that these responses seem to be analogous to a great variety of other learned adjustments which it is difficult or impossible to study with the same precision.

As has appeared in this discussion, the important generalization is not from simple to complex behavior, not even that simple formulas which are found in the eye-movements apply to a multitude of miscellaneous adjustments, but rather that formulas of apparently widespread application to human behavior can be studied and recorded in such relatively simple phenomena as the movements of the eyes.

Our muscle records of rapid finger oscillation and photographic records of eye-movements in pursuit of an oscillating object both clearly demonstrate that even relatively simple systematized overt acts do not spring into existence immediately as adequate adjustment patterns. Both show a preliminary warming-up of coördinated action which we can understand only as the rapid development of adjustment habits—an elementary learning process. Both emphasize the nature of adjustment as a systematizing process of approximation and correction. Both show the interaction of various neuromuscular systematizations, in various permutations, each influenced by refractory phase, inhibition, and relative fatigue in the development of even the simplest overt behavior.

CHAPTER VIII

THE COMPLICATION OF REACTION BY THE INTERACTION OF NEURAL STRATA

SOME time ago I witnessed the illuminating performance of a military drama in which a famous actor played the rôle of an outraged father. After denouncing a superior officer who had seduced his daughter, he suddenly drew a revolver and pointed it at the villain. Before he could pull the trigger, the consciousness of cast and military training took control of his behavior, and the unfired revolver fell to his side. The impulsive response to the relatively circumscribed stimulus-situation of outraged fatherhood was inhibited by a reaction to a wider stimulus-situation involving long training of responses to military cast.

The acting was good, the conflict was realistically portrayed, and the sequence of impulses and their control were clearly apparent. Meantime, the seducer stood like a wooden image—the unfired revolver was an everyday occurrence in his experience. All primitive reactions to a possible threat in the situation were long since outlived, if they ever existed. His attitude plainly said to the audience, "Don't be alarmed, old dears, the man won't shoot, the threat is part of the play. I have been through it before and my murder is not in the book." I saw it as a moment of complete indifference, not one of the triumph of military training and bravery over self-protective instinct. The father's part was taken by a great actor, who had doubtless studied human conflicts and their portrayal, but the illusion he created was almost destroyed for me by the indifference of the supporting actor in what should have been a moment of tragic strength.

It is not difficult to detect an important psychological problem in the difference between these two types of acting. In the given instance the better actor presumptively followed a real pattern of adjustment to situations as they develop, while the other did not. In its widest formulation that problem involves the nature of interaction between different strata of neural integration. The complete solution of the problem would involve a complete system of dynamic psychology which is far beyond our present experimental knowledge.

We are primarily concerned in this discussion only with a much more narrowly defined question which may or may not be a prolegomenon to wider interpretations, i.e., what is the nature of that interaction between the protopractic responses which we call reflexes, and the higher systematizations which involve more highly elaborated data?

The instance of good action which I have described, adequately portrayed an inner conflict and competition of discrete and intelligible systems for the domination of behavior. There is relatively little mystery in that. The instance of failure is less obvious and more instructive. What is the difference between a convincing portrayal of triumphant bravery and acting that clearly says, "You and I know the revolver will not be discharged"? This question comes very close to our problem.

The main points of this chapter may be reduced to three, as follows:

(1) The traditional classification of human responses as reflex, instinctive, habitual, and voluntary, as though they were invariant units which could be

referred to discrete levels of the nervous system, involves a simplification fallacy of far-reaching influence on the understanding of actual human behavior.

(2) Our thesis, on the contrary, is that many, if not all, adaptive acts of the adult human are really complicated responses determined at several levels of integration. They commonly begin as a relatively crude approximate adjustment of short latency which may be called protopractic, and develop into more adequate adjustments which may be called epicritic, varying according to the equipment of the individual. Unfortunately, these protopractic and epicritic stages are often indistinguishable except in experimental situations.

(3) Our third point is more problematic and is presented as a working hypothesis. In our use of the terms, protopractic and epicritic are not new names for specific units of behavior, but are purely relative terms referring to greater or lesser elaboration of the sensory data. While our discussion is based on the rôle of reflex protopractic responses in total human adjustment, a considerable variety of beginning-responses may be regarded as protopractic in contrast to those more adequate phases of adjustment that occur after maturer elaboration. In this sense instinctive and habitual protopractic reactions correspond schematically to the reflex phase as crude adjustments of short latency, which commonly pass into finer or epicritic adjustments to differentially elaborated data concerning the total environmental and purposive situation. Moral values and even science itself represent still higher strata of epicritic elaboration. In behavior, which is conditioned by these, habit and impulse would be protopractic.

The doctrine of a stratification of neural systematization belongs to the relatively stable traditions of neurology. It is commonly held that each segment of the spinal cord contains the central neural mechanisms for corresponding segmental reflexes, involving not only the motor cells of the final common path to the muscles and the peripheral synapses of the afferent projection, but also at least one systematizing link, i.e., the intermediate neurones.

This segmental stratification is roughly shown by the retained reflexes of the experimental hind-, mid-, and fore-animal, and even more delicately by reflexes of experimentally isolated segments. Experimentally usable human analogies of segmental reflexes are such responses as the knee-jerk, lid-reflex, and vestibular reflexes of the eyes. Since, however, the rest of the nervous system cannot be eliminated in normal human subjects, these reflexes are commonly complicated by the action of higher neural levels, so that simple reflexes entirely free from complication of intersegmental influence can seldom or never be demonstrated in intact humans.

In addition to its peculiar functions as a neural center, each segment is traditionally held to be connected with many others as a link in the neural mechanism for the spread of neural excitation. For example, afferent impulses reach the various association areas of the cortex while impulses from the various segmental levels coöperate in or compete for the control of the final common path. Corresponding to the intersegmental connections of the neural strata it is customary to differentiate another kind of phylogenetic stratification of neural structure in which the autonomic system, together with the cord and brain stem represent primitive or older systems, while more recent structures, such as the neo-pallium, represent superposed additions to the primitive neural equipment. This kind of stratification corresponds roughly to the doctrine of neural levels, which is so frequently found in contemporary psychologies.

While the segmental stratification provides for the inheritance of discrete response mechanisms, connections between several neural levels provide for the integration of segmental responses into more complex reaction systems, reaching a supposititious upper limit in the reactions which are conditioned by the total experience of the individual.

In addition to the various phylogenetic stratifications, there is evidence in the tradition for something like an ontogenetic stratification in human development, depending on the first appearance of specific responses in the life history of the individual. This ontogenetic development seems to be conditioned in the first instance by the successive development of various parts of the embryonic nervous system. Anatomical stratification of postnatal neural development, if it exists, is relatively obscure and complicated and can at present only be regarded as a more or less plausible hypothesis resting on certain characteristics of overt behavior and experience. For example, there seems to be some evidence in the psychiatric tradition for the ontogenetic fundamentality of certain early experiences which may color the whole later life of the individual. Such influences may originate in early infancy and perhaps even in prenatal experience. In the latter case they seem to join with phylogenetic systems in one continuous series. More complicated strata seem to originate later in the life history of the individual and to show themselves as conditioned behavior, set, habits, behavior patterns, memory, apperceptive systems, and purpose.

For systematic purposes one may then distinguish two forms of preindividual or phylogenetic stratification, i.e., the segmental and the intersegmental of the various neural levels. One may also distinguish various more or less problematic levels of individual or ontogenetic stratification of behavior and experience, such as prenatal, infantile, adolescent, and sophisticated, reaching a relatively high form in scientific thought on the side of experience and purposive reaction on the side of behavior.

On the basis of comparative and genetic analyses of behavior, it is customary in psychology to distinguish typical forms of behavior which depend on these various phylogenetic and ontogenetic strata. Reflexes are supposed to originate at the level of the cord and brain stem, instinct in the basal ganglia and brain stem, and experimentally conditioned or purposive behavior in the cortex. Our psychologies seem to be widely committed to such a hypothetical stratification of behavior corresponding with the various levels of neural systematization.

Along with the doctrine of the stratification of the neural systems and behavior, there exists also in our psychophysiological tradition evidence for a more or less extensive interplay of neural action between different levels even before birth, according to Coghill, so that a complete human response to any situation may be the consequence of widespread intersegmental excitation. Under these circumstances the overt response might be properly regarded as due to the interaction of various neural strata.

Traditionally the interrelationship between the segmental strata in the central nervous system is that between a group of higher controlling systems and a more primitive group of systems whose action the former inhibit or reinforce. The final common path is known to be constantly subjected to impulses from various neural levels, so that any response originating at a reflex level is superposed on a complex dynamic background of prereflex conditions. There is experimental evidence that this generalization, which was primarily found to hold true in animal experimentation, also holds true for man. Our simplest reflex acts are seldom or never simple muscle twitches of uniform duration and extent. Each stimulus to a reflex operates on a neural center that is already more or less excited from the interaction of various levels. As it gains control of the final common path, the reflex stimulus never finds the system on which it acts exactly alike except by accident. For example, we know experimentally that a voluntary deadening or enlivening of the knee-jerk may produce, respectively, depressed or exaggerated reflexes. The mechanism of such control of the lower strata by the higher is still a matter of more or less plausible hypotheses whose experimental investigation offers many difficult technical problems.

In the second place, the effect of any reflex stimulus may be prolonged through the action of intersegmental "delay-paths" into a more or less protracted contraction or relaxation of the reacting muscle. Such response would be illustrated by the voluntary extension of the leg at the knee in reaction to the same stimulus that evokes a knee-jerk. The overt act in such a case originated as a reflex which was superposed on a "set," and developed into voluntary behavior.

Presumptively, stratification of behavior is not an exclusive property of segments or levels. Within the superior neural organ it seems to take the form of a differentiation between systems of higher and lower excitability on the one hand and of physiological dominance on the other. In any concrete afferent system the proximate cortical stratum seems to be the projection area. From that focus the spread of cortical excitation reaches widely separated areas which are related to the projection area in terms of greater or less intimate systematic connection. This also may be thought of as a kind of stratification. Between these functional strata the processes of reinforcement and inhibition probably remain determinative and, in conjunction with the original stimulation from coöperating or competing afferent systems, probably control the extent and direction of the spread of cortical impulses in any given neural reaction.

Of these various forms of stratification only the segmental stratification has received adequate experimental verification. At the level of overt behavior it is still difficult to disentangle the intersegmental effects of the several levels and strata. We consequently still speak loosely of reflex, instinctive, and habitual acts as though the corresponding levels of neural integration were tight compartments.

