raphy will already have realized, that my connection with psychology has been unusual and almost despite myself. I never had any teaching in psychology worth mentioning. I had the inestimable advantage of the influence and inspiration of Charles Myers during that summer of 1911, and of subsequent correspondence, but cannot call myself his pupil, only his admirer and disciple. I learned a great deal from Charles Spearman, but only by crossing swords with him, not as a pupil. And I have learned from many others by their correspondence and their friendship. But I never had a course in psychology, though I took a D.Sc. degree in it in 1913 on presentation of my publications, and an oral examination by Myers and Bainbridge.

What I have enjoyed most – I enjoy any kind of teaching – has been trying to clarify in students' minds the applications of mathematics to psychology. That, with my Sampling Hypothesis, my work on the influence of selection on factors, and the fact that I have had rather a flair for inspiring and conducting big surveys, seem to have been my main contributions.

L. L. THURSTONE

THE biography of an individual scientist cannot be expected to be of general interest except when there has been a spectacular achievement or a colorful personality or both. The present case has no claim to either. Some students may find encouragement in knowing that something can be accomplished in spite of much floundering with objectives that do not seem as clear as they will in retrospect.

Both of my parents were born in Sweden. In order to get some education my father joined the Swedish army and became an instructor in mathematics and fortifications. In later life he was a Lutheran minister, a newspaper editor, and a publisher. My mother, born Sophie Stråth, had a very good voice and a strong interest in music. My sister, Adele, is two years younger than I. Both of us were started at the piano when we were quite young. My sister was the better student, both in high school and at the piano. She finished a Bachelor of Music degree.

My parents changed the family name, which was Thunström, because it was so frequently mispronounced and misspelled. I have never joined any Swedish clubs and I have had very few contacts with Swedes until recently when I have become acquainted with Swedish psychologists.

I was born in Chicago on May 29, 1887, but my elementary education was in many scattered places, including Berwyn in Illinois, Centerville in Mississippi, a public school in Stockholm, Sweden, a boys' school in Stockholm, a grade school and a high school in Jamestown, New York.

At the age of fourteen it was expected that I would be confirmed in the Lutheran church. This was a problem because I declined to learn the catechism. When it became evident that this was really awkward, there was a conference with my father and another Lutheran minister and myself. I was offered the proposition that if I would select any three questions in the catechism to which I was willing to learn the answers, then I would be confirmed. I accepted this proposal and thus I was officially confirmed in the Lutheran church. When I accepted this proposal, my seniors really won the case because I read the catechism voluntarily in order to select the three questions to which I would be willing to memorize the answers.

The only honor that I received in high school was a first prize of thirty dollars in the Prendergast competition in geometry. With the prize money I bought a second-hand bicycle and a box Kodak which was the starting point for my work in photography. This is still my principal hobby. When I was a high school sophomore I had my first publication. It was a short letter to the *Scientific American*, published in June, 1905. At that time there was a good deal of discussion about the hydroelectric power companies at Niagara Falls. The power companies were accused of diverting so much water to their power plants that the beauty of Niagara Falls was being ruined. I proposed a very simple solution for the conflict between the power companies and the tourist interests. This is what I wrote:

"How to Save Niagara"

"To the Editor of the *Scientific American:*

"There has lately been much discussion on how to save Niagara Falls. I take here the liberty to describe a method for utilizing the greater part of the energy in the falls without injuring in the least the beauty of the falls and without necessitating any engineering structures in the vicinity of the falls.

"Suppose a dam, constructed across Niagara River, a few miles above the falls or at the beginning of the river. Let the gates of the dam be closed half of the time and opened half of the time, making the river flow, say, twelve hours in daytime. There would be no danger of overflow when the gates are shut, with the large area of Lake Erie above the dam. It is evident that twice the regular flow of the river could be extracted from Lake Erie in the daytime. Let the regular flow pass over the falls and take a quantity equal to half the regular flow continually for power purposes. This would give about 3,500,000 horsepower without injuring in the least the beauty of the falls. The gates of the dam could be open, say, nine hours in the day and three hours in the night, in order to make it possible to see the falls also at night. It seems to me that if these arrangements were possible, it would give a great amount of power and at the same time save the destruction of the falls.

Louis L. Thunström.

Jamestown, N. Y., June 20, 1905."[1]

There was a comment in one of the national magazines that I was proposing a way in which we could eat our cake and have it, too.

Engineering

Every high school student has probably puzzled at some time about the old problem of trisecting an angle. As a high school sophomore I worked

[1] L. L. Thunström, How to save Niagara, *Sci. Amer.*, 1905, 93, 27.

out a French curve which could be used with a straight edge for trisecting any angle but, of course, the solution was not within the restrictions of Euclidian geometry. In a freshman class in analytical geometry at Cornell, I learned how to write the equation for that curve and I showed the solution to my instructor. Professor Hutchinson told me that he knew over twenty solutions to that old problem but that he had never seen this particular one before. The solution was published in the *Scientific American*, and this was my second publication.[2] I also learned a good deal of physics in high school by puzzling about the old problem of perpetual motion.

At Cornell I started in civil engineering but changed to electrical engineering. Perhaps I should have majored in physics, instead. In the basement of Rockefeller Hall I worked with one of the physics instructors, Dr. Nasmith, who was studying the singing arc. I set up some experiments in the transmission of sound through a light beam by projecting the sound into an arc and receiving the light beam on a selenium cell. The idea was eventually to record the variations in light intensity on the edge of a motion-picture film by means of a cylindrical lens, but we never got that far. At the same time I was playing with a new design for a motion-picture camera and projector. In this design every point on the screen is continually lighted so that there is no dark interval or flicker. The film moves uniformly through the projector without any intermittent motion. These two effects are accomplished by means of two rotating sets of mirrors which keep the distance from the film to the objective constant, even though the film is moving continuously. This machine was actually built and demonstrated. But it was not until several years later that I demonstrated it in New York.

In the engineering school I had great admiration for Professor Dexter Kimball. His course on machine design was probably the best arranged instruction that I have even seen. For example, when several hundred students were working on the design of a shaper, he had the problems so arranged that no two students were working on exactly the same problem. In his lectures on machine design I acquired many ideas that have been useful in other contexts. He pointed out, for example, that in a design problem one starts at the cutting edge and that the frame is the last thing to be designed. The uninitiated probably sees the frame first and his impulse might be to design the frame and then to hook the mechanism onto the frame, which is the most ineffective way to proceed. Kimball's admonition that one should start to solve a problem at the cutting edge is a useful idea in many other contexts. In a committee session one can sometimes be helped by formulating as precisely as possible what is to be accomplished. That is the cutting edge of the problem. An organizational outline might then correspond to the frame of a machine.

[2] L. L. Thurstone, Curve which trisects any angle, *Sci. Amer. Suppl.*, 1912, 73, 259.

In one of his lectures Professor Kimball described some psychological characteristics in the history of a machine such as the sewing machine or the lathe. In the early stages in the development of a machine the designer introduces decorative effects which have nothing to do with function. It is as if the designer were trying to compensate for the inadequacy of the design even though he may not be aware of it. In the more mature stages of a machine its beauty is found in the close relation between design and function. These ideas were well illustrated with lantern slides of the history of mechanical devices. I remember thinking at the time that the curlicues on automobiles were certainly examples of nonfunctional additions. If we look at the automobile designs today (1950), we must admit evidence of immaturity even now. The useless and expensive shapes of automobile bodies and the distracting decorations on the automobile dashboard are evidence of the immaturity of present automobile design, and this is forty years after Professor Kimball's lectures on that subject.

