Robert Choate Tryon: Pioneer in Differential Psychology

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Chapter 22 in

Portraits of Pioneers in Psychology

Kimble, G.A., Wertheimer, M., and Whire C.L. (Eds.),
American Psychological Association, 1991

Chapter 22

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My name is Robert C. Tryon. I died many years ago and I must say that your invitation to come and talk to you took me very much by surprise. Incidentally, you did not send me a plane ticket, and travel to New Mexico from my new place of residence is very expensive. It is also very difficult to obtain an exit visa; they don't even quite know what sort of passport to issue. Anyway, it is for these reasons that I hesitated before accepting your kind invitation, although it is still not clear to me why you asked me to come. I finally did decide to come, motivated largely by curiosity over your invitation. I assume that you wanted me to come because you felt like hearing something about my time, my life, and my work. I will try to oblige.

BIOGRAPHY

I was born on September 4, 1901, in Butte, Montana. I came from what may be described as "poor working stock." My father earned his living as a plumber. Neither my father nor my mother was well educated, both having finished only elementary school. None of my siblings, of which I had seven, went beyond high school. We were always poor, and to the day I died I always thought of myself as a "working stiff." I died on September 25, 1967, in Berkeley, California, at 66.

*Photograph of Robert C. Tryon courtesy of the Archives of the History of Psychology

Education

When I was still very young my family moved to Los Angeles. I don't remember the move at all; it must have occurred sometime during 1904 or 1905. I grew up in Los Angeles, then a very lovely small town with the mountains in the background and the ocean not too far away. I was a good student, finishing high school with top grades. As a reward, and as the first member of my family ever to do so, I entered college—the University of California, Southern Branch, today known as UCLA. My major was in English. At that time one could only do one's freshman and sophomore years there, and if one wished to continue one had to transfer north to Berkeley.

This is what I did and I continued to major in English. My ambition then, and always, was to become a writer. During my career as an English major I met some very interesting people. They were mostly San Francisco journalists who formed a stimulating "Bohemian" crowd. We would discuss politics, philosophy, and literature endlessly, consuming enormous amounts of beer. Beer, I must tell you, became and remained one of my great passions. Parenthetically, I might just add that during the last 30 or so years obtaining a cold glass of beer has been very difficult. I also met my first wife, Carolyn, who introduced me to psychology, which was her major. She convinced me to take a class from one of her professors. I did. I was fascinated. I gave up my ambitions to become a writer—when one is young one's ambitions change rapidly—and I began to read and major in psychology. The professor's name was Edward Tolman.

Upon graduation I was invited to become a graduate student at Berkeley. I accepted and obtained my PhD jointly in psychology and genetics in 1928. My fellow graduate students were people some of whom you might remember; let me mention the names of some of those who were my friends: Yoshioka, Macfarlane, and Tinklepaugh. Our teachers were Tolman, Franzen, and Stratton. From Tolman I learned psychology, from Franzen I learned statistics, and from Stratton—well from Stratton I did not learn very much.

While a graduate student, I published two papers, both on the effects of unreliability of measurement on the differences between groups (Tryon, 1926, 1928). Both papers were published in the *Journal of Comparative Psychology*, and both papers made something of an impression on the psychological community in general, and on Professor Tolman in particular. My dissertation, titled "Individual Differences at Successive Stages of Learning," was accepted by the psychology department in 1928.

In those days, most psychologists studied learning. Multifarious experiments on the learning process per se were being published. In addition, Lashley used learning in his attempts to study cortical localization of function, in his search for the engram. Using learning experiments, other psychologists were studying drives and incentives, and so it went, on and on; everyone was studying learning.

At that time people thought it necessary to study learning as a means of measuring mental capacity. Therefore, or so one would have thought, an accurate and quantitative description of learning would have been available. But that was not the case. Learning was always described in terms of a "learning curve," which represented the average performance of subjects plotted against successive stages in the learning process. This seemed to me inadequate and I resolved to do something about it. I tested hundreds of rats in two very difficult mazes, investigating individual differences in learning very carefully, and correlating performances at various stages of the learning process.

My dissertation was a modest success. It convinced me that reliable and valid measures of learning could be obtained, and that one could learn a good deal about the process of learning by a careful examination of animals at various stages of acquisition. My dissertation also convinced Professor Tolman that I knew statistics and that I should be hired to teach this subject to my fellow graduate students. We discussed an offer for me to become instructor of psychology at Berkeley.