Unfortunately, comparative and genetic analyses are not always applicable to overt adult behavior. Analogy tends to emphasize the segmental differences. Direct experimental analysis of the interaction of various strata in complicated overt adult behavior is, I believe, a desideratum. There seems to be at least one promising but as yet relatively unexploited method for such analysis—that is, the temporal separation of different phases of muscle contraction.

As far as I know, there are only two groups of published records of temporarily stratified human responses when cortical reaction was superposed on a reflex act of the same mobile member. One group was contained in my *Theories of Inhibition,* Part I. These records show lid-reflexes to sound with superposed voluntary closure of the lids. The matter seemed so important that a considerable number of similar records were taken subsequently from several subjects. All show the same characteristic dicrotism and discontinuous action-pattern. The first reaction consists of a reflex partial closure of the lid with a latency of 35–40σ, followed by a partial recovery, and then by another lid closure of cortical origin with a latency of from 90–150σ or more.

Differentiation of the temporal stratification of reaction is relatively difficult or impossible to observe and record in the movements of heavy limbs where the various phases of muscle contraction tend to fuse to a smooth line in the record, due to the physical characteristics of the motion of heavy bodies. They are more easily shown, however, in the records of corresponding muscle thickening. Similar dicrotism has been reported in analogous experiments with the knee-jerk. In this case the records were made by the thickening of the quadriceps.

A group of records showing the temporal sequence of reflex and cortical control of the final common path to the external eye muscles was published in *Adequacy of Reflex Compensatory Eye-Movements Including the Effects of Neural Rivalry and Competition.* These records not only show the temporal primacy of the reflex movements of the eye and the delay of antagonistic cortical control, but they also differentiate the manner by which a protopractic phase passes into an epicritic, and reasserts itself when the visual data cease on which the epicritic phase rests.

Another quite different set of records for the analysis of the behavior flux by the temporal sequence of protopractic and epicritic muscle phases resulted from an unpublished investigation of responses to pain under the patronage of the Committee on Migration of the National Research Council. The general experimental setting was as follows. The subject was stimulated by sine-wave faradic currents of various more or less painful intensities. Reactions of the quadriceps muscles were recorded by a sensitive photographic system whose special construction need not at present concern us. The essential feature is that a photographic recording mechanism rested on the quadriceps muscles of both thighs and operated in such a manner that equal simultaneous thickness changes in both muscles neutralized each other. This automatically eliminated most of the effects of circulation and respiration on muscle thickness and recorded small differences in relative tonicity of the two quadriceps. A telegraphic key in the hand of the subject provided him with the means of discontinuing the painful stimulation at any moment.

These and related experiments provided a considerable amount of data concerning the effect of pain on the tonicity of muscle, both neighboring and remote, and on the extent and latency of the reflexes which were simultaneously evoked, with certain probable implications as to the effect of disagreeable experience on migration. Only a small fraction of the data, however, is relevant to the present discussion. With a suitable anticipatory set on the part of the subject, quite painful stimuli could be endured for some time before the pain-producing electric circuit was voluntarily broken. Slight stimulation produced little or no effect on muscle thickening and was endured indefinitely with relative quietude. As the intensities increased, the first recorded effects were certain characteristic changes in respiration and more or less widespread uncoördinated waves of muscle contraction with secondary superposed waves of shorter period. Whenever we found any effect at all on muscle tonus, it was always first in the direction of muscle thickening—that is to say, of muscle contraction. The effect on the reflexes was more complicated, depending on the temporal incidences of the respective stimuli. Even intensities evoking widespread waves of muscle thickening and reflex reinforcement could be endured for some time before the subject relieved the painful situation voluntarily by releasing the telegraph key. At high intensities of stimulation the practicable endurance was very short, while the antecedent muscle contractions were more sudden and of greater amplitude.

Two aspects of the experimental results are of especial importance in the present discussion. As a result of a preëxperimental set to endure the painful stimuli quietly, there was a decrease of involuntary and crudely adaptive writhing, and a delay of the more highly adaptive release of the telegraphic key. There was always a conspicuous temporal separation between the involuntary writhing and the more adaptive behavior which discontinued the painful stimulation.

Other records showing the sequence of protopractic and epicritic phases in cortically initiated behavior are contained in the paper on the *Antagonistic Muscle Action in Voluntary Flexion and Extension.* These records, which have already been described in another connection, show how a protopractic phase of cocontraction precedes and passes into the regular rhythmic series of muscle contractions and reactions involved in the voluntary oscillation of a mobile member.

I believe that with proper techniques such sequences of adjustment will prove to be very common, if not universal phenomena. First comes an epicritic set for or against the reflex, or other protopractic response, modifying the resultant latency and amplitude of relevant reflex muscle contraction. Then, with increasing sensitivity to stimulation or some inner reinforcement, the protopractic response may be continued into a more or less complex phase of more adaptive epicritic adjustment.

While experimental data are still inadequate, I conjecture that much the same scheme may be found in so-called habitual acts, and that there are, in adult human behavior at least, no pure habitual responses uncontaminated by reaction set and the epicritic elaboration of the sensory data. The degree of that elaboration is limited only by the intellectual resources of the organism and the time that is available before the response to one change in situation passes into response to another.

Our picture of human adjustment is not a mosaic of reinforcing or conflicting reflexes, instincts, habits, and voluntary acts or a succession of discrete responses under these various categories, but a dynamic continuum, a sort of spiral process with a relatively simple front of overt reaction at any given moment and a highly complex background. Adequate experimental analysis would probably show that each overt reaction is really a complex of approximate beginning reactions and elaborated adjustments. The beginning reactions are evoked by current stimuli superposed on the remains of consummated responses to past stimuli by which they are inhibited, reinforced, or qualitatively modified. Each protopractic phase as it emerges is modified by the effects of more or less elaborated sense data from the same situation-stimulus, which in turn may modify the next protopractic phase of response.

Our common experience is full of incidents in which the reaction passes from protopractic to epicritic adjustment. When an automobile horn sounds close behind one at a street crossing, the protopractic start-reflex passes into successive stages of epicritic adjustment according to the elaboration of the sensory data. It may be quite different according to the visibility or location of the automobile, the congestion of traffic, and the set of the victim. Elaborated response commonly begins by turning the eyes for more data in the estimation of relative distances. The later epicritic response may be any or all of a large number of reactions within the repertoire of the individual. He may stand still and raise his hand, run to one side or the other, according to traffic conditions and the estimated speed of the different vehicles, or the distances of the sidewalk and safety zones. He may even take the number of the automobile, if conditions warrant it, and call for witnesses of violation of traffic regulations with a view to reporting the driver. The delayed and elaborated responses are epicritic. The choice of reaction depends on set, habit, and purpose, as well as on circumstances.

The more or less widespread muscle contractions of the protopractic start-reflex may also be a common initial phase in all kinds of thwarted, nonadaptive, and maladaptive behavior, as well as for useful epicritic adjustments according to the violence of reaction, the configuration of the inhibiting and reinforcing stimuli, and the individual's capacity to organize them. If the epicritic adaptive adjustments are inhibited and the protopractic responses intensified or reinforced, muscle cocontraction phase may pass into the complete paralysis of fright, into the rhythmic automatism of precipitate flight, vituperation, or whatever corresponds to the personal equation of the victim.

Cases when a protopractic *habit* fails to produce adequate adjustment are numberless. One starts to dress for some social function and suddenly must inhibit the preparation to retire. In the adaptation of speech and manners to a changed environment, any sudden emergency may evoke long-standing or habitual protopractic responses contrary to intent and expose the normal social *milieu* and habits of the reactor before the fitting or intended response can become operative. The old soldier who according to long psychological tradition disclosed his military training by dropping his bundles at an unexpected command to "attention"; the parvenu who lapses into habitual mannerism as he tries to ape the manners and speech of a new culture; the tendency to lift one's hat by the brim after wearing a straw; colloquialisms, slang, or grammar lapses; reaching for one's pipe in spite of New Year's resolutions; writing in familiar patterns in spite of intention to write better, or to disguise one's chirography, all illustrate the outcrops of protopractic response. Subsequent epicritic correction of these outcrops often serves only to call attention to the lapses.

I venture the working hypothesis that most normal human overt adjustments to environmental changes include both protopractic and epicritic factors. The hypothesis of stratified total response is directly opposed to the doctrine that human instinctive acts are merely the product of inherited psychophysiological patterns. In adult humans, at least, there seem to be no instinctive patterns at all in the sense of uniformities of configuration of response. The primitive phylogenetic phase of an instinctively initiated act seems to be only an approximate or protopractic beginning, so that instinctively initiated response commonly starts in phylogenetically predetermined directions, but the development and completion of such acts is an ontogenetic variable dependent on habit, experience, conditioning, an understanding of the circumstances, and intent.

Similarly, our hypothesis denies necessarily mutually exclusive categories of instinctive and rational human reactions. Probably most rational reactions have protopractic beginnings, but no adult human reaction, except in extreme emotional inhibition, is completed on the primitive level on which it starts.

Certainly it is not wise scientific terminology to name an act according either to its beginning or its terminal phase. For example, to classify any cross section of the behavior flux, such as eating, as reflex, instinctive behavior, or habitual action, is misleading. In the ingestion of food there is an instinctive phase, but there are also several reflexes and a great number of habitual and occasionally deliberative phases with clear purposive sets. The same analysis would be true of sex behavior, of combat, submission, and fright; in short, of practically all human behavior with instinctive protopractic beginnings. Just as an adequate particularizing description of behavior would involve a qualitative and perhaps a quantitative statement of the sequence and interplay of these various phases, so adequate names for the behavior should not emphasize unduly any one of the several factors, but should somehow indicate their range and possibly their order. Thus a concrete case of fright beginning with a start-reflex and ending in complete intellectual paralysis clearly differs from another case which began without start in a gradually dawning consciousness of the frightfulness of the situation. The former might be called a reflex-emotional response, while the latter might be an inference-emotional-partially-adaptive act. They are not merely qualitatively different degrees of fright. Specific cases may differ in origin and sophistication, as well as more or less completely in sequence. Many of our actual episodes of emotion follow the former scheme. Many of our laboratory experiments in emotion follow the latter.

The formulas for human and possibly for infrahuman behavior ought to express the sequence of the several factors in the behavior flux. Especially in the description of individual and racial differences it seems desirable to factor out the protopractic beginning reactions as well as the epicritic systems as the various more elaborated factors emerge and succeed one another in overt behavior. This may be a difficult thing to do. It certainly would be much simpler to note the resultants in overt acts. But something significant will be lost unless the description includes the temporal sequence of their development.