Ever since my undergraduate days I have been interested in the psychological aspects of machine design, especially as regards human limitations in visual-motor coordination in the controls. During the Second World War a lot of military equipment was designed under pressure of time with inadequate consideration for this problem. The results were often serious. One does not have to go far to see examples of design defective because of psychological factors.

While in the engineering school, I became interested in the possibility of studying the learning function as a scientific problem. Partly in this connection I visited several lectures in psychology. One of these was a lecture by Professor Bentley on the higher thought processes, and I heard a lecture by Titchener. I remember being interested in his lecture but curious about his extremely formal and pompous manner. I certainly had no idea that I would myself be lecturing in the same subject. Boring finished his engineering degree at Cornell several years ahead of me, but I did not know him at that time. In the senior year I was elected a member of the electrical engineering fraternity, Eta Kappa Nu, an honor that I appreciated all the more because I did not earn it on scholarship.

The motion-picture machine problem had interested me off and on for several years during high school and college. Since Thomas Edison was manufacturing one of the best known motion-picture projectors at that time, I arranged to demonstrate my model in his laboratory in East Orange, New Jersey. A demonstration was arranged in 1912 and I went there with my working model. I met Mr. Edison and his chief engineer, Bliss, and several other men who expressed considerable interest in the model. They spent a good deal of time on it and they were evidently considering the possibilities of marketing this type of projector. They told me finally that it would be necessary to retool their whole plant for the manufacture of a machine of such radical design and that they were unwilling to do so; they said they had no doubt that the new type of projection would eventually be generally used, because it entirely eliminated the flicker. At that time the flicker was much more of a problem than it is in present machines. It was then that Mr. Edison offered me an assistantship in his laboratory. Immediately after being graduated with a mechanical engineer's degree, I went to work in Mr. Edison's laboratory in East Orange. I saw him daily and I had a very good chance to observe his work habits.

Thomas Edison was a man of strong convictions and he did not have much admiration for university education. For every experimental failure he seemed to produce three more experiments to try. In this sense he seemed to be tireless. The cot in his office was probably used for lying down to think about his problems as often as it was used for sleep. Thomas Edison seemed to have a startling fluency of ideas, which often ranged far from the immediate problem. He seemed to have an absolutely endless array of stories; very few of them were fit for publication. If problem-solving ability is to be studied scientifically and experimentally, it will be advisable to include studies of different kinds of fluency. Edison was interested in educational motion pictures but he had rather inadequate ideas on that subject. Even now, nearly forty years later, motion pictures have not found their proper place in the teaching process. When motion pictures are used in teaching, they usually cover so many ideas for each minute of the film that they are intelligible only to those who already know the subject. Effective teaching must be much more deliberate and it must include judicious repetition and summary. I have seen few motion pictures that satisfy this fundamental criterion for teaching effectiveness.

In the fall of 1912 I decided to return to a university with a good graduate school, and I accepted an instructorship in descriptive geometry and drafting in the engineering college at the University of Minnesota in Minneapolis. In my freshman classes I had two students who have won distinction in their respective fields and who are now on the University of Chicago faculty. They were Karl Holzinger, who is professor of Education, and Thorfin Hogness, who is now professor of chemistry and director of the Institute of Radiobiology.

While teaching in the engineering college for two years, I had my first instruction in psychology from Professor Herbert Woodrow and from Professor J. B. Miner. Woodrow taught experimental psychology and he was very generous with the engineering instructor who became interested in the experimental study of the learning function.

Graduate Study at Chicago

In the summer of 1914 I started graduate study in psychology with Professor Angell at Chicago. I recall one of my first impressions of graduate students of psychology. When they were asked a question, they would start to talk fluently, even when they obviously knew nothing about the subject. I was sure that engineers had higher standards of intellectual honesty. One of my first courses was called advanced educational psychology and it was taught by Professor Judd. I used to wonder what the elementary course could be like if the course that I was taking was called "advanced." I soon became accustomed to the fact that prerequisites did not mean anything and that there was no real sequence of courses in psychology, even though they were listed by number and title to give the appearance of a sequence, in which one course was supposed to build on another. I never had an elementary course in psychology or in statistics. My first degree was M.E., and I was never flattered when it was interpreted at Chicago as a master's degree in Education!

One of my accomplishments during that year was to learn how to carry five soup plates over my head through the swinging doors at the University Commons where I worked as a waiter. The alumni waiters from the University Commons include a very large group of professional men.

One of the most interesting among the graduate students in psychology was Beardsley Ruml, who has earned more distinction than any of the rest of us. He left psychology to do it. Even as a graduate student he was very much interested in economic theory. I recall one evening at the Midway Gardens when the discussion turned to economics. Ruml, armed with a stein of beer, declared emphatically that we would be just as well off if we dumped all the gold in the ocean. It was prophetic of later times.

Carnegie Institute of Technology

In 1915 Walter Bingham interviewed graduate students at Chicago to find assistants for the newly established Division of Applied Psychology at Carnegie Institute of Technology in Pittsburgh. He was assembling his staff for that important and interesting new development. He asked me about psychological research. I told him about my interest in the galvanic reflex and its possibilities for experimental psychology. With some hesitation I also told him about my interest in the psychological problems concerned with melody. At that time I was considering writing a master's thesis on the Hindu ragas, in which the melody covers a very small pitch excursion so that occidental standards of tonality are not involved. The psychological problem is then to determine, if possible, what constitutes the perceptual unity of such a melody. I had no idea at the time that Bingham was himself interested in such problems, that he had worked with Stumpf and von Hornbostel, and that he was interested in collections of phonograph records of exotic music. When I received a telegram from Bingham appointing me to an assistantship with an annual stipend of one thousand dollars, I was probably more pleased than I was twenty-three years later when I was promoted to a distinguished service professorship at Chicago.

Bingham's venture was to establish the first department of applied psychology in this country. The work was challenging at every turn and, while the orientation was always toward practical applications of psychology, there was a generous interest in theoretical problems. It was a privilege for a graduate student to be closely associated with the staff which included, besides Bingham, Walter Dill Scott, Clarence Yoakum, G. M. Whipple, E. K. Strong, Kate Gordon, W. W. Charters, and J. B. Miner. Bingham once told me that I was a good assistant but that I was not dependable in looking after details. He was right. After the first year as assistant, I was told about President Hamerschlag's comment that I had not made enough of an impression. Nevertheless, I received a reappointment for a second year as assistant in 1916. In 1917 I received a doctorate in psychology at Chicago and, after that, promotions in rank and salary came annually. Before I left Carnegie, I had a full professorship and was chairman of the department of psychology. It has often seemed strange that I did not undertake any fundamental theoretical problems during the seven years at Carnegie. Such interests must have been incubating without my realizing it because, when I later came to Chicago in 1924, such work seemed to get under way with a great deal of pressure.

All of our objective psychological test material at Carnegie was made available for the Army in the First World War. My own assignment was to work in the trade-test division in Newark, New Jersey, in the design of objective methods of appraising the oral trade tests. My main contribution in that assignment was the key-word principle for scoring oral trade tests. I wrote a memorandum[3] on the key-word principle for Beardsley Ruml, who was director of the Newark trade-test office. There was a lot of discussion in our staff in Newark as to whether the key-word principle would be feasible, but it was given a field trial with favorable results. A large number of oral trade tests for army use were then designed on the key-word principle.[4] In the application of this principle, the examiner asks a question which he reads from the manual. If the respondent uses any of the specified words

[3] Thurstone, *Oral trade tests*, Committee on Classification of Personnel in the Army, Trade Test Division, 1918.

