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EDWARD CHACE TOLMAN

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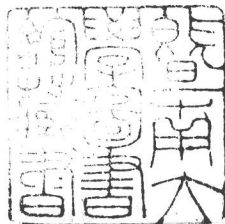
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BEHAVIOR AND PSYCHOLOGICAL MAN

Essays in Motivation and Learning

BY EDWARD CHACE TOLMAN

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FOREWORD

[This foreword was prepared from suggestions and comments of several of Professor Tolman's colleagues and students. It is hoped that what it lacks in coherent style because of this mongrel parentage is compensated for by the variety and vitality of its understanding and appreciation.]

THIS COLLECTION of papers by Edward Chace Tolman, sponsored by a group of his former students and present colleagues, is not only an appropriate commemoration of his more than thirty years at the University of California but an important psychological document which shows the development of his systematic theory from its early neonatal stirrings to its later more consolidated but still-maturing phases. The development of a system has more than historical interest. It throws light on the very processes by which knowledge is expanded, integrated, and given meaning.

Tolman's complete bibliography contains over eighty items. The problem of selection was therefore a considerable one. Since it was felt, however, that he himself was in the best position to make the most meaningful and useful selection, he was persuaded to pick out the articles which to him meant steps in the development of his theoretical system. The result can best be characterized as a "Progress Report." Tolman is a system builder who not only theorizes, but also experiments. This means that his work is never done. He is constantly accumulating new data in his own laboratories and studying the data of other workers. He is continually adding and subtracting concepts, revising propositions, reorganizing ideas, and manipulating them creatively to see what new gestalten they might form. Tolman's now classical *Purposive Behavior in Animals and Men* was published in 1932. But that book was a milestone, not a capstone, for he has continued to expand, revise, and reformulate

his system; and these papers, which include several systematic articles written before the book's publication, should enable the reader to glimpse, with Tolman, the nature of the changes which take place in a systematic purposive behaviorism as it is modified to encompass the data of an expanding field.

This collection of papers is not only as a significant scientific publication, but also a revealing human document. Tolman is one of those rare beings among system builders who has a sense of humor about himself and his theorizing. System building for him is not a grim business. It is a happy, gay, creative activity, and his papers express all of this to the full. No matter what the subject, how abstract the treatment, his wit, humor, magnanimity, and tolerance are written into each analysis. He is constitutionally incapable of writing dogmatically or of publishing a polemic. The papers in the present book not only trace explicitly the history of significant ideas but also portray a person with grace.

Tolman's system is characterized by two major attributes: the first of these is the breadth and all-inclusiveness of his psychology. Above all else, he has insisted that behavior is multidetermined and that an adequate system must encompass all psychological data. He believes that a theorist's job is to try to describe and account for the entire field which lies within his discipline, and not to restrict it arbitrarily to the more amenable areas. Tolman has never been interested in writing a "small scientific theoretical system"—he has always sought the complete formulation. This has meant the witting rejection of attempting finalistic formulation at this stage of the science, and the characteristically cheerful acceptance of a programmatic role.

The second major characteristic of Tolman's system building—his use of intervening variables and other hypothetical constructs—has been decisive in forcing theorizing in psychology out of peripheralism and into centralism. The genius of his theorizing lies in his clear understanding that attempts to deal with correlations between stimulus field and resulting behavior can be understood only in terms of postulated intervening variables, hypothetical constructs, dynamic central processes. The problem of science, for him, is not that of seeking correlations but of seeking the systematic meaning of the observed correlations.

The papers in this volume testify to Tolman's respect for the multifaceted data of psychology. Some give the impression that Tolman is primarily a learning theorist; others, that he is primarily concerned with the problems of motivation; still others, with the problem of perception, or clinical experience, or with social psychological problems. If these papers are analyzed in terms of theoretical approaches rather than in terms of problems, a similar breadth is seen, for in some of them are overtones of Gestalttheorie; in others, of behaviorism, or of operationalism, or of purposivism, or of Freudianism. And yet, when all the papers have been read, one cannot leave this collection without a feeling of closure, of having read through a carefully thought-out, consistent, provocative, and original integration of concepts—in other words, a scientific system.