The course of the sequence is neither simple nor invariable. Successive instances of similar situations never find the neural mechanism in identical conditions. Probably, also, there are complicating processes of development of reaction in which the epicritic phase of one stage of development becomes protopractic in another; of refractoriness in which a differential duration of the refractory phase favors one kind of repeated stimuli over another; and of relative fatigue.

The grounds of our working hypothesis are partly experimental and partly theoretical. Theoretically, it seems impossible to prevent the progressive spread of afferent stimulation to neural centers of greater elaboration. The first phase of response may and probably generally does involve the shortest neural path and less elaboration than later phases. Conversely, those phases which are based on the greatest elaboration of the sensory data and on sophistication tend to come later in any concrete development of adjustment. It must not be supposed that we assume an exact correlation between elaboration and latency. Epicritic adjustment may also appear in the form of anticipatory set, but there is a general tendency, consequent to each change in external vital conditions, which brings in the epicritic adjustments after some delay. Moreover, what appears to be antecedent epicritic set may on investigation prove to be the product of an antecedent adjustment. The two are often indistinguishable in the behavior flux, except on the basis of careful experimental analysis. As we have pointed out, the hypothesis also rests on some experimental records of the reflexes. There is also, I believe, no inconsiderable support in other psychophysiological traditions which have led to various close approximations to our formulation.

Our working hypothesis relieves us of certain embarrassing categories and assumptions of invariant instinctive and habitual behavior patterns and furnishes a schema which is elastic enough to cover the description of a great variety of observations. In addition, certain apparent exceptions to the sequence of protopractic and epicritic phases furnish a new viewpoint for the description of such anomalies as emotional reinforcement and hypnosis.

One can imagine a kind of general schema for the description of behavior, providing for the specific description of all phases of the total adjustment, their latencies and durations, the set out of which they arise, their mutual conflicts and reinforcements, as well as for a statement of the evoking stimuli. Until some such general formulas are available, corresponding in the field of behavior to Pavlov's analysis of the interconnection of stimuli, it seems a little premature to speak of a scientific behaviorism or, on the basis of what we now know, to neglect arbitrarily any of the conditions of the complex behavior flux. Most of our present psychological descriptions of behavior patterns represent relatively narrow parts of such a schema. They seem to me to be inadequate either for a description of the behavior flux in normal human adjustments, for interpreting their abnormalities, or for improving their adequacy.

Until closer approximations to such a formula are available, we lack, I believe, the elementary foundations for a psychology of social relations, or a scientifically conditioned ethics. I conjecture, moreover, that the development of an individual or of a civilization by education is a task which will be found to be closely connected, not with suppression, but with the supplementation of protopractic responses by suitable or desirable epicritic systems, as well as by conditioning the reflexes to favored stimuli. There seems to be no necessary antagonism between the two phases of adaptive adjustment, as is sometimes held. In the general sequences of approximation and correction both may be and probably often are useful.

CHAPTER IX

CORTICAL SYSTEMATIZATION

IN addition to a finer approximation of adjustment to environmental changes, the main difference between behavior which is conditioned primarily at superior, and that at lower, levels is the infinite capacity of the former to develop new combinations. While variability is, as we have seen, a universal phenomenon of human reactions at all levels, only in the higher does it reach the majestic proportions of invention and poetic imagination. Much of this wonderful realm is at present inaccessible to direct experimental attack. Still more cannot be objectively recorded or subjected to measurement. This makes those instances all the more valuable where experiment and precise records are both possible.

The most complex cortical reactions which were included in our experimental study of variability consisted of a group of word reactions in process of systematization which might be called memorization. In the familiar tradition of memory experiments series of letter groups or words are commonly presented serially, one at a time as by a revolving drum. With complete memorization, speech reactions within the series no longer follow the original stimuli. The effective stimulus becomes some systematically connected fact of experience. By a process of association, conditioning, or systematization, whose neural nature is still a matter of conjecture, the reactions become anticipatory. When the reactions are uniformly anticipatory for each member of the series, memorization is traditionally held to be complete.

Unfortunately, the method of complete learning is time consuming. Moreover, it usually fails to record those variations in reaction in the course of development which were the main problem of these experiments and which we hoped would throw some light on the nature of the process itself. Satisfactory technique, consequently, must give some measure of decreasing reaction latency as learning progresses. An adequate measure of changes in the reaction latency incidental to partial learning would probably obviate the necessity for complete learning.

Our method of eliciting and recording these reactions to systematized word series was somewhat analogous to the traditional method of paired associates. In our method, however, the measured associations were continuous instead of being merely paired. This made the method a kind of multiple prompting with recorded reactions. The technique was first described in *Psychological Effects of Alcohol,* by Dodge and Benedict. In brief, it consisted of the following essentials: A series of twelve four-letter substantives was exposed letter by letter, beginning with the last letter of the last word and proceeding backward to the first letter of the first word. Reaction to each word as it was thus exposed backward was recorded by means of a voice-key and an electric marker on base lines parallel to the series of words. During the first presentation of any series of words, adequate reactions could only occur after the presentation of each word was complete or sufficiently complete for identification. In subsequent presentations of the same series, identification and consequent reaction occurred progressively earlier. After sufficient repetitions each word was spoken, not only before its presentation was complete, but before it began. Such anticipatory reactions could occur only on the basis of some associative process. Decrease in the reaction time thus became a measure of the memory factor in the reaction.

For the sake of uniformity these reaction records were measured and reported in terms of space rather than in terms of time, i.e., in terms of the part of the word which was exposed when the reaction occurred. Any reaction after the complete exposure of the word was recorded as positive reaction latency. A reaction coincident with the complete exposure of a word was recorded as of zero latency. Anticipatory reactions were given a negative sign; and all reactions which occurred before any of the word was exposed were grouped together as though they occurred coincident with the beginning of the exposure, at −15 mm. This last notation indicated that the learning of that particular sequence was complete. Such notation may not be ideal, but it expressed the experimental facts simply and accurately and presented all the data in a single series of values.

Compared with the other measurements our previous experience with this memory experiment was quite limited and its technique relatively unstandardized. The lack of uniformity in the stimuli precluded direct comparison of the results with the other measurements of the series. In their bearing on our main problem—namely, the elementary conditions of human variability—these data are, however, of peculiar significance. They constitute our only measurements of complete, cortical resystematization. It was conceivable that the variability of cortical reactions might follow quite a different course from that of the lumbar reflexes. Just how the two really differ was part of our main inquiry.

Certain sources of error in the technique which appeared in the conduct of these experiments may be divided into three groups as follows:

(1) The main source of technical error was a shift of emphasis during the long experimental series from letter to word associates. This change appeared also within each word series as it became memorized. It seems to be an inevitable incident in the learning of words by any process of successive exposure. Apparently in the earliest records I was much more literally-minded than in the last. Undoubtedly this change of emphasis on different aspects of the learning process constitutes an important source of variability from series to series. It shows clearly in the difference of incidence of reaction as between the distribution of early experiments and those of the later records. The facts seem to be that notwithstanding uniform spatial contiguity, two systems of prompting were involved in our experiment. They are, respectively, the sequence of letters within each word and the sequence of words. That is to say, word reaction may be prompted by the letters of that word as they appear seriatim from the end of the word toward the beginning. But the last exposed letter of one word never operated consciously as the stimulus for the next word. The associations from each word to the next were associations of wholes. By a kind of inner shift of emphasis attention to letters seemed to decrease. In its place there occurred a word association, which seemed to be based on a kind of fitness or naturalness of the word sequence not otherwise describable at present. I did not permit myself to form narrative associations or connections. But the words seemed to form situation wholes with a kind of meaning background which I have called fitness. In reviewing these series several years after the experiments, I still found a certain naturalness or

fitness between some of the words or word groups. Sometimes, but not always, these late groupings corresponded with the quickly learned associations of the experimental period. It is probable that the naturalness or fitness of word sequences was not objective in the series, but was related to some unrecognized experience factor. I believe that such fitness groups might be a particularly valuable aid in probing for residual experience, and capable of somewhat more precise experimental control and measurement than the free-association test. Word sequences, which could be most quickly memorized, should correspond to residual systematic groupings and complexes.

(2) As the experimental series progressed, there was a further, though probably not unrelated, change in the persistence of the mental image of the word to which I had just reacted. Toward the end of the series attention seemed to be rather evenly divided between the past stimulus and the expected word. This was especially true after a series was partially learned. When a word was spoken before its exposure began, the time during which its exposure progressed was not a mental blank, but a continuous search for the next word of the series. This search consciousness would doubtless repay controlled analysis. It sometimes ended in assured anticipatory reactions. Sometimes it led to conflict and competition, which was only settled after the partial exposure of the next word, sometimes to wrong reactions.

(3) Notwithstanding our care not to admit familiar associations in making up the word groups, words differed widely in the way they fitted, or seemed to belong to each other in the several series. This was not a regular or constant factor. It served to make each group of words unique. This contamination of the

experimental presentation by the residua of preëxperimental experience was not peculiar to our memory experiments. It operated in all cortical reactions and possibly also in the reflexes. In cortically systematized reactions, however, its influence was vastly more apparent.

The first step in preparing the stimuli for these word groups was to make a list of four-letter substantives. From this list groups of twelve were selected by chance with the arbitrary precaution that no two words in a group began or ended with the same letter. Each group was then arranged in a series quite arbitrarily so that no two successive words made an obviously familiar combination or had any obvious connection when the list was read backward. The words of each group were then typewritten in spaced capital letters along the upper edge of a strip of white paper which was about 12 cm. wide and 50 cm. long. The space occupied by each typewritten word, including 5 mm. free from printing, was approximately 20 mm. The series of twelve words did not occupy the total space, but left, when wrapped on the drum of the kymograph, an empty space of approximately 45 mm. in length. A signal for the beginning of the series was typewritten about midway in this space. The space below the typewritten words was used for reaction records. The exposing kymograph stood approximately 50 cm. from the subject's headrest, on the main apparatus table at one side of the eye-movement recording camera. The tube of the voice-key for the memory experiments served as part of the headrest in the eye-movement and eye-reaction measurements. As combined with the other experiments the memorization experiments were consequently conducted without special preëxperimental reorientation on the part of the subject, except a slight turning of the head to the left.

