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EDWARD CHACE TOLMAN

1886—1959

A Biographical Memoir by
BENBOW F. RITCHIE

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Biographical Memoir

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Edward C. Tolman

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April 14, 1886—November 19, 1959

BY BENBOW F. RITCHIE

EDWARD CHACE TOLMAN made several singular contributions to psychology:

1. He was the first to recognize that an understanding of animal behavior requires that the concepts of purpose and knowledge be open to experimental treatment.

2. His ideas and his experiments with rats exposed many of the ambiguities and confusions hidden in the widely held belief that rewards are necessary for learning.

3. His ideas and his experiments with rats also exposed the essential vagueness in the widely held belief that every learned association is a kind of stimulus-response connection in which some response or set of responses is conditioned to some stimulus pattern.

Tolman made two further contributions to the advancement of his science and his profession:

1. During the year 1949-1950, "The Year of the Oath," Tolman led the faculty of the University of California in a battle that saved academic freedom at that university.

2. His books, his papers, and his lectures on psychology are models of honest, clear, and unpretentious exposition, and his autobiographical sketch, published in 1952, seven years before his death, shows us how an honest and imaginative scientist can understand himself and his work.

I have decided to make his self-sketch the body of the present biographical memoir because it illuminates the mind and heart of its

writer more brightly than could any biography of Tolman written by one who knew him less well than he knew himself. What follows, then, is Tolman's autobiography, to which I have appended two annotated quotations that will clarify further the nature of his contributions.

I was born in Newton, Massachusetts in 1886. I went to the Newton Public Schools, which were then, and still are, considered to be unusually good, and then went to the Massachusetts Institute of Technology, where I obtained a B.S. in electrochemistry in 1911. I went to M.I.T. not because I wanted to be an engineer, but because I had been good in mathematics and science in high school and because of family pressure. After graduating from Technology I became more certain of my own wants and transferred to Harvard for graduate work in philosophy and psychology.

My family was, I suppose, what now would be called "upper middle" or possibly "lower upper." My father was president of a manufacturing company and my maternal uncle president of a similar company. My brother, who was five years older, and I were, first one and then the other, expected to go into our father's business. Hence, we both went to M.I.T.; my father had been a member of the first graduating class and was a Trustee. My brother, however, escaped by becoming a theoretical chemist and physicist and I, having read some William James during my senior year at Technology, fancied that I wanted to become a philosopher. Upon graduating from M.I.T., I went to the Harvard summer school and took an introductory course in philosophy with Perry and one in psychology with Yerkes—both then young assistant professors in the combined department of philosophy and psychology. I decided then and there that I did not have brains enough to become a philosopher (that was still the day of great metaphysical systems), but that psychology was nearer my capacities and interests. It offered, at that date, what seemed a nice compromise between philosophy and science.

The fact that my brother and I both avoided family expectations

and chose academic careers, instead of going into the factory, and the further fact that this led to no family quarrels and that we were even financially supported during the process, probably tells a good deal about the nature of the family setup and of the general cultural milieu in which we lived. Our immediate family consisted of a warm, loving, but in some areas Puritanical mother and of a kindly, affectionate, but very much occupied father—who was depressingly energetic and excited about his business, so much so that when he tried to get us boys interested in it he merely wore us out—and of a still older sister who, as far as I was concerned, was already leading a grown-up life outside my ken. This seems the sort of setup which the recent studies of ethnocentrism suggest may be conducive to the developing of ambitious but non-authoritarian personalities. Although we lived in a well-to-do conventional suburb with stress on appearances, there still persisted in our family and in those of some of the neighbors the legacy of reformism, equal rights for Negroes, women's rights, Unitarianism, and humanitarianism from the earlier days of the "Flowering of New England." These social tendencies were combined with the special Bostonian emphasis on "culture" together with, in our family, a special dose of moral uplift and pacifism. Typical mottoes of my father were, on the one hand, "Tend to business" and, on the other, "Man does not live by bread alone." There was relatively great freedom of discussion between children and parents and close ties to the wider family. What I am trying to say is that the rebellion of my brother and of myself against parental domination was in directions which the parents themselves could not too greatly, or too consciously, disapprove. We were choosing the professions. We were set to increase the sum of human knowledge and presumably were to apply such an increase of knowledge to the betterment of mankind. Furthermore, we would be living up to the Puritan tradition of hard work and to the Quaker tradition, on our mother's side, of plain living and high thinking. This is not to say that our parents were not deeply and basically disappointed that we did not really adopt the

other strain in their own natures and in the New England culture at large—that of making money and taking advantage of the expanding national economy. But it *is* to say that, since in large measure we were merely following what they had preached, they could not show their disappointment too strongly either to themselves or to us. Undoubtedly this typical parent-child tragedy of America was mitigated for them, as it is for so many American parents, because we, the children, were striving towards what, at least in New England, could be considered a form of upward social mobility.

