THE DEVELOPMENT OF A RESEARCH PROGRAM IN MENTAL DEFICIENCY OVER A FIFTEEN-YEAR PERIOD

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The Wayne County Training School opened its doors in 1926. Those responsible for its creation had set as its purpose increase in salvage among the higher grade mentally deficient. Institutional treatment and care of the mentally deficient in America had seventy-five years' experience back of its ideas and methods. If the purpose of the establishment of this new institutional venture was to be attained, research for possible better ways became a conditio sine qua non.

Research, in whatever field, is a call for the future. It cannot be undertaken otherwise than with a feeling of discontent with the achievements of the past and the hope for a possible improvement in the time to come. There are, however, limitations in research especially under institutional conditions: limitations with regard to the worthwhileness of the research project in respect to time and money required; limitations as to the immediate need; and limitations as to the possibility of practical applicability of the theoretical results. It seemed worthwhile, today, after fifteen years to review and evaluate the results of such a research program, originated admittedly with a selfish purpose solely to help the administration of the institution in the search for a new program, but which has sought none-the-less never to forget the major objectives of science.

Where did we stand in our scientific knowledge on the problems of moron and borderline defectives when the Training School opened in 1926?

Our knowledge on mental deficiency is derived mainly from four different disciplines. They are:

1. Social science, including administration.
2. Psychology including psychopathology.
3. Education, especially special education.
4. Medicine, especially neuropsychiatry.

Let us consider sketchily each field separately and discuss the main problems involved.

The original purpose behind the creation of public institutions for the mentally deficient was ameliorative. At the risk of delaying progress in our theme, I would urge you to read John Greenleaf Whittier's essay in 1848 on Peculiar Institutions of Massachusetts, where his deep religious soul discusses Dr. Samuel G. Howe's first mental hygiene survey on the problem of feeblemindedness in the Commonwealth. Practical forces, some might say natural, have determined, perhaps too largely, an asylum type of development in this field. It is within our memory that parole was formally recognized in law. And yet, even today, within the classic institution no particular differentiation of program for the higher grade deficient has been evolved to prepare him the better for his attempted return to the community.

If we were to increase salvage among the higher grade deficient, where the community objectionable characteristics of this type of individual are largely his threatening aggressive behavior, the program could not be satisfied with attempting merely to impart more academic knowledge or increase vocational skills but must be more vitally concerned with helping these individuals in their interpersonal relations, in their development of different emotional attitudes and in their creation of different mores.

Various experiments in the organization of our community life were made in many directions from the very opening of the training school. It may be sad, but it is none-the-less true, that negative aspects seem the easiest patterns for institutions to adopt. Emphasis is much more easily laid upon what these children cannot do and why they shouldn't be permitted to try to do anything outside the narrowest possible circle of pre-
scribed activities, particularly if the doing is the least bit noisy or otherwise disturbs an adult or an adult's viewpoint, rather than focusing on what they might do.

The attempt to develop a democratically self-determining cottage among the boys was made on several occasions, from the second year of operation on, but without continued success. The reasons were various but mainly related to the fact that while self-government was existent, it was existent only up to a point but not beyond that point; it was a form but not a whole substance. In 1935 we established in an area more than half a mile removed from the group of other cottages the Homestead Cottage. Whatever the principles of mental hygiene had taught us should be expected from an experiment in child living, but which we had not achieved in our earlier trials, has been obtained at this self-determining cottage.

The so-called Iowa studies were unknown to us at the time the Homestead Cottage was projected and started. Our gain in administrative relations with this cottage had been enough: so-called disciplinary problems with our older boys had been greatly lessened; the boys were happier; our employees in the other cottages were furnished a laboratory course of instruction constantly at their disposal; actually fewer employees are required in these two cottages (a second cottage had been established a year later). But actually, in addition to social growth, significant acceleration of intellectual growth became even more significant as later certain specifically designed educational ventures were introduced into the cottage program. Kephart and Ainsworth have reported on these studies.