Early Career

Stratton, then the department's chairman, had very different ideas. It seemed that he was dead set against me. I don't know why, but I suspected that he considered me less than a gentleman, unworthy of holding an academic appointment, especially one in his department. Stratton pointed out that I always wore a shirt to work, and the same shirt at that. Worse, he thought that my fingernails were dirty. This was proof that I was not a gentleman. In any event, he strenuously opposed my appointment.

Events conspired to frustrate Professor Stratton's wishes. Tolman had been offered the chair at Harvard in 1928. He took the offer very seriously, and went to see President Sproul, of the University of California. Tolman agreed to stay if three conditions were met. First, that Charlie Honzik be appointed his full-time research assistant. Honzik had worked as the departmental "shop man," and in an accident had lost some of the fingers on his right hand. Worker's compensation did not exist, and Tolman felt obligated to help him find other employment. Second, he asked for a small salary increment for himself. And, third, he insisted that I be appointed instructor of psychology. All three requests were granted, and I—son of a plumber, dirty shirt, dirty nails and all—was appointed to the distinguished position of instructor of psychology, University of California, Berkeley. I never left, except during the war, and to give lectures here and there.

I began to teach. I never liked it—- formal classroom teaching, that is—although some have said that I was pretty good at it. In those early days my graduate students were Ghiselli, who went on to make quite a career for himself as an industrial psychologist; Crutchfield and Ballachey, who became social psychologists; Charlie Honzik, who became a clinician; John Gardner, who went on to become secretary of health, education, and welfare in the Kennedy and Johnson administrations, and some others. In later years my graduate students included Jerry Hirsch, a behavioral geneticist now at Illinois, and Dan Bailey, a quantitative psychologist now at Arizona State.

RESEARCH

I also began to do research, and a lot of it. I was interested in two inter-related issues in psychology. I wanted to study the heritable causes of behavior, and I wanted to describe the learning process quantitatively. Quantitative descriptions, I always thought and still think, would provide an adequate theory of learning. I published such a description, in fact I published 12 such descriptions, all in the *Journal of Comparative Psychology* (Tryon, 1930, 1931a, 1931b, 1931c, 1931d, 1939, 1940a, 1940b; Tryon, Tryon, & Kuznets, 1941a, 1941b). I will describe some of my findings in a minute, but here I would like to interject that it always was one of the keenest disappointments of my life that no one took these descriptions for what I thought they were, namely a comprehensive theory of learning. And not a bad one at that because one could make important predictions about the behavior of rats in mazes based on my theory.

I think that I have made contributions to several domains of psychology. I have contributed to behavioral genetics, I have contributed to our understanding of the learning process, I have made some modest contributions to statistical procedures useful in the behavioral sciences, and I have used these statistical procedures to contribute to what others have called behavioral ecology. Let me describe my work in each of these areas very briefly.

Behavioral Genetics

When I first arrived at Berkeley, Tolman had just published the results of a selective breeding experiment, in which he had tried to obtain two populations of animals one of which learned easily and another which made many errors. The experiment was a qualified success: Animals in the first selected generation responded to the selective pressure; however, animals in the second selected generation regressed to the mean.

I thought that Tolman had made two mistakes: Most seriously, the selective breeding was based on performance in a maze of questionable reliability. Second, he stopped the experiment too soon. Those of us with training in genetics knew that interpretations based on only two generations of selective breeding are pretty shaky. In fact, Tolman made a third mistake. He inbred while trying to breed selectively. This is a serious error because inbreeding exhausts genetic variance on which selective breeding depends. I made the same mistake myself, not knowing that it was an error. This mistake was pointed out to me years later, by Jerry McClearn, who joined the faculty here at Berkeley in the later 1950s.

My Selective Breeding Study. In any event, Jeffress, another student, and I convinced Tolman to try again, this time with a maze of known reliability. We built a 17-unit T-maze that was indeed reliable. When running 22 hours hungry and for food reinforcement, the odd–even reliability of our measurements, in terms of the number of errors the animals made, was 0.93. We published our results, to my knowledge the first time that reliabilities of the measuring instrument used in animal learning studies had ever been reported (Tryon, Tolman, & Jeffress, 1929).

Tolman and Jeffress became impatient. Selective breeding experiments take a long time. I proceeded with the remainder of the experiment by myself, not realizing that the experiment would last for more than a decade.