Turn now to a more particular question. Why did I, personally, go into psychology rather than choosing to follow my brother into physics or chemistry. I suspect the following factors were involved. First, during adolescence it seems to have been my brother with whom I identified and picked as my model rather than my father. Thus, I was set to follow my brother into the academic world. On the other hand, I did not dare compete with him in his own field. An older brother is both a tremendous example and a very frightening rival who, because of his advantage in years, has one licked intellectually before one starts. Secondly, I suspect that, although I was considered by my teachers to be as good as my brother in mathematics and science, my mind was in fact less rigorous and less logical. Third, as the youngest in the family I had been over-babied and over-protected—being made into a shy adolescent—who had therefore been led, perhaps, to become especially sensitive to and interested in human relations. Further, at the age of seventeen I was taken out of school for two years because of a functional heart disorder which at that time was laid to too rapid physical growth; the more probable psychoanalytic explanation I leave to the reader. This left me much time to introspect, to become somewhat morbid and to imagine myself as a potential “writer,” “humanitarian,” or “saver-of-souls”—in, of course, a chaste, rationalistic, Unitarian sort of way. Again, as a late maturer and one who had always been poor at sports and one who, no doubt due to the influences of a

mother of Quaker origin, was afraid of bodily competition and of masculinity in general, I probably had suffered a sufficient number of rejections from all but a small intimate group of boyhood friends to have had another reason for needing to "understand" human reactions. Although I had thought I wanted to be a philosopher, I can remember the excitement I felt in that first course in psychology with Yerkes in which we did little "experiments" on reaction time, mental images, and the like. I felt that here one was going to learn what made people tick. It would be much more successful than preaching at them. (I had gone through a phase of thinking that I wanted to be a Unitarian minister.)

In the fall of 1911, therefore, after only one summer session course in philosophy and one in psychology, I began at Harvard as a full graduate student (unthinkable in these days) in the joint department of philosophy and psychology. The courses I remember most vividly were: Perry's course in Ethics, which laid the basis for my later interest in motivation and, indeed, gave me the main concepts (reinforced by a reading of McDougall's *Social Psychology* as part of the requirement of the course) which I have retained ever since; Holt's course in Experimental (largely two-point thresholds and epicritic and protopathic sensations) which I took my first semester and which proved a terrible letdown from the really humanly important problems which I had supposed psychology was to be concerned with; Langfeld's course in Advanced General, using Titchener as a textbook, which almost sold me temporarily on structuralistic introspectionism; Holt's seminar in Epistemology in which I was introduced to, and excited by, the "New Realism"; and Yerkes' course in Comparative, using Watson's *Behavior—An Introduction to Comparative Psychology*, which was just out, as a text.

In addition to these more or less formal courses there was the graduate research done under Münsterberg with Langfeld doing most of the actual supervision. This, if I remember correctly, I began after one year only of graduate enrollment. It had, of course,

a tremendous influence upon me. Münsterberg was then at the height of his interest in applied psychology. And most of the research projects which were being carried out in the laboratory involved primarily objective measurements of sensory-motor skills. And even my own research dissertation, which was assigned to me, and which involved the learning and relearning of nonsense syllables under pleasant and unpleasant odors, according to Ebbinghaus's Learning and Savings Method, was primarily objective in nature. I used the then up-to-date *Rupp-Lippmann Gedächtnis Apparat* and all I had to do was to sit and count revolutions. Yet in spite of this objective character of practically all of the research being carried out and reported in the weekly laboratory meetings, Münsterberg several times made little opening speeches to the effect that *the* method of psychology was *introspection*. We were expected to ask our subjects, the other graduate students in the group, for introspections, and we took these introspections down in our protocols. But, as far as I remember, none of us was able to make much use of them in his final write-up. And this troubled my theoretical mind. If introspection were "the" method of psychology and we weren't doing it, shouldn't I really go to Cornell where Titchener taught one to do it properly? This worry about introspection is perhaps one reason why my introduction in Yerkes' course to Watson's behaviorism came as a tremendous stimulus and relief. If objective measurement of behavior and not introspection was the true method of psychology I didn't have to worry any longer that we were not doing the latter, or, at least, not doing it in any consistent and approved way.

I say that this was a great relief to my "theoretical mind." As to just when and why this theoretical orientation developed I am not clear. It may be in part constitutional, whatever that may mean. But I am more inclined to believe that it developed from my early fear of, and awkwardness in, manipulatory activities; I had never been especially good in the laboratory at M.I.T. Such fear and awkwardness were perhaps induced as a reaction against my father's extreme

interest and proficiency in such matters and also against my brother's slightly greater identification with our father's pattern.

Whatever the explanation, I have always wanted simple and wide-reaching, if not too precise, explanations and have always bogged down in the face of a multiplicity of facts. I can learn facts if I have to, but I forget them equally easily. And in any argument, academic or otherwise, I always find myself handicapped by having forgotten the factual details which alone would buttress my stand. I can make a parade of scholarship, but I find it tiresome and the parade is, I suspect, usually a phony. All the necessary facts are just too many for me to keep in mind. I suspect that I also have some weakness, innate or acquired, in verbal imagery. This is the reason I feel comfortable only when I have translated my explanatory arguments into diagrams. I always did like curves better than equations. Analytical geometry was a lot more fun than advanced algebra. (They used to be separate courses in my day.) I am very unhappy whenever I do not have a blackboard in my office.

