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PROBLEMS OF HUMAN VARIABILITY[1]

When I was first introduced to the literature of experimental psychology, there was one characteristic, frequently recurring phase of the numerical expression of experimental results that commonly aroused a mild resentment. Along with central values, which were then almost universally expressed in terms of the average, there was usually a smaller numeral which bore the curious legend of "mean variation." As a student I had a suspicion that the legend had some connotation of disapproval and regret. In later laboratory experience, of course, that early resentment gave way to a complacent tolerance. The mean variation came to be a legitimate part of the game. But I never outgrew the suspicion of disapproval and regret. As the years have passed, this suspicion has grown into a conviction that contentment in the mass expression of human variations is not good science.

As my scientific interest developed into experimental investigations of my own, variability loomed more insistently and menacingly as a great barrier to real science. Notwithstanding all practicable care to preserve the constancy of stimuli, and notwithstanding the increasing reliability of recording techniques, the experimental shadow was never lost. It was only slightly reduced. Every effort to state the effects of experimental investigation in exact terms sooner or later encounters the same anomalous limitation. If, peradventure, in consequence of a great number of measurements and their statistical treatment, a point is reached where further data have relatively little effect on the central value, the fact of variability still remains to invalidate the application of that abstract central value to the next actual facts of experience. Apparently variability is quite as real as the central value. Notwithstanding our accumulated fund of painstaking measurements, there are conspicuously few dependable constants in psychology. For a science that seeks to express itself in terms of invariants the facts of mental life are woefully unaccommodating.

Two scientific experiences strengthened my conviction that variations in psychology must be taken seriously. The first was technical. In the study of fatigue, as well as in the study of the neuro-muscular effects of alcohol, the most conspicuous feature of the

[1] Address of the vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, Cincinnati, December, 1923.

relatively exact records was their variability. The surprising thing to me was that any central tendency at all was discoverable. No two successive eye-movements took the same time, followed the same path, or had the same beginning or end. No two successive lid reflexes, knee-jerks, word reactions or thresholds gave the same measurements. Identical records apparently did not occur. It seemed to me that if the experimental modification of neuro-muscular processes were to develop, some one must undertake the arduous task of studying the conditions and range of their normal variability.

The other experience was more theoretical. It developed under the dual influence of Sherrington and Verworn. Out of it came the belief that neuro-muscular variations were not artifacts to be statistically lumped and treated as though they were errors of measurement, but that they represented realities which might well be quite as significant as the conscious aims of measurement in which they occurred.

The practical importance of variability needs only to be mentioned. The capacity to learn by experience is fundamental to the healthy human mind. It is a measure of intelligence. Stereotypy in thought or action is a symptom of abnormality. Complete invariability of response is observed in no living organism.

Variability is a factor also in life's tragedies. The operation of a power-press is one of the more monotonous automatized occupations. Close escape of the operator's fingers is common. An extreme in the normal distribution of escapes means disaster. Doubtless all accidents may be reduced to terms of position in the normal distribution of human variations. The automobilist cuts a little too close. The machine worker brings his hand a little too near to the gears. The locomotive engineer thinks of extraneous affairs a little too absorbedly. If accidents were modal or close to the mode, working conditions would be intolerable. The prevention of accidents as an ideal requires a system of checks and safeguards that raises the entire distribution area of variations above the limits of safety. This seems fundamental to the theory of accident prevention.

Human variability has been regarded scientifically in three typically different ways. Historically, the first, and probably still the most widespread reaction of the scientific consciousness to the variability of human nature, is doubt as to the possibility of a science of the human mind. If I remember rightly, Kant held against the possibility of a science of psychology because of the impossibility of reducing mental phenomena to mathematical formulae. A consciousness of difficulty, however, like a consciousness of ignorance, is more apt to stimulate scientific endeavor than to paralyze it. Herbart, Fechner, Donders, Cattell, Ebbinghaus, Spearman, Thorndike and the long line of those who have sought numerical expression for mental events form an interesting and suggestive sequel to Kant's dictum. But there are still real differences of scientific opinion as to whether psychology may properly be called a science in anything but aim.