Although Tolman is a psychologist who is interested in all behavior and all psychological problems, his experimental work has been confined primarily to the rat; so it is perhaps appropriate to add here a parenthetical note about him as a "rat psychologist." His use of rats for the experimental mediation of theoretical problems in part reflects the era in which he developed as a psychologist; in part, it represents his own strong belief that the concept which cannot be studied by natural-science methods and demonstrated in lower animals is not a useful or valid concept for a basic science of behavior. And like the anthropologist who establishes his basic principles through the study of relatively simple primitive societies, the psychologist finds simple organisms to be useful tools. In humorous appreciation, Tolman flaunts his rodent orientation by dedicating his major work, *Purposive Behavior in Animals and Men*, to *Mus norvegicus albinus*. The rank and file of contemporary American psychologists, mainly interested in the applied fields and the complexities of man's environment, have tended to regard theory-oriented experimentation with rats as "rat experiments" with little of human relevance. They have failed to appreciate, until recently, when the need for systematization to integrate the mass of empirical data forced it upon them, the difference between the phenotypic nature of data and its genotypic meanings in the hands of a creative theorist. Tolman, the "rat psychologist," can truly say with Terence, "*Homo sum: humani nihil a me alienum puto.*"

That nothing human or psychological is foreign to Tolman is very clearly illustrated by the following historical note which was prepared by one of his students.

In noting Tolman's theoretical experimental work on "universal" or "normative" psychological laws, one should not forget that he was among the first animal psychologists to make extensive use of the methods of differential analysis. The importance of differential psychology is now accepted so much as a matter of course that one is likely to forget that around 1920 the really dignified and important psychology was experimental, of which animal psychology was really a psychobiological offshoot, and that anyone working with tests and measurements, statistics, clinical psychology, or personality carried the taint of being an "applied" psychologist or worse, and, if a statistician, was regarded as a sort of glorified clerk.

Those who then worked in this field can attest to Tolman's active support of study in these areas. He supported it because, as always with Tolman, everything that happens has to be accounted for—in fact, he *had* to get these facts, viewpoints, approaches, into his system. He was one of the first, if not the first, to study hereditary differences among rats in maze ability, and substantial studies of heredity by his students spring from this interest. He was one of the first to make a statistical study of the reliability of individual differences among rats in learning ability.

His systematic thinking in this area at this time came to a head in the final section of his book, *Purposive Behavior in Animals and Men*, where he attempted an integration of differential and normal psychology, one of the few such systematic integrations in psychological literature. His active sponsorship of quantitative methodology contributed to the University of California's early development of a balanced emphasis on measurement in its curriculum and research laboratories.

Clinical psychology and personality psychology are today in the ascendance. But twenty-five years ago, under the dominance of experimental psychology, clinical psychology was generally considered as a rather disreputable field—something with which psychology unfortunately had to suffer because of practical and social pressures. Tolman never shared with other respectable psychologists these experimento-centric prejudices. One has but to read his earlier papers oriented around the nature of motivation to note that he long ago considered the problems of the clinician and personologist to be *necessarily* a part of the field of psychology, and he has given active and understanding support to the development of this field.

As for Freud and Freudian concepts, this was for years an area of downright taboo. Yet students and associates have never heard Tolman offer disparagements of Freud and psychoanalysis. His questions have always been: How must we fit into a systematic body of psychological

thought the dynamic mechanisms these people are concerned with, disregarding the rather colorful language they use to describe them? How can we, specifically, frame critical experiments better to study and describe them? An examination of the writings of Tolman and of the many experiments by him and by his students show plainly the early influence of these then-tabooed types of thinking and work.

Tolman's early acceptance and support of the thinking and work in these *verboden* aspects of the field of individual differences, clinical psychology, Freudian theory, etc., is not mentioned here in order to reveal magnanimity, tolerance, permissiveness, and intellectual courage—though, these attributes he possesses in high degree. A measure of his stature is that he grasped at once the essential relevance of these subject matters, and was soon busily at work eagerly absorbing them into a broad, systematic treatment of psychology because they simply *had* to belong.