At the end of my first graduate year at Harvard I went to Germany for the summer to help me prepare for the required Ph.D. examination in German. I have always been enormously intrigued by foreign languages although I have no natural talent for them. For I have a poor ear and always have to learn phrases and vocabulary by seeing the words and not just by hearing them. At Langfeld's suggestion I spent a month in Giessen with Koffka, who had been a fellow student of Langfeld's in Berlin and who was then a young *Privatdozent* in psychology at the University of Giessen, and so got my first introduction to Gestalt psychology—although at that time I sensed only vaguely what it was all about. Nevertheless it prepared me to be receptive to Gestalt concepts when after the first World War we began hearing about them more fully in this country through the writings of Wertheimer, Köhler, and Koffka. And in the fall of 1923 I went back to Giessen for a couple of months to learn more.

After getting my doctor's degree at Harvard in 1915 I was in-

structor for three years at Northwestern. I had a compulsive drive from the beginning to do research and to write. I think this was due in part to my brother's example, who was already hell-bent on research and academic success. This compulsion for research and writing, although it did not result in a very large output at the time, interfered with my learning to become much of a teacher. I was still relatively self-conscious and inarticulate, and was afraid of my classes. Also my difficulty in—or dislike for—organizing and remembering a large array of facts was already a handicap. Further, this was just before we got into the first World War and my pacifist training, plus my own problems about aggression, kept me in a terrific emotional turmoil so that I did a still poorer job. I was called before the Dean sometime during the winter of 1917-18 because I had given my name to a student publication, circulated in the Middle West, that was concerned with "war aims," and which had, no doubt, something of a pacifist tinge. The Dean, in leafing through an issue in my presence, did not feel any less hostile because the leading article turned out to be by no less eminent a person than David Starr Jordan. In any event, I was dismissed at the end of that academic year on the grounds of war retrenchment and my not too successful teaching. But I have always thought that my near pacifism had something to do with it. I escaped the first draft by being a couple of months too old. But the second draft came along and my pacifist principles and my doubts about the war did not prevent me from signing up and trying to get a commission. But I was already too late to get into the psychological testing service organized by Yerkes. In the early fall of 1918 I was offered a commission to work with Dunlap and Stratton on the screening of air force candidates. But by that time everyone knew that the Armistice was on its way. So I did not accept.

In the meantime, during the summer of 1918, I was without a job but by luck plus Langfeld's good offices I was offered in the fall an instructorship at California. And here in Berkeley I have stayed extremely happy ever since until very recently. From the very first

California symbolized for me some sort of a final freeing from my overwhelmingly too Puritanical and too Bostonian upbringing. The "Freedom of the West," whether real or fancied, at once captured my imagination and my loyalty and has claimed them ever since—although with the years I have, of course, become aware that all is not gold that glitters—even in California. In any case, there are features about the climate and the landscape which seem to me better as a steady diet than those provided by any other place in the world. Particularly the Bay Area (although it produces its share of tonsils, allergies, and influenzas) seems absolutely ideal as an all-year-round working climate. Whatever my early psychological instabilities, they have all but disappeared. Whatever my increasing psychological maturity—and there has been some—I like to credit most of it to the social, intellectual, and physical virtues of Berkeley plus an extraordinarily happy marriage.

I have never been comfortable or efficient in administrative or committee activities and have in large part managed to escape them. My drive has gone into trying to be creative and in my earlier years, whenever I was feeling inept on some social or academic occasion, I can remember going home and talking to myself in some such words as: "Well, I'll show them, I will be better known in my field than they will be in theirs." And then I would return to the laboratory, or the study, with an enhanced drive. This compulsive academic ambition, which has, of course, lessened with the years—this self-ideal of someone going to be truly successful in the academic world—came, I suppose, from the fact that in childhood and boyhood I was always successful in school, but never on the playground, and from the fact that, as already indicated, I identified with my older brother. Furthermore, Academe was for me a protected haven in which one could release one's aggressions, of which I undoubtedly have my share, and stick one's neck out on paper without its being too obvious either to oneself or to the other fellow.

Having thus tried to think out, as a very amateur clinical psychologist, what kind of a person I think I am and how I think I got that

way, let me try to present a brief history of my psychological interests and concepts. Presumably these have been affected by the structure of my personality; but whatever the interconnections, these are beyond my ability to unravel. I shall present now, therefore, merely as objective and straightforward an account of my ideas as I can.

In my three beginning years as instructor at Northwestern I was still thinking largely in terms of classical introspective and associationistic problems. For, although I had been, as I said, tremendously excited by Yerkes' introduction to and criticism of Watson's behaviorism, the behavioristic point of view had not yet really got into my blood. Thus the first papers I turned out were concerned with such pre-behavioristic problems as retroactive inhibition, imageless thought, and association times for pleasant, unpleasant, and neutral words.

When, however, I joined the department at Berkeley as instructor in 1918, I found it was up to me to suggest a new course. Remembering Yerkes' course and Watson's textbook, I proposed "comparative psychology." And it was this that finally launched me down the behavioristic slope. Only a few students enrolled and at first a lot of time was spent in arguing against anthropomorphism and the Clever Hans error. But, before too long, I actually acquired some rats from the Long-Evans strain which had been developed in the Anatomy Department. And I and a few graduate, or advanced undergraduate, students began trying out minor experiments in learning. (Even though I had been clumsy in the physical and chemical laboratories at M.I.T., I *could* build mazes.)