In front of the kymograph a movable support carried two electric markers. These markers were adjustable on the vertical support and wrote in ink on the strip of paper which carried the words. One marker recorded 2″ intervals. It was electrically synchronized with the laboratory clock. The other marker was operated by an electric circuit which was broken by a voice-key. A duplicate of this voice-key is described with estimates of its latency and of its adequacy in *Psychological Effects of Alcohol.* In addition to the two markers, the support also carried a metal screen which hid the kymograph from the view of the subject. A short slit in this screen at the height of the words was just wide enough to expose one letter at a time. Four word-series were used in every four-hour session. Each series was exposed twice in immediate succession, thus corresponding to the paired stimulations of the reflexes.

With respect to the question of differential variability at different levels of the nervous system, this form of cortical reaction was unique in our experimental series. As was intended, the main variation in successive presentations of a word group constituted a kind of learning process which is only faintly foreshadowed in the reflexes. The whole process, as we have already noted, involved a higher form of adaptation to the conditions of the experiment. This is expressed in terms of the experiment by changes in the distributions of the reactions. Typical distribution surfaces show a skewed distribution area with two peaks, one at approximately 2 mm. after the word was fully exposed, namely, at approximately one-half second after the exposure was complete; the other peak at -15 mm., namely, before the exposure began. In some distributions the anticipatory reaction peak is especially high. This shows rapid and relatively complete memorization and resulted from repeating the series on successive days. Even on the first repetition some word sequences were learned.

Such distributions support our subjective impressions of the nature of the two systematizing processes with which we were dealing. In the narrower system, progressively smaller fragments of each word became the adequate stimulus for that particular word. In the wider systems each word as a totality became the stimulus for its successor.

The protracted influence of previous exposures overshadowed all other factors of variability in these cortical systematizations. This influence may be expressed as follows: (1) In successive exposures of a series of words, even though they were separated by months, our reactions always showed some effect of previous exposure. (2) This effect is expressed by the decreased average reaction time of the series—frequently in the first of the paired repetitions, regularly in the second. (3) Its influence on various parts of the series is quite unequal. Each part of the series of words ran an individual course quite independent of the rest. It is probable that the effects of otherwise unanalyzed experience made each word pair a special situation. (4) In general, the effect of each exposure on subsequent repetitions does not appear as simple arithmetical summation, but follows a more complicated course, in which time between exposures plays a dominant but not an exclusive rôle.

Comparison of these memory processes with the knee-jerk and other reflexes might seem to show that cortical processes completely reverse the processes of the lower nervous system.

Throughout the experimental series stimuli were presented for both knee-jerk and lid-reflex. Notwithstanding hundreds of repetitions of these paired stimuli the second stimulus was never anticipated by a reflex act. There was no shortening of the reflex latency. On the contrary, the only change in reflex latency seemed to be a slight increase. A definite experimental effort to develop a resystematization of the reflex arc in the direction of a crossed reflex was unsuccessful. In spite of hundreds of cases of simultaneous stimulation, true crossed reflexes were never elicited. Similarly, simultaneous stimulation of the right and left final common paths to the quadriceps by voluntary effort and by normal reflex stimulation, respectively, also failed to produce any reconfiguration of the mechanism of the knee-jerk. The lid-reflex was somewhat less refractory to experimental resystematization. It proved possible to produce a lid-reflex of an approximately normal latency by a knee-jerk stimulus, after a considerable number of simultaneous excitations; but that may have been due to the faint noise of the blow on the tendon.

A review of the average reaction latencies for each presentation of the various word sequences indicates a number of significant facts concerning the cortical systematization processes as they are measured by the method of multiple prompting, and shows that no two repetitions of our word-series had the same effect in shortening the reaction latency. This is true, not only of each sequence of words, but also of each individual word.

While it seems impracticable to trace all the vagaries of such a fluid systematization process, certain tendencies were apparent. As we have already noted, the time between repetitions is a conspicuous condition of decreasing reaction latency. A marked decrease of reaction latency regularly appeared at the second exposure of a series which had already been repeated a number of times, even after long intervals. The first exposure after an interval of several weeks showed very much less of the preservation effects of previous repetition than was shown by a second exposure which immediately followed the first. The phenomenon is interesting. It seems to indicate a latent systematization which persists after the lapse of a considerable time, but is not disclosed completely until reinforced by the first of the paired exposures. As the systematization became more stable after many repetitions, this latent effect largely disappeared. When the systematization was almost complete the effect of the second repetition was much smaller, showing a kind of diminished return.

If one groups the word-reaction latencies according to the time interval between successive exposures, the greatest decrease in latency occurred in immediate repetition. Sixty-five per cent of this effect persisted for an hour; 14 per cent persisted twenty-four hours; an average of 4 per cent persisted for from two to ten days. An average of 1 per cent lasted throughout our long experimental series.

Complete learning in these experiments was not approached by even steps. The decrease in reaction latency is rapid at first, then more and more gradual. An analogous change in latency appeared in our reaction-time records for both eye reactions and speech reactions as they approach their lower limits. It was also shown in the changes in decreasing amplitudes of the lid-reflex and of the vestibular deviation of the eyes. The two latter processes were interpreted as due to the development of persistent refractoriness. To different extents and with different accelerations, all the variations which may be regarded as resystematization processes appear to follow the same general course. There seems to be too close an analogy between them for mere accidental coincidence.

A cortical barrier to repetition was indicated both in the memory experiments and in simple speech reactions. The adequate stimulus for speech reaction was the exposure of a word which was brought suddenly into place without perceptible vibration by the action of a pendulum stop mechanism. Each word remained exposed for a second or more, or until the operator removed the word in readiness for the next exposure. In spite of the continuous exposure, the subjects uniformly spoke the exposed word only once. Similarly, in the memory experiments the exposure was protracted. Reaction occurred progressively at earlier and earlier moments in the exposure process, but it is noteworthy that after reaction occurred the previously adequate stimuli no longer functioned as a stimulus to reaction. They met some barrier to reaction analogous to a relative refractory phase.

In the memory experiments, as in the word reactions, the barrier obviously depended on the specific arrangement of the experiment. It would doubtless have been possible to arrange an experiment in which the subject would have continued to repeat the word as long as any part of it appeared in the field of view. The barrier to repetition in this case would have been reduced to the relatively short refractoriness of the motor system. The instructions occasioned a neuropsychological system whose barrier was of much longer duration. The subject did not keep repeating the just-exposed word, though the recency of the exposure should theoretically have rendered it eligible for repetition. The experimental set overbalanced the effects of recency. We would reëmphasize the presumptive importance of such barriers in ordinary life. Without it, all our lives might be spent in the repetition of the first chance experience. Thus, cortical barrier to repetition appears to be an important condition of resystematization. Where the barrier is weak, as in stereotypy, learning is at a minimum. From these considerations it seems to be a reasonable working hypothesis that some barrier to repetition, analogous to refractory phase, is a condition of intellectual development.

We have already noted the evidence in the reflexes that refractoriness to the just previously given stimulus may develop into an increased sensitivity if the interval between stimuli is prolonged beyond a critical period. This zone of increased sensitivity (the "supersensitive phase") is relatively unexplored in psychophysiological tradition and its relation to the refractory phase barrier is practically unknown in psychology. From physiological experiments we might expect both consequences of previous stimulation, heightened sensitivity lasting longer than refractoriness. We conjecture that such heightened sensitivity is an important positive condition for resystematization, while refractory phase is a negative condition; and it is probable that each condition has its own temporal incidence. There is a mass of evidence in the psychological tradition of learning which seems to support this conjecture, but direct experimental investigation is lacking.

CHAPTER X

CONSEQUENCES OF PERSISTENT CORTICAL SYSTEMATIZATION

SINCE the classical experiment of Ebbinghaus, psychology has concerned itself intensively with the determinants of recall. The consequent laws of memory represent the temporal conditions of the development of persistent cortical systems which are effective for recall. While the question as to the anatomical and physiological nature of these systems is at present far from being satisfactorily answered, the consequences of the residua of previous excitation on perception and behavior have been studied in many experimental settings.

Casual observation indicates that the repetition of identical stimuli at the perceptual level never evokes identical conscious events or reaction processes. Take, for example, two combinations of letters *competition* and *tmienopioct*. The former is at once familiar and meaningful. It connects with various chains of mental events in which it has played a part, and tends to evoke a multitude of mental reactions in neurological, social, and economic systems whose precise description does not now concern us. The latter is presumptively strange and quite new; at least that was my intention. Notwithstanding its lack of obvious relation to established systems, the effort to systematize it in some way is quite strong, though relatively unsuccessful. That particular group of letters doesn't fit any language known to me. It doesn't fit into my familiar experience, though some of the letter combinations are more suggestive than others. Looked at a second time, however, the consciousness it evoked has already radically changed. In the second instance it is no longer entirely new but is recognized as the just previously seen combination of letters which we have been discussing, with some more or less familiar and suggestive letter combinations.

Similar analyses of perceptual developments are familiar enough in psychological tradition. Precise experimental investigations of the rôle of the residual systematization of past experience in perception are somewhat less common.

In collaboration with my great teacher and friend, Benno Erdmann, I helped to investigate the differences between familiar and unfamiliar stimuli in our study of the psychology of reading. Out of that investigation developed the importance of letter, word, and phrase configuration which has been so extensively exploited in the pedagogy of reading and which probably represents an early approximation in the development of the theory of Gestalt. We found that word configuration might be the dominant condition of perception under conditions when no isolated letters were perceptible and an important condition where individual letters were clear. In a later experimental analysis I found evidence for a prefixational excitation process in reading which was corrected and made precise by an inhibition-reinforcement process incident to ocular fixation.

Our present task requires the recapitulation of some of the results of the reading investigation or of some other equally clear experimental data on the consequences of persistent cortical systematization.

Erdmann-Dodge confirmed the findings of earlier investigations that several times more letters could be read from a single short exposure when arranged in familiar word patterns than when not so configurated. In opposition to the Goldscheider-Müller doctrine of reading by dominant letters they found evidence of the existence and perceptual significance of systematized residua corresponding to word "form."

The perceptual significance of word "form" was demonstrated by the following experiments.

Letters were exposed at gradually increasing distances until they became illegible owing to the smallness of the retinal images. When familiar words were exposed at this distance they were found to be generally legible. These results were verified in more exact tachistoscopic exposure which eliminated the possible influence of gross eye reactions and accommodation changes. Since words were still read "the experiment proved that the words were not read from the recognized letters because they were not perceptible, but from the total recognized word picture.