A second reaction to human variations has been to regard them as accidents and to express them in terms of the theory of chance error. This is the reaction in which psychology proceeds by analogy with the physical sciences. There is, however, an important difference between the statistical elaboration of a group of physical measurements and the elaboration of variations in a psychological experiment. In the former it is assumed that there is only one real value and that this reality, or an indefinitely close approximation to it, together with a statement of the probable error of the approximation, can be determined by the statistical treatment of the data of numerous more or less imperfect measurements. In the latter case—that is to say, in psychological experiments—even when one arranges for records of the highest technical reliability, the variations are not eliminated. They are, then, not mere accidents of measurement, but are inherent in the facts which are under investigation. The term "probable error" under such circumstances is an anomaly.

While the emphasis on unsystematized variability as such tends to skepticism or agnosticism with respect to the possibility of a science of mind, the search for mental invariants to be statistically elaborated from the data of experiment leads to artifacts. While some students of the exact sciences are still frankly skeptical with respect to the scientific character of psychology, those whose need of a knowledge of human nature is great, as the psychiatrist, the educationist and the personnel manager, point out that most psychological generalizations are relatively barren in the treatment of individual cases, or when applied to the complexities of actual life as they find it. There would seem to be possible a clear way out of the dilemma in a third attitude—namely, in the systematic exploration of the anomalies that are commonly grouped in some statistical measure of variability. Analogy from the psychology of individual differences would seem to justify the expectation that real variations are just as significant as uniformity, and that the investigation of human variations is an investigation of reality.

Much of the recent service of psychology in education, in mental disease and in personnel has been due to the readjustment of its scientific aims to the peculiarities of the materials with which it deals. Treating each individual as a special combination of capacities, experiences and accomplishments has been found to be vastly more efficient than treating individuals as though they were all alike. This service of individual psychology seems to have only just begun. It is reaching almost daily into wider fields of science and practice. But it is still handicapped by recurring popular demands for a system of stable invariants, such as mental age, experience, habits, instincts, sensations, memory types, emotions and the like.

Differences in the attitude toward human variability has resulted in some confusing differences of emphasis in the scientific accounts of some of the best known facts of our mental life. When the repeated stimulation is met successively by similar reactions, the product of the search for relatively stable objects leads to the doctrine of habit. In the traditional accounts of habit, variations are commonly ignored. When noted, they are treated as disturbances of the real process, as relatively insignificant accidents in the great tendency to human uniformity.

As every one knows, the facts of human-nature are never so simple as the schemata of habit imply. Probably no two repetitions of a human reaction would ever prove to be identical, if the records were complete and the units of measurement were fine enough. The stabilization of long training greatly limits variability, but even in the best-trained performances, like a musician's execution of classical music, no two instances seem to be exactly alike. Within the limits of virtuosity there are apparently instances approaching genius and moments approaching mediocrity, a best performance and a poorest one, probably never two that are identical. In less thoroughly standardized reactions there is an indefinite variety of transfer, substitution, elision and addition, a continuous evolution of new patterns in which the old patterns are partial conditions rather than archetypes.

On the other side of the methodological dilemma, the traditional doctrine of learning emphasizes the variations of reaction to a succession of similar stimuli. Quantitative investigations of learning show that invariant habit is never found in any adequate record of human performance. Habit functions as a limiting concept of learning. It is analogous to the hypothetical final plateau of development, which is seldom, if ever, quite reached in experimental investigation.

Analogously, the traditional scientific accounts of sensation, perception, memory, instinct and reaction time tend to overemphasize the hypothetical invariants, whereas practical needs and practical insight make it plain that invariability in human life is a myth. It is a great service of the modern German school to show that sensations as invariant elements of mental life do not exist and would be valueless if they did exist. We long ago abandoned the hypothesis of mental faculties, but the spirit that postulated them still survives in our traditional account of habits, reflexes, instincts and ingrams, as well as in our efforts to test human capacities.

It is a sign of hope that wherever variability has been investigated, as, for example, in the fatigue curve, the curve of forgetting, the curve of work, the effects of drugs and climate, motivation and the unconscious conditions of consciousness, the results have regularly increased our working knowledge of human action, although at the cost of simplicity of our scientific generalizations. Contrariwise, the treatment of mental facts as relatively fixed constants or systems of objects has led to grave errors in the history of psychology. It has been the tendency which underlies the recrudescence of hypothetical faculties, types, instincts and atomic sensations. The description of mental facts as more or less complexly conditioned integrative processes keeps close to the facts, even if it fails as yet to satisfy our demands for scientific simplicity.