Still in the realm of the social science discipline, we have made carefully documented studies of all boys and girls discharged from the training school up to a date three years before the beginning of the study so that conclusions could be reached of their social acceptability after at least three years on their own in the community. Forty-three per cent of all boys from the training school, who had been returned to the community with our approval prior to July 1, 1933 were found in a very carefully conducted personal survey made during the winter of 1937-38 to be totally self-supporting—even in these depth-of-depression days. Nineteen per cent of this entire number, in addition to supporting themselves, were also aiding in the support of others. An additional 29 per cent were classified as partially self-supporting. Kephart and Ainsworth and Bijou and Ainsworth have reported on these studies.

We are now engaged in testing out a manual on cottage operation written as a teaching device to facilitate the interpretation to cottage workers of the reasons for certain attitudes and methods based upon these investigations: this manual was prepared by Melcher-Patterson and McCandless.

What was the challenge in 1926 from the contribution of psychology to the field of mental deficiency?

Binet's revolutionary attempt to "measure" intelligence had been translated to America by Goddard and further developed by him to fit American conditions. Stern's formula of the intelligence quotient had been adopted to make possible an apparently exact measurement of an individual's mental inventory. The 1916 revision of the Binet scale by Terman had made possible the use of "the test" by any school teacher who could afford to buy the book! For the keen observer, however, a rigidity of thinking about retardation in mental development became more and more apparent.

Was the I. Q. something to be accepted as constant and invariable? Was the currently accepted deterministic viewpoint of low I. Q. as settling all future personal problems of the child so tested to remain unchallenged? Any deviation from that viewpoint was sacrilegious in most educational circles at that time. And yet, if our efforts in the rehabilitation of the mental defective should be sustained in his readjustment to community life, it was necessary to demonstrate that this then general belief was a misconception.

We mention two very significant studies by Hoakley: the first in 1932 entitled "Variability of Intelligence Quotients" based on a study of the results of 1469 Stanford Binet examinations made on 550 different children at intervals varying from six months
to twelve years. This study, important as it is in many respects, is most important because of the inescapable evidence it presents that not even sixteen years is the upper age limit for intellectual growth. The other study, published in 1935 and in all respects a continuation of the same fundamental problem, was a "Comparison of the Heinis Personal Constant and the Intelligence Quotient." One of her conclusions from this study, that the P.C. is a more constant and more representative index than the I.Q., would, if adopted generally, serve to answer many of the questions so perplexing where such laboratory terms have to be used in courts and similar situations before laymen where intellectual constants and such "tom-foolery" details are just as much out of place as the details of Wassermann procedure would be.

More than just variability in I.Q. could be shown. Was the institutionalized mental defective condemned to "lower his I.Q.," as had been stated so often, or could proper and adequate training in an institution provide the necessary stimuli for a fuller development of such a child's limited capacities? The studies by Kephart, just mentioned, and several others gave ample proof that, especially in the familiar type of mental defectives, the success of such efforts could even be measured by a "heightened" I.Q.

Yearly examination of all children with a broad battery of standardized intelligence tests, non-verbal as well as verbal and collateral educational achievement tests, combined with psychiatric observation focused upon personality reactions and psychogenetic origins, served only to mordant our impressions that knowing more about differences in manner of learning of our children was more important for our purpose than any other question confronting us and that until we could clarify this seemingly perpetual maze of qualitative vs. quantitative differences in our children's mental activities, we were to continue befogged. If we were to make even a step forward in the assignment given us, to increase salvage, research in education was inescapable.

In 1926, one hundred twenty-five years had passed since Itard's grandiose attack upon the educational possibilities, even of an idiot child, had shaken the pedagogical world. His attempt in spite of its failure—nowadays we may say he did not have the facilities to diagnose this child as severely brain-injured—awakened the educators to explore a hitherto forgotten field. Seguin's efforts contributed much to popularize special education in the United States. But the field remained quite sterile until, at the beginning of this century, Montessori in Italy and Decroly in Belgium, both psychiatrists, opened new avenues of educational approach. Unfortunately both seemed dissatisfied with their success in the field of mental deficiency and turned back to work with normal children. The Montessori method of "sense training" is still in use for the low-grade child; the Decroly method never became popular in the United States because we developed independently the "activity program" and "progressive education." For all purposes neither method seemed to offer promise: the one was too specific and the other too general in its scope.