Seen from the point of view of his appreciation of various theoretical approaches rather than from that of his responsiveness to all kinds of data, Tolman's theorizing is the first major systematic effort based on a clear recognition of the compatibility of the "Gestalt," "depth," and "behavioristic" approaches. This contrasts most sharply with the traditional cleavages in psychological theorizing—cleavages which still exist for many. He was quick to make use of the many significant contributions of all three major systematic efforts in the construction of his own original and provocative system. A brief comparison of Tolman's approach with the other three illustrates this convincingly.

The intuitionists, or "understanding" psychologists, have agreed that, although the natural-science approach may be adequate for the study of lower sensory functions, it most certainly could not do for the study of higher mental processes, such as insightful thinking, or of personality. Tolman refused to accept such a dichotomy and has demonstrated the fruitfulness of attacking the higher mental processes experimentally and in a natural-science manner. Although such groups as the various denominations of the Gestalt persuasion have not explicitly subscribed to the cleavage of the "understanding" psychologists, and although they too have applied themselves to experimentation, quantification, and law finding in the higher mental processes, they have tended to shy away from the study of these processes through analysis of behavior. They have to a con-

siderable extent remained introspectionists or phenomenologists in principle. Tolman has accepted many of their molar and field concepts, but, experimentally, remains a behaviorist rather than a phenomenologist. Because Tolman is so convinced of the multiplicity of variables which determine behavior, his theory, unlike that of the more orthodox Gestaltists, is just as concerned with motivation, personality, and learning as it is with cognitive and perceptual factors. Thus, in lieu of the concept of Gestalt, he has invented the more functional concept of sign-gestalt.

Tolman is much more given to psychological constructs in his system building than to physiological ones. In this, he is much closer to the depth psychologists than to the Gestaltists. Unlike the Gestalt psychologists and like the depth psychologists, Tolman does attempt to deal with genetic problems, but unlike the depth psychologists, he does so without falling back upon purely historical answers to systematic questions. He seems to have accepted many of the dynamisms of the depth psychologists but has balked at their constructs such as "ego," "ego-ideal," etc. More parsimonious in listing and classifying needs than they, and less concerned with layers of personality, he has addressed himself more directly and more clearly to the conceptual representation of the differences between enduring and momentary structures. And in this he again departs both from Kurt Lewin and the more orthodox Gestalt psychologists with their heavy emphasis upon situationalism.

In elaborating his system, Tolman has, of course, found it necessary to refashion behaviorism. As a behaviorist and as a "rat psychologist," it is Tolman's paradoxical contribution that he has returned man to psychology by insisting upon molar behaviorism, purposively organized, as the unit of psychological analysis. He gave back to psychology its mind, lost through the mischief of a naïve behaviorism, by inventing a method for inferring mental factors from behavioral events which has both scientific respectability and philosophic sophistication. And finally, it must be noted that his concept of the sign-gestalt—a concept which is basic to his whole system—is one of the major theoretical forerunners of the current dynamic perceptual school.

This eclecticism of Tolman's approach to psychological data and theoretical orientations is made possible primarily by the second

major characteristic of his system—his free and creative use of intervening variables or hypothetical constructs. At several points in his publications—most clearly in his American Psychological Association presidential address which is reprinted in the present volume—Tolman has demonstrated that psychological science must proceed by stating and manipulating a system of independent variables of which behavior is a function, and that basic to an understanding of the functions of these independent variables are hypothetical constructs which intervene between stimulus field and behavior. But, reflecting his behavioristic orientation, he has always insisted on seeking for an operational definition of these constructs. Tied in with both an independent variable and a construct is the necessity of citing the defining experiment in which the independent manipulation is made and which justifies the hypothetical construct. It is only because of his faith in the centralist approach that the courage for an operational redefinition of higher-function terms can be mustered. And Tolman and his students have succeeded, in experiment after experiment, in redefining such terms as “purpose,” “insight,” “hypotheses,” “expectations,” etc. It should, however, also be pointed out that his search for an operational definition does not enslave him to operationalism and does not lead him to the sterility of the extreme operationalist who would see in a construct nothing but a set of pointer readings.