"It is clear that the individual letters under these conditions of exposure were not recognizable because their retinal images were not large enough. The word, on the contrary, constituted so large an object that the number of excited retinal elements guarantees recognition, provided that the significant form of its total picture, i.e., *the optical type of* the word, is as familiar as the typical character of the individual letters." Under these circumstances "we read in such word types."

The very mistakes in identification showed the importance of the word "form."

The conditions of ordinary perception made these results probable even before the experiments were undertaken. "We recognize a house, for example, not because we apprehend the individual bricks, a hedge not because we perceive the separate twigs. On the contrary it is the typical character of the total arrangement or total configuration which insures the identification. So we recognize in an individual letter, as long as it appears plainly to us, not the individual lineal elements which constitute it but the configuration of its totality.

"These experiments leave no question that under the chosen conditions of exposure the optical total form of words determined our recognition and not the perceptibility of the single letters which constituted them.

"Our visual perception offers phenomena of simultaneous contrast whenever objects are variously colored or where monochromatic objects stand out from differently colored backgrounds. A figuration of stimuli of complicated color character which conditions such a contrast phenomenon appears also in every perception of printed characters. Even a letter is a totality which we perceive as such, not on the basis of the optical parts into which it may be analyzed, but rather in consequence of the configuration of these parts which are peculiar to it. On this account we must apprehend this totality in its wider and at the same time in its more detailed sense. It does not consist primarily of fine black surfaces, but also of the more or less broad variously formed white surface elements of its background which close it in and surround it.

"These parts of the white background are for the total figuration not less essential than the black. It is sufficient to exemplify what we have just said in a few simple forms. A *5* could be analyzed into the black forms ⌐ ' ɔ but forms like this we would not recognize as a *5*. < | is no *K* although both forms have the same elementary black lines. These remarks might seem self-evident. They are not quite that when they are transferred to visual word wholes, and they must be transferred to these word wholes as well as to all of our visual perception in general, which is characterized by the arrangement of similarly colored parts on a differently colored monochromatic background. Printed words get their typical character through the configuration of the lineal parts in contrast to their white background. The letter complex

l
h
ü
f
e
G

gives us the word "Gefühl" but we have difficulty in reading the word although it contains the series of letters. Moreover, in this order we read it letter by letter. The familiar spatial picture "Gefühl" has a totally different form character, because the former phenomenon is a strange one, and because it contains a series of totally unfamiliar contrasts. The visual word picture, in which there is a familiar figuration of its parts, cannot help exercising a determining influence on the perception of words just because its several letters make an optical whole. We may summarize as follows: The recognition of visually familiar printed words under conditions which preclude the recognition of individual letters has its basis in the typical total form which remains a characteristic of each word."

Certain theoretical implications of these and related experiments were given by Erdmann shortly before his death in his *Grundzüge der Reproduktions-Psychologie.*

If one sought the most fundamental principle of the Erdmann theory of perception it would probably be found in the doctrine of "nonindependent reproduction by apperceptive fusion." This is an immediate, necessary, and universal process in every perception, and consists of the arousal of certain representables and their fusion with the immediate effects of stimulation. This should not be confounded with the associative fusion of sensory data into wholes possessing qualities, or with the associative interweaving of percepts by preëstablished neural paths, by personal experience, or by similarity. It is a precondition of them all.

Any *Reproduktions-Psychologie* must start with the postulate of psychophysiological residual systems. Their nature still remains a matter of debate. That they exist as unconscious dispositions to new moments of consciousness and behavior, that they may remain for a long time as unconscious dispositions for the associative interweaving of the original experience, there is abundant evidence in habit—motor, intellectual, and emotional.

Sense perception regularly involves memory factors for which there is no direct sensory stimulus. These are commonly associated supplements. The consequent perception may be called associatively supplemented (*ergänzt*) perception. A still simpler and more fundamental supplemented perception is found in tachistoscopic experiment and occasionally in daily life. It occurs when attention is concentrated on the perceptual content, and also when objects are casually noticed in cases of diffused attention. In spite of the narrowness of this kind of perception, it is often clearer than the present stimuli can account for, and it commonly involves an identifying cognition. Either fact would imply the interaction of reproduced sensory experience. The notable peculiarity of such reproductions is that they never appear in consciousness independently, but always fused with the immediate results of stimulation. The term "apperceptive fusion," which may be applied to such reproduction, must not be confounded with associative fusions of conscious factors. It refers not to conscious contents but to the conditions of consciousness. Apperceptive fusion involves two moments which may be called, respectively, the stimulus component and the residual component. In any given fusion the two components are simultaneous. Dynamically, the stimulus component is primary. The residual component, however indistinguishable to introspection, is essential to the fusion.

According to the *Grundzüge,* apperceptive fusion is the condition of all cognition. It determines the course of attention and is the cause of many of the illusions of normal and abnormal experience. All cognition is recognition. No perception (even the most undeveloped) is entirely free from it. In adult consciousness it underlies the serial development of observation, introspective as well as sensory.

The reproductive processes that begin in apperceptive fusion commonly lead to "mediate supplemental reproduction" by associative "interweaving." Of these, remembered, abstract, and imagined presentations are the simplest forms. Such reproduction is associative, but not in the sense of Hume's association of ideas. Only residual systems are associated with the immediately aroused residual component of the apperceptive fusion through which they are reproduced. In mediate-supplemental-interwoven reproductions neither the associated residua nor the condition of their reproduction need be a conscious content.

Of the various forms of supplemental interwoven reproduction the most momentous is the perception of symbols. The cognition of symbols presents every form of sensory cognition from apperceptive fusion to the more complicated associated thought processes. In the discussion of these supplemental reproductive interweavings Erdmann restates in the *Grundzüge,* partly in the form of equations, his contributions to the interrelation of thought and speech, as well as to the psychological organization that underlies formulated thought. The argument is too condensed for recapitulation.

Not only may reproduced factors be conditioned by nonindependently aroused residua, now fused with sensory factors, and again interwoven as associated supplemental moments, but also the products of reproduction may remain unconscious though stimulated. This occurs in apperceptive preparation for, or in attention to, a coming unknown stimulus, in the silent elaboration of speech, and as Erdmann's self-observation indicated, in the lack of meaning consciousness antecedent to reasoned familiar utterance. The understanding of sense impressions, of speech, and of reading matter may on occasion involve widespread conscious reproduction of "agglutinated residua." When the material is sufficiently familiar the stimulated agglutinated residua may remain unconscious. These unconsciously stimulated residua may be represented in consciousness by emotional states, of which the feeling of familiarity is an example.

The climax of apperceptive completion appears in the sublogical processes such as abstraction, comparison, expectation, and combination, which are the psychological foundation of formulated thinking, both inductive and deductive.

We have traced the consequences of persistent cortical systematization in reading and speech in some detail, not because they are found there in a peculiarly high degree, but because of the historical significance of the precise experimental data on which they rest and their general theoretical importance. Persistent cortical systematizations are discoverable in all perceptual and thinking processes, but they are not constants. On the contrary they are modified more or less by every related experience and behavior, and subject in successive instances of their arousal to all the modifying influences of refractoriness, inhibition, and reinforcement, relative fatigue, and resystematization. In a unique way they appear at the very heart of our consciousness and behavior.

CHAPTER XI

THE RELATIONSHIP BETWEEN MIND AND BRAIN

THE last chapter has prepared the way for the proposition that human variability is not merely an important psychophysiological fact, and a condition of mental development, but that as represented by apperception, it indicates a fundamental condition of the integration of intellective consciousness.

Psychophysical parallelism. One of the most common hypotheses of the relation between mental phenomena and the action of the nervous system is involved in the concept of psychophysical parallelism. A modified parallelism probably expresses in a convenient way the duality of two systems of intellectual integration known as psychology and neural physiology. Certainly psychological and physiological description use very different apperceptive systems in the perception of, and different systems of concepts in the expression of, their common data. I believe, however, that it has been and is an unfortunate working hypothesis for an investigation of the relationship between mind and brain.

In the history of inner psychophysics, the hypothesis of parallelism has been almost sterile. In a period of rapid development of both psychological and physiological experience and theory, one seeks in vain for a single notable advance in psychophysical theory that is the direct outgrowth of psychophysical parallelism. On the contrary, in four respects at least it seems to be a handicap.

1. The principle of parallelism is inhibiting rather

than stimulating. If at the outset of investigation we assume the validity of some theoretical relationship between mind and brain, like thoroughgoing parallelism, we discourage rather than encourage the investigation of fact.

Unfortunately, as far as actual experience goes, thoroughgoing parallelism is open to serious question. The phenomena of consciousness cease to exist in the only form in which they are given to us directly, while some of the processes of neural biology may be demonstrated to continue. Furthermore, there seems to be little doubt that intellective consciousness at least is more intimately and directly connected with the activity of the superior cerebrospinal ganglion than with any others. But if consciousness, in a form in which we know it directly, is correlated with specific nervous processes in specific parts of the nervous system, it is the business of science to discover the peculiar characteristics of the neural process which appear to be necessary conditions of that consciousness which we know. This is a plain, straightforward, scientific problem. Much remains to be done toward its solution. But it is obvious that advance in our knowledge will not be stimulated by the belief in a thoroughgoing parallelism.

Perhaps each segment of the spinal cord, each neuron or even each electron, may possess a "consciousness" of its own. However improbable the supposition may appear, it is thinkable. It is, moreover, apparently incapable of disproof. Supposititious cord consciousness, however, is utterly inaccessible to us. It is not our consciousness. It is not even comprehensible in terms of our consciousness. The special hypothesis of a special kind of inaccessible consciousness to correlate with every kind of neural process is quite indefensible in empirical science. It confuses the issue, and would be legitimate only if we could show that the various neural processes actually possess those characteristics which in the cerebrum are the conditions of consciousness as we know it.

Furthermore, psychic units and psychic elements never have been, and probably never can be, correlated with physiological units and elementary neural processes. A psychic element, if there is any such thing, always appears as the consequence of complex neural antecedents. That is to say, a color or tone which for consciousness is not further analyzable, is physiologically still highly complex. Beginning in the sense organ and ending in the cortex there are at least three links in the chain of neural happenings. In the case of color the number of links is probably higher, and in none of the links is the process a simple one. In the last link it probably involves more or less widespread cortical disturbances with a complex interplay of excitations and inhibitions. Certainly it is not an unanalyzable event.