[4] Thurstone, *Aid for interviewers*, Issued by the Adjutant General of the Army for use of personnel officers, Trade Test Division of the Committee of Classification of Personnel, Orange, N. J., 1918.

that are listed as key-words, then the respondent is given credit for the question. In this manner the examiner can give a preliminary screening of the candidates without himself knowing anything about the trades involved. The same principle was applied to picture tests in which the respondent answered questions concerning the numbered or lettered parts in pictures of trade equipment and processes.

One publication, written at Carnegie, was a short monograph on *The Nature of Intelligence* (1924). It was published in the International Library of Psychology, Philosophy, and Scientific Method in London. The monograph was the result of ideas initiated by Professor Mead's lectures at Chicago. Professor Mead's lectures in social psychology probably had a greater influence on my psychological thinking than any other course. It certainly had nothing to do with the social psychology of 1950. I became interested in focal consciousness as representative of the incomplete act. I tried to relate the concepts of the incomplete act with the concepts of intelligence. I tried to show that the degree of intelligence is associated with the degree of incompleteness of the act at which it becomes focal in consciousness. The more incomplete the act, the greater is the range of possible overt expression. If the act becomes focal in consciousness at a very incomplete and abstract stage of development, then the conscious choices control a wide range of overt resolution. The less intelligent act approaches completion before it becomes focal in consciousness and therefore it controls a very narrow range of possible overt expression. This interpretation of intelligence is sometimes listed in the textbooks as one of the numbered theories that students are expected to memorize. I have never seen a textbook summary of this theory which is intelligible to me.

The department of applied psychology flourished for eight years at Carnegie, but in 1923 it was no longer in favor and the research activities in applied psychology were discontinued. In these early days of applied psychology, Walter Bingham's group at Carnegie made substantial contributions towards the eventual acceptance of applied psychological research.

Washington in 1923

In January, 1923, I went to Washington to help initiate some studies in civil service personnel methods in the Institute for Government Research which was supported by a foundation grant. The purpose was to assist civil service commissions throughout the country to write better civil service examinations. My assignment was to prepare materials and manuals from which civil service commissions might prepare improved examinations with the new objective methods. Some of the commissions were very receptive to the idea of having a central agency prepare materials for use in different

commissions with a considerable saving in expense and time. At that time very few commissions had psychologists and qualified examiners who were familiar with the objective methods. Much has been accomplished since that time and many of the civil service commissions now have technically trained psychologists and examiners on their staffs.

While in Washington, in 1923, I discussed with some friends in the Navy the possibility of investigating experimentally the problem of learning during sleep. This was evidently a new idea and I was invited to carry out a preliminary experiment on learning the telegraphic code. The first experiment was made with the Navy in Washington. An instructor gave code practice at night when students were asleep. The practice was given in half-hour periods with alternate half hours free from practice. The speed was adjusted each night to about two words a minute faster than the average speed of the class during the previous day. It was found that the class completed the course in three weeks less time than was expected, and, as a consequence, there was great interest in the idea. I was invited to set up a more complete and controlled experiment at Hampton Roads, and that was done. I made only occasional visits to Hampton Roads, so that I was not there to supervise the work which was carried out by noncommissioned officers. After the experiment was under way, I received a letter from Captain Smith at Hampton Roads explaining that the experiment had probably failed because of the lack of comparability of the control group and the experimental group. The instructors of the control group were afraid that they would be judged as to their teaching efficiency if their class did not keep up with the experimental group which was getting code practice during sleep. In order to overcome such a supposed handicap, they gave several hours' additional practice daily to the control group in the hope of keeping up with the experimental group. That was about the time when I left Washington so that I could not be at Hampton Roads to set up a new experiment and to explain the procedures more adequately to the instructors.

Return to Chicago

It was in the summer of 1924 that Thelma Gwinn and I were married and we returned to Chicago where I had been appointed an associate professor of psychology. (At that time, and several years later, we had the opportunity to go to Berkeley, California.) Both Thelma and I had been graduate students at Chicago and we were thrilled to return there. Since I had been teaching statistics at Carnegie, I volunteered to give such a course at Chicago. Professor Carr was willing for students in psychology to take a one-quarter course in descriptive statistics if they wanted it. Although this course was a novelty in the department at Chicago, it was not important work. My main attention went to the teaching of mental test theory. In all of the American colleges this subject was taught mainly from the various authors' manuals, and practically all such courses were confined to detail of the Stanford-Binet test. Neither instructors nor students seemed to have any interest in the theory of this subject, and this circumstance was probably responsible for the low prestige of mental test work. I decided to make some contribution toward improving this situation, and I now had the definite objective to start work on fundamental problems in psychological measurement. Most of my previous work had been concerned with descriptive and applied aspects of psychological measurement.

There was general discussion about the normality of the distribution of intelligence at point age and I investigated the application of this assumption to various educational scales that had been constructed. In the early educational scales it was assumed, in effect, that the distribution of any educational test was the same for young children as for educated adults and that they differed only in the mean. Turning to the description of general intelligence, I assumed two parameters for each age-group, namely, the mean and a measure of dispersion. Applying this idea, a scaling method for psychological tests was developed and this was my first paper on the theory of psychological measurement.[5] I regard that paper as one of my best.

The next problem was to examine the mental-age concept which had previously been criticized by Otis and others. In another paper[6] I described the logical difficulties of the mental-age concept. In scoring the test performance of a child there is always some uncertainty as to whether the child is failing in a test item or whether he is merely distracted. In an attempt to minimize this source of error in the total score, I wrote a paper proposing that the score should be a scale value which is exceeded by as many successes as there are failures below it.[7] Interesting things are often discovered accidentally. At one time I asked my research assistant, Annette McBroom (Wiley), to plot two curves for some psychological test data, namely, the relations between mean-test performance against age and the dispersion against age. Both of these were determined first by scaling. Although I had not asked for it, she also plotted the relation between the mean-test performance and the standard deviation for each age after these values had been obtained by scaling. I then noticed that the relation was linear for the successive ages. Capitalizing on this simple relation, I located a rational origin for the scale of intelligence. This was done by extrapolating the linear relation until it reached a base line of zero dispersion. I reasoned that if we locate a point on a scale at which variability of test performance vanishes,

[5] Thurstone, A method of scaling psychological and educational tests, *J. educ. Psychol.*, 1925, 16, 433–451.
[6] Thurstone, The mental age concept, *Psychol. Rev.*, 1926, 33, 268–278.
[7] Thurstone, The scoring of individual performance, *J. educ. Psychol.*, 1926, 17, 446–457.

then such a point ought to represent a rational origin because the dispersion cannot be negative. This idea is perhaps remotely analogous to some ideas in the kinetic theory of gases. I tried this procedure on a number of psychological tests that had been scaled, and I found that the age at which the rational origin is located turns out to be several months before birth. My neurological friends assured me that such a finding could be justified and a paper on this subject was published in 1928.[8] Next we turned attention to the problem of the mental growth curve. The special difficulty with this problem, in contrast with similar curves for physical growth, is that in psychology we had no metric for appraising intelligence. Since the scaling method provides such a metric and a test for its internal consistency, we decided to construct a mental growth curve by a scaling method and with a method of locating a rational origin. That material was published in 1929.[9]