It was Watson's denial of the Law of Effect and his emphasis on Frequency and Recency as the prime determiners of animal learning which first attracted our attention.¹ In this we were on Watson's side. But we got ourselves—or at least I got myself—into a sort of in-between position. On the one hand I sided with Watson in not liking the Law of Effect. But, on the other hand, I also did not like

¹ The Law of Effect was Thorndike's name for the belief that satisfying rewards are necessary to strengthen habits.—B. F. R.

Watson's over-simplified notions of stimulus and of response. Nor did I like his treatment of each single stimulus and each single response as a quite insulated phenomenon which has practically no relation to any other stimuli or any other responses. That is, I was already becoming influenced by Gestalt psychology and conceived that a rat in running a maze must be learning a lay-out or pattern and not just having connections between atom-like stimuli and atom-like responses "stamped in" or "stamped out," whether by exercise *or* by effect. In fact, my objection to Thorndike's Law of Effect was not to the importance of motivation as a factor in learning, but rather to his wholly mechanical notion as to its operation by way of effect. According to Thorndike, an animal learned, not because it achieved a wanted goal by a certain series of responses, but merely because a quite irrelevant "pleasantness" or "unpleasantness" was, so to speak, shot at it, as from a squirt gun, after it had reached the given goal-box or gone into the given *cul de sac*. And it is this same quite mechanical and irrelevant notion as to the operation of the modern successor of Effect—"Reinforcement"—which underlies, I believe, my main objection to *it*.² I have, that is, always found difficulty in conceiving how a completely past and divorced "pleasantness," or a completely past and divorced "need-reduction" (*i.e.*, reinforcement), can act back upon and selectively strengthen the appropriate synaptic connections merely because these synapses happen, quite irrelevantly, to have been the ones which have functioned most recently in time.

It was also during this early period at California that I began developing certain more basic theoretical concepts. These were initiated by a growing belief that a really useful behaviorism would not be a mere "muscle-twitchism" such as Watson's. It soon appeared to me that "responses," as significant for psychology, are defined not by their physiological, muscular, or glandular details but rather by the sort of rearrangements between organism and environment or between the organism and its own internal states which they achieve. It also

² The Principle of Reinforcement was Hull's name for the belief that need-reducing rewards were necessary to strengthen habits.—B. F. R.

seemed to me that "stimuli" as actually used by psychologists are defined in most cases not in terms of the details of sense organ stimulation but in terms of environmental "objects" and "situations" identifiable only in relatively gross, often merely commonsense terms. That is, I was beginning to have the as yet rather vague notion that there was something which I wanted to call "behavior *qua* behavior." This would be something other than and different from the mere muscle contractions and gland secretions and the mere punctiform sense-organ stimuli underlying such behavior.

The further notion that purpose and cognition are essential descriptive ingredients of any such non-physiologically defined "behavior *qua* behavior" I borrowed from Perry. He pointed out that behavior as such is both persistent and docile. (Thorndike's cat exhibited persistence and docility relative to getting out of the box.) The cat's behavior has to be described and identified in terms of these purposive and cognitive features—but in a quite non-metaphysical and non-teleological sense.

During this period I also spent considerable effort in trying to translate some of the familiar pre-behavioral concepts, such as "sensation," "emotion," "ideas" and "consciousness," into these new, non-physiological behavioral terms.³ And in the course of so doing I came to use the term "molar" to designate "behavior *qua* behavior," as contrasted with the term "molecular" to designate the underlying physiological units of sense-organ stimulation, central neural activity, and final muscle contraction or gland secretion. This pair of terms—"molar" vs. "molecular"—was suggested to me by Professor Donald C. Williams, then a graduate student in philosophy and psychology in Berkeley.

³ Tolman's position was non-physiological only in a special sense—the different purposive and cognitive dispositions of the animal name different ways of behaving, not different kinds of internal events or processes. Tolman recognized the important relevance of physiological findings, but he denied that an understanding of behavior must *begin* with beliefs about the nature of the internal events or processes that may later be found to be relevant to a more complete understanding of this behavior.—B. F. R.

The above ideas, expanded and elaborated, were finally brought together in the book, *Purposive Behavior in Animals and Men* (1932). As I survey now ambivalently that extensive tome, I find a number of features in it which strike me as probably still worth calling attention to.

First, it is to be noted that I spent what would now seem an unconscionable amount of space and effort in attacking "mentalism" and in defending an objective approach to psychology. Bridgman's book had, to be sure, appeared in 1927.⁴ But I had not as yet read it, so I did not use the term operational; but it was obviously an operational set of concepts for a non-physiological behaviorism which I was groping for. Today, however, the operational battle has so largely been won that to the average psychologist it no longer seems worth arguing about. In other words, today we are practically all behaviorists. In some loose sense we practically all subscribe to the doctrine that the only psychological statements that can be scientifically validated are statements about the organism's behavior, about stimulus situations or about inferred, but objectively definable, intervening variables.