If some kind of unknown elementary conscious process parallels the migration of each separate ion in neural tissue, such processes are unknown. We can form no conjecture from our inner experience as to what they might be like. They belong to that large but usually discredited class of hypotheses, that spring, not from the facts, but from theory. Utterly unprovable and irrefutable, they have no proper place in natural science. On the contrary it is clearly a part of the business of psychology as science to ask, What is the peculiar complication of the physiological manifold which conditions observable mental fact?

2. Both in the *Elemente* and in the *In Sachen der Psychophysik,* Fechner promulgated the now widely

accepted doctrine that the "threshold" of outer psychophysics has a counterpart in inner psychophysics. It seems to be generally agreed that even the peculiar nervous correlate of normal consciousness may be stimulated subliminally. Fechner held that the subthreshold waves have a definite configuration, which represents their mutual interdependence and determines their advent into consciousness. The same view finds various expression in current psychological theory.

Just what subliminal excitation may mean in view of the all-or-nothing law leaves us in some perplexity. In other connections it is commonly held to mean the restricted spread of response in neural systems. Even if it be granted for the sake of argument that consciousness is a quantitative variant of the unconscious, thoroughgoing parallelism would still need a certain correction. Any given conscious content is obviously not correlated with the subthreshold stimulation of relevant parts of the nervous system, but only with a nervous response, i.e., to suprathreshold stimulation. We must face the phenomenal problem. What really happens after the threshold is reached is a matter for investigation rather than for speculative presupposition.

3. Further evidence for the inadequacy of parallelism as a working hypothesis for inner psychophysics is the fact that it breaks down when we really use it, and leads to absurdity. We are utterly unable to reason successfully either from known nervous facts to consciousness, or from consciousness to its nervous correlates. Phenomenal parallelism as a working hypothesis for inner psychophysics assumes too much. It is equally embarrassed by the question where in the scale of organic existence a consciousness comparable

to our own begins, as well as by the question concerning the specific conditions of the only consciousness which we can know directly.

Wherever psychology has ventured to express itself uncritically in supposedly parallel physiological concepts, it has made serious mistakes. At the present time, at least, it should use the terms energy, work, fatigue, inhibition, excitation, or facilitation with great discretion in their physical or physiological connotation. For example, we know very little of the metabolic energy equivalents of mental work and nothing at all of what a fatigue of consciousness might mean. A consciousness of "fatigue" may be a very different matter from physiological fatigue. Because of such sources of error, animal physiology has for some time been trying to rid itself of uncritical implications of consciousness in its concepts and in its technology.

If it is retained at all, the hypothesis of psychophysical parallelism must be tinkered with before it will fit the facts. It cannot be trusted anywhere, and is consequently a debatable theory rather than a serviceable working hypothesis.

4. Finally, parallelism in inner psychophysics is at heart a confession of scientific impotence. Du Bois-Reymond's "Ignorabimus" may be the final word. But it was never a satisfactory starting point for experimental science. It may be that "astronomical knowledge" of neural processes will forever fail to discover consciousness. Meantime, however, it is pertinent and legitimate to ask what characteristics of the nervous processes are essential conditions of our normal consciousness as it is described in psychological relationships.

A legitimate part of the task of every descriptive science is to describe the sum total of the conditions of the various phenomena that it investigates. Psychology has no especial obligations and no exemption if it would be a science, even though the task is beyond our present ability.

Some of the conditions of the various phenomena of consciousness seem to be given in introspection. In no case are they completely given there. Even consciousness is always something more than the sum of the factors that we can introspectively analyze out of it. Wundt's principle of creative synthesis must be given a place in a purely descriptive psychology. It is true in the simplest psychological configurations. It is still more conspicuously true in meaning and personality.

The conditions of consciousness itself can probably never be accessible to direct introspection, since as conditions of consciousness they must also be conditions of introspection. Direct analysis can reach only so far as introspection reaches. If, in spite of the paralyzing dogma that consciousness is unanalyzable and undefinable, we persist in asking the scientific question concerning the conditions of consciousness that make introspection possible, we must use some other technique.

Similarly, analysis of the neural process will doubtless never reach beyond its own descriptive frame. Certainly physiological analysis by itself will never reach the psychical, since our human consciousness is correlated, not with elemental metabolic processes, but with specific organic complications of metabolic processes in a specific organ.

From both the physiological and psychological standpoints the problem of consciousness appears as a synthetic one. If we would ultimately discover what the integrative complications involved in consciousness actually are, it will do us little good to catalogue the elements of which it is composed.

It is the business of a profitable working hypothesis to point out a direction of investigation that looks promising. Under the circumstances it seems to me that the first step may well be to make some reasonable estimate of the kind of organic integration that would approximate consciousness as we know it.

Approximation to the principle of apperceptive integration. It is probable that the integrative principle that underlies intellective consciousness can never be separated out by introspective analysis, since in all intellective consciousness it is presupposed not as a factor but as consciousness itself; nevertheless, if we are to discover any clue to the integrative principle it must be from consciousness. We may reasonably expect that it will be reflected in the fundamental organizations within consciousness that are accessible to us.

If we should ask what has generally appealed to psychologists as the most essential characteristics of intellective consciousness, memory and association would probably be the favorites by a large margin. Without either, intellective consciousness as we know it could not exist. Modifications of either by accident or disease effect grave changes in the soul life, not merely in its stuff, but in its organic character, however fully the elements are retained.

In what seems to most psychologists an extreme and untenable position, Loeb held that associative memory is consciousness. The psychological offense in Loeb's formulation consists in an apparent *petitio principii.* Psychology knows no association except the association that may occur between facts that are already conscious. Similarly in memory, what is remembered must have previously been a conscious fact. But it gives no real clue to the nature of consciousness to say that consciousness is the revival and association of conscious facts. We are bound to ask the previous question: What constituted consciousness in the original of the associated and remembered fact?

I conjecture that Loeb's answer to the difficulty would be to insist that both memory and association are also biological concepts—that memory is a function of all organic matter and that association is a widespread characteristic of living tissue. But in this sense, i.e., the only sense in which Loeb's principle ceases to be a *petitio principii,* it ceases to be an entirely satisfactory account of mind. If association and memory are general biological principles, we must inquire as to the peculiar characteristics of the associative memory that constitutes our consciousness. These characteristics, it seems to me, are given in the type of associative reproduction which, for want of a better name, we may call apperceptive.

In using the word apperception we should make it clear that we are not using it in the restricted sense of Wundtian theory, but rather in the more general sense indicated in Erdmann's description of the facts of perception. By apperception he means

> that complex reproduction process that occurs in every perception of developed consciousness, in which the present stimulus immediately arouses and fuses with the residua of similar past stimulation, and mediately arouses and is interwoven with the residua of past experiences which are associated with it.

In order to use even Erdmann's formula as the schema for all intellective integration we must abstract from the specific function in whose service the apperceptive process was originally exploited, i.e., perception. Our generalized principle might be restated as follows: Apperceptive integration is the general name for a complex reproductive process which occurs in every moment of intellective consciousness. The present neural excitation, in so far as it conditions consciousness at all, arouses immediately the residua of previous similar excitations and fuses with them; while it also arouses mediately the residua of previous excitations originally connected with them.

The question of the validity of our working hypothesis is a triple one: Could such an integrative principle give us the organization of consciousness as we know it? Does it correspond with the psychological facts at our command? Does it correspond with available physiological data?

Hypothetical questions in synthetic psychology. 1. If we postulate a living tissue of such character: (1) that n changes in its environment (nS) tend to excite n characteristically different reactive modifications in the life history of its elements and their organic interrelations (nR); (2) that every new S tends to reproduce every previous R in definite sequences, though in various degrees of completeness; (3) so that every new R becomes a part of a relatively slowly changing system of reactive modifications, with which it thus becomes organically integrated and to which it adds its peculiarity, should we not, with such a reproductive organization of its reactive modifications, grant to our tissue a kind of intellective consciousness, in which the qualities of the original nR are logical accidents, depending on the number of discrete reactive modifications of which the elements of our hypothetical tissue are capable within the limits of their organic integration?

2. If we further postulate a circular excito-reactive process by which the directly excited factors of the system tend to reinforce the S which aroused them, and to emphasize the corresponding R, together with other R's immediately connected with it, while it tends to inhibit less directly connected processes, within the limits of relative fatigue, would we not therewith grant our tissue a kind of selective attention?

3. If we further postulate a relatively slowly changing group of emphasized R's, which are regularly reproduced with every new R, must we not therewith grant our tissue a kind of personality?

The psychological problem. As is indicated by their form, our questions make no pretense of giving an elaborated psychological theory. They aim to focalize the problem of the organic structure of intellective consciousness, and to suggest a synthetic solution to the problem.

The fundamental principle which underlies the questions, that intellective consciousness may be known as a kind of integration, is by no means new. Psychology at the very beginning of its modern period undertook responsibility for analyzing the complex forms of integration which are directly observable within consciousness. It has made substantial additions to positive knowledge, not only in the fields of perception, memory, and association, but also in the more complex fields of language and secondary identification, meaning and coördinated response, and the thought processes themselves. The limits of progress seem to be determined only by the precision of our problems and the development of adequate technique.

Our thesis, however, goes farther. Not only are there discoverable forms of organization within consciousness which characterize the systematic groupings of experience into units, temporal and spatial, practical and scientific, logical, ethical, and religious, etc., but we hold that consciousness itself is not essentially different from the observable phenomena of consciousness. It is conspicuous that we can never catch conscious facts except in organized form. Unintegrated conscious elements never have been discovered, and probably never will be. They are scientific abstractions. Moreover, there is abundant and excellent psychological precedent from Aristotle to the present, for believing that consciousness itself may be cognized as a kind of organization.

James put the matter in his striking way:

> Consciousness connotes a kind of external relation, and does not denote a special stuff or way of thinking. . . . The peculiarity of our experiences, that they not only are but are known, which their conscious quality is invoked to explain is better explained by their relation to one another.

The thesis is not lacking direct scientific evidence. The experimental analyses of hysteria and the "subconscious," of divided consciousness and multiple personality clearly indicate that any given configuration of consciousness is neither intrinsic nor necessary, but functional. Consciousness and personality never appear as homogeneous quanta of some specific stuff. They always involve the unification of discrete, more or less highly differentiated factors. Schizophrenic disorders, divided consciousness, and dual personality have the common characteristics of a disturbance of that unifying process.