Shortly after coming to the University of Chicago, I had an opportunity to join the staff of Dr. Herman Adler at the Institute for Juvenile Research on a part-time basis. That experience was profitable in many ways. My work with Dr. Adler was largely advisory on problems of personnel and research. It was at that time I discovered Richard Jenkins, a medical student, in one of my classes. I offered him a research assistantship at the Institute for Juvenile Research and he started to work on the problem of intelligence in relation to family size and birth order. His work was so outstanding that I made him a co-author of a monograph on that problem.[10] Dr. Jenkins was later director of the Institute. At that time I met Andrew W. Brown who joined the Institute staff. My work at the Institute was terminated when I was offered, through Professor Charles Merriam, a promotion to a full professorship at the University of Chicago. It was a curious circumstance that the promotion to a professorship at Chicago did not come through the department of psychology. Ever since my promotion to a professorship, our work has been in the Social Science building and we have had the friendly interest of our colleagues in sociology, political science, and economics. The Social Science Research Committee has given us several research grants and some space. I have especially appreciated that in all these relations Dean Ralph Tyler of the Social Science Division has been helpful and friendly to our research projects.

Since there was no textbook on the theory of mental tests, I assembled a lithoprinted booklet on the reliability and validity of tests,[11] which was useful for teaching. Most of the material was drawn from journal articles on statistics, including Spearman's early work on reliability. The pamphlet was

[8] Thurstone, The absolute zero in intelligence measurement, *Psychol. Rev.*, 1928, 35, 175–197.
[9] Thurstone and L. Ackerson, The mental growth curve for the Binet tests, *J. educ. Psychol.*, 1929, 20, 569–583.
[10] Thurstone and R. L. Jenkins, *Order of birth, parent-age, and intelligence*, 1931.
[11] Thurstone, *The reliability and validity of tests*, 1931.

prepared for temporary use in teaching because of the complete absence of any textbook on mental test theory. Today, twenty years later, although that pamphlet has been out of print for many years, we have frequent inquiries for it. The reason for this circumstance is that until now there has not been any textbook on this important subject. It is difficult to understand why this should be the case, because all departments of psychology give instruction in psychological tests. I am rewriting that old pamphlet in the form of an elementary book on test theory, and fortunately Gulliksen of Princeton has written a rather complete treatise on this subject which has just been published.[13] The absence of any suitable teaching material for elementary courses in test theory may indicate a continued lack of interest in the principles of this subject. The curricula of psychology departments still have many courses of instruction in test theory, but I fear that most of them teach the student only how to give particular tests, especially the Rorschach which is now so much in vogue.

In 1925 I had some evening discussions with Dr. David Levy in regard to the Rorschach test which he was introducing in this country. We discussed the possibility of objectifying the scoring and the interpretation. I was interested by the proposal to undertake this study, but I decided to gamble my time on other problems in psychological measurement because I had in mind the scaling methods for tests and the rewriting of psychophysical logic. The original monograph by Rorschach is well worth reading, but few of his enthusiastic followers seem to have contributed anything of consequence to psychological theory and science. In psychological science there is now no group that is more cultish and superficial than the Rorschach followers, and yet they dominate a large section of clinical psychology.

Psychophysics

Now we turn to psychophysics which is psychological measurement proper. It is another line of development that was started soon after our arrival at the University of Chicago. When I started to teach psychological measurement in 1924, it was natural that I should encourage the students to learn something about psychophysical methods. The standard reference was, of course, the two big volumes on quantitative psychology by Titchener. The determination of a limen was the basic problem in old-fashioned psychophysics. In order to be scholarly in this field, one was supposed to know about the old debates on how to compute the limen for lifted weights to two decimal places with a standard stimulus of one hundred grams. One could hardly worry about anything more trivial. Who cares for the exact determination of anybody's limen for lifted weights? In teaching this subject I felt

[13] H. Gulliksen, *Theory of Mental Tests*, 1950.

that we must do something about this absurdity by introducing more interesting stimuli. Instead of lifted weights we used a list of offenses presented in pairs with the instruction that they should be judged as to their relative seriousness. The subject checked one of each pair of offenses to indicate which he thought was the more serious. Instead of selecting one of the offenses as a standard, we asked the subjects to compare every stimulus with every other stimulus. It was now apparent that the classical method of constant stimuli is a special case of the more complete psychophysical method of paired comparison. We followed this same procedure with a list of nationalities that were also presented in pairs. For each pair of nationalities the subject was asked to check the one which he would prefer to associate with. I did not realize at the time that it was going to be necessary to rewrite the fundamental theory of psychophysics in order to cope with data of this type.

The starting point for our work in psychophysical theory was a square table with rows j and columns k. In examining this square table of proportions for any experiment, I set myself this theoretical problem, namely, that if some of the entries were erased, then it should be possible in an overdetermined experiment to fill in the missing entries so as to be consistent with the given entries. The first impulse might have been to solve this problem in the statistical manner by plotting one column against some other column, or perhaps against the sum of the rows. A linear or non-linear regression might then be fitted and one might be able to make an estimate about the missing entries. A statistical procedure of this kind is useful, especially when one has no theory or idea about the underlying order. In this case my ambition was to formulate a theory about comparative judgments, so that the missing entries could be supplied on a more rational basis. This problem haunted me for a time because it implied a subjective metric which required, of course, a subjective unit of measurement. No solution was acceptable unless it could be operationally defined with checks for internal consistency. I wrote a solution to this problem in a paper on psychophysical analysis, which I regard as the best paper that I have produced. The resulting equation was called a law of comparative judgment. The principal difficulty was not in the mathematics which turned out to be extremely simple, but rather with the psychological concepts. By the method of paired comparison we were able to allocate each stimulus to a point on the subjective continuum. In addition to a mean scale value on this continuum, each stimulus was also characterized by a dispersion which was called the discriminal dispersion. This dispersion was in the subjective or experienced quality of a stimulus and the standard deviation of this dispersion for a standard stimulus could be chosen as a subjective unit of measurement. Subjective scaling of this kind is completely defined operationally and tests for internal consistency were readily available. For example, the sum of the subjective separations between the stimulus pairs AB and BC must be equal to the experimentally independent determination of the separation AC. If the continuum was undimensional, then this simple type of check would establish the fact. Five papers in 1927[13] describe my efforts to establish some law and order in psychophysics, but I should never claim that they are more than a beginning in the development of psychophysical theory.