In 1926 Orton's publications from Iowa on strephosymbolia were attracting considerable attention. They seemed to offer a starting point to learn more about these undoubtedly differences in manner of learning.

For several years reading disability among our children was the point of attack. The results were fruitful. We discovered that cases of reading disability identical with reading disability of normals existed among our children: that they existed among them in about the same proportion as found among normal children, about 11 per cent; that these disabilities could be overcome; that methods of treating the disabilities of normal children were not applicable to our children; indeed, that the very method perhaps, used today to teach normal children to read might be a prominent factor in producing the disability; that specific methods of treatment could be devised to teach our children whereby spectacular results followed; that children six, seven and eight years in school without learning to accomplish even first grade reading could be taught to read up to their mental age level at a rate two to three times as fast as the normal child learns to read: that a method of prevention was probably possible.

This field of investigation was very fruit-
ful. It stimulated the imagination of many persons important to the support of the total project who had been perhaps lukewarm to the possibility of anything but a relatively barren future. The results of remedial teaching could be measured and described in terms that anyone, not merely an educator or a psychologist, could understand.

An impressive series of publications of original investigations by Hegge and his associates resulted from this area of disabilities. The most important book yet written on the teaching of reading to this type of handicapped child grew out of these studies—the book by Kirk.

We would call particular attention to the transformation that takes place in the rebellious, anti-socially-headed non-reader child, twelve, thirteen, or fourteen years of age, when he comes to recognize that at last he is going to learn to read.

Perhaps the most important conclusion from our researches in this field was the conviction that reading disability, so far as the higher grade defective was concerned, was largely the result of the attempt to teach the child to read before he was prepared for the experience.

Upon the theory that the most vital educational need for the younger children entering the training school is not more academic drill, but a longer preparation period in which to acquire more of the foundation of unfold experience with which normal children just naturally start their academic training, we began a few years ago to create an environment which shall meet the younger child’s needs and stimulate him to mental activity in accordance with the principles which have been so successfully demonstrated in pre-schools for very young (normal) children and perhaps kindergartens.

This program has been under the continuing direction of Ruth T. Melcher-Patterson. Its results in terms of effect upon intelligence, social maturity and personality adjustment, also later academic achievement, have been fully reported in detail in the literature as well as the details of the organization of the unit and its methods.

You may ask why I have not discussed earlier the contributions and challenges we received from the medical sciences. Isn’t Tredgold who wrote the first and still standard book on mental deficiency in English, a physician? Is not his classification of primary and secondary amniation still in use? Is not his concept of social competence of the feebleminded the most widely used criterion in the circles of lawyers and laymen? No one would, or could, minimize the contributions of Tredgold, or of Fernald in the United States. However, for our special problem—the rehabilitation of the high grade mentally defective—the usual fields of brain pathology, biochemistry, genetics and clinical syndromology offered no prospects. Child psychiatry outside the child guidance movement, which has always had thumbs down on the mental defective, was little known. Homberger’s standard work had just been published in Germany. Kanner’s outstanding treatise was to follow only many years later.

There should be no wonder that our first studies in the medical field were medical researches pure and simple and not in any sense research in the field of mental deficiency as such.

H. S. Willis and associates reported on sensitization from repeated injection of the new PTP tuberculcin, a hitherto unobserved phenomenon turned up in the course of studying the incidence of tuberculosis in our population.

Ferry and associates, among a long series of studies in immunology, worked out important developments in the treatment of meningococcal meningitis which were to reduce the mortality of that disease another 50 per cent below the point where Flexner had left it.

Huddelson, and a group of research associates of the Rockefeller Foundation spent several days a week for two years studying the possible presence of brucellosis in our population.

Bunting directed over a period of several years a rather elaborate study on dental caries.