The first great problem of human variability—namely, what to do with it—seems to me to have only one answer that is congruent with a true scientific attitude. As far as they prove to be real we must treat them as real without apology and without regret. The least satisfactory treatment would be to leave them massed in a statistical penumbra.

Doubtless law constitutes the aim and end of all scientific thinking. Nothing that I have in mind is opposed to that generalization. The difference between a science that seeks a relatively stable system of things and their qualities and a science that would investigate variability is not merely that the equations of the latter are more complex. The latter regards the changing complex sum-total of conditions not simply as a changed system, but as a system of changes. The former asks, What are the unanalyzable elements of mental life? What is a given person's reaction, mental age or intelligence quotient? What is his span of attention, etc.? The latter asks, What are the differential consequences of successive stimulation? How is a reaction affected by antecedent and concurrent processes? What are the integrative processes that we call attention, consciousness and personality? This is what some of us mean by dynamic or conditional psychology.

The General Physiology of Sensation

The moment one adopts a general scientific attitude toward the fundamental problem of human variability a host of special and particular problems assume new significance. Between general physiology and the psychology of sensation there is a great gulf, especially in the conception of the stimulus. Our standard psychologies still commonly state that the stimulus for visual sensation is light. The fact that visual sensation disappears if light falls continuously on any part of the retina is laid to fatigue or adaptation. The physiological fact seems to be that living tissue, whether it is sense organ or nerve muscle preparation, is excited only by a change in its external vital conditions. Light is not the stimulus. It is change in illumination that excites reaction. While the sense organs seem to be peculiarly differentiated for long-continued reactions to single changes, a constant sensation from constant energy is never discoverable in any concrete instance. The normal invisibility of the retinal blood-vessels illustrates the importance of change in the concept of the stimulus. The eye-movements during continuous fixation are almost equally suggestive. A law that correlates energy change with a uniform sensory intensity would be of equal theoretical importance to the Weber-Fechner law.

The general physiology of sensation has yet to be written. It never will be as long as we cling to the delusion that sensations are stable entities of the mental life. I believe that we need a thorough-going revision of the tradition of sensation from the standpoint of the general theory of irritability.

The Problem of Repeated Stimulation

A second group of problems of human variability of scarcely less fundamental importance concerns the effects of the repetition of stimuli. When the patellar tendon is struck twice in rapid succession, a curious phenomenon is observed. If the muscle responses are measured accurately in terms of muscle thickening, the second response equals the first only very rarely. We know little enough about the facts, still less about the reasons. Briefly, it appears that if the second knee-jerk stimulus follows the first within half a second, it finds the reflex arc in a condition of decreased irritability, called the relative refractory phase.

First discovered in the heart muscle, refractory phase appears to be a universal phenomenon of irritable tissue. Its characteristic duration and some of its modifications have been shown in nerve and in some of the reflexes.

Several years ago, with the support of the Ernest Kempton Adams Fellowship, I undertook an exploration of some of the elementary conditions of human variability. Neither the considerations that influenced the selection of processes, nor the methods of stimulating and recording them need now detain us. Some of the results, however, are directly relevant to our present discussion.

In all the processes which were measured there appeared more or less clear analogues of the relative refractory phase of the knee-jerk. These analogues are of especial theoretical interest in the unsystematized and systematized cortical reactions. In the latter they play an amazing rôle.

Refractory Phase of Voluntary Reactions

There is no standard technique for investigating the refractory phase of a cortical reaction. There is no general agreement that such a phenomenon exists. On the contrary, one might suppose that there is no limit to the frequency of repetition of voluntary reactions. Theoretically, however, it is highly improbable that refractory phase is an exclusive phenomenon of the lower neural arcs. The evidence from general physiology would be entirely opposed to such a conjecture. Refractory phase seems to be a universal phenomenon of sensitive tissue.

I believe that something very much like refractoriness regularly appears in cortical reactions in spite of appearances to the contrary. Take a hypothetical case for illustration. If several visual stimuli were to follow each other at a fixed interval of 0.5″, reactions to the recurring stimuli undoubtedly would not be retarded or diminished by the fact of the sequence. On the contrary, the latency of reaction would tend to diminish to a vanishing point. It should be noted in this hypothetical case, however, that the rhythmic succession of identical stimuli is not merely a repetition of the first stimulus. It becomes part of a total stimulus situation which is responded to as a whole by a systematized series of acts.