One very important aspect of Tolman's stress on generality and centralism is seen in his treatment of the learning problem. As a colleague points out:

In the special field of animal maze learning, Watson's classical behaviorism of the 1910's and the 1920's with its search for “the” locus of learning—thought to be sensory or motor, but in any event peripherally channeled and focused—has given way to a recognition of the generality, and thus of the central character, of the learning mechanism. This progress was carried by Lashley and his group, and by the “molar” behaviorism of Tolman and his collaborators.

To point up the nature of Tolman's contributions to learning theory and to clarify what is meant by his generality and his centralist approach to learning (so important for educational psychology as antidotes for Thorndike's specificity), it is necessary to examine, briefly, his reasons for rejecting the two theories of learn-

ing which were dominant at the time he wrote *Purposive Behavior in Animals and Men*—the conditioned-reflex theories of Pavlov, and the connectionist theories of Thorndike and others. Tolman, in discussing the conditioned-reflex theories, points out that one of the basic difficulties with such theories lies in the fact that the learned response is practically never the same as that made to the unconditioned stimulus. He has never let anyone ignore the fact that the learned response varies appropriately with the situation. His emphasis on the adaptive and changing character of the learned response distinguishes his viewpoint from that of all the stimulus-response psychologists. Of the latter he might say that they seem so absorbed in computing habit strengths, momentary effective reaction potentials, and the like, that they fail to notice the qualitative changes that occur during the learning process.

Tolman's criticism of the connectionist theories is based primarily upon experimental data on latent learning. Numerous experiments, he asserts, suggest that learning is not a matter of direct and immediate connections between stimuli and responses, but that the organism, instead, learns "what leads to what." Tolman's interest in the adjustive, changing, and generalized character of the learned response, together with his demonstration that what is learned is only partly revealed in the performance of the organism, form the basic tenets of his theory of learning. Experimental data made it necessary for him to seek for the answer in such hypothetical constructs as "expectations," "hypotheses," "sign-gestalten," "cognitive maps."

Tolman's viewpoint has produced a profound change in our way of thinking about learning theory. Before we can proceed to formulate any quantitative laws of learning, we must first make sure that we know the character of the learned performance, unearthing the central processes which determine the changes in the character of the learned response. In the study of human problem solving and thinking such an understanding of the cognitive structures underlying the observed behavior is essential, if we are to gain any insight into the nature of human thought. The recognition and insistence that it is this problem which comes first, and the invention and experimental use of techniques for solving this problem, constitute perhaps Tolman's greatest contribution to contemporary learning theory.

The ingenious use of hypothetical constructs has not been limited by Tolman to learning problems. He has applied this technique to problems of motivation, to social problems, to personality problems. His "intervening variable" has become one of psychology's most powerful conceptual tools and has made it possible to make a fresh approach to all of psychology's problems and to think more clearly about those mediational processes which are involved in man's commerce with the world about him.



No assessment of Tolman the scientist would be complete without at least a brief note on Tolman as a teacher. It is in that capacity that most of us have known him best, and it is in that role that we have seen him display so clearly those attributes of character and mind which can be seen in the papers in this collection. From a colleague who has watched his ways with students over the years comes this statement about him and his teaching: "His scientific and personal flexibility, his youthful zest and energy for ideas, for work, for friendships have created students who, while devoted to him, are not devotees. Instead, they have largely gone into fields far removed from rat experimentation, and most of them have developed theoretical organizations of their own somewhat different from his. He has not been a 'master' with followers, but as a New England liberal mellowed in the West he has inspired students to become independent psychologists and to carry on in their own ways with the open-minded and zestful spirit which he helped them discover and develop in themselves." Those of us who have had the benefit of having been in close contact with Professor Tolman have all had the feeling, at one time or another, that we have contributed to great creations. He has done this for us because he does not over-teach. His lectures cannot be described as smoothly finished, carefully prepared expositions, but as galvanizers to creative thinking by his students. His classes (which frequently evolve into loud free-for-alls in which student and teacher cannot be differentiated) reveal the searchings and fumbings of the creative scientific mind rather than a digest of conclusions already reached, organized, and neatly filed away. Nothing has ever been authoritarian, static, or finished either in his systematic psychology or in his personal relations. For Tolman, as for so many artists, the fun of the game is in

the act of creation. This leads to the sharing of his ideas with his students so fully and so consistently that he has frequently given credit to students for ideas which he actually evolved himself. The picture many of us have of Tolman is that of a man who with high, good fun plays with constructs and models to his delight, to the inspiration of his students, and to the profit of psychology.