The next problem was to investigate the relations of these new psychophysical formulations to Fechner's law and Weber's law. Fechner's law states that the subjective allocations of the stimuli are a logarithmic function of the physical stimulus magnitudes. The law of comparative judgment enables one to allocate each stimulus to the subjective continuum, even when the stimuli vary in ambiguity or discriminal dispersion. Perhaps its most important feature is that a subjective metric can be established even when there exists no physical stimulus magnitude for the attribute that is judged. The law of comparative judgment enables us to measure social, moral, and aesthetic values where we have no corresponding physical stimulus magnitudes. Weber's law does not involve the subjective continuum, because it states a relation between two physical magnitudes, namely, the stimulus magnitude and the physically measured error. This relation is stated statistically in terms of the relative frequency with which a given error magnitude is to be expected. A customary statement of Weber's law contains a number of ambiguities which make it indeterminate as to how it should be experimentally verified. I attempted to state Weber's law symbolically in such a manner that its experimental verification became operationally defined. It also became apparent that equally often noticed differences are not equal unless the discriminal dispersions are equal. The reason why this circumstance had not previously been found was no doubt the fact that experimental work was confined to simple stimuli whose ambiguities were all equally small. An experimental demonstration of the separation between Fechner's law and Weber's law could be easily set up by varying the stimulus ambiguities. These should markedly affect Weber's law, but theoretically they should have no effect on Fechner's law. At the same time I questioned the Phi-Gamma hypothesis,[14] which was demonstrated on logical grounds to be necessarily false. It was demonstrated to be false if one assumed Weber's law or any other law in which the observational errors are any monotonic increasing function of the stimulus magnitudes. The method of rank order

[13] Thurstone, Psychophysical analysis, *Amer. J. Psychol.*, 1927, 38, 368–389; The method of paired comparisons for social values, *J. abnorm. soc. Psychol.*, 1927, 21, 384–400; Equally often noticed differences, *J. educ. Psychol.*, 1927, 18, 289–293; A law of comparative judgment, *Psychol. Rev.*, 1927, 34, 273–286; Three psychophysical laws, *Psychol. Rev.*, 1927, 34, 424–432.

[14] Thurstone, The Phi-Gamma hypothesis, *J. exp. Psychol.*, 1928, 11, 293–305.

was brought in to the same theoretical structure and it was shown to be consistent with the method of paired comparison.[15]

Subsequent developments in psychophysical theory were concerned largely with the measurement of social values. Two of the later studies will be considered here because they are concerned with psychophysical theory. One of these deals with the number of dimensions in a psychophysical domain, a problem that was frequently discussed in our seminars. The initial attempt to rewrite psychophysical theory assumed a unidimensional subjective continuum with adequate tests for unidimensionality. Since some psychophysical problems deal frankly with a multidimensional domain, there was necessity for setting up a suitable experimental procedure to establish a multidimensional subjective metric. Methodologically this problem was solved by Marion Richardson,[16] who introduced the method of triads as a psychophysical method. The subject is presented with sets of three stimuli and he is asked to indicate which two are more similar and which is, therefore, the odd stimulus. The subject does this for each presented triad and the proportions of paired comparison can then be inferred. It should be carefully noted that the subject's task is not to say that one stimulus is x'er than another as to some specified attribute x. Such a question implies unidimensionally. In the method of triads the subject is merely asked to indicate which pair of stimuli are more nearly alike. This gives him freedom to make his judgments in a multidimensional subjective domain. The analytical problem of determining the dimensionality for a set of data by the triad method was solved by Young and Householder[17] who contributed an ingenious matrix solution. Therefore we are no longer limited to a unidimensional domain in dealing with psychophysical problems. Psychological measurement can be carried out with a multidimensional subjective psychophysical metric.

About twenty years later I returned to a different type of psychophysical problem. In psychophysical analysis that has been discussed here so far, the problem has been to allocate each stimulus to a point in the subjective continuum whether it be unidimensional or multidimensional. If we assume that this job has been done, then we can ask whether any predictions can be made as to how people will behave in relation to the objects or ideas that they have been judging. This is, in a sense, an obverse psychophysical problem because, instead of allocating the physical stimulus to the subjective continuum, we now want to use the subjective allocations in order to predict overt conduct. A simple examination of this problem leads to some rather

[15] Thurstone, Rank order as a psychophysical method, *J. exp. Psychol.*, 1931, 14, 187–201.

[16] M. Richardson, Multidimensional psychophysics, *Psychol. Bull.*, 1938, 35, 659–660. (Abstract.)

[17] G. Young and A. S. Householder, Discussion of a set of points in terms of their mutual distance, *Psychometrika*, 1938, 3, 19–22, 126.

startling and interesting conjectures which can probably be experimentally verified. This is what I have called the prediction of choice.[18]

These inferences seem to have immediate practical application in a variety of problems, such as the prediction of elections, the study of consumer preferences, and the prediction of relative consumption of competing objects. Consider two political candidates who have the same average popularity. Their subjective scale values are, therefore, the same. Let one of these candidates have a much more variable dispersion in the sense that some people are enthusiastic about him while others hate him. The other candidate may be assumed to have a smaller dispersion so that there are not such wide differences of opinion about him. In an election these two candidates would split the vote. The more variable of the two candidates can insure a plurality by introducing a third candidate, even with lower average popularity, on the assumption that the correlations in popular estimates of the three candidates are not very high. The less variable of the two principal candidates has no such leverage on this situation. The same logic applies in the prediction of choice among competing commodities. Lately it has been found that the extremely simple psychophysical method of successive intervals can be used for survey purposes in the prediction of choice. The method of successive intervals is essentially a variant of the method of single stimuli with multiple categories of judgment. It seems almost certain that psychophysical theories and methods will invade an increasing territory of practical application. By these methods we can honestly say that we are measuring moral and social values.

Social Psychology

Although I have not attempted to gain competence in the general field of social psychology, our work in psychological measurement has naturally turned to the measurement of social values. This was largely due to our attempt to introduce some life and interest in psychophysics, which was dominated for a long time by the trivial problems of lifted weights and limen determinations. The extension of psychophysical methods to the measurement of social values was especially tempting when it turned out that the law of comparative judgment is entirely independent of the physical stimulus magnitudes. This circumstance enables one to use the law in the measurement of social and aesthetic values where physical stimulus measurement is entirely irrelevant. Our work on attitudes was started when I had some correspondence with Floyd Allport about the appraisal of political opinions, and there was discussion here at that time about the concept of social distance which was introduced by Bogardus. It was in such a setting that I speculated about the possible use of the new psychophysical toys. I wrote

[18] Thurstone, The prediction of choice, *Psychometrika*, 1945, 10, 237–253.

a paper entitled "Attitudes can be measured."[19] Instead of gaining some approval for this effort, I found myself in a storm of criticism and controversy. The critics assumed that the essence of social attitudes was by definition something unmeasurable. There followed a number of other papers on the construction of particular attitude scales and on methodology, including a little monograph on *The Measurement of Attitude* by Professor E. J. Chave and myself (1929). There was a good deal of interest in the subject and a lot of attitude scales were constructed for particular issues. These included attitude scales on the treatment of criminals, patriotism, Sunday observance, the church, war, the Negro, prohibition, unions, communism, public office, constitution of the United States, social position of women, immigration, birth control, the Chinese, the Germans, the law, censorship, evolution, capital punishment, economic position of women, and others.

Our best work in this field was a study, supported by the Payne Fund, on the effect of motion pictures on the social attitudes of high school children. The only adequate description of that work and the principal findings was in a lithoprinted monograph[20] which has long since been out of print. About thirty experiments were carried out on a large number of films. The procedure was to arrange with a local theater to run a particular film on a particular evening. The films were selected by special previews here in Chicago. Those films were selected which might have some effect on the social attitudes of high-school children on some debatable issue. Free tickets were distributed in the local high school and students were told that they must write their names on the tickets to validate them. In this way we had a record of the students who saw the film. A few days before and a few days after the film was shown, we gave attitude schedules in the high school on the issue which might be affected by the film. In this way we demonstrated, for example, that the film "The Birth of a Nation" has a very strong effect in making high school children less friendly toward the Negro. Similar studies were made with other films on other issues. At Mooseheart we succeeded in demonstrating by experiments the summation effect on social attitudes. When a single film did not give a statistically significant effect, and when two films a week apart gave a barely noticeable effect, we demonstrated that three films showed a significant effect. The summation effect is an important principle in a propaganda program with material in which a single film may not be adequate to demonstrate a significant shift in attitude.