As things turned out, my first job was to run all over the Berkeley campus to find rats: to the Anatomy Department to pick up a few, to the Zoology Department to pick up a few more, and so on until I had collected a base population, a *heterogeneous* base population, of about 100. As I have already indicated, selective breeding depends on genetic variability in the population, so the fact that my base population was heterogeneous is important. In the genetic sense, the word heterogeneous has Greek origins, *hetero* meaning different and *geneous* referring to the genetic material, genes. I tested these heterogeneous animals in my maze and obtained a distribution of error scores, some rats making many errors as they learned the maze, others relatively few. I then selected for breeding the *brightest* rats and mated them *inter se*, and I did likewise with the *dullest* rats. In due course, these matings produced offspring, which I again tested in the maze when they were of the appropriate age. I then mated the brightest rats from within the bright line, and the dullest rats from the dull line, and tested the progeny, and so on, for 21 generations of selective breeding.

The experiment was a success. The lines I had selected differed from each other greatly in terms of the number of errors they made in learning the maze. In fact, by the seventh selected generation the distribution of error scores was so different that they no longer overlapped. The dullest rat from within the bright line made fewer errors than the brightest rat from within the dull line (Tryon, 1940c).

Significance and Generalizability. The experiment, as I have said, was a success. It was also an immediate success and it remains a success today, judging by the number of references to the experiment that continue to appear. The experiment is discussed in almost every introductory psychology textbook published these days, much to my delight.

The results of the experiment were significant, and for three reasons: First, because they demonstrated that learning ability, which clearly is a phenotype that depends heavily on environmental factors, has a genetic substrate. Second (and please remember the times, the heyday of behaviorism and American learning theory), these results were important because they clearly indicated that Watson's, and Kuo's, and Dunlap's idea of writing a psychology in which heredity had no place plainly would not work. Third, and most important, these results were significant because we had created biological material on which we, and others, could perform many other experiments.

We did just that; we performed many experiments on the rats we had selected. One question we felt compelled to answer was whether the differences in our selected lines could be accounted for by drive variables. We showed that they could not. Rather, the animals differed in terms of habit strength, to use the language of the 1940s.

Searle, another graduate student at Berkeley, showed that rats selected as bright and dull, respectively, were *not* generally bright and dull. Rather, we had selected for a very special trait, namely, for performance in mazes the mastery of which depended on the correct utilization of spatial cues. This reaffirmed my belief that mental capacity, intelligence, needs to be conceptualized in terms of clusters of independent, though correlated, abilities (Tryon, 1932a, 1932b, 1935). In this regard I differed from Spearman, who thought in terms of generalized intelligence, some sort of "g" factor. We engaged in a public debate about our different views. This was the only "scientific" fight I ever had with Spearman. The other fight was more personal: He was the editor of a journal to which I had submitted an article, which, when it appeared, contained a spelling error. Given my undergraduate training in English, this caused me considerable grief—this and Spearman's glee in pointing out the error. I went back to my original manuscript to discover that I had spelled the word correctly, and that the mistake was the printer's. Spearman never believed me and continued to make fun of my spelling.

Lastly, these experiments also had an impact on learning theory. Tolman, when he presented his new theory of learning during his presidential address to the American Psychological Association in 1937, felt obliged to introduce four individual difference variables in his account of learning. These individual difference variables were (1) *H*eredity, (2) *A*ge, (3) previous *T*raining, and (4) special *E*ndocrine, drug, and vitamin conditions (or HATE).

Learning Experiments

During these years, the 1930s, I also worked on the learning process. In all I published 10 papers in this area, and 2 more manuscripts remained on my desk. The first paper in this series was published in 1930; it was titled "Studies in Individual Differences in Maze Learning Ability: I. Measurement of the Reliability of Individual Differences." The last paper was published in 1941; it was titled "Studies in Individual Differences in Maze Ability: X. Ratings and other Measures of Initial Emotional Responses to Novel Inanimate Objects" (Tryon, Tryon, & Kuznets, 1941b).

Structure of Learning. The best paper in this series was the seventh one (Tryon, 1940a), in which I tried to formulate what I called a general theory of learning. My theory was an attempt to account for individual differences in maze learning on the basis of the general psychological components of learning that were deducible from observing the behavior of animals at various stages of learning. The particular psychological components underlying learning, I thought, could be inferred on the basis of the behavior of rats, provided that the performance satisfied criteria of internal consistency, goodness of fit, prediction of other behaviors *in situ*, and in other relevant situations. In total, I inferred the existence of 10 such components, some examples of which are directional set, exit gradients, conflicts, and food pointing.