In such a case, a reaction elicited by the systematization before its natural stimulus has been given will not be revoked with a normal reaction latency by its natural stimulus. When the normal stimulus appeared, it would not be reacted to at all. It would fall within the relative refractory phase of the anticipatory reaction. So anticipatory reactions often displace normal responses and confuse or inhibit them.

This tendency of an anticipatory systematized reaction to inhibit natural reactions is well illustrated in the pursuit reactions of the eye to a moving pendulum. When the pendulum begins to move, there is a single reaction latency of approximately 0.2″. Thereafter there is no observable reaction latency. A normal person picks up the rhythm of the pendulum swing and follows it with adequate ocular pursuit movements until fatigue sets in. That is to say, the successive swings of the pendulum are not reacted to as isolated events at all. Anticipatory reactions to the rhythm of the pendulum inhibit the discrete reactions with normal latency.

There is some evidence that a similar relative refractory phase exists not only in experimental reactions, but throughout normal mental life. If there were no relative refractory phase of the cortical processes, the last experience, whether objectively or subjectively conditioned, should theoretically tend to reiterate itself in endless repetition, on the basis of recency. That is to say, without something like a refractory phase, an idea or memory image would maintain itself endlessly in consciousness until some stronger stimulus eliminated it. This is directly contrary to the fact. Only occasionally does a mental image persist so tenaciously that one can think of nothing else. Such instances are conspicuously rare in normal mental life and are usually conditioned by emotional reinforcement.

Somewhat more often, motor acts take on the characteristics of stereotypy. There seems to be real difference between motor and sensory refractoriness. Witness the difference in the effects of continuous rewhistling of the same melody on the whistler and his auditors. A good story or joke is perennially good to the one who tells it, but it soon becomes intolerable to the one who is forced to listen to its repetitions.

Art differs from other forms of experience in the brevity of its relative refractory phase and our readiness for repetition. A joke is stale on repetition. A great work of art is enjoyed over and over again. This may be due to emotional reinforcement, though we do not yet know how pleasure could operate to diminish refractory phase. We do not ordinarily care to read the same novel twice. We tend to avoid the immediate repetition of a walk or any other experience. This antipathy to immediate repetition shows itself in the craving for diversity of experience, for change in work, in dress, in recreation. One's appetites and desires follow the same general scheme. The recency of an experience acts to strengthen the disposition for its return, but it also operates to inhibit its immediate repetition. Any calamity may be preferred to being bored by endless repetition.

Our difficulty in experimentally demonstrating the existence of a refractory phase in voluntary reaction we have already mentioned. It appears to be impossible to apprehend a succession of discrete identical stimuli as disconnected units. They naturally and inevitably fall into more or less complicated rhythms, groups and totals. Rhythm, generalization and even science itself express this tendency. It is certain that our minds refuse to keep separate a mass of isolated repetitions. It is fairly clear that the repetition of a word stimulus is not a simple repetition. The second instance differs from the first in that it tends to form a series with the first instance. Apparently the impossibility of reacting to a series of identical stimuli as though they were isolated events manifests itself in system building and in systematic memory. We have experimental evidence of this function both in our word reactions and in the memory experiments.

By a peculiarity of our exposure apparatus in the word reactions an exposure once made was permanent until the experimenter reset the exposure mechanism. This circumstance will doubtless be criticized by many of those who are learned in tachistoscopic procedure. Protracted exposure has seldom been used in reaction experiments. This is not the place for an elaborate defence of our technique. One may say in passing that it corresponds more nearly to normal reading than very short exposures. From the standpoint of our present discussion the significant fact is that notwithstanding the long exposure the word was regularly spoken but once. There was abundant opportunity to repeat it many times. Why was it said but once?

Analogous inhibition of repetition occurs in normal reading. The just fixated word does not disappear immediately after one has read it. On the contrary, it remains visible in the peripheral visual field for some time, but it is seldom read a second time and practically never reread without definite intention and refixation. Just as it remains more or less indistinctly visible in the visual field, it also remains more or less indistinct in the fringe of consciousness. Reading requires a certain persistence in consciousness of what one has just read. Residual effects of this sort may last some time.