Analogous phenomena are familiar enough in normal mental life. Adequate stimuli of many kinds are constantly affecting the sensorium. The fact that the vast majority fail to come to consciousness is not due entirely to any inherent characteristics of the stimuli themselves. The same stimuli under slightly different subjective conditions may give rise to conspicuous features of our experience. In the former case available material failed to be taken up into the momentary organization of clear consciousness. Even though it fails to clear up independently, there is evidence that it may leave significant revivable residua. Apparently, however, much is lost forever, and never becomes a part of our organized experience.

Similarly, the evidence seems conclusive that the rearousal of nervous residua is no guarantee of cleared-up consciousness. Specific uncleared residua may even function in the process of association. If the functioning residua at one time fail to be included in consciousness and at another time succeed, the difference must lie in something outside the residua themselves. We can understand it in terms of our theory as a variation in the completeness of the integrative processes. The stuff of consciousness was available in both cases. In the former instance the unconsciously aroused residua fail of being taken up into the particular moment of organization and unification in which our present consciousness consists.

The principle of apperceptive integration makes no claim to be a principle of explanation. The particular physiological conditions of organization are left indeterminate. But if intellective consciousness itself, as well as its various aspects, can be regarded as a kind of integration, the special problems of inner psychophysics are rendered more concrete and definite.

The second claim to attention is that our principle is drawn directly from fundamental mental processes which are accessible to observation and experiment. Perception itself, and consequently introspection, shows the process in completest form in the processes of apperception and assimilation. Various aspects of the process are conspicuous in the familiar abstractions of conscious memory and association, in habit, in the conscious controls, and in conduct.

The process of perception we have already discussed. In memory and association the application of our principle is fairly obvious.

Habit and conduct have always offered peculiar difficulties to the application of psychophysical formulas. The degradation of consciousness in habit was always an offense to parallelism as well as to the motor theories of consciousness. If we once admit, however, that consciousness is a particular kind of integration of organic reactions, then it is evident that a simplification of the organization, by the development of more direct arcs, either within the cerebrum, or in the lower ganglia, may lose the peculiar characteristics of the conscious organization. Secondary automatic acts will participate in consciousness only in the same way as the simplest reflexes, like the knee-jerk and the protective wink.

In so-called voluntary activity, on the other hand, with its conscious suppressions and reinforcements, the resulting activity may, and in its highest form will, represent the resultant of the completest practicable integration of the individual's past and present experiences and his intentional set. Any relatively constant factor of experience may add to the complexity of the regularly reproduced residua which make up the reacting personality.

Conversely, in organisms whose nervous systems contain only relatively simple reflex mechanisms, there is neither ground nor excuse for assuming the complex reproductive and recapitulatory processes involved in intellective consciousness. The application of our principle to the problems of animal consciousness is at least a clear-cut issue.

The physiological problem. From the standpoint of biology and physiology, I believe that the conception of consciousness under the schema of a specific kind of integration is a conspicuous outgrowth of the available data. Early biological theories of the conditions of consciousness suffered from the general preëxperimental confusion between psychological fact and psychological hypothesis. Moreover, they often confused the issue with mechanical or materialistic interpretations.

After the discovery of the electrical currents of action in nerve and muscle, theories of nervous organization tended to follow the analogy of the telegraph and telephone. The cerebrum corresponded to the central exchange or switchboard. Energy was supposed to enter the system in sense stimulation. Variously combined and modified, it found exit as a motor impulse. Consciousness was a by-product or correlate of the switching process.

Traces of this conception are still found in popular thought and even in psychological textbooks. But the present scientific view of the action of the nervous system is fundamentally different. Each of the millions of neurons whose various clusters make the anatomical units of the nervous system seems to be a rather independent little body, and leads a somewhat independent life in addition to its relations with the rest of the neural system. It has its own supply of energy, regulates to a considerable degree its own nutrition, reacts in its own peculiar way to the chemical and nervous stimuli to which it happens to be sensitive, and, finally, it may die without necessarily involving the destruction of its neighbors. In general each neuron seems to be particularly sensitive to some particular varieties of stimuli. But its action in every case seems to be relatively independent of the kind of stimulus that excites it. The mode of its reaction appears to be determined primarily by its own specific form of reaction. Each neuron may in turn be variously stimulated. The end of a chain may be gland, muscle, or nervous tissue.

Whatever the final outcome, each step in the process, including the last, is determined by the character of the participating links. The results, as measured at different points of the process, may bear widely different ratios to the energy of the original stimulation. It may become zero, or perhaps even negative by inhibition. It may reach enormous proportions by various reinforcements. Take, for example, the familiar phenomenon of the knee-jerk. Slow pressure against the tendon produces no reaction, however intense it may be. A small amount of energy in the form of a quick tap may, on the contrary, evoke a reaction more than twenty-five times the force of the tap. If, however, the same blows are given in a series of ten per second, the reactions may almost, or entirely, disappear, due to the development of refractory phase.

Similarly, ether vibrations of barely threshold intensity, corresponding to the light reflected from a thin column of smoke in a partially darkened theater, may produce widespread neural excitations. It may lead to the maximum activity of the main musculature in the body, or to no reaction at all, according to its interpretation. Neural activity is no longer regarded by the oriented as the central reflection and final motor emergence of the afferent energy derived from sense stimulation. Each link in the chain is a magazine of potential energy which reacts to adequate stimulation in its own way. Every sensory-motor reaction may thus be traced through the nervous system as a chain of stimuli and reactions to stimulation. Somewhere in that process our consciousness arises. There is evidence that its locus is not in the peripheral sense organ. Though there have been recent attempts to connect part of it with the basal ganglia, the weight of evidence forces us to regard intellective consciousness as conditioned by complex cerebral activities.

The Cartesian theory of a single point of contact between soul and body is now clearly inadequate. It was long ago abandoned as a tenable hypothesis in physiology, but its effects are still felt. True, there is no single place discoverable where consciousness as such is located. No circumscribed or focal lesion necessarily destroys consciousness as a whole, though Southard found the integrity of the parietal lobes peculiarly important. It is not uncommonly held, however, that certain kinds of consciousness are located in discrete cortical areas. Certainly, there is no warrant for such an assumption in our present knowledge of the localization of function. It is indeed true that each of the several sensory-projection fields has especial functions with relation to the rest which no other field can vicariously effect. But we cannot say that a certain kind of consciousness is here or there. If a given field is destroyed, consciousness will lose some of its possible content, but consciousness may remain. It may be a distorted, crippled consciousness, with more or less conspicuous differences from normal. The brightness and the song may be gone, but a very vigorous mental life without either vision or audition is a conspicuous reality. The assumption that visual projection areas contain the correlate of a visual consciousness, which might persist if these areas could be isolated, is without a shred of evidence. The anatomical provision for the intricate organization of each part with all the rest is certainly not meaningless. Its obvious lesson is confirmed by some of the most assured products of psychological experiment, which indicate the complexity of the conditions of even the simplest concrete facts of consciousness.

While some of the recent tendencies in physiology appear to emphasize the processes within the cell units, the physiology of the cerebrum never more clearly faced the problem of the importance of the systematic interrelations of the neurons. This is especially conspicuous in the substitution of the concept of an area of maximum vulnerability for the old concept of localization in speech disorders. The assumption that consciousness is somehow connected with the individual cells is certainly not correlated with any clear notion of how the intraneuron processes are related to human consciousness. I conjecture it is rather because it is recognized that the cerebrum is an integrating organ and it is assumed that the elements must have somewhat the same characteristics as the integrated whole.

The stuff of consciousness. The question as to the nature of the unorganized stuff of consciousness is not really a part of our present discussion, but it is bound to be asked whether there is a peculiar soul stuff or not. For myself, I must confess the problem is especially intriguing, but it seems to me that this is an added motive to guard against methodological error.

Whatever the answer, as far as science is concerned, it must be in the form of a hypothesis. There will be a characteristic difference between this hypothesis and the hypotheses of our questions. The latter appear to be not principles of explanation but working hypotheses. The motive for the former is not practical as far as I can see, but purely speculative. The question will be asked. It will be answered. The speculative demands of human reason must be satisfied or the completest organization of consciousness will be lacking. But the question and its answer are not directly pertinent to the present question at issue.

On the other hand, our working hypothesis does effect a certain pragmatic selection from possible answers. It is obvious that on this basis, the stuff of consciousness cannot be sensations, memory images, or any other direct products of introspective analysis. These cannot be the unintegrated stuff of consciousness because they are already conscious.

It is widely held that certain moral needs require that the stuff of consciousness be a peculiar sort of soul stuff. The demands arise in the effort to escape the determinism of a materialistic universe. These needs certainly deserve a hearing. But it never seemed to me that the hypothesis of a special soul stuff helped matters much if we are forced to deal with that stuff and its combinations under the category of causality. Neither does it seem to me that the stuff of our material world-construct can be held responsible for its organization. It cannot supply the principles of its organization without being more than stuff.

One is justly suspicious of cycles and epicycles of hypotheses. There are good methodological grounds for demanding that explanatory hypotheses be as simple as possible. In the present case these grounds constitute a motive for the hypothesis that the stuff of our physical constructions on the one hand, and of our consciousness, on the other, have no necessary and intrinsic differences. There may be no peculiarity of the stuff of consciousness, except that it is capable of that particular kind of organization. I can see no intrinsic reason why the same stuff should not be capable of organization in other systems beside consciousness. There is no negation given or implied. As far as I can see the stuff of consciousness might enter into all sorts of different systems as any given atom in the body is also in various systematic relations with the inanimate.

In one very fundamental sense intellectual parallelism was not affected by our critique of parallelism as a working hypothesis for inner psychophysics. It seems to me that we might well hold that the stuff of the universe is capable of parallel systematization in many different scientific settings, that it may be at once a soul stuff and a matter stuff. But this sort of parallelism makes no pretense to knowledge of the one form of systematization from the knowledge of the other form. On this basis it would be absurd to hold that the organization of the primitive stuff in consciousness was paralleled by the organization of some more of the primitive stuff in brain tissue. The concept would seem ludicrous and I believe it is ludicrous.

To sum up, my main contention is that the stuff of consciousness is a logical accident. Whatever it is, the right kind of integration would constitute consciousness. I can see no intrinsic reason why any stuff in the universe may not enter into a similar kind of organization, if the proper conditions are given.