Secondly, I am still amused and bemused by my neologisms. They have, of course, never been as offensive to me as to others. Further, I would point out that a number of them have gained some currency in the literature. Thus "*discriminanda*" and "*manipulanda*," appear here and there in the writings of others. Further, the more basic central term of "sign-gestalt-expectations," while seldom picked up in its entirety, seems to have been the source of the now widely current term, "expectancy." It should be pointed out, however, that "expectancy" as used by others is probably a more atomistic concept

⁴ The post-Watsonian Behaviorists, Tolman, Hull, Spence, Guthrie, Skinner, and others took P. W. Bridgman's *The Logic of Modern Physics* as their methodological bible. This book described the intimate relation between the meaning of any physical magnitude, like spatial length, and the operations required for determining any particular value of this magnitude. The post-Watsonian Behaviorists used this idea to develop a neo-pragmatic theory of meaning which they called "operationism."—B. F. R.

than was intended in my original meaning of "sign-gestalt-expectation." Finally, the term "means-end-readiness" seems also to have found favor among some rat runners and even among some personality psychologists.⁵

Thirdly, I still would want to emphasize the distinction to which I was trying to draw attention by the use of the two concepts "means-end-readiness" and "sign-gestalt-expectation." This was a distinction which few readers, if any, seem to have understood or, at any rate, to have remarked upon. By the introduction of these two concepts I was trying to say that what the organism acquires in a given *concrete* situation is *first* an "expectation" that by responding to this spatially and temporally located *concrete* sign (or means) by a given behavior it will arrive at a further concrete "significate" (or goal) and, *secondly*, that the organism is also acquiring a general "readiness" thereafter to accept this same general *type* of sign or means as leading to the same general type of significate or goal. The sign-gestalt-expectation is limited to, and goes off in, the particular concrete situation. The means-end-readiness is a more universalized disposition which, once acquired, the organism carries about with him to new situations. I today would still hold to this basic distinction, although I would now phrase it somewhat differently. Instead, that is, of now talking about concrete sign-gestalt-expectations and correlated, governing means-end-readinesses, I would speak, rather, of concrete "behavior-spaces" of the moment and of governing, controlling "belief-value matrices." These new terms seem to me to emphasize better another essential point of the doctrine which is that each single sign-gestalt-expectation is always part of a larger *field* of expectations and that any single means-end-readiness—belief-value unit—is also part of a larger field or *matrix* of such units.

Fourth, another feature involved in the concept of sign-gestalt-expectations, which seems not to have been understood by most

⁵ These terms, for Tolman, denote different kinds of dispositions—dispositions to discriminate, to manipulate, to prepare for, or to connect stimulus objects in definite ways.—B. F. R.

readers was that a sign-gestalt-expectation is not to be conceived as just an S-S association. It is to be thought of rather as a single interconnected and interacting whole—hence the term gestalt—such that the discriminated character of the sign will within limits affect that of the significate and vice versa. Thus, I was postulating, for example, that a perceived door, which, it has been learned, led to food, probably has, in some degree, different immediate discriminable properties from a perceived door, which, it has been learned, leads to electric shock. Further, I was emphasizing, in contrast to classical Gestalt theory, that, even from a phenomenological point of view, the smallest unit of experience is not just a mere sensory-perceptual pattern but such a pattern suffused with instrumental meaning.

Fifth, in connection with my new notions of the “behavior-space” and of the “belief-value matrix,” I would now find a certain further defect in the book in that the concepts “sign-gestalt-expectation” and “means-end-readiness” did not allow for the “self” as an object within such an expectation or such a readiness. It is probably the influence of Lewin,⁶ with his concept of the “Life-Space” and of the “Psychological Person” *as in* the Life Space, and the influence of psychologists and sociologists, who have been investigating group phenomena, which have led me now to substitute a behavior space, which contains both a behaving self and goal-selves and a belief-value matrix, which may also contain universalized self-images.

A sixth feature of the book which still fascinates me is that it attempted to provide a theoretical scheme for summarizing and interrelating a great many types of learning experiment. It was hoped at the time the book was published that it might be used as a textbook in courses in animal psychology. And a few hardy individuals did so use it. I myself never was able to. It always turned my stomach. I had just torn out my vitals and exposed them to the world. I was therefore unable to look upon those vitals without suf-

⁶ Kurt Lewin (1890-1947) hoped to describe the way that motivation influences behavior by conceiving of different motives as forces that push or pull the person in different directions in what he called the person's “life-space,” by which he meant the environment that the person pictures himself in.—B. F. R.

fering either extreme shame or unseemly pride. I wanted to vomit at it and often still feel so inclined. There was too much self-consciousness and affectation of style. However, I still think that the book provided what was at the time a stimulating organization of empirical data on animal motivation and animal learning.

Finally, the last feature in the book, which I now wish to draw attention to, is the fact that it was also an attempt to lay out a scheme for the lines of interaction between all the various variables determinative of behavior. Among these variables were what I then called the "immanent determinants." These were my first step toward what later I conceived of as "intervening variables." I felt vaguely at that time that the cognitive and purposive features of behavior, which I was postulating, were somehow statements about the shapes of the functions connecting the final dependent behavior to its various independent determiners of environmental stimuli and physiological drive states. Therefore I said the cognitive and purpose features were "immanent" *in* these connections or functions. It was only later that I hit upon the notion of breaking up the total functions into two or more successive steps and inserting "intervening variables" as intervening events or processes or states between such successive steps or functions.⁷ So much for the book.