There was heavy correspondence with people who were interested in attitude measurement, but they were concerned mostly with the selection of attitude scales on particular issues to be used on particular groups of people.

[19] Thurstone, Attitudes can be measured, *Amer. J. Sociol.*, 1928, 33, 529–554.

[20] Thurstone and Ruth C. Peterson, *The Effect of Motion Pictures on the Social Attitudes of High School Children*, 1932 (obtainable on microfilm from the Univ. of Chicago Library, Dept. of Photographic Reproduction, Film No. 1696).

There seemed to be very little interest in developing the theory of the subject. The construction of more and more attitude scales seemed to be unproductive, and I decided to stop any further work of this kind. Incomplete material for a dozen more attitude scales was thrown in the wastebasket and I discouraged any further work of that kind in my laboratory. I wanted to clear the place for work in developing multiple factor analysis.

Our social psychological studies have been opportunistic in a sense, because they have consisted in applications of new psychophysical methods on suitable occasions. Rarely have we set out to devise a measurement method for an existing social psychological problem. The question can be raised to what extent the development of a young science should be under pressure of the major problems of the day. Perhaps the principal reason why social psychology has very low prestige is that many authors in that field reveal that they have an axe to grind. It is doubtful whether one can be a propagandist and a scientist in the same field and at the same time. Similar comments can be made about many social studies. The excuse is often made that social phenomena are so complex that the relatively simple methods of the older sciences do not apply. This argument is probably false. The analytical study of social phenomena is probably not so difficult as is commonly believed. The principal difficulty is that the experts in social studies are frequently hostile to science. They try to describe the totality of a situation and their orientation is often to the market place or the election next week. They do not understand the thrill of discovering an invariance of some kind which never covers the totality of any situation. Social studies will not become science until students of social phenomena learn to appreciate this essential aspect of science.

Learning

Although the field of learning was my first interest in psychology, I have not been productive in that field. It has always seemed to me that we have missed something essential in learning which is not represented by the ordinary studies of rote learning. My doctor's dissertation on the learning curve equation[21] was a very simple study, and a paper on variability in learning[22] related to a current controversy at that time. Something more elaborate was developed in a paper on the learning function,[23] in which it seemed that the learning curve for rote learning should be S-shaped. Some of these ideas were incorporated in a study of the relation between learning time and length of task,[24] in which I was pleased to find that nine experimental studies

[21] Thurstone, The learning curve equation, *Psychol. Monogr.*, 1919, 26, No. 114.
[22] Thurstone, Variability in learning, *Psychol. Bull.*, 1918, 15, 210–212.
[23] Thurstone, The learning function, *J. gen. Psychol.*, 1930, 3, 469–493.
[24] Thurstone, The relation between learning time and length of task, *Psychol. Rev.*, 1930, 37, 44–53.

in the literature fitted the theoretical expectations according to which the learning time varies as the 3/2 power of the number of rote items in the list. Another study of the error function in maze learning[25] was an elaboration of the same theme in another setting.

Multiple Factor Analysis

The work on multiple factor analysis was started in 1929, but it did not get under way seriously for another year until completion of other commitments. The original observation equation for multiple factor analysis was written in Pittsburgh before 1922, but it was ten years before I started serious work on the problem. Much has been written on multiple factor analysis, so that this discussion will be limited to some of the incidents and accidents concerned with the development of the main ideas. Our early work was supported by annual grants from the Social Science Research Committee at the University of Chicago. We had a number of research grants from the Carnegie Corporation for research assistants and for the purchase of calculating machines. One of the Carnegie grants was specifically for the development of a matrix multiplying machine. We investigated some of the new calculating equipment that was being designed at Cambridge, but we finally decided to use a modified form of IBM scoring machine which could be adapted for matrix multiplication. The machine was built by the IBM Company in Endicott, and it has been in daily use in our laboratory for many years. As far as I know, that is the only matrix multiplier of this type that has been built. The machine was designed largely by Dr. Ledyard Tucker, and we had the interest and assistance of Professor Eckert of our Physics Department at that time.

When it became evident that the development of multiple factor analysis would require special research grants, I decided to consult Dr. Keppel on one of my trips to New York. I explained to Dr. Keppel that I needed some research funds to develop what I called multiple factor analysis and that it was a big gamble. I told him that I could give him no assurance that this gamble would be successful but that I expected to give my major time to this problem, perhaps for several years. He gave me an initial grant of $5,000, which was a great help. Subsequently we had several grants from the Carnegie Corporation for this work. I did not realize at that time that I would be giving major effort to this problem and its application in identifying primary mental abilities during the next twenty years.

Many of the turning points in the solution of the multiple-factor problem depended on minor incidents. On one occasion, when I was having lunch with Professor Bliss, chairman of the Mathematics Department, and with

[25] Thurstone, Error function in maze learning, *J. gen. Psychol.*, 1933, 9, 288–301.

the astronomer Bartky, I asked them about some arithmetical operations that I was doing on rectangular tables of numbers. I asked them if there was any kind of mathematics that could be useful in operations of that kind. They both laughed and told me that I was extracting the root of a matrix. When I asked what was meant by a matrix, they suggested that I talk with Professor Barnard, who was teaching three courses in this subject. Professor Barnard took a friendly interest in the problem and helped us a great deal. I appointed Patrick Youtz as a research assistant, and he tutored me in the elements of matrix algebra. Youtz was then a graduate student of mathematics, and he is now on the staff at M.I.T. At a later time Bartky gave valuable assistance when I was working on the principal axis solution. I was trying to solve a problem in least squares with a conditional equation, although I had not at that time put the matter in so simple and direct a manner. He told me that this was an old problem in celestial mechanics and he gave me the solution. Then I discovered that I had myself studied that solution in theoretical mechanics, but I did not think of the solution in connection with my own problem. I described the principal axes solution at an A.A.A.S. meeting in Syracuse in 1932. These incidents illustrate the erratic way in which research can be done, in spite of the limitations of the investigator.

Beginning with Spearman's famous paper in 1904, there was a quarter of a century of debate about Spearman's single factor method and his postulated general intellective factor g. Throughout that debate over several decades, the orientation was to Spearman's general factor, and secondary attention was given to the group factors and specific factors which were frankly called "the disturbers of g." Even now much British writing and some American writing on factor analysis are oriented toward the general factor and the group factors which constantly disturb the general factor g. The development of multiple-factor analysis consisted essentially in asking the fundamental question in a different way. Starting with an experimentally given table of correlation coefficients for a set of variables, we did not ask whether it supported any one general factor. We asked instead how many factors must be postulated in order to account for the observed correlations. At the very start of an analysis we faced very frankly the question as to how many factors must be postulated, and it should then be left as a question of fact in each inquiry whether one of these factors should be regarded as general.