As the animals learn the maze, these psychological components form higher order coalitions, which are dynamic, changing with practice. In order to explain these changes in components with practice, I was required to make six assumptions: (1) retention, or memory, of the components, (2) memory of the consequences of the components, (3) memory of the effort entailed in utilizing the components, (4) existence of multiple goals, (5) weighing some of the components because of emotional support, and (6) selection of those components that involve the least effort.

In sum, my theory of learning involved a shift in the way these components were put together into higher order coalitions with practice.

TIME OUT FOR WAR AND ADMINISTRATION

My research in learning and behavioral genetics was interrupted by the entry of the United States into World War II. Gardner Murphy, the eminent personality theorist, had read some of my statistical works, and called me to Washington to work for the Office of Strategic Services, the equivalent of the CIA. I was appointed deputy chief of the planning staff of the OSS, charged with developing measuring instruments that would be useful in selecting individuals who would make good spies, saboteurs, liaison officers with the various resistance movements in occupied Europe. Now I can tell you that selecting bright and dull rats is quite different from selecting good spies, but we were successful. Very few of the men we dropped behind enemy lines were ever caught.

World War II ended, and I returned to Berkeley. Because of my administrative experience, or so I was told, I was elected chairman of my department. The previous individuals, Stratton and Tolman, had been heads rather than chairs and I insisted on becoming a chair. I began to introduce democratic procedures into the administration of the department. I thought of this as progressive, and so it was, but it did not work. Given our faculty, everyone wanted his or her own way, and when they got their way, as a result of my enlightened democratic regime, chaos was the result. The department suffered, suffered greatly, and it took a very long time to recover.

CLUSTER ANALYSIS

After the war I never really went back to my research in behavioral genetics or learning. Rather, I became very interested in computers, in statistical procedures, especially in their application to "relevant" problems in the social sciences. I introduced the computer into my classes, into both my undergraduate and graduate courses in differential psychology. Computers removed dreary hours of desk calculator work, which previously had had a deadening effect on students. They could now spend more time on ideas. Let me give you one example: One of my graduate students, Stevenson, broke new ground when he studied the structure of creativity in a sample of 33 engineers, then cross-referenced these finding across *all* of them by a master cluster analysis. We calculated that this study would have taken 15 years of analysis by desk calculator. It was done in 8 hours by the computer. Here we have an illustration of how the computer permits one to tackle an experimental problem that would never have been attempted without it.

I worked very hard on cluster analysis (Tryon, 1967, 1968a, 1968b). This method, in its final form, was my last public accomplishment. In fact, it was published posthumously (Tryon & Bailey, 1970). Cluster analysis serves four goals: (1) The condensation of many variables into a few basic dimensions that capture the general covariation among variables, (2) Selection of homogeneous subsets of variables that are observable representations of the basic dimensions, (3) Description of the statistical properties of the dimensions and clusters, and (4) Geometrical, or graphic, descriptions of the cluster structure of the data.

In the mid 1950s, I began to work full time on the development of cluster analysis, although some of you may recall that I had written my first monograph on the subject way back in 1939. Now, however, I really went to work on cluster analysis *and* on developing the computer program to run it. This required me to spend literally thousands of hours in front of my desk calculator. The final result of this painfully difficult work, painful because I was never very good at programming, was my book on cluster analysis, written with Dan Bailey, *The BC TRY System of Analysis*.

BEHAVIORAL ECOLOGY

I now had the tools in hand with which to study how human beings group together in large metropolitan areas. Living and working in Berkeley, I naturally chose the Bay Area as my laboratory. I used the 1940 and 1950 census data for my investigations.

In this work I made only one assumption: that in large urban areas people aggregate, or form social groups, on the basis of certain defining behavioral characteristics. I gave these social groupings names such as "The Segregated," "The Workers," and "The Exclusives." This research yielded three interesting findings. First, these social groupings remained relatively constant. There was a remarkable similarity in the data based on the 1940 and the 1950 census, and this despite the socially disruptive intervening world war. Second, these social groupings were to a certain extent differentiated in a biological sense; the groups were reproductively isolated. One can imagine the consequences of such assortative matings in human populations, especially if these conditions persist over long enough periods of time. And, third, I found this work in behavioral ecology interesting because it allowed me to investigate the incidence and the characteristics of psychiatric disorders in these social areas.

CONCLUSION

During those years, the 1950s and 1960s, I also started work on a general introductory psychology text. I had always felt that there was a real need for a short book of this type, say 200 pages or so, which would present the main concepts of general psychology and illustrate these with the solid methods of our field. My feelings were that students get terribly confused by the crazy quilt of "this experiment shows . . . ," and "that theory states . . . " style of writing, a style that is more or less characteristic of many present-day encyclopedic general textbooks. I thought that I could write my style of book, and in fact I did. The page proofs were on my desk the day I died. The manuscript was never published.