Both in our experiments and in normal reading the tendency against repetition may be regarded as a consequence of the systematic connections in which the stimuli appear. It would be entirely possible to arrange for a different kind of reaction in which the word was repeated "n" times after exposure. Such a complicated reaction must, however, not be confused with the repetition of reactions. When a complicated reaction of this sort was completed (after "n" acts), it would not naturally be repeated except in response to a special demand.

It is in consequence of the general disinclination to repeat reactions that words and phrases are not commonly reiterated in close juxtaposition in good writing. To do so is not conducive to fluent reading. A mechanism for protection against useless repetitions is not difficult to imagine. The circumstances clearly require some agency that operates on the analogy of a relative refractory phase. If it is not a relative refractory phase, one would have to postulate some other mechanism with identical characteristics and functions.

REFRACTORY PHASE IN THE DEVELOPMENT OF REACTION SYSTEMS

The most complex cortical reactions which were included in our program consisted of a group of words in process of systematization. This would be commonly classified as memorization. In the familiar forms of memory experiments, series of letter groups or words are commonly presented one at a time by a revolving drum. When memorization is complete, speech reactions within the series no longer need to follow the original stimuli. The actually effective stimulus is not the visual presentation of the word which is spoken, but some more or less remotely connected fact of experience. By a process of association or conditioning, 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 commonly held to be complete.

The method of complete learning is time-consuming. Moreover, it usually fails to show the variations in reaction which were the main problem of our 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 learning would obviate the necessity for complete learning.

Our method of eliciting and recording these reactions to word series was analogous to the traditional method of paired associates. In our experiments, however, the measured associations were continuous instead of being merely paired. This made the method a kind of multiple prompting. In brief, it consisted of the following essentials: series of twelve four-letter substantives were 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 backwards was recorded by means of a voice-key and an electric marker on 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 systematizing process. Decrease in the reaction time thus became a measure of the memory factor in the reaction. When a series was completely memorized, all reactions were anticipatory. That is to say, each word was spoken before any part of it was exposed.

In their bearing on our main problem these experiments are of peculiar significance. They constitute measurements of a complete cortical resystematization. It is conceivable that the variability of cortical reactions follows quite a different course from that of the lumbar reflexes. Just how much the two really differ was part of our main inquiry.

Throughout the experimental series paired stimuli were presented for both knee-jerk and lid-reflex. Notwithstanding hundreds of repetitions of these paired stimuli the second stimulus was never anticipated. There was no shortening of the reflex latency. On the contrary, the only change in reflex latency seemed to be in the direction of increasing it. This was true both for the knee-jerk and the lid-reflex. A definite experimental effort to develop a reconstruction of the reflex arc in the direction of a crossed knee-jerk was a complete failure. In spite of hundreds of cases of simultaneous stimulation true crossed reflexes were never elicitated. Similarly, simultaneous stimulation of the right and the left final common paths to the quadriceps by voluntary effort and by normal reflex stimulation, respectively, also failed to produce a reconfiguration of the mechanism of the knee-jerk. The lid-reflex was somewhat less refractory to experimental reconditioning. It was possible to produce a lid-reflex of an approximately normal latency by a knee-jerk stimulus, after a considerable number of simultaneous excitations. Partial cortical resystematization occurred in the memory experiments after a single repetition of the series.

A condition of cortical refractoriness similar to that of the speech reactions is discoverable in the effects of these memory experiments. It will be remembered that the stimulus for speech reaction was the exposure of a word which was brought suddenly into place 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. It is noteworthy that after reaction occurred the previously adequate stimuli no longer functioned as stimuli to reaction. They found the reaction system in a relative refractory phase.

In the memory experiments, as in the word reactions, the refractoriness obviously depended on the particular arrangement of the experiment. The set incident to the task determined both the reaction movement and the refractoriness. 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. Refractoriness in this case would have been reduced to the relatively short refractoriness of the motor system. The instructions produced a neuro-psychological system whose refractoriness was of a peculiar order. 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 task overbalanced the effects of recency. We would reemphasize the presumptive importance of this in practical life. Without it all our lives might be spent in the repetition of the first chance experience. In other words, refractoriness to repetition appears to be the first condition of resystematization. Where such refractoriness is low, as in echolalia and in stereotypy, learning is at a minimum.