CHAPTER XII

MIND WITHOUT BRAIN

NOT infrequently a person is brought to a neurological clinic with a strange disturbance of speech. He has suddenly developed an inability to understand hitherto familiar spoken phrases and to read. If the patient is right-handed and his hearing of noises is unimpaired, the brain surgeon will proceed with great confidence on the assumption that there is a neural disturbance on the left side of the brain at about the level of the temple. This disorder of speech is a fairly common one and is called sensory aphasia. If a patient has become unable to see objects in the left half of his field of vision, the brain surgeon would assume with confidence that the cause of his left-sided blindness is a disturbance of the right side of the brain, on the inner surface, at the back. The life and usefulness of human beings is daily conserved by brain operations based on the general assumption that certain definite disturbances of conduct and thought are connected with disturbances of definite brain areas. The assumption is in accord with scientifically known facts. It is justified by practical experience.

It is unfortunately true, however, that the exact neural processes which are involved in some of the simplest mental acts are still unknown. The neurophysiology of discriminating red from green, hot from cold, or sweet from bitter is just as obscure as that of remembering yesterday's dictation, the shift of attention from business to golf, and the control of conduct by the intention to be honest. All efforts to picture these mental processes in neurological terms are mere hypotheses, scientific guesses, and should be aimed to arouse investigation rather than self-satisfied belief. Nothwithstanding the limitation of our information, no one who is acquainted with the scientific evidence doubts that some kind of brain action is a condition of our mental life.

Not all the consequences of the confident assumption of the dependence of mind on brain are as salutary and helpful as the practice of brain surgery and psychophysiological research. On the contrary, some apparently logical conclusions from the premises seem quite alarming, since they are opposed to one of the most fundamental articles of our social faith. One of these more or less alarming conclusions proceeds as follows: If all mental action is conditioned by brain action, then either there is no mental life after death or it must be of some other order than our present. Furthermore, with such a premise, the possibility of believing in a superhuman mind seems out of the question.

The answer of Fundamentalism. Motived entirely by the desire to be honest with himself and to think straight, a scientifically-minded person might very well say to his brother who still clings more or less blindly to conventional belief: "Show me the brain of your God. Unless I can find his nerve cells or the tissues that include them, unless I can put my finger on his nervous system, common honesty prevents my believing in a superhuman mind." Several courses are open to the believer. He may say to his scientific brother: "Have faith and let nothing interfere with it. Forswear all impious scientific evidence and believe as I do or as I try to." That is the essence of Fundamentalism. It threatens a return to propaganda by force, though at present its modes of torture are limited to social ostracism and economic disability. There are good psychological grounds for the belief that propaganda by force can never prevail. Now, as of yore, human souls will not hesitate to suffer for the sake of intellectual integrity. The moral daring of the martyrs is not dead.

The double truth. Another course for the believer and his scientifically-minded brother might be to follow the example of medieval compromises and adopt the principle of the "double truth." That is to say, they might adopt the principle that one is free to believe in religion what is doubtful in science. Now there is excellent philosophical precedent for the expedient of leaving to the practical reason what is unknowable. Something similar is commonly done when the religionist points out to the scientist the minute cosmic area that his science covers and with a sweep of the arm to indicate the indefinite unknown, solemnly proclaims, "There, is God!" Personally, I think the gesture is an impiety. I could have but scant regard for a god who inhabited only the unknown. Moreover, with the gradual encroachment of the known on the unknown, a god of the unknown would be pushed farther and farther into the outer darkness, out of contact with practical life. Some thoughtful people believe that this is what is happening as science develops. It is the great excuse for Fundamentalism. To stand by and see God gradually crowded off the sidewalk by a parvenu is not edifying for any normal person. It naturally arouses all the finest loyalty of the believer and makes him fighting mad.

The scientific attitude. It is possible that in addition to these two unhappy sequels there is a third ending to the story that is less repugnant to our sentiments. Suppose, for example, the believer requests his brother to look deeper into the too familiar facts rather than away from them. It is a good maxim in everyday life that the cure for darkness is not less light but more. It is an equally good scientific maxim that the best scientific investigation opens the doors for further and still more profitable research. True science never sets a limit to its progress. Now, our believing brother might ask several pertinent neuropsychological questions whose consideration is illuminating, even if definite answers are not yet available. He might ask, for example, just where in the chain of neural occurrences does consciousness arise. When a patient answers the request to give his name, we can trace the course of the nervous impulse with considerable assurance from the inner ear, through the ganglia at the base of the brain, to the appropriate projection area of the left hemisphere, which was disturbed in the case of the first patient mentioned.

If we ask at the various levels whether consciousness resides there, we shall make the somewhat disconcerting discovery that consciousness resides nowhere in the neural chain. It can hardly be in the ear, for if the ear is isolated from the brain there is no awareness of sounds. For a similar reason it is not in the big eighth nerve, nor in the basal ganglia, nor even in the relevant temporal convolution. Each of these links is essential, but each may be intact without awareness of sounds if it is isolated from the rest of the nervous system. Something occurs in each link of this chain of neural events that is much like what occurred in the preceding one. In no link is consciousness found, yet something happens in the final one that is capable of being included in that form of integration which we call consciousness and in the relatively more permanent and more inclusive integration which we call mind. There is no necessity for this inclusion. It is more or less completely absent in sleep and under the influence of such narcotics as chloroform or ether.

A second question that might be suggested is not unrelated to the previous one. Are mental processes correlated with specific kinds of matter or with specific kinds of organization? If with the former, then, since there is no element exclusively in brain, the same matter outside the brain should guarantee existence of extraorganic minds. There is, however, no evidence anywhere that consciousness is dependent on a given kind of stuff, but rather on its peculiar systematization. If mental processes are correlated with the specific kinds of organization, then it is a privilege and a scientific duty to inquire what are the characteristics of that particular kind of unity and whether it occurs only in the cerebrum.

This kind of questioning is getting us ahead a bit even when it uncovers our ignorance, so let us advise our believing brother to press his inquiry and ask what kind of integration it is that is mental. On this we have some relevant data.

Characteristics of mental integration. We know many kinds of integration which give no indication of being conscious. Some of them occur in the nervous system. Of these the reflexes would be an example. We also know some of the characteristics of that kind of integration which is conscious. We can be reasonably sure that it is dynamic rather than static. By this we mean to say it progresses and has direction. It is like a chemical reaction rather than like an arch. One might be tempted to say that this dynamic integration is vital, but the one differentiating characteristic of vital chemistry is the reversibility of its self-regulated metabolism. That is to say, vital metabolism rebuilds what it breaks down. There is no known characteristic of mind that necessitates its exclusive correlation with reversible metabolism. On the contrary the only known neural reactions to external stimuli are dissociative.

Two further characteristics of mental integration that we know are, first, that it is cumulative, like a rolling snowball, and, second, that it is recapitulative. That it is cumulative is shown in the summation of memories to which each new experience adds something that was not there before, leaving its predecessors more or less intact. There are many examples of cumulative integration in our universe. One of the big ones is our solar system, which is constantly picking up stray waifs on its way between the stars. But the rolling snowball integration is not the most characteristic factor of mind. It is apparently essential to mentality, but not a differential attribute.

That mental integration is also recapitulative is shown in the nature of personal experience. Out of each present experience I not only get a new memory factor that builds itself on to a past, but the new stimuli revive that past more or less completely. It is I with all my past experiences who hears the noises in the adjoining room. It is I that adds them to my memory treasury and in that process I recapitulate the sum total of my past. These are the only characteristics of mental integration that seem fairly well authenticated. Mental processes are characterized by associative memory and apperception.

Synthetic minds. Suppose we had the engineering problem of planning a mental organism along these lines. The first task would be to plan for a few differentially reacting mechanisms. The primary ones we would call sense organs. The exact number and kinds of sensory reactions might be variable as they are in humans. The precise energy changes to which they might react are theoretically quite immaterial. The second task would be to integrate these primary reactions into a unit that preserves each new reaction by uniting it with the old in such a way that in each moment of total reaction the old is recapitulated. Now, if we had such a group of summative recapitulating reactions, however bizarre the materials might be, would we not have something so closely resembling remembering personality as to be indistinguishable from it? If, in addition, we could add that remarkable projection of the past into the future, which we call intent or purpose, our synthetic personality would take on spiritual significance.

If we think of consciousness, mind, and personality in this manner as a kind of integration whose objectively observable aspect is behavior, these concepts cease to be a matter of indifference in any scientific psychology. They represent its ultimate problems.

The purport of our questioning is not obscure. If we refuse to allow ourselves to be quite satisfied with the crude doctrine that mental processes are conditioned by brain action and insist on asking what kind of action in the brain actually conditions mental events, we follow a strictly scientific procedure that is not without its practical utility. Apparently, the answer must be that not the brain itself but some form of integration that may go on there is the real condition of mental processes.

The great hypothesis. There is now opportunity for one more related question. Is there any evidence that elsewhere in the universe there are conditions for a similar form of integration of any factors whatsoever?

Dynamic cumulative integration is a common phenomenon within the limits of our knowledge. Recapitulation is certainly rarer. It apparently recurs in the development of the embryo and possibly in some astronomical events. The hypothesis is not entirely fantastic that each embryo, as it grows and develops, recapitulating the history of its race, represents a conscious moment in some supraindividual mind; and that each developing nebula conditions an idea in some spirit of the universe.

Farther than this we may not go with our present scientific knowledge. One may say it is all nothing but a more or less plausible hypothesis. But it fits the facts better than any other that I know, and the way is apparently open for conscientious faith to say, "I will conduct myself as though the great hypothesis is true." This provides a place for its pragmatic sanction. Scientific investigation cannot be said to have developed a practical working faith. It probably never will. Its business is the investigation of phenomena. But it certainly does prescribe conditions for a practical working faith. The only disaster to faith and science would be the assumption that our present scientific information was complete and that further investigation was unnecessary.

The task we set ourselves to accomplish is done. In brief outlines and in systematic, though frequently in schematic, form, we have traced those conditions and consequences of human variability which our scientific explorations have made probable. We have found variability to be not a mere accident impeding the development of a true science of behavior, experience, and personality, but rather an aspect of the observable facts that is quite as important as their commonly noticed relative stability. The conditions of variability reach into the elementary processes of neural action until one may say that a differential variability is the hypothesis that best fits the observable neuropsychological facts.

The consequences of variability seem to reach into the very heart of mental reaction and mental development. One may say with some confidence that without variability there would be no mental development, either in the race or in the individual. The evidence suggests an even more fundamental relationship. One is tempted to substitute for the older materialistic doctrine, "Without phosphorus no thought," a newer psychophysiological generalization, "Without variability, no mind." But the variations must apparently be of definite and distinctive kinds, connected in a specific manner with systematizations of relative persistency.