To summarize, my substantive contributions have all been in the area of individual differences. I tried to understand the learning process by investigating individual differences. And I tried to develop quantitative methods to describe and understand individual differences. I am a differential psychologist, and I hope that I have made this clear. I hope that I have also given you something of the flavor and content of my academic life. Thank you very much and good luck.

AUTHOR'S PERSONAL COMMENT

I now feel obliged to speak to you as Kurt Schlesinger, more or less in order to establish my "credentials" as a biographer and expert on Robert Choate Tryon. Please allow me these few personal remarks.

I did know Professor Tryon, albeit not very well. I was not one of Professor Tryon's graduate students, although he had many, and you would have done much better inviting one of them to give this lecture. I was, however, a graduate student at the University of California at Berkeley, and I did audit several classes that he taught. We often had discussions about the state of psychology in general, and about one of his great interests, behavioral genetics, in particular. He served as a member of my comprehensive examination committee.

Although Robert Tryon was not one of my official teachers, he served as an academic advisor. I think of him as a friend, but I do not know whether this feeling was reciprocated. Nevertheless, I have lasting memories of Robert Tryon, memories that are now more than 25 years old, and memories that are very warm. I have always held him in the highest regard. He was a dedicated scholar, a first-rate psychologist, and a truly gentle man.

I would like to share one personal experience with you, an experience that I now remember, many years later, with considerable amusement. The incident I am about to relate occurred in 1962. It was a day on which the Bay Area was in the middle of a heat wave, one of the worst on record. It was also the day scheduled for the oral part of my doctoral comprehensive examination, to take place at 1 P.M. in Tolman Hall. The custom, in those days, perhaps it was even a requirement, was for candidates to appear in their best suit, which in my case made little difference since I possessed only one. I am wearing the jacket of the suit I wore on that occasion, but the pants and vest that went with it have long since disappeared. I don't know whether you agree with me that it is/was a very handsome suit, but as you can see it was clearly meant for a winter wardrobe. The heat of the day, the winter suit, and the emotions one feels when one is about to take an oral examination, all made me perspire a good deal.

As I said, my examination was scheduled for 1 o'clock, and I was punctual, but my committee, out to lunch, was not. After I had been pacing the halls for about an hour, they appeared around the corner of the hall and entered the room set aside for my orals. I was told to wait outside while the committee planned their tactics. A few minutes later I was asked to enter and I witnessed the following scene. There were the five members of my committee, seated around a table, my chairman in the middle. His head was on the table and I swear that he was sound asleep. The committee chairman's function was to run the examination and, as much as possible, to keep the student out of trouble. My chairman's "position" added to my distress. The proceedings continued, another member of the committee took charge, and decided that the proper way to continue would be to let people ask me questions, beginning with Professor Tryon. He asked me a fine question, fine not the least because I happened to know the answer. Having asked the question, Professor Tryon got up and left the room. At this point I did not know how to proceed, but I was instructed to answer Professor Tryon's question. I gave what I think was a very good answer, the conclusion of which coincided exactly with Professor Tryon's entry into the room, coffee cup in hand. The remainder of the examination proceeded without any other untoward incident; my chairman never woke up, I passed, and everyone congratulated me.

Others have written,

> Tryon was a tolerant man. His acceptance of the human condition was almost that of a naturalist. He was rarely censorious of human frailty. His friends knew him as gentle, affectionate and playful. His humor was tender and self-mocking. To a remarkable degree, he lived and worked and judged himself by his own standards. He was his own man. (Krech, Crutchfield, & Ghiselli, 1969)

Let me now apologize for these lengthy personal reflections. In his introduction to *The Comic Mark Twain Reader* (1977), Charles Neider discusses Twain's essay *How to Tell a Story*, reminding us that there are essentially three ways to tell a story. The comic story is English, the witty story is French. Both these ways of telling a tale are short and both end in a point, and both are told with "pathetically eager delight." The third way of telling a story is humorous, and Twain called it "the American yarn." It is told at great length, in a deadpan style, and it "wanders as it pleases and it need not arrive anywhere in particular. A yarn is most successful if it goes on and on, the audience is most attentive, but the teller, now tired, goes to sleep, to awaken to an audience still spellbound, waiting to hear the end of the tale." Well, my remarks certainly did not arrive anywhere in particular, but of this I am certain: Robert Tryon definitely liked to tell and listen to yarns.

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