Let me consider now the development of this concept of intervening variables as I attempted to elaborate it in several articles which succeeded the book. Intervening variables I conceived as hypothesized states or processes between the variables of stimulus situations, physiological drive conditions, heredity, age, past training, etc., on the one hand, and the final dependent variable of behavior, on the other hand. This notion of "intervening variables" as constructs defined in part in terms of their postulated functional relationships either to the

⁷ Tolman introduced the idea of the intervening variable to explain what he meant by the dispositions that direct the animal's behavior. These dispositions are shaped by the animal's environment, both internal and external, and they, in conjunction with the animal's other dispositions, shape the animal's behavior. Thus they "intervene" between the environmental causal conditions and the behavioral effects we observe and record.—B. F. R.

independent variables, on the one side, or to the dependent variable of behavior, on the other, was presented in three articles. In the first of these articles, "Psychology versus Immediate Experience," the emphasis was upon finally laying the ghost of a subjective, primarily introspective, definition of the intervening variable. The second article, "Operational Behaviorism and Current Trends in Psychology," was further concerned with the functional, mathematical dependencies of the intervening variables upon the independent variables and with operational ways of measuring these intervening variables. It also raised the further problem of the functions by which such intervening variables were to be conceived to interact with each other and to produce the final dependent behavior. Finally, in the third article, "The Determiners of Behavior at a Choice Point," an attempt was made to use this sort of an analysis for bringing together all the more important rat experiments to that date (1937). The attempt was made to show which functions—those between independent and intervening variables or those between different intervening variables or those between these and the final dependent behavior—the various individual rat experiments were respectively concerned with.

More recently, it has been argued by MacCorquodale and Meehl that a distinction is to be drawn between "intervening variables," defined solely in terms of functional relationships to the independent or dependent variables, and "hypothetical constructs," defined in terms of constitutive properties attributed to the intervening states or processes as such. It is claimed by these writers that Hull, who has borrowed and also found useful the notion of intervening variables, and I myself have both built our theories primarily on "intervening variables" in their sense and not upon "hypothetical constructs." It is my present contention, however, that all theories really use "hypothetical constructs." We theorists have differed merely in the explicitness with which we have indicated either to ourselves or to others just what the assumed constitutive properties of our hypothetical constructs are. In other words, I do not agree that there are two separate kinds of theoretical concept—intervening variables, on the

one hand, and hypothetical constructs, on the other. I accept the importance of the distinction between the assumed functional relationships of the intervening variables, or, if you will, hypothetical constructs, and their assumed constitutive properties. And MacCorquodale and Meehl did us a great service in emphasizing this distinction. Yet I do not believe one can fruitfully argue about the one feature, of functional relationships, without also arguing, or at least implying, something about the other feature, of constitutive properties. Thus the constitutive properties which, my type of theory assumes, are those implicit in a topological, electromechanical model. I have called it elsewhere a pseudo-brain model. But it is far more "pseudo" than it is "brain," even though it is full of hypothetical constructs in the narrow MacCorquodale and Meehl sense.

I would like to turn now to some of the kinds of experiments on rat learning done in the Berkeley laboratory which seem to have influenced, or been influenced by, my theoretical position. Theory is viable and to be justified only in so far as it stimulates, or is stimulated by, research. My theoretical pronouncements have, to be sure, usually been phrased merely loosely and programmatically. And so they have seldom made possible any precise theoretical deductions which could then be specifically subjected to experimental test. Nevertheless, these theoretical meanderings have conditioned me and my students to be interested in certain *kinds of experiment*. The theory, though loose, has been fertile; perhaps fertile primarily because loose.

Now, for the experiments themselves. In trying some two years ago to summarize the major directions of research in the Berkeley laboratory it seemed to me that a majority of the experiments could be grouped under five main headings: (1) "latent learning," (2) "vicarious trial and error" (VTE), (3) "searching for the stimulus," (4) "hypotheses in rats" and (5) "spatial orientation." I shall not attempt to catalogue these experiments here nor shall I attempt to give the credit which is due to the individual students and research workers who actually had most of the specific ideas, developed the

experimental designs, and did the actual work. Rather, I wish to suggest merely that all of the experiments were in one way or another supportive, on the one hand, of a field theory of rat learning, and, on the other hand, of a theory which asserts that the animal brings to the stimulus situation certain cognitive sets (e.g., hypotheses, means-end-readinesses, needs-to-solve, VTE's, and searchings-for-the-stimulus) as well as specific states of motivation. These cognitive sets and motivational states cause him to react selectively and actively to the then and there presented stimulus array and determine the behavior space which he comes to perceive and the new means-end-readiness (belief-value matrices) which he will carry away. All of these experiments from my point of view, if not from that of their authors, have reinforced the general notion of the essentially cognitive character of learning. The original crucial experiments in this doctrine of the cognitive character of learning were those on "latent learning" initiated by Blodgett.

Turn now to a quite different problem. I have always been obsessed by a need for a single comprehensive theory or scheme for the whole of psychology. And I have also always wanted to be something more than a mere learning or rat psychologist. I have wanted a scheme which would cover not only rat learning but also one which would be pertinent to the problems of human thought and of human motivation. In *Purposive Behavior in Animals and Men* I was already tempted into pronouncements concerning primary and secondary drives, demands, insight, and ideation as well as concerning learning *per se*. And under the impetus of a general human concern for social events and of a need to discover how man is ever to achieve a stable, or even a merely satisfying, society I have a number of times been tempted into relatively *ad hoc* assertions concerning drives or needs and concerning complex motivational dynamisms both in individuals and in society.⁸

⁸ During the Second World War Tolman published *Drives Toward War* (1942). This book not only sought an explanation for the motives that impel man to war, but also imagined the kinds of social controls that a warless society would have to enforce.—B. F. R.