We have already noted evidence in the reflexes that refractoriness to the just previously given stimulus may develop into an increased sensitivity, if the interval between stimuli is carefully adjusted. This zone of increased sensitivity is relatively unexplored, and its relation to the refractory phase is practically unknown. Both are obvious residua of previous stimulation. Increased sensitivity lasts longer than the refractoriness. It may well be that the heightened sensitivity is a positive condition for resystematization, while refractory phase is a negative condition, and that each condition has its own temporal incidence.

THE PROBLEM OF CONCURRENT PROCESSES

The problems of variability in those reactions which are incident to the repetition of identical stimuli and their systematization are enormously complicated in actual life by various concurrent reinforcements, inhibitious and controls. These conditions of variability are practically unexplored.

We are coming to believe in a natural daily rhythm. But its causes, extent and incidence at the various levels of the cerebro-spinal system are unsolved riddles. If there are weekly, seasonal and yearly rhythms, they are even less well understood.

There are better known respiratory and vascular rhythms, which complicate all threshold experiments and limit the effective delicacy of recording devices of the motor processes. However influential these complications of reaction may be, I conjecture that the most important theoretical implications of our experimental attack on the problem of variability concern those variations that are produced by the interaction of the neural delay paths in the higher neural levels.

One of the surprises of this experimental series was the discovery of an arbitrary depression of the knee-jerk by the voluntary depression of the motor system of the thigh. The hypothesis that this was due to a decreased tonus has utterly failed of verification by the most delicate technique that I could devise. One can not arbitrarily dismiss the hypothesis of a centrally aroused decrease of irritability of the reflex arc. The opposite is undoubtedly true. An increase in the irritability of a reflex arc may be centrally conditioned. Voluntary reinforcement of the knee-jerk may produce every possible degree of quadriceps thickening. Contrary to expectation again, this reinforcement may appear as a smooth contraction without noticeable break between the reflex and the voluntary phase.

Controlled reinforcement occurs in the reflex ocular compensation to rotation when the eyes are open. Records from closed eyes show that the reflex compensation may occasionally be entirely adequate—that is to say, it may adequately compensate for the precise angle velocity of rotation. Usually, however, the reflex compensation is quite inadequate until it is controlled by the more accurate visual data of rotation. In case the vestibular and visual data are contradictory, the latter eventually win the competition and control the final common path.

Analogous transfer of the control of the eye-movements from primitive to fine sensori-motor systems occurs in coordinate compensatory eye-movements.

The data are sufficiently clear and sufficiently numerous to permit generalization. In the muscle reflexes and in reflex ocular compensations, reflexes merely initiate motor responses. They have the practical advantages of a low latency response in a presumptively useful direction. That initial response may be and usually is quite inadequate to meet the situation which aroused it. The subsequent fate of the reflex act, its control, reinforcement, inhibition or reversal depends on higher systematizations and the elaboration of data that do not enter into the reflex.

In the case of the knee-jerk this scheme provides for a reasonable understanding of an apparently imbecilic reflex. Any sudden stretching of the muscle such as would occur if one unexpectedly stepped off a low platform evokes reflex contraction within 40 sigma. Whether that reflex shall be sustained, inhibited or reversed depends on the slower cortical elaboration of the available data.

The picture is not merely a pretty mechanism of great flexibility and efficiency. It points towards a paradigm of all instinctive action. The reflexes represent the simplest and most mechanically persistent of instinctive acts. If they regularly meet rivalry, competition, suppression and reinforcement from superior systems, the more fluid instincts might still more reasonably be expected to meet analogous modifying circumstances and controls. It looks as though the reflexes and other instincts might well reduce to primitive initial responses in predetermined directions. I doubt if their supposititious driving power is more or less than that. In the simpler and more mechanical instinctive acts there is clear evidence that higher centers regularly assume control of the initiated response without disaster, pathological implication or any other disturbances except a moment of systemic rivalry and competition.

The data which I have laid before you are necessarily very limited. I can not hope that all the details are convincing, but I shall be disappointed if we can not all agree on the fundamental thesis that human variations are worth rescuing from the scrap heap of mass statistics by concerted and systematic attack.