I feel impelled to apologize. The above, far too flattering foreword I first saw only after it was in page proof. I basked and I preened. I wished that a mere half of the kind things said had been true.—E.C.T.

CONTENTS

1. A New Formula for Behaviorism	1
2. Can Instincts Be Given Up in Psychology?	9
3. A Behavioristic Account of the Emotions	23
4. Behaviorism and Purpose	32
5. Purpose and Cognition	38
6. A Behavioristic Theory of Ideas	48
7. A Behaviorist's Definition of Consciousness	63
8. Sign Gestalt or Conditioned Reflex?	69
9. Gestalt and Sign Gestalt	77
10. Psychology versus Immediate Experience	94
11. Operational Behaviorism and Current Trends in Psychology	115
12. The Acquisition of String Pulling by Rats	130
13. Determiners of Behavior at a Choice Point	144
14. Physiology, Psychology, and Sociology	179
15. Prediction of Vicarious Trial and Error by Means of the Schematic Sowbug	190
16. Psychological Man	207
17. A Drive-conversion Diagram	219
18. A Stimulus-expectancy Need-cathexis Psychology	228
19. Cognitive Maps in Rats and Men	241
20. Bibliography	265

1

A NEW FORMULA FOR BEHAVIORISM

[*Psychological Review*, JANUARY, 1922]

THE IDEA OF BEHAVIORISM is abroad. In the most diverse quarters its lingo, if not its substance, is spreading like wildfire. Why?

In the first place, it is to be observed that ever since the days of Ebbinghaus's experiments on memory the inadequacy of the merely introspective method as such has been becoming more and more obvious. And the recent work in mental tests and animal psychology has strengthened this conviction. In the second place, there has always been a formal logical difficulty about the introspective method which has troubled certain minds. That is, the definition of psychology as the examination and analysis of private conscious contents has been something of a logical sticker. For how *can* one build up a science upon elements which by very definition are said to be private and noncommunicable? And, thirdly, the introspective method is practically arduous and seemingly barren of results. It is these three features, then, which seem to have been primarily responsible for the spread and catching of behavioristic categories.

What, now, does the behaviorist offer as a substitute? We turn to the archbehaviorist, Watson. Behaviorism, he says, will be the study of stimulus and response such that given the stimulus we can predict the response, and given the response we can predict the stimulus. Very good! But how does he define stimulus and response? He defines them, he says, in the terms in which physiology defines them; that is, stimuli are such things as "rays of light of different wave lengths, sound waves differing in amplitude, length, phase and combination, gaseous par-

ticles given off in such small diameters that they affect the membrane of the nose," etc., and responses are such things as "muscle contractions and gland secretions."¹ We turn, however, to a later chapter² and read with astonishment, in a footnote, that "it is perfectly possible for a student of behavior entirely ignorant of the sympathetic nervous system and of the glands and smooth muscles or even of the central nervous system as a whole, to write a thoroughly comprehensive and accurate study of the emotions." But how can this be, we ask, if, by very definition, behavior is a matter of "muscle contractions" and "gland secretions"? How, on the basis of this definition, can a person "ignorant of glands and muscles" write a behavioristic account of anything? That he can write such an account we would admit. The only difference between our point of view and Watson's would be that we should insist that such an account would be the only truly behavior account, and that an account in terms of muscle contraction and gland secretion, as such, would not be behaviorism at all but a mere physiology.