All of these pronouncements have been somewhat abortive because of lack of training on my part in the other social sciences and even in personality psychology and in social psychology. Yet I do not wish to disown them. The pronouncements may have been naïve; but I do not think that any of them has been basically wrong or mischievous. They constituted my first steps towards a more complete conceptual scheme—a scheme which would allow me to handle not only simple learning but also problems such as the operation of innate or socially acquired secondary and tertiary needs, the operation of the psychoanalytical and other dynamisms, and, finally, the explanation and prevention of individual and social maladjustments. I have been concerned throughout with man's basic needs, biological or social, and with the question of how these needs become modified through social learning as a result of given cultures and given training procedures within a family. Why is it that individuals and cultures go astray? Why is it that a social system seems to seldom to allow for reasonable satisfactions in most of the individuals involved in it? Can we arrive at some naturalistic definitions of the good life or of different kinds of good life? And, having arrived at such definitions, can psychology and the social sciences eventually agree upon ways to produce such lives? These are the sort of questions which I would seek to raise and would like answers for. . . .⁹

In conclusion it would seem meet to indicate the main sources from which I think my ideas have come. First of all most of the credit, if it be credit, should go to all the students whose ideas I have shamefully and consistently adopted and exploited throughout the years, and ended up by believing to be my own. Secondly, it should go to my teachers at Harvard who taught me to think, to be critical, to be complicated but to remain naturalistic. Thirdly, it should go to

⁹ I have here omitted approximately 900 words of the original text, which are a résumé of some of Tolman's later theoretical concepts. As Tolman himself says of this passage, "The preceding paragraphs are, I realize, too condensed and . . . too complicated for such a brief account to have much intelligibility." For full treatment of these ideas see Tolman's last paper, "Principles of Purposive Behavior" (1959).—B. F. R.

all the members of the department of psychology at Berkeley who have always given me untold moral and intellectual support in spite of considerable tolerant skepticism as to the worth of my final outpourings. Next, it should go to the Gestalt psychologists, but especially to Kurt Lewin whose ideas I have borrowed time and again and absorbed into my very blood. Again, it should go to my year's stay in Vienna and especially to Egon Brunswik, who opened my eyes to the meaning and the viability of the European psychological tradition, both academic and psychoanalytical, and who gave me new insight into the essentially "achievement" character of behavior. Still further, it should go to all my colleagues, old and young, in the Assessment Program of the Office of Strategic Services. There once and for all I finally became addicted to PSYCHOLOGY and no longer content to think merely of rats and of learning. I there acquired an aspiration level relative to personality psychology which I have since been striving for but have, of course, not achieved. And, finally, my thanks must go to the Department of Social Relations at Harvard University, which during the year 1949-50 taught me something of sociology and of anthropology and of personality and of social psychology, and set me wondering about ways in which my rat concepts might eventually become amalgamated with those of the scientists in these other fields. For, if we are to advance, we must first understand, and then attempt to incorporate into our own, the perspectives of our sister sciences—not merely of those sciences which pertain to physiology but also and even more of those which pertain to social living.

FURTHER NOTES ON EDWARD CHACE TOLMAN

I. Tolman was a great teacher. The following quotation, a warning to the reader who is about to enter the concluding section of Tolman's *Purposive Behavior in Animals and Men* (1932), shows the sort of teacher Tolman was, and, in showing this, explains why

his students and colleagues loved him dearly:

It is obvious that the preceding pages have attempted to offer a new "system" of psychology. But system-making is very properly open to suspicion. It is the resort of arm-chair hidiers from reality. And, once set up, a system probably does as much harm as good. It serves as a sort of sacred grating behind which each novice is commanded to kneel in order that he may never see the real world save through its interstices. And each system is so obviously bound to be wrong. It is twisted out of plumb by the special cultural lack of building materials inherent in the time and place of its origin, as well as by the lack of skill of its individual architect or architects.

An apology, therefore, is in order. We can, in short, merely hope that the propositions summarized in the succeeding pages, when set up in front of you as a pattern of mullions through which to observe the psychological landscape, will serve (but only temporarily) to limn into prominence for you new areas for the gathering of data.

But may neither you nor we ever seek to hold up these propositions, save in a somewhat amused, a somewhat skeptical, and a wholly adventure-seeking and pragmatic behavior-attitude.

2. Not only was Tolman an honest man, he was a brave one. The following quotation from *The Year of the Oath: The Fight for Academic Freedom at the University of California*, by George R. Stewart, describes the atmosphere above and around the university when this fight began.