Lewis studied the urine of almost a thousand children with particular interest respecting the presence of phenylpyruvic acid.

Watson and Moehlig investigated the growth pattern in a group of children showing extreme retardation in physical growth.
as well as endocrine anomalies and then the results of administration of specific growth hormone. These studies are still in progress under Watson and Strauss.  

None of these studies touched our essential problem.

The very earliest impressions we gathered of our children were those of extreme heterogeneity. In a group of mental defectives from which all idiots and imbeciles were excluded, to use the descriptive expression of heterogeneity may seem strange but to the clinical psychiatrist no other impression would be possible.

With respect to this early perplexity over the very question of qualitative vs. quantitative differences in the learning processes, notwithstanding the tremendous advances in our knowledge of our children from the extensive research I have just sketched so briefly, we still continued, in 1937, perplexed.

The National Research Council through its Committee on Psychiatric Investigation had published in 1934(5) a report on the Problem of Mental Disorder. Feeblemindedness is barely mentioned anywhere in all that exhaustive report of practically four hundred printed pages. Meyerson(6) can be said to have been the only contributor even to mention the subject and I quote from his contribution to “Current Points of View”(7):

... as a matter of fact, there is no such unity as is implied in the term feeblemindedness.

There are, as a matter of fact, groups of individuals who present as part of the syndrome by which they are differentiated from the normal the symptom of lowered intelligence. Thus the cretin, the Mongolian imbecile, the feebleminded with organic brain-disease, brain-injury, postencephalitis, etc., represent groups not at all biologically related to one another, except in the appearance of mental defect.

The quality of the research done in feeblemindedness is on the whole inferior because it has been dominated by the misleading concept that feeblemindedness is some kind of biological unit. Here and there work has been done on the anatomy of the brain of the feebleminded. Very creditable studies have been done on the cretins because this subject linked itself up with the definite subject matter of endocrinology. A few radiographic studies have appeared. Sporadic biochemical papers appear in the literature. Most of the work on feeblemindedness appears in psychological studies, which really only measure the quantitative effect but give us no hint as to causation. Surveys galore have been made at great expense largely to bolster up preconceived ideas. It is a striking commentary on the schools for the feebleminded that very few of them have clinical directors, and one can count on the fingers of one hand the places where pathology, biochemistry and physiology are used in the studies of the hypophrenias. This great problem needs clinical investigation in all its forms. The psychological and sociological studies should go on; but the basic investigations into the nature of the individual who is feebleminded can hardly be said to have started.

In 1937, through the support of the McGregor Fund, research at the training school was greatly expanded by the addition to the research staff of Heinz Werner, experimental psychologist, and Alfred A. Strauss, child psychiatrist. From the collaborative endeavors of these two men and their associates, has come much to answer this persistently perplexing problem of qualitative vs. quantitative differences.

Typology had not hitherto been considered a matter of very great significance beyond the question of expressing etiology. Even Meyerson didn’t suggest any importance attaching to the subject beyond differential diagnosis of origin.

Strauss, a pupil of Goldstein, had started in 1930(8, 9) to apply the findings of brain-neurology in the field of mental deficiency. After years of study in developmental neurology he has succeeded in establishing clinical criteria for differentiation between the brain-injured, exogenous, and the familial, endogenous, type of feeblemindedness.  

By evaluating isolated reflex disturbances in brain-injured children, usually overlooked or pushed aside as not significant, he has crystallized an organic behavior syndrome diagnostic even in children where these residual isolated signs were absent.

In collaboration with Werner(27-40) who contributed his vast experience in genetic and experimental psychology, the foundation was laid for a psychopathological analysis of perceptual and conceptual differences between the exogenous and the endogenous type of mental deficiency. Those differences in fundamental aspects of behavior, now confirmed by objective laboratory demonstration, give new and vastly different significance to the old problem of type of defectiveness. The results of Strauss and Werner’s investigations are ample proof of
the usefulness of Gelb and Goldstein's\(^{(10)}\) pioneering approach relative to the psychopathology of brain-injured adults on the basis of Gestalt-psychology.