In 1931 and 1932 some of the present writers on multiple-factor analysis were still concerned with the problem of this general factor and with such related problems as the standard error of the tetrad difference. At one time I decided to relate the work on multiple-factor analysis to the earlier work of Spearman, and for this purpose I wrote the tetrad difference equation on a piece of paper. I expected to spend a good deal of time on this problem. As I looked at the tetrad difference equation, it dawned on me

that it was nothing but the expansion of a second-order minor. If all of the second-order minors vanish, the rank is, of course, unity, and immediately one can then ask the corresponding question about the vanishing of third-order minors, fourth-order minors, and so on. If the question had been asked in that manner, multiple-factor analysis would probably have developed many years earlier. The work in multiple-factor analysis introduced several ideas which extend the earlier work of Spearman. These ideas include the interpretation of Spearman's single factor theorems as a special case of unit rank, the matrix formulation of the factor problem, the communalities, the simple structure concept, the rotation of the reference frame for scientific interpretation, the desirability of interpreting primary factors as meaningful parameters, the use of oblique reference axes, and the principles of configurational invariance. Later work introduced the second-order factors and studies in the effects of selection on the factorial structure. All of these ideas are concerned with methodology.

Throughout this work, the emphasis has been on factor analysis as a scientific method distinguished from problems of statistical condensation of data, which we have considered to be of secondary importance for most scientific work. There are, of course, entirely legitimate problems in which statistical condensation is the essential purpose of a factorial analysis. But this is not the type of problem to which we have given principal attention in the Psychometric Laboratory at Chicago. My first paper on multiple-factor analysis was published in 1931[26] and a multiple-factor analysis of vocational interests was published in the same year.[27] The principal publications in this field from our laboratory have been *The Theory of Multiple Factors* (1933), *The Vectors of Mind* (1935), and my APA presidential address of the same title.[28] The first volume was rewritten in more extended form with the title *Multiple-Factor Analysis* which was published in 1947. In the last fifteen years multiple-factor analysis has attracted the attention of many competent students, so that there are now available a number of texts on this subject. For some reason that I have never been able to understand, the principal concepts of multiple-factors analysis have met severe criticism. Among these the greatest surprise was the criticism and ridicule of the introduction of communalities, the simple structure concept, and the oblique reference frame. These concepts and methods were introduced to resolve troublesome problems of factorial indeterminacy. The striking results that have been obtained in a large number of scientific studies with these methods have reduced the severity of criticism, but these concepts are by no means generally accepted. A curious type of criticism has been made of my attempt to give meaningful

[26] Thurstone, Multiple factor analysis, *Psychol. Rev.*, 1931, 38, 406–427.
[27] Thurstone, A multiple factor study of vocational interest, *Person. J.*, 1931, 10, 198–205.
[28] Thurstone, The vectors of mind, *Psychol. Rev.*, 1934, 41, 1–32.

interpretation to the factorial parameters. I can hardly imagine a more absurd type of criticism and yet it is very commonly made.

Primary Mental Abilities

As soon as the methods of multiple-factor analysis had been developed to the point where practical application seemed feasible, we started work on such a project. The development of a large battery of fifty-seven tests for various aspects of intelligence was a large undertaking. When this job had been done, the whole battery was given to a group of 240 volunteers in the spring of 1934. Analysis of these records constituted our first attempt to identify primary mental abilities. A short paper on this experiment was published in *Psychometrika*[29] and a more complete report constituted the first issue in the *Psychometric Monograph Series.*[30] Although my first text on multiple-factor analysis, *The Vectors of Mind,* had previously been published (1935), with a development of the concepts of communality, the rotation of axes, and the use of oblique axes, I hesitated to introduce all of these things in the first experimental study. In particular, there was strong advice from Thorndike, Kelly, and other men for whom I had respect, that an oblique reference frame would be completely unacceptable. Instead of proceeding according to my convictions, that first factor study was published with the best fitting orthogonal frame, although we knew about more complete methods. This was an effort to avoid the storm of controversy that we feared in the introduction of so many different procedures in the first experimental study.

In the last fifteen years the identification of primary abilities and traits has proceeded at an entirely unexpected pace. Fortunately the problem has attracted the attention of some mathematicians and mathematical statisticians. A number of the papers are so technical that they are beyond the comprehension of some of the rest of us who were concerned with the development of these methods in their primitive stage.

The correlations of the primary factors can be factored, just like the correlations among tests. When this is done we find several second-order factors. One of these seems to agree very well with Spearman's general intellective factor g. The critics feature our support of Spearman's g, but they ignore the fact that this work represents at least a modest gain in unraveling the complexities of mental organization.

When a number of the primary factors had been identified with some degree of assurance, it was challenging to develop some tests of primary abilities for use in the public schools. We hope that it will be possible to get teachers and psychologists to describe children in terms of their mental profiles instead of the single intelligence quotient. To develop tests of primary abilities for use in the schools introduces new problems. As usual there is always the limitation of time for psychological testing. The practical question is, then, how many of the primary abilities can be appraised in the amount of time that is allowable for psychological testing in the schools. The pressure is always to reduce to a minimum the time limit for each test so as to cover as many abilities as possible within one or two class periods. We have tried to make practical compromises in this regard in order to make available in the schools our findings about primary mental abilities. The results of these efforts, issued first in a set of tests of primary mental abilities, were distributed by the American Council on Education in 1938. Subsequently the procedures were simplified with various scoring devices and the tests were shortened considerably. The distribution of this test series was taken over by Science Research Associates where Lyle Spencer and Robert Burns are directors. It has been a pleasure to work with these men in the distribution of psychological test material because they have a genuine interest in the scientific values involved and also a realistic recognition of the practical demands in the schools. It was Robert Burns who was largely responsible for initiating the three-year research project on mechanical aptitude which was supported by the Office of Naval Research. In that study we verified rather clearly that the second space factor, identified by Guilford in his factor studies for the Air Force during the war, was the main component in the complex that is called mechanical aptitude.[31]

In all of our studies in psychological measurement and especially in the theoretical and experimental work on the primary mental abilities, I have been very fortunate in having my wife as a partner because she is a genius in test construction and related problems. With the assistance of Katherine Byrne and Katherine Vitato, she assembled a set of seventy games that could be given to five-year-old children who have not yet learned how to read. These game-tests were given to an experimental population of five- and six-year-old children and a multiple-factor analysis was made of the scores. That unpublished study showed essentially the same primary factors at the kindergarten age which we have found in other studies for adults. A new set of tests was constructed for kindergarten children.

Examination in the College

Before the reorganization of the college at the University of Chicago, Dean Ernest H. Wilkins had several committees at work on the problem of cur-

[29] Thurstone, The factorial isolation of primary abilities, *Psychometrika*, 1936, 1, 175–182.
[30] Thurstone, Primary mental abilities, *Psychometric Monogr.*, 1938, No. 1.

[31] Thurstone, An analysis of mechanical aptitude, Psychometric Laboratory Report, No. 62, 1951; also in *Psychometrika* (in press).

riculum construction. When Mr. Hutchins came to the university, he developed aggressively a revised curriculum for the college and many other new arrangements. When it was proposed to introduce comprehensive examinations for the determination of grades, I wrote a memorandum to Dean Works, in which I suggested certain principles that should be adopted in writing those examinations. I was asked if I would help to start the new examination procedure as chief examiner for the college. I accepted with the idea that I would help to get the system started, but I did not leave this assignment until Professor Ralph Tyler came to Chicago seven years later. I proposed some new principles to be used in the construction of college examinations, and these were accepted. One principle was that the examinations should become public property as soon as they had been given. The purpose of this system was to eliminate bootlegging of examinations in fraternity houses and elsewhere. One of the consequences was that a new examination had to be written each time, and here several novel ideas were introduced. No question was used in a comprehensive examination if the instructors did not know the answer. If the instructors started to argue about the answer to a question, it was either eliminated or revised until the instructors agreed about the answer. The identity of the student was not known by the person who assigned the grades. The grades were determined by the distribution of scores before the identities of the students were known. Some departments objected that new examinations could not be written each time that a course was given. Our response was that if a new examination could not be written at the end of each course, then there was no justification for the course.