On June 14, 1949, the day when the Academic Senate met first in Berkeley to consider the oath, these were among the headlines in the *San Francisco Chronicle*: "Atom Inquiry," "Are You A Communist?," "Russ Answer U. S. British Balkan Notes," "Hiss Perjury Trial," "Condon To Be Called In Coplon Trial," "U. N. Official Sotirov Denies He's Russ Agent," "Business View Of Red China," "U. C. Loyalty Oath New Pledge Scheduled For Heated Debate In Academic Senate Today," "Three Loyalty Oaths In U. C. Contro-

versy," "‘Dangerous Ideas’ and Wellesley," "U. of Illinois Says Its Loyalty Oath 10 Years Old," "Maryland Professors Must Sign By July 1," "Phi Beta Kappa Opposed To Oath." . . .

In the midst of such whirlwinds, blowing from all ends of the earth, the faculty of the University of California tried to raise a voice in the cause of the freedom of the human mind. Small wonder that voice was not heard clearly. . . . What chance was there for deliberation, for even tempers, for clear thinking, and for sensible judgments on eternal values, in a period when this whirlwind of headlines symbolized the world tempest in which we were caught?

Every headline of *Communist* (even if that Communist were in Indo-China), every headline of *Investigation* (even if that investigation were in Washington), every headline of *Disloyalty* (even if that disloyalty existed only in some vicious imagination)—every such headline meant that a state of mind was induced and then transferred to the next headline, which read, *U. C. Oath Controversy*. These were the whirlwinds of struggles for world mastery and graspings for power. Yet education—in a democracy—should not be the servant of power.

On that same afternoon (June 14, 1949) Edward Tolman spoke to the Academic Senate about the special Loyalty Oath which the Regents had appended to the 1949–1950 contracts.

The issue that I am concerned with involves [he said] certain ambiguities . . . [that] make it difficult to be certain just what we [are] being asked to commit ourselves to. . . . As a psychologist I . . . assert that a party or organization . . . [can] advocate or teach but it cannot believe. Only individuals can believe. . . . If this phrase is left in . . . this is neither good psychology nor good civil rights. . . . This [is] the principle of guilt by association. Does the University . . . want to . . . [endorse] that principle? [After pointing out further dangerous ambiguities in the special oath he concluded] I cannot and will not sign [this] oath. . . . I hope, of course, that enough other members

of the Senate will join with me in this protest to demonstrate to the Regents the seriousness with which we view the oath as a threat to academic freedom, and indeed as a threat to mere decency and the honest use of the English language.

In this brave speech Tolman touched the mind and heart of every teacher, every scientist, and every man of good will. Ten years after this speech, on Charter Day, March 20, 1959, the president of the University of California, Clark Kerr, awarded Tolman an honorary LL.D. and expressed the gratitude of the University in the following words:

Eminent psychologist, brilliant investigator of purposive behavior in animals and man, student of cognitive processes, pioneer in systematic theory construction, developer of a major and comprehensive theory of learning.

A man of tolerance and humor, dedicated to rigorous methods of scientific psychology and at the same time hospitable to all imaginative and original ideas. A great teacher who has inspired generations of students and colleagues to high creative effort.

When one recalls the long and painful struggle at the University over the special loyalty oath, this action of President Kerr's testifies not only to the greatness of Edward Tolman but also to the greatness of his University. In 1962, exactly forty-four years after Tolman first came to Berkeley, the Department of Psychology at the University of California began the fall semester in a new building—Tolman Hall. In this building the behavior of countless generations of pigeons, mice, rats, dogs, cats, monkeys, students, and men will be touched by the ideas of a great teacher and a brave and honest man, Edward Chace Tolman.

HONORS AND DISTINCTIONS

- President, Western Psychological Association, 1922.
Representative of the American Psychological Association, National Research Council, 1936-1939.
President, American Psychological Association, 1937.
Member, National Academy of Sciences, 1937.
President, Society for the Psychological Study of Social Issues, 1940.
Penrose Lecturer, American Philosophical Society, 1941.
Faculty Research Lecturer, University of California, Berkeley, 1946.
Member, American Philosophical Society, 1947.
Member, American Academy of Arts and Sciences, 1949.
Honorary Sc.D., Yale University, 1951.
Honorary Sc.D., McGill University, 1954.
Honorary Fellow, British Psychological Society, 1954.
Co-President, Fourteenth International Congress of Psychology, 1954.
Honorary LL.D., University of California, Berkeley, 1959.

KEY TO ABBREVIATIONS

- Am. J. Psychol. = American Journal of Psychology
 Am. Psychologist = American Psychologist
 Comp. Psychol. Monog. = Comparative Psychology Monographs
 J. Abnormal Psychol. = Journal of Abnormal Psychology
 J. Abnormal Soc. Psychol. = Journal of Abnormal and Social Psychology
 J. Comp. Psychol. = Journal of Comparative Psychology
 J. Comp. Physiol. Psychol. = Journal of Comparative and Physiological Psychology
 J. Educ. Psychol. = Journal of Educational Psychology
 J. Exptl. Psychol. = Journal of Experimental Psychology
 J. Gen. Psychol. = Journal of General Psychology
 J. Phil. = Journal of Philosophy
 J. Soc. Psychol. = Journal of Social Psychology
 Phil. Sci. = Philosophy of Science
 Psychol. Bull. = Psychological Bulletin
 Psychol. Monog. = Psychological Monographs
 Psychol. Rev. = Psychological Review
 Univ. Calif. Publ. Psychol. = University of California Publications in Psychology

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