I hope, furthermore, that most of us would accept the related proposition that the search for integrative processes is quite as promising as the search for mental elements, thresholds, reflexes, instincts, complexes or any invariant artifact whatsoever, whether introspective or behavioristic.

Raymond Dodge

Wesleyan University

JOHN MAXSON STILLMAN, 1852–1923

John Maxson Stillman, professor of chemistry and vice-president emeritus of Stanford University, was born in New York City on April 14, 1852, the son of Dr. Jacob Davis Babcock Stillman and Caroline Maxson (Stillman). He was graduated from the University of California in 1874, and received the degree of Ph.D. from his Alma Mater in 1885. A student in chemistry in Strassburg and Würzburg, in 1875 and 1876, he returned to this country as instructor in chemistry at the University of California, where he remained until 1882. He then went to Boston as chemist of the Boston and American Sugar Refining Companies until 1891 on the foundation of Leland Stanford Junior University, in which institution he served as professor of chemistry for twenty-six years, becoming vice-president in 1913, and retiring under the age limit as emeritus in 1917. He was the author of numerous articles on chemical matters, covering especially the organization of certain vegetable compounds, the ammonia compounds of inorganic chlorides, the molecular lowering of the freezing point in diphenylamin and naphthylamin, the precipitation of calcium and magnesium in the purification of water, the poisonous elements in whisky. Most of these impressed him as little worse than ethyl alcohol itself. In his later years he made a specialty of the history of chemistry, an important piece of research being on the life and work of Paracelsus (1921). Lately he completed a volume on the early chemists and alchemists, soon to be published. He was a member of the American Chemical Society, of the American Institute of Chemistry, of the Deutsche Chemische Gesellschaft, and a fellow of the American Association for the Advancement of Science. He died at Stanford University, December 14, 1923.

Such is the condensed academic record of one of the most scholarly of chemists, most devoted of teachers and most lovable of men. As an intimate associate for half a lifetime, I can speak feelingly of his strength and virtue, and of the indebtedness to him of the new university through all its early growing pains.

At the opening of Stanford University on October 1, 1891, Dr. Stillman was one of the fifteen teachers chosen at the modest but ambitious outset, one of the three senior members in a remarkable group, who remained with it for a generation. Without invidious comparison, I may note these members of this first faculty who have stood steadily in the first rank in respect to scholarly attainments, productive work, educational wisdom and friendly helpfulness. These were the late John Caspar Branner, professor of geology, first vice-president and second president; John Maxson Stillman, professor of chemistry, the next vice-president; and Charles David Marx, professor of civil engineering, vice-president after Dr. Stillman's retirement as Emeritus, in 1917. All three were unusually capable in each of the respects I have enumerated, but Dr. Stillman's especial virtue lay in the line of wisdom. No better faculty man ever helped a university and the need of sound judgment and wise administration was never greater than in the six lean years ("the long fight") which followed the death of the founder (1893 to 1899), when the entire prospective endowment was tied up by wanton litigation. There is an Albanian proverb, "Open a cask of sugar and flies will come all the way from Bagdad." The most insistent of these litigants was the United States Government itself, which claimed the entire endowment in view of the indebtedness of the Central Pacific Railway, not then due and which was paid in full with interest as soon as its bonds had matured. The government was three times non-suited in Federal Courts—at last in the Supreme Court, the university meanwhile living from hand to mouth under conditions of supreme difficulty.

It may interest the thousands of Dr. Stillman's friends and students to know that of all the professors at Stanford he was the only one in any degree selected by the founder himself. Governor Stanford said to me that his old friend, Dr. J. D. B. Stillman, has left a son who had been a teacher in the University of California and was then a professional chemist, living at Brookline, Massachusetts. With this hint, I visited Boston to see Dr. Stillman, and being thoroughly pleased, I offered him the chair of chemistry. This he as promptly accepted, declining to consider an advance in salary for his company, on the ground, as he said, that "it would only tend to confuse his mind." We thus secured (as I have elsewhere stated) "one of the wisest teachers I have ever known and one of the most thoroughly beloved. His dear wife (Emma Rudolph Stillman), I may add has ably seconded him in every relation, and few other Stanford homes have contributed as much as theirs to the social well-being of the community." (Days of a Man, I, 398).

David Starr Jordan