In the initial work of the Board of Examinations we were fortunate in having an exceptionally good staff of examiners. On the staff we had Wolfle, Richardson, Gulliksen, Kuder, Adkins, Stalnaker, and Russell. I have always been proud of the fact that I collected this group of examiners when they were graduate students and recent post-doctorates. All of them have arrived professionally. The standards of workmanship were exceptionally high, and I believe that we had the good will of the faculty.

Studies of Personality

One of my principal interests in psychology to which I have returned several times has been the study of personality. Soon after completing the doctorate, I turned my attention seriously to the study of abnormal psychology, and I read Freud and a good deal of the psychoanalytical literature. My conflict here was that, on the one hand, the center of psychology probably was the study of personality, but, on the other hand, I was unable to invent any experimental leverage in this field. That was the reason why I turned to other problems that seemed to lend themselves to more rigorous analysis.

During my first year at the University of Chicago, Beardsley Ruml asked me to spend a quarter in Philadelphia to work with Elton Mayo. My assignment was to work with him daily and to try to decipher his psychological system. I found it an extremely profitable experience to spend the mornings with him at the Philadelphia General Hospital and the afternoons at Ardmore when he went there to work with patients. I became convinced that no one should ever receive a doctorate in psychology without such an experience, no matter what his major field might be. As to the assignment, I had to report that to systematize Elton Mayo was an impossible job. By that I did not mean to depreciate Elton Mayo for whom I had the very highest regard, but I did not see the possibility of any textbook exposition of his ideas.

Teaching

In 1948 we had an unusual experience when both Thelma and I were appointed as visiting professors at the University of Frankfurt in Germany. Our group was the first one to go from the University of Chicago to Frankfurt. Our principal motivation for that enterprise was the opportunity to help, even in a very small way, to repair what is left after the physical and moral destruction in Europe. Our lectures and seminars in Frankfurt were scheduled on the first three days of each week so that we had every week end for visiting lectures at Marburg, Heidelberg, Münster, and other places. We have never had more grateful students and colleagues. We brought American books and we were informed that these were the first books from outside Germany to reach their laboratories since before the war. We admired the efforts in reconstruction against terrific odds, including hunger, lack of supplies, and living quarters built by hand in the rubble.

We have had a number of foreign students in our laboratory. These included Charles Wang and E. H. Hsü from China, who have been productive. Mariano Yela from the University of Madrid, Spain, who spent two years here, was one of our best students. From South Africa we have had three superior students. John Karlin remained in this country and is now on the staff of the Bell Telephone Company laboratories in Murray Hill, New Jersey. Mrs. Melany Baehr was sent here from South Africa by the National Bureau of Personnel Research. Mrs. Baehr's dissertation was written here for a doctor's degree that was awarded in South Africa. We have had similar cooperative arrangements about several other dissertations for degrees that were awarded in other universities. Mrs. Carol Pemberton came here from South Africa and is now completing work for the doctorate with a dissertation on the closure factors in relation to personality traits. According to our last information several years ago, Nicholas Margineau was still a political prisoner in Rumania.

At the present time we have an exceptionally promising group in the Psychometric Laboratory. Among the advanced students are Thomas Jeffrey, who is in immediate charge of the Laboratory, Andrew Baggaley, Fred Damarin, Robert Fantz, William Harris, Ray Hartley, Thomas Johnson, Father Lawlor, John Mellinger, and Jonathan Wegener. In addition to these advanced students we have an exceptional group of Fellows: Professor Allen Edwards for the Social Science Research Council, Dr. Lyle Jones on a National Research Council fellowship, Dr. J. E. Birren on a Public Health Service fellowship, Jean Cardinet from Professor Piéron's laboratory in Paris, and Per Saugstad on a fellowship from Norway. As this manuscript is being written, Dr. Horacio Rimoldi is preparing to go to Uruguay on a new professorship.

The graduate students of psychology who have a major interest in research will probably find it advantageous now, as in the past, to serve an apprenticeship in teaching. To teach is probably still the best way to master one's subject and to recognize its major research possibilities. When I was promoted to a professorship at Chicago in 1927, I was offered the opportunity to devote full time to research without teaching obligations. I chose instead a program that implied the teaching of one course each quarter, and I still believe that was a wise decision, partly because it has enabled me to keep in touch with promising talent among graduate students and to select associates in my laboratory. The research men who are completely divorced from teaching are often isolated professionally unless they have served for some years in active teaching before withdrawing to their laboratories. Seldom is a young man destined to professional recognition if he withdraws from teaching immediately after the completion of the doctorate.

Psychological measurement is generally regarded as a field of specialization, but this is an unfortunate circumstance. Those who specialize in this field often regard mathematical statistics as their basic subject matter and the result is that they often forget psychological theory and problems. It would be more fortunate if the quantitative aspects of psychology were treated as integral parts of psychological theory and experimental procedure. Psychological measurement theory would not then be relegated to separate courses. It would be part of psychological subject matter in social psychology, the cognitive functions, personality theory, learning and forgetting, the sensory and perceptual functions, and the rest. It is a challenge to develop further the quantitative aspects of psychology itself. Mathematical statistics is a useful tool but it is an entirely different subject. It is in no sense a substitute for psychological measurement theory which is part of psychological science.

This biography has been concerned primarily with the development of psychological ideas that have guided my work. But all of my time has not been at the office and laboratory. Our family has lived in the same house for twenty-four years, and much of our interest has centered there. Our oldest son, Robert (23), has just been graduated in electrical engineering at Illinois Institute of Technology; Conrad (20) is completing the second year in medicine at the University of Chicago; and Fritz (18) is completing the sophomore year in physics at California Institute of Technology. Our policy has been to encourage their projects in a basement machine shop and electronics laboratory, the grinding of telescope reflectors, and the assembly of their own television set, a bedroom radio station that barely left room for a bed, and many other enterprises. For many years we have spent long vacations in a summer colony, Wabigama, on Elk Lake near Traverse City, Michigan. That unusual group of twenty families of scientists and professional men have individual cottages on the lake, and we have had an unusual experience in community living with one of the most friendly groups we have ever known. The boys have had the experience of knowing well these men and their families in the informal life of that summer colony, a privilege that they appreciate the more as they reach maturity. Summers have been more than vacations. They have been rich episodes in living, including fishing, fly-tying, rod-making, sailing, house construction, work in the woods and orchard, outboard motors, and help in all the emergencies. Outside of my own connection at the University, the associations in Wabigama have had the most important impact on our family.

Thelma has the outstanding achievement in our family in managing an active household at the same time that she was professionally active. She has been a partner in every research project in the Psychometric Laboratory. For many years she was in the laboratory daily, helping to plan the projects, supervising most of the test construction, and participating especially in the psychological interpretation of results. In 1948 she left this work to become director of the Division of Child Study in the Chicago Public Schools. This report should really have been written as a biography for both of us.