It should be noted that the possibility of a behaviorism which shall be not a mere physiology but something different has apparently already occurred to a number of writers. Thus, for example, Holt says that "the phenomena evinced by the integrated organism are no longer merely the excitation of nerve or the twitching of muscle, nor yet the play merely of reflexes touched off by stimuli. These are all present and essential to the phenomena in question, but they are merely the components now—the biological sciences have long recognized this new and further thing and called it 'behavior'"³ Mrs. de Laguna also explicitly states that what we want is a behaviorism which is not mere physiology. "In order to understand behavior we must resolve it into a system of interrelated *functions*, just as in order to understand the physiological workings of the human body we must envisage the complex of chemical and mechanical processes as falling into such fundamental groups as digestion, circulation, etc., constitutive of the physiological economy. Now just as there is a physiological economy, so there is a larger vital economy in closest union with, yet distinguishable from it. This is the system of behavior, by means of which the being, animal or human, maintains his relations with the environment and forms a factor in its transformation. The science of behavior has the task of tracing the lineaments of this larger economy."⁴

¹ John B. Watson, *Psychology from the standpoint of a behaviorist*, Philadelphia, Lippincott, 1919, p. 10.

² *Ibid.*, chapter vi, "Hereditary modes of response: emotions," p. 195.

³ E. B. Holt, *J. of Phil., Psychol. & Sci. Methods*, 12 (1915), 366.

⁴ Grace A. de Laguna, "Emotion and perception from the behaviorist standpoint,"

A. P. Weiss also seems, to some slight extent at any rate, to lean towards this same view of the desirability of a nonphysiological behaviorism. For example, the following: "The investigation of the internal neural conditions forms part of the behavioristic programme, of course, but the inability to trace the ramification of any given nervous excitation through the nervous system is no more a restriction on the study of effective stimuli and reactions in the educational, industrial or social phases of life than is the physicist's inability to determine just what is going on in the electrolyte of a battery while a current is passing, a limitation that makes research in electricity impossible."⁵

The two essential theses which we wish to maintain in this paper are, first, that such a true nonphysiological behaviorism is really possible; and, second, that when it is worked out this new behaviorism will be found capable of covering not merely the results of mental tests, objective measurements of memory, and animal psychology as such, but also all that was valid in the results of the older introspective psychology. And this new formula for behaviorism which we would propose is intended as a formula for all of psychology—a formula to bring formal peace, not merely to the animal worker, but also to the addict of imagery and feeling tone.

But how can this be done? By what single common set of concepts can we possibly take care both of the facts of gross behavior and of those of consciousness and imagery?

Psychol. Rev., 26 (1919), 410-411. See also other articles by the same author. "Dualism in animal psychology," *J. of Phil., Psychol. & Sci. Methods* 15 (1918), 617-627; "Dualism and animal psychology: a rejoinder," *J. of Phil., Psychol. & Sci. Methods*, 16 (1919), 296-300, and "Empirical correlations of mental and bodily phenomena," *J. of Phil., Psychol. & Sci. Methods*, 15 (1918), 533-541.

⁵ "The relation between physiological psychology and behavior psychology," *J. of Phil., Psychol. & Sci. Methods*, 16 (1919), 626.

⁶ Attention should be drawn to two other very significant attempts to begin a detailed "working out" of such a behaviorism in addition to Mrs. de Laguna's in the article on "Emotion and perception from the behaviorist standpoint" already quoted from. These are to be found in a series of articles by J. R. Kantor: "A functional interpretation of human instincts," *Psychol. Rev.*, 27 (1920), 50-72; "Suggestions toward a scientific interpretation of perception," *Psychol. Rev.* 27 (1920), 197-216; "An attempt towards a naturalistic description of emotions," *Psychol. Rev.*, 28 (1921), 19-42, and 120-140; "A tentative analysis of the primary data of psychology," *J. of Phil.*, 18 (1921), 253-269. And in a series of articles by R. B. Perry: "A behavioristic view of purpose," *J. of Phil.*, 18 (1921), 85-105; "The independent universality of purpose and belief," *J. of Phil.*, 18 (1921), 169-180; "The cognitive interest and its refinements," *J. of Phil.*, 18 (1921), 365-375. It must be pointed out, however, that whereas both these authors are giving yeoman strokes in the direction of just such a nonphysiological behaviorism as the writer is contending for, neither of them seems himself to be wholly self-conscious of this essential difference between such a true behaviorism and a mere physiology.