Several of these researches have been presented before this section: in 1939, "Behavior Differences in Mentally Retarded Children Measured by a new Behavior Rating Scale";\(^{(28)}\) in 1940, "The Mental Organization of the Brain-Injured Mentally Defective Child";\(^{(82)}\) in 1942, "Comparative Psychopathology of the Brain-Injured Child and the Traumatic Brain-Injured Adult."\(^{(86)}\)

Others have been presented before the American Association on Mental Deficiency and psychological groups. We are convinced that these researches meet Meyerson's\(^{(11)}\) challenge that "basic investigations into the nature of the individual who is feebleminded can hardly be said to have started." The appearance in the current literature of the terms exogenous and endogenous in the sense developed in these studies, shows how quickly the usefulness of these differentiations has been recognized by workers in the field.

To touch one important implication of this new classification we must turn back to the field of education. The classical system of instruction for mentally defective children is based overpoweringly upon the quantitative concept of mental deficiency. Hollingworth\(^{(12)}\) has stated that "feebleminded children differ from ordinary children only in amount of ability, not in the kind of abilities, they possess. No mysterious or unique matter or method is necessarily required in the task of training them. They can learn the same things that other children learn up to the limits of their capacity." Some variety of that formulation governs most educational thinking and practice in this general field even today.

Statistical studies\(^{(24,26)}\) in our own school showed that the endogenous, or familial, defective child tended to advance educationally through the school opportunity given him while the brain-injured, or exogenous, defective child tended to lag behind: i.e., the quantitatively defective profited, the qualitatively different child, so far as brain function is concerned, lagged behind.

There was a challenge here that could not be avoided. The results of the applications of methods that Strauss\(^{(13)}\) had developed in the education of individual cases earlier as adapted to class situations with brain-injured defective children is just now reaching publication after three years' developmental experimentation.\(^{(71-73)}\)

In the field of special education of the feebleminded in general, Werner,\(^{(13-18)}\) the experimental psychologist, brought his techniques to bear for the first time and gave us the opportunity to see what functional analysis of psychological processes might contribute to the understanding of differences in learning of mine-run feebleminded children.

I quote from a letter which Allport, professor of psychology at Harvard University wrote me:

> We all know that the empirical, rough-and-ready devices employed by mental testers have a limited value in disclosing the true nature of mental deficiency. Indeed, these tests are now known to mask important functional disorders that presumably underlie mental handicap. Through their fresh attack upon the subject and through their discovery of some basic patterns of mental handicap, I believe that Werner and Strauss are making significant discoveries that promise eventually to improve our understanding and therefore our methods of treating the different kinds of mental retardation.

I have tried to present to you the high lights of the development of a research program in mental deficiency over a fifteen year period under the conditions as outlined in the beginning. We are well aware of many gaps in its texture: of many existing problems we have not touched. Many ventures I have not even mentioned: excursions into drug and allied therapy,\(^{(66-68)}\) speech disorders,\(^{(60-64)}\) electroencephalographic studies now under way and some others.

The most important problem of all—which will become still more significant in the future—concerns studies on personality and group relationship. We have utilized, for example, the measurement of interpersonal relationships in groups by the Moreno technique:\(^{6}\) we have studied group formation under democratic and autocratic leadership according to Lewin's hypotheses;\(^{4}\) a study on the personality make-up of the brain-injured child\(^{(77)}\) is to be published soon. These results all enter into administrative usefulness. The most important problem of
the personality of the high grade defective, his reactions and his constitutional deviations, is far from being solved. We see in Sheldon's new books(14, 15) on constitutional psychology one possible approach to it.

Whatever the future will bring we do not know. We have followed and will follow in our research efforts principles which have been so excellently expressed on a similar theme "The World We Want," in the Christian Science Monitor recently, by the great Spanish philosopher and politician, Madariaga(16). He says: to see the way clear, we have:

(1) To define what is desirable,
(2) To define what is possible at any time within the scheme of what is desirable,
(3) To carry out what is possible in the spirit of what is desirable.

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